HAY-FEVER A KEY TO THE ALLERGIC DISORDERS

JOHN FREEMAN
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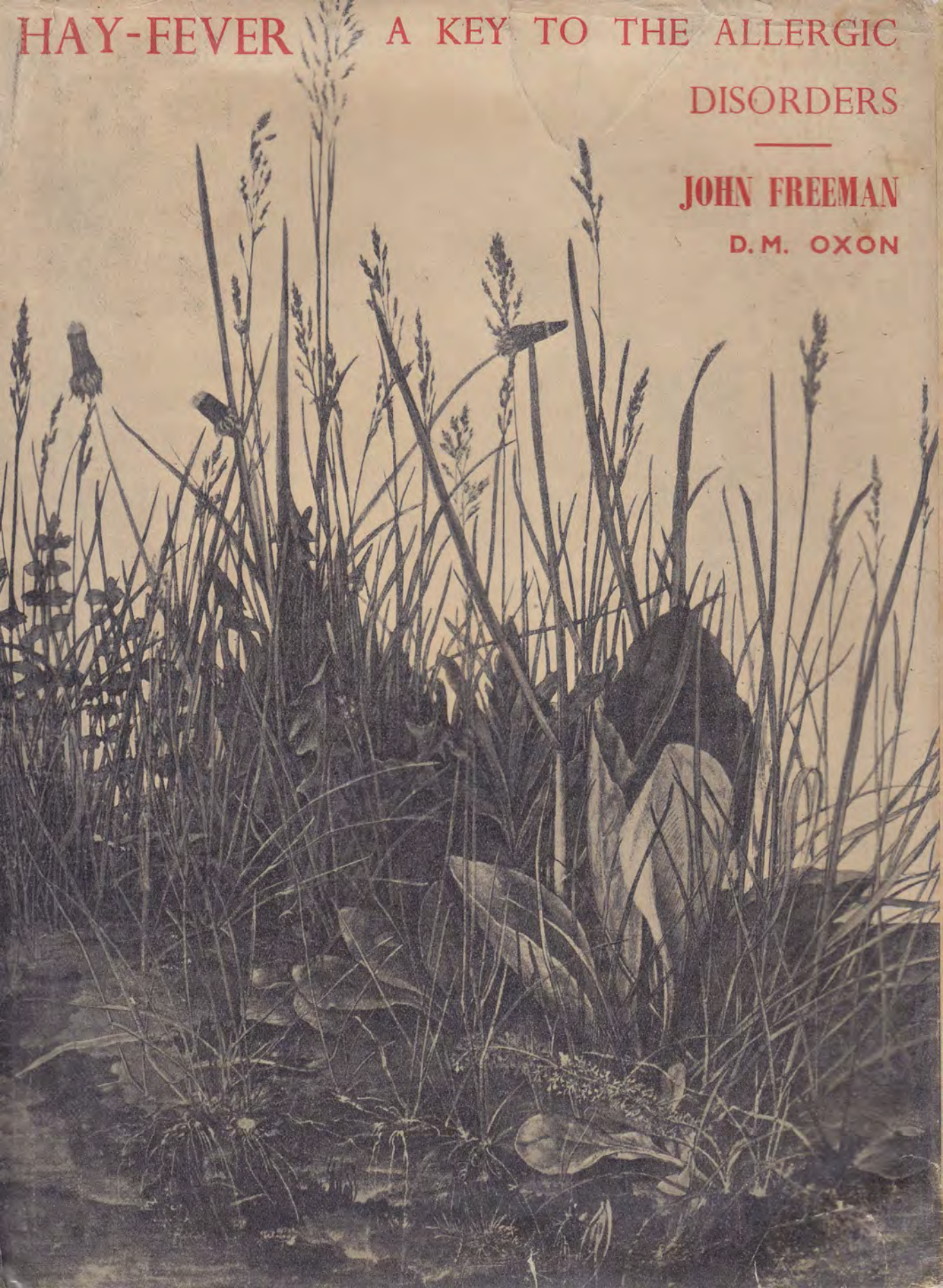

HAY-FEVER

LEONARD NOON

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A KEY TO THE ALLERGIC DISORDERS

by
JOHN FREEMAN D.M. (Oxon)

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To

L. N.

THIS ACCOUNT OF MY STEWARDSHIP

On ne peut se flatter d'avoir le dernier mot d'une théorie, tant qu'on ne peut pas l'expliquer en peu de paroles à un passant dans la rue.

J. D. GERGONNE (1771–1859)

FOREWORD

Our old Chief, Sir Almroth Wright, used to say to me across the Inoculation Department tea table, ' Before your ship of life founders be sure you get all the goods up out of the hold and onto the quay-side.' Alas, he did not manage to do that himself; he died one and a half years ago, at the age of eighty-six, leaving the philosophy book which he had been writing for fifty years in such a chaotic state that even now his grandson is having great difficulty in getting it out. Wright used to say, ' You must be warned by me, J. F. : you finish your book *somehow*! '

This book from first to last has taken a good twenty-five years to write, and it is certainly none the better for that. At moments of brief leisure much of it has been re-written a dozen times over in the amateur way, till I hardly know what I have left in or left out.

I hereby make apologies to my wife, children and (later on) grandchildren, and to so many kind friends for countless holidays devastated by what quickly got known to them as ' The B.B.', i.e. that Bloody Book. They have all been very patient and helpful—when I could let them help.

Leonard Noon. In 1908 Noon, my lifelong friend, began considering investigations into the cause and treatment of hay-fever and of the Isle of Wight Bee-disease; in the communal spirit of those days these projects (together with dozens of others) were thoroughly canvassed by all workers in Almroth Wright's laboratories.

As for the bees, the favourite flippant suggestion was for immunisation by massage, a general line of treatment I was working on at that time (pp. 230–232). That Bee investigation got no further; but headway was made with hay-fever.

Blackley had demonstrated many years previously the specific reaction which all hay-fever subjects gave to the pollen of the grasses when this substance was brought into effective contact with their living tissues. Blackley's great monograph on ' Catarrhus Aestivus ', published in 1873, was Noon's spring-board for research.

Noon, after a protracted illness, died of phthisis in 1913. Perhaps the greatest disaster that befell the old Inoculation Department was this untimely death at thirty-five years of age. From the research worker's point of view, his was probably the best brain that ever entered our famous laboratory.

I was closely associated with Noon in his early experiments; and, when alas he fell ill, the work was entirely in my hands. This book is an account of my stewardship.

I have not tried to write an exhaustive text book on allergy by making a re-hash of all the contradictory statements that could be found written in the ' literature ' of that complicated subject (hence the absence of a bibliography); on the contrary, I have kept fairly closely to what has actually been done by myself

and colleagues in the old Inoculation Department as we developed more and more from Noon's pregnant beginning with hay-fever.

Wright's Inoculation Department. It is only just that I should say something of the novel Organisation (though unkind colleagues sometimes called it a Disorganisation) which not only permitted, but even encouraged, first Noon and then me to do the work which is described in this book.

Wright, warned by his recent, almost insuperable. difficulties in persuading the Army Medical Chiefs to adopt his new prophylactic treatment for Typhoid Fever, saw that the necessary close collaboration between clinic and laboratory, when adopting new methods, would hardly have a fair chance if left to the hospital set-up of his day. He did not rest content that his research laboratory should be unhampered by clinical orthodoxy and by economic pressure ; his (I think) unique plan was that treatment derived from his laboratories should be applied to the sick by *himself*, and (under his careful guidance) by his young co-workers too : his new Inoculation Department was in fact, though not in name, a minute but completely independent Research Hospital living in symbiosis with the main Hospital. This ' Inoculation Department ' owed its freedom to its financial independence ; for forty and more years it was kept on its financial legs by the money-earning work of its inmates—generously supplemented by rich gifts from several of our well-to-do private patients.

Laboratories and Clinics. I should indeed have been culpably dull if I hadn't learned a great deal when working in a well equipped and live laboratory, and also with many thousands of patients—first with hay-fever and later with asthma or some other of the Toxic Idiopathies—flowing through the clinics which, little by little, we found it necessary to set up in conjunction.

The success of these clinics has been my bane, for they have attracted too many people, all needing help ; now that I am busy coiling down ropes to leave everything shipshape at the hospital, we have each week eight or more ' Clinics for the Allergic Disorders ', and places in them are booked up for months ahead, through thousands of practitioners all over the country.

Unfortunately such work is costly in time and trouble ; we calculate, for example, that every case that comes to us now for ' examination and report ' uses up, all told, a full three man-hours of work—some skilled and some of course comparatively unskilled—before that report can be posted to the patient's doctor.

Our clinics were started without any with-your-leave or by-your-leave because they were found necessary for our research and, though they have developed far beyond our needs, and perhaps beyond our resources, I claim that they have always ministered to such research. Similarly our laboratory work has always had a practical application in treating the sick who crowded to us. At the present stage of development I hold that laboratories and clinics are symbiotic : they are mutually necessary if the work described in this book is to go on.

Nomenclature. I have used in this book the language which I have for so long used to myself and to colleagues—including the practitioners who have sent me

cases for ' examination and report '. This has included some neologisms which I hope may be forgiven ; they were coined to meet what I thought was a pressing need in clear thinking.

I have already in this Foreword used the term ' Toxic Idiopathies ', which will be defined on its introduction on p. 21. The Toxic Idiopathies are identical with these ' Allergic Disorders ' of my book title, which is to this extent catch-penny, that you can hardly put an unfamiliar and perhaps forbidding neologism into a book title and expect anyone to read that book.

All the same, I am not proud of using the phrase ' Allergic Disorders ', even when decorated with inverted commas, because the word allergy is in such a linguistic mess. When it first began to creep into English laboratory conversation in its new and twisted meaning, I determined I wouldn't use it in this book because of its obvious ambiguity ; then, later on, this second meaning became the more general usage in clinical circles, and it seemed Canute-like to object to its use flooding in on us in the laboratory.

However, a year ago at a meeting in Oxford to discuss the proposed ' Association of British Allergists ', I heard enthusiastic but undiscriminating medical men boasting that ' allergy covered nearly all medicine ' ; so I started to weed the word allergy out again as too dangerous for general use. I have, for example, replaced the equivocal word ' allergic ' by ' idiotoxic ' which, though not so euphonius, can only mean what I mean it to mean. I believe the word ' allergy ' obscures more than it illumines in medicine.

' The Inoculation Department ' of St. Mary's Hospital is another of the phrases which may need excusing. That was its name from its cradle in what is now the Haematology Laboratory of the Hospital, through its lusty youth in the Clarence Wing, till its reincarnation after Wright's death as ' The Wright-Fleming Institute of Microbiology ' in the new Medical School buildings. Nearly all the book was written before our research laboratories were re-christened with that new name, so I haven't made the correction. I hope I may be forgiven for that too.

Medical Orthodoxy. When should we bow in the House of Rimmon, and when should we refuse to do so? Just at what point should we risk a bite from a sleeping dog by trying to get it out of our way? I don't know the answers of course ; I can only say what I have done that is at present heterodox in the practice of medicine.

I don't as a rule examine the chests of my asthma patients (unless they ask for it) because I believe that the average medical man can get singularly little accurate information from this procedure—and practically nothing useful from the asthmatic.

This belief in the stethoscope and in palpation which is still taught so sedulously to all students is, I fear, another example of the Emperor's Cloak phenomenon : each medical man—consciously or unconsciously—hesitates to say that he in particular can't hear or feel all that he thinks he should from the appearance and overt symptoms of the patient. If the reader is shocked at my medical blasphemy, why, I have my evidence.

I don't make a routine practice of getting sciagrams taken of asthmatic chests ; although I have examined very many of such photographs, and of course have read

carefully the reports of the experts thereon, I can call to mind few instances of any useful information gained thereby : I regard routine sciagramming as often an expensive piece of window-dressing. As for the coincidence of phthisis and asthma, this occurs so seldom that I have come to regard an idiotoxic diathesis as rather contra-indicating a T.B. infection ; at any rate, such coincidences seldom come my way.

I don't ' sterilise ' the skin before a puncture of it, but that is fully dealt with in Chapter VI.

Chapter VI is almost entirely concerned with teaching patients to inoculate themselves. I have been hauled over the coals for that too ; but I am unrepentant, and in fact try to insist on it for reasons given.

Ignorances. Wright used to say, ' When you are turning over the goods in your basket for inspection, if you can bring yourself to say in addition that something is missing which should be there, you will take high marks in Heaven.' Occasionally I have tried to live up to that in this book.

References. A reader will not find many. Where I have worried points out for myself I have not as a rule hunted to find out who, if anyone, was the first person to put that into print. Where I have consciously got an idea from someone else I have made acknowledgment ; where I have failed to do that it is not a claim to ' literary priority ' ; it is just ignorance.

Safety. I have not played for my own professional safety in stating my experiences and views, and if that seems egotistical I am sorry but am unrepentant. Unless a writer gives a strong lead as to *his* own views he shouldn't be writing a book at all.

Ego. While I am about it, let me apologise for the constant use of the first personal pronoun. For a year or two it was ' We '—Noon and I ; then for nearly a generation it was chiefly ' I ' ; latterly and increasingly it has been ' We ' again, and I should be deficient indeed in respect and gratitude for the very gallant help afforded me by colleagues both in times of crisis and in the humdrum day-to-day labour, if I didn't feel and acknowledge that debt. Let me mention especially work by Dorothy Noon, Ronald Hare, A. B. Porteus, D. Harley, H. Hughes, N. R. Pooler, A. W. Frankland, D. Harkins, Lady Broadbent, Mary Wright and Elizabeth Budd—of these I am especially grateful to Harley and Hughes who came to my rescue when I had, in the middle of the last war, my one and only breakdown in a long life.

My thanks are also due to my friend Rowan Davies and my son Tristram Freeman ; between them in the past three years they have read and then destroyed whole mounds of unwanted alternative versions, false starts, unfinished paragraphs, re-written stories, etc. Latterly they have been invaluable in proof readings and in finding cross-references. Of these, by the way, I find I have made many too many : I'm sorry for that, and hope it won't unduly annoy any reader. They can always be skipped.

The Future. Can work such as I here describe, with its combination of laboratory and clinical research under one's own control, go on? It could hardly have started but for Almroth Wright's heretical leadership.

Wright grew old and is now dead: his views of the full functions of the Pathologist are being lost sight of, or so it seems to me. Unfortunately, too, pathologists in general are diffident. They cherish two misconceptions. The first is that any intimate knowledge of a science of medicine must mysteriously invalidate their clinical qualifications. Quite recently a spokesman for London University, and himself a noted pathologist, when asked if in his view pathologists should have control of cases, replied with emphasis—'Most unsuitable, *most* unsuitable!'

Their second misconception is that they believe their clinical colleagues to be as infallible in their powers of diagnosis as the pathologists know themselves to be fallible; this delusion persists in spite of many sad disclosures at post-mortems. Naturally their clinical colleagues take care not to disillusion the poor fellows. An amusing friend of mine, a man who ended his brilliant medical career as senior physician to the Hospital, said to me one day—'Surely, my dear Freeman, pathology is the most degraded and degrading branch of our profession'. Partly a joke no doubt, but not entirely.

Can the conditions Wright won for us ever be repeated? The nearer I come to the launching of this book, the more I doubt if such unhampered and undirected researches as I here describe can find a place in a planned, and therefore stereotyped, society. Is there any place for an unconventional Almroth Wright in the Brave New World?

No doubt all old men have these misgivings, but ——

CONTENTS

ILLUSTRATIONS

CASE HISTORIES AND ANECDOTES

CHAPTER I

HAY-FEVER ITSELF

What do we mean in this book by ' hay-fever ' ? It is necessary to settle that, and then to use the term with exactly the same meaning throughout, or we shall never quite know what we are talking about.

Like other diseases, hay-fever is recognised in two ways : (*a*) by the complex of clinical symptoms which are worrying the patient, or (*b*) by using as our criterion some feature, and if possible some unique feature, in the causal mechanism of our disease.

For example, tuberculosis is either (*a*) a wasting disease with a hectic temperature, etc., etc., or (*b*) the resultant between an infection by the bacillus of Koch and the defences of the infected person.

The symptom complex method of recognition is nearly always historically the first to be adopted because it is all that can be managed till something of the causal mechanism is understood ; again, classification by symptoms is all that is necessary if only symptomatic treatment is to be attempted. This method breaks down, however, as soon as treatment becomes radical.

(*a*) **The clinical** picture of hay-fever would be something like the following :

A conjunctivitis-cum-rhinorrhoea—among other things ; often coming on in ' attacks ' and going off in the same way ; occurring only between mid-May and mid-July—at any rate in the South of England ; suffered only by a selected few of the population, and these few distributed among the population by heredity.

Unfortunately other disorders might be, and often are, included by this somewhat indefinite definition ; therefore, standing alone, the clinical classification is ambiguous and dangerous.

(*b*) **The causal** classification is more reliable, and fortunately there is in the causation of hay-fever an unique criterion to guide us—the idiotoxic response to grass pollen. Using this criterion, hay-fever would be defined aetiologically somewhat as follows :

Hay-fever is the clinical resultant of the interaction between, on the one hand the pollen of the grasses, and on the other hand the living tissues of those people who are sensitive to it.

Or, shall we say more colloquially :

Hay-fever is what happens to people who are sensitive to grass pollen when the pollen meets their living tissues.

These two methods of classification, the clinical and the causal, do not quite cover the same ground ; as I have said, the clinical method often includes cases not

caused by grass pollen, while the pollen-cause criterion sometimes picks out cases which might not be called hay-fever by the clinician because they do not show the clinical signs of classical hay-fever. Later on, I shall be calling these cases Cryptic Hay-fever.

In ordinary practice, of course, we rely on a combination of the two methods : first, we suspect that the trouble may be hay-fever, or the patient does, because appearances suggest it. To test the aetiological definition given above we observe the response to intimate contact with grass pollen—whether the natural accidental contact at midsummer or by laboratory experiment. It is the experiment with grass pollen which must settle the business : this is a necessary stipulation if we are to apply successfully the Prophylactic Thoroughgoing Desensitisation (P.T.D.) for treatment as described in Chapter V.

Verbal Ambiguities. Here I must stop to point out that the literature of hay-fever has often got into a serious muddle concerning what is hay-fever and what is not. It is so important to clear up the ambiguity that it is worth a page or two to get our words clearly defined.

Lest I should be thought to exaggerate the difficulties, let us turn to the big *Oxford English Dictionary*, remembering that this can only reflect what reputable people have put into print.

The *O.E.D.* says that hay-fever is a disease of early summer ; that is true enough if we are confining our attention to the disease in England, but it is not true if we are considering the hay-fever of tropical countries where it is the rains rather than the summer which control the growth and flowering of the grasses. *O.E.D.* gets into deeper water still when it allows that hay-fever can be caused by the scurf from the skin of horses ; horse-scurf is with us all the year round wherever there are horses, therefore its pathological action would certainly not be confined to the early summer.

As we shall see later, we needn't stop at horses. Cats, dogs, cattle, pigs, goats —with a little industry one could collect dozens of animal substances which (with the right people) can produce symptoms resembling those of hay-fever (see pp. 47 and 280). And we need not confine ourselves to animal scurfs : sawdust from this or that wood, germinal spores from one of a hundred different moulds, flour-dust in a granary or a bakery, women's face-powder—in short, any animal or vegetable particulate matter suspended in the atmosphere can on occasion, and in the correspondingly sensitive person, produce symptoms resembling those of hay-fever—if it is merely clinical resemblance that we are looking for.

Para-Hayfevers. Let us admit at once that there are large numbers of disorders which owe their symptoms to a causal mechanism which is very similar to that of hay-fever, but yet these have nothing whatever to do with grass pollen. The tendency of writers to call these disorders ' hay-fever ' discloses to us the muddle-making deficiency of the terms in use. Clearly, if our main treatment for hay-fever is to be by the manipulation of a grass pollen sensitiveness, we must not call all the other conditions ' hay-fever ' or we head straight for trouble.

We can only clear up the verbal muddle by having a self-explanatory name to

over all these other clinical conditions where the symptoms and causal mechanism are very similar to those of hay-fever, although grass pollen plays no part. Years ago I borrowed from the jargon of the bacteriologist, and called the other conditions the ' para-hayfevers '. First I would propose to include under that term the troubles which are caused by pollens other than the grass pollen. Thus the comparatively slight trouble we get here in England from the pollen of the autumn composite flowers (chiefly the Michaelmas Daisies and other asters) I would call a para-hayfever ; and we can particularise this as being an aster para-hayfever if we wish.

Similarly, the more severe disease of the North American fall—much worse than hay-fever itself we are told—which derives from the pollen of the Ambrosiae and other wind-pollinated composites of that hemisphere, I would call a para-hayfever from Ambrosia pollen ; though perhaps such a devastating disease as this is reported to be deserves to be called Ambrosia-fever in its own right. Anyway, it would be one of our para-hayfevers.

Then we should go on to speak of horse-scurf para-hayfever, and so on ; but *para*-hayfevers let them all be, not *hay*-fevers.

Idiotoxic Rhinorrhoeas. Writers, especially ear-nose-and-throat surgeons, often make use of the one term ' rhinorrhoea ' when they wish to avoid implying a necessary grass-pollen causation ; this is not very good because rhinorrhoea is a concomitant of so many conditions—e.g. a cold in the head, or the after effects of taking snuff. The surgeons would probably wish to add the epithet ' allergic ' to denote the nature of the rhinorrhoea ; I should perhaps prefer ' idiotoxic ' as I have headed this paragraph, but allergic rhinorrhoea should be readily understood nowadays.

If we wish to have one phrase to include hay-fever and all the para-hayfevers we might say a rhinorrhoea-cum-conjunctivitis caused by airborne idiotoxin ; but as that is a large mouthful let us leave it as an allergic, or idiotoxic, rhinorrhoea.

'Hay-' and '-Fever'. One consideration more, perhaps, we may give to the name hay-fever before describing the disease itself.

The prefix ' hay- ' indicates sufficiently clearly not only that our disease has to do with grass, but also indicates roughly the time of grass pollination as being essential. But why should we tolerate the incorrect ' -fever ' when in uncomplicated hay-fever there is no rise in temperature? This well-known error in clinical observation has long been embedded in popular language all the world over (e.g. heufieber, fievre du foin, fiebre del heno, etc.) ; since it involves no aetiological mistakes it will not embarrass any radical treatment. I think we should lose more than we should gain by trying to correct this popular word ; we must not go tilting at every verbal windmill, so ' hay-fever ' let it continue to be for us.

Symptoms. We have said that hay-fever is usually first suspected from its clinical symptoms ; let us review in detail what these are.

The symptoms are either local or generalised by the blood stream. The local troubles will occur wherever the airborne pollen irritant makes physiological contact

with the living tissues of the patient ; this can only be at the places open to the air, and even so will only be in certain areas of our body because for the most part our skin is too thick and the surface scales too dead to permit of this physiological contact. The prominence of eye and nose symptoms, therefore, is due to a physiological and botanical accident, and not due to eye or nose abnormalities as the surgeons tend to think (see p. 243).

These local troubles, chiefly of the eyes and nose, are most conspicuous to outsiders ; they do not, however, always give the most distress to the patients—though usually they do so. The first symptoms are subjective and take the form of a tickling, first of the eyes and then of the nose ; that may be all that happens in a very slight case.

The eyes from this incipient tickling may proceed to a smarting and soreness in a minor attack. This very unpleasant condition may work up to intolerable pain and photophobia in the extreme cases.

In the eyes the first thing to be noticed objectively is that the inner angle, and then progressively the whole eyeball, becomes inflamed. As the inflammation develops and spreads, a brisk lachrymation begins ; this, at first at any rate, flows down the lachrymal duct, thus helping to supplement the pollen irritant in the internal nares.

The nose is having trouble too. Simultaneously with this conjunctivitis, pollen-laden air is being aspirated into the nose, and most of the pollen grains that enter get caught there ; for one of the main jobs of the nose is to act as filter for the lungs. This, as in the eye, produces first a faint tickling and then a distracting irritation.

The growing irritation is accompanied by a brisk catarrhal discharge from the mucosa, and the patient usually sneezes vigorously in an attempt to dislodge the irritant pollen ; the increased discharge gradually amounts to a rhinorrhoea which is augmented via the lachrymal ducts by the pollen-laden involuntary tears. The mucosa of the convoluted bones in the anterior nares first become inflamed, and then engorged and swollen, thus tending to block the nasal air-channel and make the patient breathe through the mouth.

In these milder cases the symptoms are not unlike those of a baddish head cold where the symptoms have developed with uncanny suddenness in the form of an ' attack ' ; in fact they are often described as a cold for the first year or two when they occur in childhood, and especially in a family whose members are not sufficiently acquainted with hay-fever, and therefore don't believe what the children tell them.

If the patient is by habit a mouth-breather, or if he becomes one through the blocking of the anterior nares as above described, then the pollen-laden air is aspirated through the mouth and throat ; the palate and tongue become itching and sore and the lips may feel burning hot, and inflamed spots and blebs develop on the hard palate.

It will be appreciated that if these symptoms have developed through the blocking of the nose they are usually found in cases of greater severity, though of course this blocking must depend to some extent on the size of the air passage which the patient has normally at his disposal, and on his breathing habits.

In the milder attacks the symptoms of eyes, nose and throat usually go no further than is recorded above ; but if the attack is more prolonged and intensified, the whole eyeball becomes suffused and red.

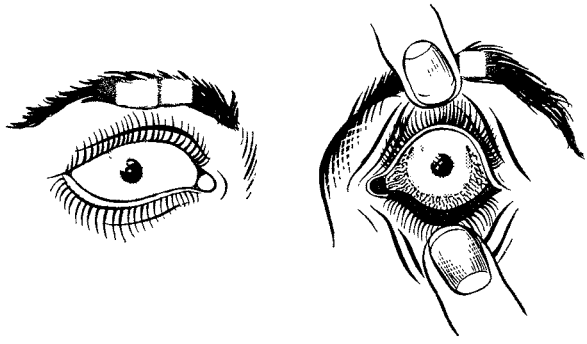


FIG. 1
Shows a hay-fever patient's eye reacting to grass pollen extract which has been dropped on to the conjunctiva.

This conjunctivitis is naturally accompanied by a serous leak (see Chapter VII) which tends to ' gum up ' the eyelids in the early morning and there may be a difficulty in parting them. Involuntary tears carrying pollen with them flow more freely till the lachrymal duct, the walls of which have in their turn become so swollen as to occlude the lumen, will no longer allow the fluid to pass, and the tears course freely down the face. The itching becomes intolerable, and patients say they want to rub their eyes out.

Simultaneously with this, the nose and throat itch more and more, and a prolonged bout of sneezing usually occurs to expel the irritant and to clear the air-passages of the nose. In the worst cases and with the most highly sensitive persons the conditions are rapidly aggravated up to, and far beyond, what the luckless sufferer would have believed possible. Blebs of serous fluid may form on the surface of the conjunctival sac, and, at the lower fold of it where the pollen grains tend to collect, the conjunctiva begins to flake off until the surface of the eyeball is raw and ragged.

As for the nose, the sneezing may go on and on for fifty or sixty times until the patient falls exhausted on the floor in a semi-conscious condition. In the days when women's long hair was invariably done up, and kept up, with innumerable hairpins, I was frequently told they would ' sneeze their hair down ' (then a minor solecism) and would shoot the pins all over the room by these sneezing fits.

If the luckless person is, or has, become a mouth-breather, the state of the lips, mouth and throat becomes seriously uncomfortable.

I remember one young woman telling me that her throat and the upper part of her mouth was itching ' as from the continuous biting of millions of ants '. On examination, all the palate was fiery red with inflammation and the hard palate had been excoriated by the scratching with the finger nails of the sufferer in her attempts to stop the intolerable irritation produced by the exudation of the oedema fluid.

Patients, I have been told, have been so seriously affected that they have had their eyes packed with ice-bags ' to save the eyesight ' and their bodies packed with hot water bottles in an attempt to check the rigors that were continually shaking them. I have heard of lives despaired of (see p. 250) but never of any deaths from even the worst attacks of classical hay-fever. I have heard of several cases where children with hay-fever have got stranded in a hay field or have lagged behind the grown-ups who were with them, and they have fainted amongst the tall grass as the result of their paroxysms, and when found have been quite unconscious. Certainly hay-fever can make midsummer into an utter misery for pollen-sensitive persons.

The skin, even of the face and even of the thinnest-skinned of women and children, reacts less than mucous membranes will do ; but all over the body it may tingle and itch from the direct action of the pollen which lands on it and gives the sensation of goose-flesh. (Especially this happens where the protein content has been extracted from the grains by sweat or tears ; then this pollen extract may penetrate the upper dermal layers to some extent and so reach the living cells of the cutis vera and there produce an inflammation and oedema.)

In this way the surface of the face and neck itches and becomes swollen and inflamed ; and of course any scratching will drive the irritant protein deeper into the skin. As described above, the tears tracking over the cheeks will soak into the skin and may carry the irritant pollen extract to the deeper layers ; the resulting oedema gives a bloated and inflamed look as well as intense irritation. Women often refuse to appear in public and complain that their faces during the hay-fever season look as if they had been drinking heavily ; they certainly may attain a most dissolute and woe-begone appearance.

This action of the grass pollen through the thickness of the intact epidermis is especially likely to happen where the pollen can lodge and also where the surface is naturally moist. Very noticeably this occurs in the external auditory meatus. The ears also give trouble by the blocking of the eustachian tube and patients may get earache, deafness or giddiness therefrom ; but this belongs, of course, rather to the nose and throat than to trouble on the external skin.

Other places affected are—the base of the scalp hairs, the armpits, and between the breasts. Also there may be special trouble wherever there is a lodgment of pollen between the clothing and the skin, e.g. at the neck.

When skin inflammation and swelling has gone down again, the surface looks dry and scaly—suggesting eczema—and indeed it might reasonably be called eczema.

If the pollen lodges at any abrasion or wound of the skin surface (e.g. scratches on the legs or arms of a child) then this break in the normal defensive skin exposes the underlying tissues to the action of the pollen and they become puffy and inflamed and intensely irritated. The lesion is not of course necessarily septic, though it looks like it.

All the foregoing suggests clearly enough that the whole body is sensitive to the irritant pollen grains and that their action is not confined to the eye and nose areas ; but for the most part the local suffering *is* in the eyes, the nose and the throat—in that order of precedence.

By way of summing up the relative importance to the patient of the local troubles, I might give some figures of the *first* area patients have complained about to me. I used to keep tally of these complaints and, if patients did not volunteer any, I prompted them by asking which area was annoying them most. I got the following distributions :

Eyes	40%
Nose	37%
Throat	16%
Skin of face	3·5%
External auditory meatus	2·5%
Skin of neck, shoulders, scalp, arm-pits, chest, hands, feet and ' the entire surface of the body '	1·0%

The above-shown ascendency of eye troubles over nose troubles is slight but steady. This alone should be enough to discount the idea of some ear-nose-and-throat specialists that hay-fever (or any of the para-hayfevers for that matter) belongs exclusively to the nose department or can be removed by nasal operations. Bad mechanical conditions *might* be improved thereby—though always at the risk of trauma (see p. 127), and anyway they are of little importance in hay-fever.

It is a useful general rule that if eye irritation is slight or absent, then an air-borne irritant (such as grass pollen) can only be responsible for a small percentage of the patients' troubles : it is not a clear case of hay-fever.

General Distribution. If the pollen extract made by tears or sweat or in the nasal and upper air-passage exudates gets into the blood stream it will be carried all over the body and may produce symptoms anywhere.

The lower air-passages and the lungs may be affected both directly (i.e. by the pollen grains arriving there in the inspired air) and also by the pollen extract arriving in this way via the blood stream. It is difficult to separate the two methods of attack and that is why the larynx, bronchi and bronchioles have not been included in the above statistics concerning the distribution of *local* troubles. For the matter of that, symptoms in eyes, nose or throat would also be reinforced by pollen extract in the blood stream, but probably only in the heavier pollen attacks.

Pollen grains in the inspired air can cause hay-asthma direct : if through habit, surgical interference, or through disease (including hay-fever itself) the patient becomes a mouth-breather then some pollen would go down into the lower air passages. This asthma effect of breathing pollen grains through the mouth down into the lungs can be demonstrated experimentally.

That asthma can be produced if the extracted protein of the pollen grains gains access to the blood stream can be proved by the simple experiment of putting it there. A very little pollen extract injected from a syringe into the blood stream, or a much larger quantity allowed to infiltrate via the lymphatics from a subcutaneous injection of a grass pollen ' vaccine ', will readily produce (among other things) transient symptoms in the susceptible person, but will have, of course, no effect on the non-hayfever person. I should perhaps warn the reader that this is a highly dangerous experiment unless carried out carefully.

Asthma does not stand alone as a complication of hay-fever. We saw above that the skin can be affected by the airborne pollen grains, especially where these can lodge and be extracted by the sweat and tears ; those areas must have a reinforcement of trouble if the pollen extract comes to them also by the blood stream. It is clear that all areas of the body can be attacked from within via the blood, and it is probable that most generalised irritation of the skin will be derived in this way ; this will occur, however, chiefly with people who tend to get urticaria from causes other than grass pollen. Angioneurotic oedema, which is only a specialised form of urticaria, may be conveniently added here as one of the probable results of grass pollen getting into the blood stream of a susceptible person.

All forms of urticaria and angioneurotic oedema can be produced artificially by an experimental grass pollen toxaemia in a hay-fever subject at any time of year.

Migraine is another symptom which frequently accompanies true hay-fever, and again it occurs chiefly with people who are prone to migraine at other seasons, i.e. from specific poisons other than grass pollen. By injecting pollen extract into the blood stream of a hay-fever subject, migraine can be produced at any time of the year artificially just like urticaria, in fact the two usually go together. Neither can be produced by such injections into the normal person.

All the diseases to be discussed in the next chapter (all the toxic idiopathies, in fact) can, given the right conditions, appear as symptoms accessory to those of classical hay-fever. I don't want to stress them unduly ; they are noted here to emphasise the point that hay-fever is so closely associated with them as to be occasionally identical. There is one hay-fever variant, however, which should be noted because, if found accidentally, it may give rise to misunderstanding. All these troubles—hay-fever, asthma, the para-hayfevers, urticaria, migraine and the rest—are characterised by a serous leaking from the blood vessels under the influence of the specific poison (pollen in our case now). Not only will all oedemas be increased, but the kidney vessels will leak serum too, and we get an albuminuria. This is not often found because it is not often looked for, since there is no kidney disease to direct the physician's attention to the urine. When found by accident in a hay-fever case at midsummer I have known it to cause perturbation in the mind of the discoverer who usually attributes it to ' back-pressure ', or goodness knows what. This albuminuria occurs in severe cases of hay-fever, and will of course go when the pollen goes from the blood. After an experimental introduction to the blood stream of pollen extract I have seen the albuminous sediment fill three-quarters of a test-tube on ' boiling up ' the urine. Such an albuminuria leaves no after-effects and has no sinister significance. It only occurs during an attack.

Cryptic Hay-fever. For the last page or two we have been discussing asthma, urticaria, migraine, etc., which may accompany ordinary hay-fever and indeed be variants of it.

We can also have such diseases minus all the symptoms of classical hay-fever but caused to some extent by grass pollen. If our aetiological definitions given on p. 1 hold, we should call these pollen-induced states hay-fever also ; to avoid muddle let us call them Cryptic Hay-fever.

Patients coming to the Asthma Clinics at St. Mary's Hospital often declare that their asthma, migraine, or whatever it is, though possibly present off and on all the year round, invariably gets much worse at the end of May, and subsides again suddenly at mid-July ; though they have no symptoms of classical hay-fever, yet when tested on the skin with the grass pollen extract (see pp. 15 and 256) they may give the diagnostic wealing and are therefore cryptic hay-fever. The dating of their extra trouble at midsummer may be only coincidence, in which event, of course, they do not give the diagnostic wealing, and it has nothing to do with hay-fever.

The rationale of the business is that they have a residual tendency to oedema and serous leak due to causes other than grass pollen ; and, when this pollen to which they are sensitive arrives in the atmosphere in the course of nature at midsummer, then it increases this residual tendency to asthma and the rest rather than producing classical hay-fever.

General Symptoms are sometimes even more distressing than any of the foregoing localised troubles. Frequently a feeling of exhaustion and lassitude is produced by the pollen toxaemia. Some patients complain that their digestion goes wrong during the midsummer months, or that they have a feeling of ' liverishness '. They may feel a disgust for food or tobacco.

Patients assert, and it is probably true, that they may lose as much as a stone in weight during the short two months of the English hay-fever season. Severe cases usually say they feel very run-down and, not unnaturally, they suffer from loss of sleep. Here are a few phrases contributed by such cases when describing their general hay-fever symptoms in the consulting room : ' weak and can hardly walk ', ' very slack, and occasionally done to the world ', ' weak as a rat ', ' feel floppy and can't do things ', ' feel worn out ', ' utterly fagged in the evening ', ' my legs seem to get stiff and cramped with it, and it becomes too much exertion to move '. In fact, a feeling of exhaustion and lassitude is almost universal with severe cases.

Some say that they feel feverish and that they sweat easily ; but seldom, if ever, do they show a raised temperature in hay-fever unless the disease is complicated by infection.

Dr. Blackley, himself a hay-fever subject, relates in his monograph (1873) how he produced this constitutional disturbance experimentally by drawing pollen grains down into the lungs when breathing through the mouth rather than through the nose. After describing briefly the coryza and asthma which naturally followed such an inhalation, he goes on to say :

' In the course of 5 or 6 hours I began to have aching and a sense of weariness over the whole body, with pain in the head and spinal column. A very restless night was passed ; the pulse rose from its normal (68) to 100. Occasionally there was a slight cough with expectoration of thin frothy sputum, and for 24 hours I felt as if passing through an unusually severe attack of influenza. During the succeeding night a violent perspiration set in, and as this proceeded I began to feel more easy. The pain in the head and the weariness gradually abated, and at the end of the second day I was fit for duty again.'

Emotional Factors. I come now to the mental changes brought about by the grass pollen intoxication.

A mental derangement is not easily to be separated in our minds from these feelings of general exhaustion which we have been describing ; and this mental derangement will in turn tend to augment all the other disabilities, both local and general, in the mind of the sufferer as he broods on them ; he will tend to make the worst of his troubles.

Just as we all feel stupefied and heavy with a cold in the head (see p. 220) so does the hay-fever patient feel mentally poisoned ; presumably this is for the same reason, i.e. toxaemia affecting the central nervous system (story on p. 250).

The loss of mental balance may go much further than a dull stupid feeling. Patients feel depressed and in despair, and they are noticeably irritable to their friends. Psychology was a word not so much in general use when I began working at hay-fever about forty years ago, and was the last thing I expected to hear about from patients when I invited them to tell me of their troubles. I soon found, however, that patients related such mental symptoms to me unasked ; and though I did not give much credence to them at first, the repetition and similarity of their stories compelled attention. Here are a few phrases recorded in my notes at that time : ' I get irritable and dislike speaking to anyone.' ' I feel such a fool I could sit down and cry all day.' ' I get an empty sinking sensation and have no heart for social duties.' ' I feel ill, tired, and depressed, and very grumpy.' ' It makes me wretched and an utter worm.' ' Irritable, dazed, and muzzy in the head.' ' It isn't so much the trouble I have with the eyes and nose, it is the terrible feeling of mental inefficiency and silliness that I get with the hay-fever.'

A critical and observant doctor when describing his own symptoms said to me : ' I get neurotic, go off food, and suffer from malaise almost as in an acute fever.' He added, and I think he spoke the truth, that outside the short hay-fever season he was mentally as well-balanced and normal as a man should be.

It is noticeable that the patients who are mentally unbalanced during the hay-fever time are quite normal at other times, and that even the sanest hay-fever patient may be mentally abnormal at midsummer. I find that professional psychologists will often hotly dispute this point, to which I will refer later in Chapter XI when discussing emotional and psychological factors.

Patients do not always know of their mental aberrations and are genuinely surprised and indignant if this is suggested to them.

Wife (sweetly) : ' Oh, no, doctor, I never allow myself to get at all irritable with my hay-fever.'

Husband (unable to restrain himself any longer) : ' Oh, yes, you do, darling ; you are really not fit to live with during the attacks ; but I don't tell you so of course.'

We all know that toxaemias produce well marked mental symptoms (see p. 220) —from alcoholic intoxication to the irritability and despondency which follow a bad head cold, from the effects of opium to the madness and delirium of high fever ; it is not surprising, therefore, that a grass pollen toxaemia should be accompanied (of course, only in hay-fever subjects) by the mental effects recorded above. Nature has with hay-fever set up a useful experiment : not only can we compare the mental state of the hay-fever sufferer at midsummer with the state of his more fortunate

neighbours at that particular time, but we can compare our patient's state at that time with his state before and after his plaguing by the grass pollen cloud.

It is, as I understand it, the doctrine of some psychologists that a constant mental abnormality or trauma can find relief in hay-fever at midsummer. That should mean that they are more abnormal mentally in winter than at midsummer. I do not think any patient would agree, but that must be discussed elsewhere (see Chapter XI).

Attacks. While speaking of the main clinical symptoms of classical hay-fever some pages back (i.e. troubles of the eyes, nose and throat), the occurrence of ' attacks ' was mentioned, and these must certainly be regarded as characteristic of our disease. Indeed, these attacks are so much a part of all the associated diseases to be discussed in the next chapter, of all the Toxic Idiopathies as I call them, that Professor Aschoff has wished to group them together under the name of ' Anfalls Krankheiten ' or ' Attack Diseases '.

Hay-fever patients may be more or less miserable all the time during the two months of midsummer, yet often this chronic discomfort works up into a sudden crescendo of an attack ; this is especially likely to happen on waking up in the morning. It usually augments on coming into a sudden bright light (or under other emotional stimulus). It will obviously increase on coming into contact with an extra heavy cloud of airborne pollen. But this attack feature of our disease is the result of a vicious spiral of cause and effect (see p. 162), and this effect becoming a cause again ; I hope to explain this in a chapter later on when I have uncovered more of the causal mechanism.

After an acute attack has reached its height it may persist distressingly, but often it goes off almost as suddenly as it began. The occurrence of such sudden attacks either of conjunctivitis or of rhinorrhoea in the case history should make the diagnostician suspicious of hay-fever or one of the para-hayfevers rather than of bacterial infection which develops much more slowly, and goes away slowly too.

Heredity. Another useful diagnostic point is its distribution by birth. Hay-fever, like all the toxic idiopathies, is hereditary ; this may not come out easily in any case history for various reasons. Sometimes hay-fever only develops late in life—sometimes after the patient's children have had it and have outgrown it (see anecdote on p. 40). It should be borne in mind also that in tracing the factor of heredity it is not hay-fever only which must be looked for ; all the disorders we shall be associating with hay-fever in the next chapter (i.e. all the Toxic Idiopathies) show the hereditary element in the disease.

The hereditary character of hay-fever is shown clearly in Fig. 2.

Note that the sixth member down in the third generation was sensitive to horse scurf as well as to grass pollen and, therefore, had a horse para-hayfever as well as hay-fever. This inheritance of the allergic diathesis rather than of any one particular manifestation of it will be very much our concern when we come to discuss the inter-relationship of the various toxic idiopathies in the next chapter.

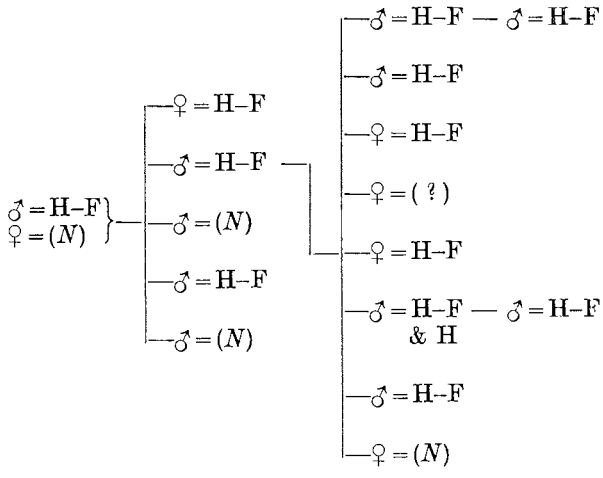


FIG. 2

The symbols in the above chart signify:
H–F for hay-fever; (*N*) for normal; (?) for not known; H for horse-asthma.

Aetiological Criterion. We come now to diagnosis by the sensitivity to grass pollen of our potential hay-fever patient. Contact with grass pollen may be of two kinds (i) the natural accident of the midsummer hay-fever season when the air is charged with pollen, and (ii) a deliberate laboratory experiment made with previously collected pollen.

About this accidental contact useful information can be got by appropriate questions: if it really is a case of hay-fever and the question is asked when the symptoms begin and end each year, the answer may possibly be rather indefinite such as ' when the weather is hot ', ' all the summer ', ' from April to August ', or even ' from March till September ', but at any rate the patient will implicate the summer season rather than the winter; in fact, if he does not, we may be pretty sure the case has very little hay-fever. On the other hand the patient may keep a diary and be precise as to facts, and I have known one such to consult his little book on being questioned, and answer that for the last few years the time of onset has varied between May 21st and May 24th and that the time of cessation has been usually on July 14th or thereabouts. Unless they do keep a note of the times, they are not likely to be very accurate in their answers, nor need that inaccuracy rule out genuine uncomplicated hay-fever.

Hay-fever, according to our definition, can only occur when there is grass pollen to cause it, and the exact time when this may be demands some local knowledge. Here in the South of England I have been accustomed to give it as from the 20th May to the 15th July; symptoms which begin much before or much after those dates in this area cast doubt on the hay-fever diagnosis.

As said above we must allow for inaccuracies and exaggeration; patients often want to show how bad they are, so that the doctor will take more trouble. Another cause of error is that people judge of the time of commencement and cessation of

hay-fever rather by their fears of it than by the actual attacks. Patients often say their hay-fever goes on till the end of July because it takes a good fortnight for them to lose their fear of it after it has really stopped for that year.

If by pollen testing the case turns out to be one of true hay-fever, but yet the actual symptoms do seriously extend beyond the time of the grass pollinating season, then the hay-fever must be complicated and extended by some other patho-

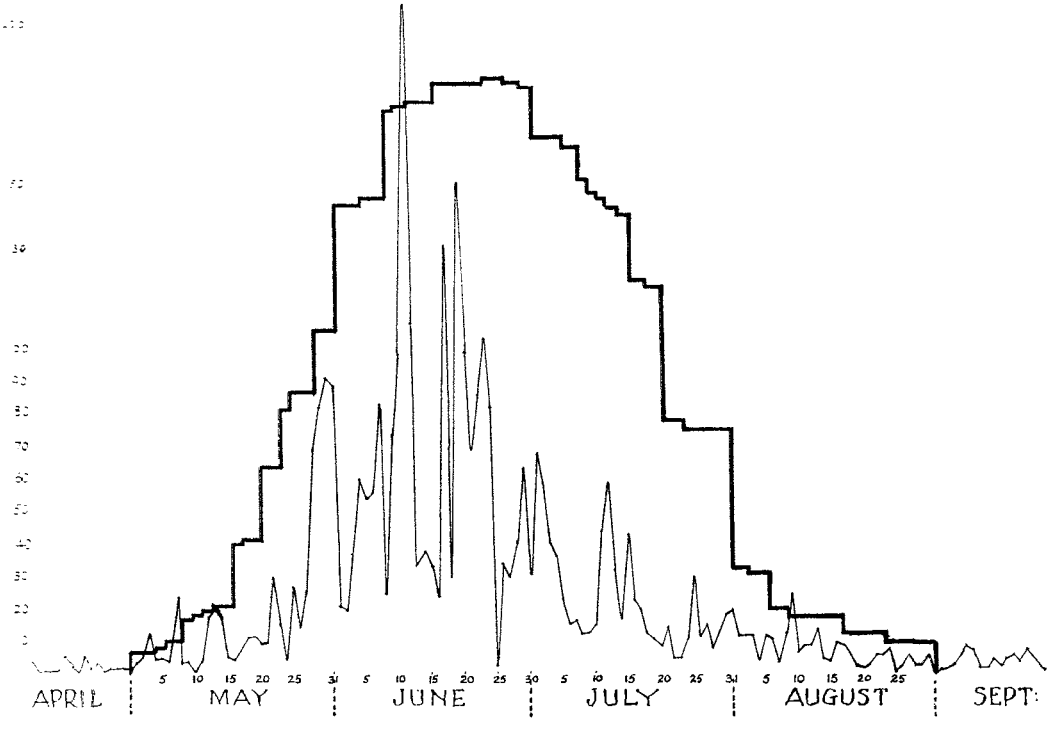


FIG. 3

Shows in a heavy line curve the times when patients *say* they have experienced hay-fever symptoms in the past few years. Patients are inclined to exaggerate the beginning and the ending of their hay-fever times.

The grass pollen incidence curve is drawn with a fine line. It shows the numbers of pollen grains caught day by day on 5 sq. cm. of the adhesive surface of slides which we exposed 80 ft. above ground level on our laboratory roof in London, W.2., throughout the year 1943. Our pollen station was situated well to the west of the centre of the town but yet with a good ten miles of built-up area between our roof top and the hayfields to the west of London. (The pollen analyses were made by Mr. H. A. Hyde and Dr. D. A. Williams as a part of their pollen survey of Great Britain, and are here published by their kind permission.)

genic factor. The patient might also be a para-hayfever patient, i.e. sensitive to some airborne substance other than grass pollen, e.g. horse scurf. (See heredity chart, Fig. 2, p. 12 : the sixth person down in the third generation was sensitive to horse emanations as well as to grass pollen, and, though it was a case of true hay-fever, that might easily produce some unusual timing of the attacks). Again there might be some bacterial infection of the upper air passages of a catarrhal nature ; the catarrhal discharge might either be mistaken for an idiotoxic rhinitis, or in my

opinion the bacteria (or even virus) of the infection might itself be a substance to which our patient is sensitive. Whatever the explanation, bacterial 'colds ' do tend to make indistinct the times of onset and cessation of true hay-fever (see p. 222).

However carefully the patient and the doctor have noted the clinical evidence including the seasonal timing, it is necessary to have an experimental testing with grass pollen or an extract therefrom in order to exclude the para-hayfevers, and this is necessary even if the symptoms are said to agree exactly in time with the local grass pollen cloud : that might be mere coincidence. Orthodox timing for hay-fever may even be an inexact statement partly derived from the dutiful wish of the patient to fall into line with the requirements of their supposed disease ; patients are generally anxious to have all the correct symptoms in order to convince the doctor.

In 1869 Blackley brought on an attack of typical hay-fever by inhaling the pollen of a bunch of grasses picked by one of his children. Remarking that it was much easier to theorise than to try experiments, he proceeded to try experiments on himself. One of these many experiments may be quoted. He put one-fiftieth of a grain by weight of pollen from the common fox-tail grass on the tip of his finger and rubbed this on the lining membrane of one nostril as far up as he could reach.

' In a few minutes a violent attack of sneezing came on ; there was also a profuse discharge of serum, which continued for some hours, gradually diminishing towards the latter part of the time. In two hours after the experiment had commenced the mucous membrane had become so much swollen that no air could be drawn through the nostril in any attempt at inspiration.'

The way of the self-experimenter is indeed hard : one-fiftieth of a grain is about 0·0013 of a gram ; this would mean that Blackley put about 1,300 Noon-units (see p. 278) up that nostril ; I compute that an ordinary routine test such as is used in my allergy clinics introduces about 30 to 40 units into the depths of the skin, and even so, such tests sometimes show quite startling results in the way of temporary oedema.

This method of Blackley's of poking pollen up the nose on a finger tip is not convenient for clinching diagnosis in the consulting room ; it was only one of a number of tests made by him, and these experiments have been altered, extended and repeated innumerable times since then.

A watery extract of pollen is easier to grade and handle than the actual grass pollen grains. The unit of measurement employed is the ' Noon-unit ' which is the amount of pollen substance that can be extracted from one-millionth of a gram of Timothy grass pollen. The Noon-units and how to make the extracts will all be discussed fully in Chapter XIV.

Blackley, and after him Dunbar, using unaltered pollen, showed that, if the dead surface of the skin of a hay-fever subject is abraded and grass pollen is placed on the uncovered cutis vera, the living cells in this area will react, producing a local oedema and erythema in the surrounding whole skin. It is this urticarial reaction which we are to note and measure, leaving out of account the effects of damage of the abrasion ; therefore the physical maltreatment should be cut down to a minimum and the strength (and so the physiological action) of the pollen should be carefully

measured to give the maximum wealing which is found convenient. These desiderata are obtained by pricking lightly through a drop of the pollen extract of 20,000 units per c.c. strength placed on a convenient site of the skin of the patient: the 'abrasion' is then practically invisible but the resulting weal of a positive test will be quite big. (For full details of the prick test, see Chapter XIII, pp. 256 to 261).

The erythema, though at times useful as an indication that a reaction is occurring, is found to differ too much from case to case, sometimes being completely absent in a hay-fever subject; it is therefore a bad indicator of reaction.

A control prick is made through normal saline to make sure that any wealing from a prick through the fluid is not due to dermographia; and, whatever area may be selected for the tests, it is as well to mark the skin with a pen or grease pencil to indicate the place and nature of each test.

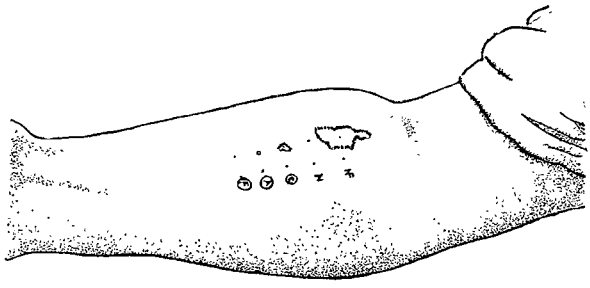


FIG. 4

Shows a prick test on the forearm of a hay-fever subject. The biggish weal, next to the bend of the elbow, is from grass pollen and clinches the hay-fever diagnosis. There is also, third down, a small and probably insignificant wealing from the compositae pollen mixture (p. 290). The other tests, including the always necessary saline control prick, are negative.

The oedema weal signifying a positive result begins to come up in about 5 minutes after the prick, and is usually at its height in from 12 to 15 minutes; the more intense the reaction the longer it may take to come fully out, but after 20 minutes the weal has usually passed its full size and is beginning to fade again. If the result is negative, then both the prick through pollen and the prick through normal saline should remain unchanged in appearance.

Dermographia sometimes causes a wealing round all the prick-marks, but is, of course, given away by occurring at the site of the negative control through normal saline; this dermographia is, however, rarely more than 3 or 4 mm. in diameter through a simple prick. Though some dermographic blurring may present difficulty in recording the exact size of a positive weal, it can hardly hide a positive reaction if this has really occurred. If there is any doubt about it, then the reaction is probably too slight for hay-fever diagnosis, and would certainly not be worth specific desensitisation.

What may we fairly deduce from such positive reactions as those shown in plates 4 and 5?

(i) If a case of hay-fever is suspected on clinical grounds and by the yearly timing of the symptoms, the positive skin test should clinch the diagnosis.

A prophylactic treatment by thoroughgoing desensitisation should certainly be attempted and success can almost be guaranteed.

(ii) If the hay-fever symptoms are atypical because they do not fit into the local timing for the grass pollen cloud (i.e. between mid-May and mid-July in the South of England) then the positive reaction indicates hay-fever, no doubt, but grass pollen is probably not the only 'idiotoxic' factor and an attempt should be made to find out what other trouble there may be and to deal with it, if possible, before undertaking any prophylactic treatment for hay-fever. Whether this treatment should be undertaken depends upon the estimate made of the percentage of total trouble caused by the grass pollen as will be explained in Chapter VI, p. 92. If the pollen accounts for, let us say, 90% of all symptoms then thoroughgoing desensitisation is strongly to be recommended. All the same the residue of troubles may well prevent what the patient would call a complete cure and will also hamper the process of desensitisation as will be explained later.

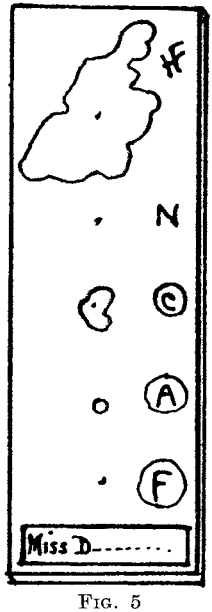


FIG. 5

Shows at life size and on a prepared microscope slide (p. 260) the tracings in ink round the weals in Fig. 4. The pricks, from above down, were through drops of (i) Grass pollen extract, (ii) Normal Saline, (iii) Compositae pollen group, (iv) Animal Scurf Group, and (v) Food Group: that is what the ink characters to the right of the slide denote. There is a minute and quite insignificant wealing to the Animal Group which is scarcely visible in Fig. 4.

If the pollen accounts for, let us say, only 10% of the patient's troubles—which is not likely to be the case with such definite reactions as those figured here in plates 4 and 5, then the patient might well lose all the hay-fever and yet hardly notice any improvement. Treatment, though technically a success, would be voted a failure by the patient. So, if a much smaller reaction has developed round the prick through the grass pollen and if the timing of yearly symptoms is unorthodox with perhaps a little asthma occurring all the year round. then it is very doubtful if pollen inoculations would do much good.

(iii) To complete the story, a third class may occasionally be detected; this is where there is a definite skin reaction with the hay-fever test, yet no symptoms are experienced by the patient. This anomalous result occurs occasionally with 'cured' cases, though not very often, I think; not having any hay-fever symptoms to complain about these people do not come for testing as a general rule and need no treatment. I do not know what the explanation may be, though one suspects that some necessary factor (other than the sensitiveness) is lacking. Psychologists assert that cases of hay-fever cured by psychoanalysis do not lose the diagnostic skin reaction. I have never had an opportunity of observing this.

Experience with many thousands of cases and extending over many years

shows that the majority of people attending a hay-fever clinic are hay-fever and nothing but hay-fever. The percentage of successful inoculation treatments will depend very largely upon how rigidly the seriously mixed, or non-hayfever, cases are rejected.

Age Incidence. If a curve is made showing the age at which hay-fever begins in the lives of the patients it is found that the greatest frequency is between the ages of eight and sixteen.

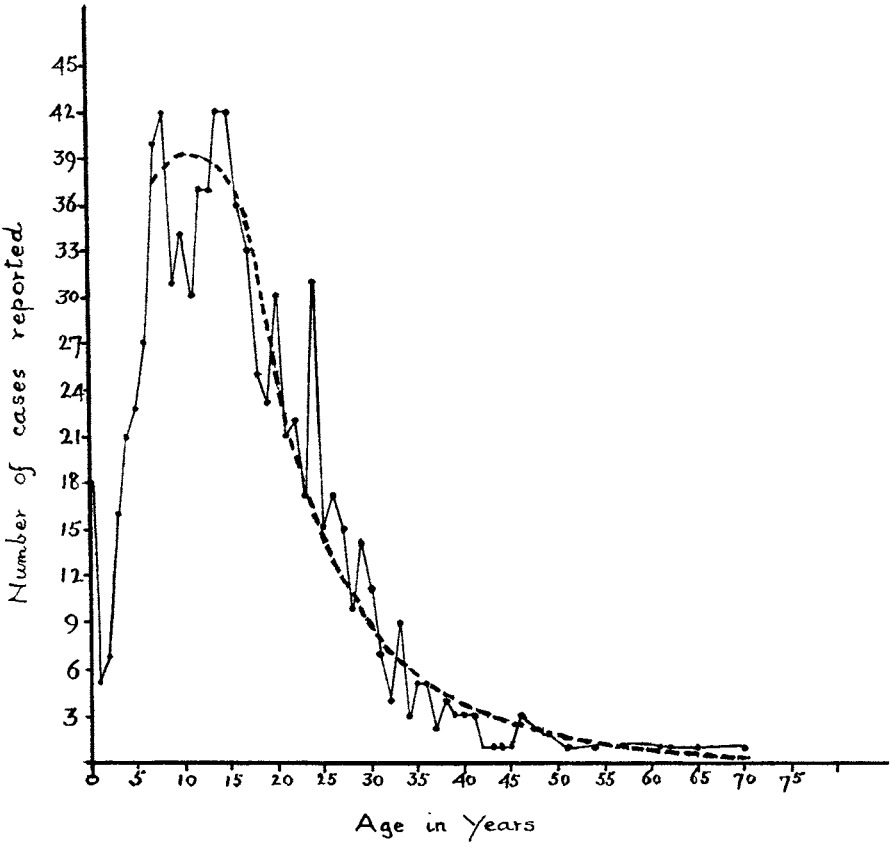


FIG. 6

Shows the age incidence of hay-fever. If the records were more numerous or more accurate, the resulting curve would presumably approximate to the dotted line.

Fig. 6 shows such a curve compounded from 1,000 genuine hay-fever cases but, of course, the age of onset I have had to take on trust from the patients or their parents.

Notice that a considerable number (18) are stated to have begun in the very first hay-fever season of their life; some of these are, no doubt, exaggerations on the part of the mother, but some must stand, I think, as correct. After close questioning, good witnesses assert that their babies born in April, May or June get undoubted symptoms their very first summer. This, if true, has great importance

with regard to an alleged sensitising process : we shall have to discuss that in Chapter XIII, p. 246.

The big fluctuations in Fig. 6 are due to inaccuracy of reporting probably, e.g. adult patients remember the age of onset in round figures, so they incline to twenty rather than nineteen or twenty-one.

The possibility of incurring hay-fever, though it becomes smaller as middle or old age advances, never quite goes ; the oldest case I have come across began genuine hay-fever at seventy-five years of age, but after forty-five it occurs too seldom for the construction of anything like a curve—at any rate, out of only 1,000 cases. I should say that the heavy dotted line in Fig. 6 represents fairly clearly the probability of onset.

Racial Incidence of Hay-fever. When I began working at the subject I was rash enough to state that hay-fever seemed to be a Northern European characteristic. My assurance in the matter was soon shaken by not only finding plenty of Spanish or Italian cases of hay-fever but also Chinese, Japanese and African natives with undoubted hay-fever. I haven't been able to get any facts as to whether racially pure American Indians, i.e. without any European blood, show hay-fever, but I think we must take it that our disease is world-wide in its racial distribution.

Sex Incidence seems to be of little or no significance ; some authorities say that it is commoner amongst men, and others that it is commoner amongst women. My own figures, allowing for the possibility that men may not go to the doctor as often as women, seem to suggest that it is about equally distributed.

I am bound to admit that sex does come into the family charts. I have charts where only males have been affected, and others where only the females ; I don't know the significance of this sex-selection.

It has certainly been my experience that the very severest cases of hay-fever, where perhaps even a fatal termination had been feared, have been men.

Social and Intellectual Status does not seem to me to affect the incidence of hay-fever so much as is generally supposed. It is often said loosely that hay-fever, and indeed all the disorders built on that pattern (see next chapter) are a hall-mark of intelligence ; I have not found this to be so. A psychologist working in my Hay-fever Clinic in 1936 used to declare that writers and painters and schoolmasters were to be found there out of all proportion to their numbers in the general population. One has to be careful because these would just be the type of people who would be alive to the opportunities afforded by a successful treatment, and would have less hesitation in availing themselves of it.

In Chapter IX we shall have occasion to show that a perturbed state of mind may precipitate attacks of hay-fever which otherwise might not have occurred, or may augment attacks if symptoms are already present. If that is so we should expect that nervy irritable people might be more likely to go to the clinics—unless of course their nerviness kept them away. I really don't think there is much in this either way ; though my psychologist friends would probably dispute my capacity for judging who is nervy and who isn't. I should be inclined to say that over a

long period of years my hay-fever patients have shown as stable a nervous system as the average run of people.

SUMMARY

The various clinical manifestations of hay-fever have been given above at some length, but we can't say that a person exhibiting such classical symptoms is necessarily a true hay-fever case unless we can show by his past history and by experiments that he is sensitive to grass pollen. As has been stated above, exactly similar clinical manifestations can be produced by foreign substances other than grass pollen. To distinguish these other conditions from the pollen-produced hay-fever they are called para-hayfevers.

As opposed to this there may be unorthodox clinical manifestations which are caused by grass pollen—at least in part—and these give none of the classical symptoms of hay-fever. These we called cryptic hay-fever.

It would seem difficult to make the mistake, but experience shows that infections of the upper air passages get called hay-fever because the patient sneezes ; and for no better reason hay-fever gets taken for an infection occurring at midsummer.

Every year perhaps a hundred cases or more are sent to our hay-fever clinics in the spring with a note from the doctor in which they may be described as ' typical hay-fever which occurs all the year round '. A negative test to grass pollen quickly shows the bogus nature of the diagnosis.

Conversely, ' colds ' which occur every year but only at midsummer are at least suspiciously like hay-fever ; a positive test to pollen would settle that question forthwith.

Long ago a Dutch patient came to see me concerning his hay-fever, which was in all respects typical. Inquiry into his family history showed a large number of cousins, uncles and aunts who all had, presumably, classical hay-fever.

There was a legend in the family that the great grandfather of my patient had had persistent colds every midsummer, but at no other time of the year ; and because so many of his descendants had developed hay-fever since his time they had no doubt what the true diagnosis of these ' colds ' should have been.

This unfortunate old gentleman was compelled, much against his will, by his strong-minded wife to wear continuously through the heat of June in Amsterdam two great-coats, one on top of the other, because he *would* persist in catching these ' colds ' !

CHAPTER II

ON THE RELATIONSHIP BETWEEN HAY-FEVER AND ALL THE OTHER TOXIC IDIOPATHIES

(*Synonyms : The Allergic Disorders, The Attack Diseases or The Atopic Diseases*)

When we started working at hay-fever in 1908, Noon and I thought we understood very well the role of grass pollen, for we had read Blackley's great monograph published in 1873. We made, however, two false assumptions.

The first was that the grass pollen in the air, *vis-a-vis* the complementary sensitiveness in the patient, was all the causal machinery that mattered in hay-fever. We shall find that this is not so. It will be necessary in the next chapter to explore other causes which affect the treatment of all allergic diseases—hay-fever included of course.

I am concerned with the second false assumption in this chapter: Noon and I thought that hay-fever stood alone as an unique disease, but it does not.

It was gradually recognised that, far from standing alone, hay-fever is only one of a closely-knit group ; though their outward manifestations may seem at first glance very dissimilar—e.g. consider the very different outward symptoms of such disorders as asthma, urticaria, and epilepsy. As I hope to show, they are in reality bound together in so many ways, and must have such a similar chain of causation, that it becomes an open question whether or no we should regard these as separate diseases or as superficial symptoms of some underlying condition which might be regarded as the ' real ' disease.

This question of regarding hay-fever either as a symptom or as a disease depends on the point of view. In practice, the clinician has to slide from one view-point to another as he considers different methods of attack. In the last chapter we established hay-fever as a clinical entity, let us now hasten to show that it may be regarded also as no more than a symptom of an underlying state.

It is important to establish this close relationship with other disorders for various reasons. It enables us to make hay-fever the key to understanding the other diseases ; and these in their turn enable us to analyse hay-fever, and understand it in a way that otherwise we should not be able to do.

Again, if we concentrate too exclusively on, say, hay-fever and angioneurotic oedema as being two clinical entities, we shall be unable to explain how much they interfere with one another or play into each others hands ; we can't explain how readily they may slide from one into another, or co-exist.

Let me put it another way. The factors which are relatively obscure in hay-fever may be much more obvious in, say, angioneurotic oedema or in asthma ; vice

versa, what cannot escape our notice in hay-fever (e.g. the idiotoxin factor which for hay-fever is the grass pollen) may not be so obvious in eczema or epilepsy ; yet, because of the close relationship, it may be presumed. If we can now establish that close relationship, we shall be in a better position to continue to discuss causation in the next chapter.

This group of disorders, of which I hope to show the relationship, have been named not too satisfactorily or unanimously, as our rather complicated chapter-heading shows ; and this muddled nomenclature has been half the trouble with them. Before, therefore, listing our diseases and attempting to group them together let us grasp that nettle-bed of nomenclature.

NOMENCLATURE

I fear I must call for considerable patience in the reader while I endeavour to clear up this really ridiculous confusion.

The Toxic Idiopathies was a name invented by me in 1919 and has been used fairly freely since. I first tried calling them the ' X ' diseases, for it became quite farcical to keep on repeating in one paragraph such phrases as ' Those diseases which have a causal mechanism similar in character to that of hay-fever ', or perhaps changing this for the more colloquial but equally inconvenient ' Asthma, Hay-fever, Migraine, Urticaria, and all that '. But the name ' X Diseases ' seemed to me affected, and had no particular meaning save perhaps in indicating my ignorance of them ; so after discussing it with my chief, Sir Almroth Wright, who had a well-developed taste for scientific neologisms, I plunged on the ' Toxic Idiopathies ' —which at least never means anything else.

These are a closely-knit group of diseases, for by my definition a toxic idiopathy is a disease which has a causal mechanism similar to that of hay-fever ; hay-fever is our exemplar disease, or archetype for these associated disorders.

By the operation of this machinery some normally harmless protein substance (e.g. grass pollen in hay-fever) becomes a specific poison for the patient—is in fact what I call the ' Idiotoxin '. Wright and I also concocted that word between us ; it seemed to be self-explanatory and exclusive of other meanings.

As reciprocal to this idiotoxin there is a hypothetical substance in the circulating blood of the patient (see Chapter V) which together with the idiotoxin makes an oedema-producing poison mixture. This hypothetical substance we called the ' Idioceptor '.

The Attack Diseases (Anfalls Krankheiten) is a name invented and used by Professor Aschoff to denote the toxic idiopathies ; I have a sneaking fondness for this name because it calls to mind what is often such a prominent feature of the toxic idiopathies, i.e. the blowing up into a sudden storm of symptoms within a few minutes from very little previous trouble. This precipitancy, however, does not always occur, and is not to be found so well marked in all of them (e.g. in eczema or in ichthyosis) ; also the phenomenon of ' attack ' is to be found in other conditions

(e.g. in anaphylaxy, q.v. below) which both Coca and I would exclude from the group ; also, and perhaps significantly, the sudden storm of anger comes as an ' attack '.

' **Atopy** ' is a word invented by Professor Coca of New York from the Greek word atopia, meaning a being out of place, and so queerness or unaccountableness. By atopy is meant : ' certain clinical forms of human hypersensitiveness that do not occur, so far as is known, in the lower animals, and which are subject to hereditary influence.' I have quoted from Coca's well-known book on Asthma and Hay-fever. I should say that atopy was not so much a synonym for a toxic idiopathy (or an Attack disease) as for the state or diathesis of the patient which renders him liable to one of these diseases ; it is, therefore, more a synonym for ' Allergy ' used strictly in its *second* meaning—a meaning which is discussed in the paragraph after next.

From atopy is derived the ' atopen ' which is synonymous with my idiotoxin. The hypothetical substance in the circulating blood which is specific for, and indeed responsible for, the poisoning action of the idiotoxin or atopen is called by Coca the ' reagin ' ; it is, however, unfortunate that this word reagin is sometimes used for the specific immune substance of infected people, e.g. for the syphilitic amboceptor : this misuse or ambiguity must, I think, be influenced by the ' Allergy I ' and ' Allergy II ' muddle which we will now discuss.

' **Allergy** ' has a multiplicity of meanings now in common use ; these are to some extent contradictory and to some extent shade off from one another, so this too popular word dominates our nettlebed of muddled immunological nomenclature. We are told that any idealistic young man instinctively feels that a beautiful woman must also be both good and clever ; in the same way we are all inclined to believe that so beautiful a word as ' allergy ' must mean something important, without concerning ourselves too much as to what that something may be. We take the pretty word at its face value, and it trips readily off all our tongues ; so popular is it that it has long ago become rather pedantic to attempt to avoid using it.

' Allergy ' has had an unhappy history. It was said by von Pirquet, who invented it in 1904 from the Greek words *allē ergeia,* to mean

> an altered reaction consequent on an infection, or on treatment by some foreign substance.

This very indefinite definition might cover most immunological responses—save perhaps what it has now chiefly come to mean. We may surmise that when he said ' an infection ' he had especially in mind tuberculosis and his own method of diagnosis by skin testing with tuberculin ; and when he used the phrase ' foreign substance ' he was thinking of Professor Richet's then recently discovered phenomenon of ' anaphylaxy '—a word we must tackle next. To use Ehrlich's pet metaphor, Pirquet was laying two research eggs which might with any luck hatch out into something later on.

Though the meaning given by Pirquet to the word ' allergy ' was so very wide, it could hardly cover the case of hay-fever ; yet, that our measure of muddle might

be full to overflowing, he said that it did—a rash inclusion, but we are mollified by his tentative inclusion of old-age and cancer : again two eggs I suspect.

In consequence of this loose upbringing, people began to use poor Allergy for whatever took their fancy, and Topley complained in his ' Outline of Immunity ' (p. 213) that it was impossible to define with any exactness the content of the group of conditions covered by the term ' allergy '.

I myself wrote somewhat petulantly in 1933 in the November number of *The Practitioner* : ' The word has been inflated till it has burst ; ' but we cannot dismiss a popular favourite in that cavalier fashion, especially if we are driven to use it ourselves.

Arnold Rice Rich of Johns Hopkins in his famous paper on bacterial allergy at the Pediatric Congress in London, July 1933, said in more measured terms, and with more authority :

' It is well for anyone who writes or speaks of Allergy to begin by defining what he himself means by that term, for the word allergy has come to be applied to the most diverse and unrelated types of altered bodily states.'

It seems clear that for anything like exactness we must ascribe not one but several meanings to the word ; these we might particularise as Allergy I, Allergy II, Allergy III, etc.

We guessed above that in Pirquet's view, in Allergy I in fact, the word signified a blend of anaphylaxy and an altered state due to bacterial infection. The anaphylaxy part we may cut out because, contrary to the prevailing views of 1904, we are hardly concerned with it now in human medicine (see p. 25). If we substitute for the very nebulous ' altered reaction ' the more precise ' state of hypersensitiveness ' we get :

Allergy I—a state of hypersensitiveness consequent on an infection.

That is, I think, the sense in which Rich, together with most immunologists, would use the word.

It should be said here that people who use ' Allergy ' in its first sense do not identify it with ' Immunity ', though Pirquet's original definition would permit them to do so. Allergy I is the *unfortunate* consequence of an infection, and is antagonistic to immunity.

Pirquet's illogical inclusion of hay-fever as an example of allergy has led to the inclusion of all the disorders with a machinery of causation similar to that of hay-fever under the group-name of ' Allergy '. This was the more likely to happen because these ' allergic disorders ' badly needed some such linking name. So in the consulting rooms and hospital clinics, as opposed to the pathological laboratories where the word started, allergy now seldom means more than Coca's Atopy, and the examples of it are my Toxic Idiopathies.

While still trying to stem the flood of random medical usage of the word, I conducted the following experiment. I asked the first twenty doctors who chanced to walk into my consulting room what they would expect to see if told that a case of ' allergy ' was being sent to them for treatment.

Most of them were confident in their reply, though some of them were wary of some

catch in my question—the ghost of Allergy I probably; however, they all answered in some such words as these: 'Oh, asthma, hay-fever, urticaria, migraine, and all that sort of thing.'

Those of them whom I knew well enough to ask where they had picked up the word couldn't tell me; it was common parlance, they said.

In these democratic days, when we settle even scientific problems by counting noddles, that settles the question. We may say:

Allergy II—that state of hypersensitiveness which leads to hay-fever and to the diseases with a causal machinery similar to that of hay-fever.

Allergy, as we all know, has long ago escaped from doctors' society; as Allergy III it has gone on the streets and is now very nearly all things to all men. On the music-hall stage and in the comic papers it signifies an antipathy: 'I am allergic to Itma', 'I am allergic to synthetic blondes'; yet even here there is a trace of surprise at the unexpectedness of this 'allergic' reaction—the last pale traces of the 'allē' I fancy. This third, or music-hall, allergy has even crept back again into medicine—'His heart is allergic (? is sensitive) to sudden shocks.'

Allergy is still going gaily downhill: Mr. Bernard Shaw seems to equate 'allergic' with poisonous.

In *The Observer* of Sunday, the 14th October 1945, G. B. S. in speaking of treatment for syphilis says: 'Iodide of potassium . . . proved so immediately allergic in many cases that it gave way to arsenic.'

The gradual debasing of verbal coinage is an ancient grumble of the pedants: with 'allergy', the changes have been more in the nature of somersaults—violent and complete. Not unnaturally they cause confusion.

I remember a visit paid to my laboratory by a worried and indignant doctor from Cleveland, Ohio, who held strong views on the causation of asthma, and had come all the way to London to maintain them at a Congress on Allergy.

He told me that at the meeting he had, to his disgust, found Frenchmen and Germans only concerned with tuberculosis and things like that! His ideas were Allergy II, and theirs were Allergy I.

Confusion is complete when a speaker or a writer uses 'Allergy' in the same lecture or paper in both sense I and II without knowing that there should be some differentiation between them.

I don't propose to use the word in the senses I and II given above more often than I can help; and when I do, if there is any ambiguity, I propose to say which I mean at the moment. Where I do not differentiate, I shall be using 'allergy' in the popular medical sense, i.e. in sense II.

When allergy is used in sense II (meaning the underlying state which is responsible for hay-fever and its like) then 'Allergen' may be used as a synonym for Idiotoxin or Atopen. The substance in the blood stream which correlates with the Allergen would be Allergin, which is of course the synonym for my Idioceptor.

I find Allergen and Allergin confusing; I shall keep to my Idiotoxin and Idioceptor.

It is difficult to contrast Allergy I with our Allergy II—largely because of the general indefiniteness of Allergy I; anyway this is not the place for it.

We shall have to consider a little how much Allergy I comes into our story when considering the bacterial factor in Chapter XII.

Anaphylaxy. The hypersensitiveness of anaphylaxy was stated above to be outside human medicine. That is more or less true, but again that depends on what twists and turns we now permit to that rather too handsome and once too popular word ' anaphylaxy '.

It was invented in 1902 by Richet from the Greek words *ana* and *phulasso*, to signify a guard or protection that has mutinied, a guard that has unexpectedly turned back on its ward like a boomerang : that which should have been a protection has gone back on us and become a danger.

It was first used by Richet to describe the troubles that beset a dog which has received its *second* injection of sea anemone juice. It was used by Theobald Smith to describe the rather different but clearly parallel troubles which beset a guinea-pig which has received its *second* dose of horse serum ; it can be used to describe the heart failure of a rabbit inoculated for the *second* time with, say, white of egg.

These untoward phenomena of anaphylaxy created a great stir in the early years of this century until it was realised that anaphylaxy could hardly be demonstrated as occurring in man and the apes, and could not be demonstrated in rats at all. Furthermore it was found that though practically any foreign protein could be used for the sensitisation, yet emulsions of bacteria would hardly serve in place of sea-anemone mush, horse serum, white of egg, or what protein you will.

I remember having a casual laboratory conversation with Sir Henry Dale (who then was, and still is I think, the greatest authority on anaphylaxy) and I used the word rather loosely and speculatively in connection with hay-fever. He retorted in pithy hyperbole, ' My good man, do keep in mind that anaphylaxy is a laboratory disease of guinea-pigs. For heaven's sake don't start muddling up our technical terms.'

It is difficult, if not impossible, to make a man demonstrably anaphylactic to horse serum by a series of hypodermic injections of horse serum—e.g. such as were given in the 1914 war in the form of antitetanic serum : often wounded men had four or five heavy shots of such serum on their way down from the front line in France to the Base Hospital in England ; but they didn't become anaphylactic, or scarcely so.

Quite different is the behaviour of horse-asthmatic patients who have acquired their tendency to violent idiotoxic responses to horse serum, not by artificial injections, but by inheriting this idiotoxic diathesis. Any accidental administration of horse serum to such patients may be most disturbing to the demonstratee, or even dangerous. Such very different hypersensitive states can hardly be the same thing —to be lumped together under the name ' anaphylaxy '.

At present the word is used chiefly by carefree writers as an elegant variant when they don't want to use the words hypersensitiveness or allergy too often in one paragraph. I shall use the word ' anaphylaxy ' in the original meaning ; and, as I have said, it barely comes into human medicine at all.

Toxic Idiopathy List. Now that we have emerged from this linguistic tangle, let us look at as full a list as possible of these toxic idiopathies, i.e. those clinical

conditions (diseases if you will, though some of them hardly amount to that) which have a causal mechanism which runs parallel with that of hay-fever.

The critical reader may justly feel that no such list can be final; cannot contain all that should go into it: that shall be discussed a little more fully at the end of this chapter. On the other hand, he may feel that the inclusion of some of the items (e.g. urticaria) is far too sweeping. Such border-line cases shall be discussed later too.

Now for this list:

The Toxic Idiopathies

The Disorder	*The Region chiefly or usually affected*
HAY-FEVER, and the Para-hayfevers from any air-borne idiotoxin	The conjunctiva and upper air passages: but only because these are the areas most open to attack by the airborne irritants.
ASTHMA, including all the Bronchitic Asthmas	Larynx, Bronchi and Bronchioles: especially after damage.
ALLERGIC DERMATITES: ECZEMA, PRURIGO, DYSIDROSIS, URTICARIA, ANGIONEUROTIC OEDEMA, ICHTHYOSIS, DERMOGRAPHIA, General Swellings, 'Up-and-down' disease, Bites and Stings of insects, nettles, and from (?) snakes, etc.	In the skin, lymphatics and subjacent tissues: two or more of these conditions may appear together, or they may show some intermediate forms, or may change from one to another. These combinations and permutations account for very many of the names in use by Dermatologists. The differences are chiefly in locality, timing and intensity of reaction.
IDIOTOXIC ALBUMINURIA	The kidneys.
PAROXYSMAL HYDRARTHROSIS	Joints and especially knees and finger joints.
FOOD IDIOSYNCRASY, DRUG IDIOSYNCRASY, IDIOTOXIC ENTERITIS	In the gut, and generally via blood stream.
MIGRAINE, EPILEPSY	The brain and the meninges.

Having made this apparently ragged list of disorders, let us now marshall the evidence which unites them into one big family of disorders.

(i) SEROUS HAEMORRHAGE. A serous leakage from the capillaries, often paroxysmal in character, is a common feature. (Chapter VII).

(ii) IDIOTOXINS. The specific idiotoxin, and its *vis-à-vis* the idioceptor in the blood, are among the factors of causation of all the toxic idiopathies. (Chapter IV).

(iii) HEREDITY. They are distributed among the population by heredity but they are inherited indiscriminately, i.e. not one but any of them. (Chapter II).

(iv) MUTATION. One toxic idiopathy may change into another. (Chapter VIII).

(v) SYNCHRONISATION. They frequently synchronise in their attacks or alternate.

(vi) SUMMATION. Co-existing toxic idiopathies can add to one another's effect. Removal of one subtracts.

(vii) METASTATIC DISTRIBUTION. If the idiotoxin for any patient is placed in the blood stream and thus broadcast over the body, it may produce almost any of the toxic idiopathies or several of them simultaneously.

(i) **Serous Haemorrhage.** If we take the symptoms of each of the toxic idiopathies in our list it is clear that a serous exudation plays a part in some of them if not in all : I assume it does so in all.

There is little difference in respect of such an effusion between, let us say, an urticaria, a paroxysmal hydrarthrosis and an allergic albuminuria. If enquiry is made into the various dermatites above (list on p. 26) it is found on consideration that a serous leakage into the skin is the dominant characteristic of all of them.

Hay-fever itself (and of course any of the para-hayfevers) is usually spoken of by the nasal surgeons as a serous rhinorrhoea, and the serous nature of the conjunctival discharge in these diseases is only hidden by the excessive lachrymation which washes it away into the nose or over the cheeks ; if the condition is very severe the serous leakage becomes obvious.

Asthma falls into line too : patients who have died in an acute status asthmaticus show so great an effusion of fluid into the submucous tissues of the larynx and bronchi that the jelly-like swellings block up the whole of the air passage ; this of itself is quite enough to cause death by asphyxiation, quite apart from any alleged spasm of the constricting muscles.

I think the point need not be pressed further : a paroxysmal serous effusion is obvious in many of the toxic idiopathies ; it is consistent with known facts that it should be present in all of them—to a greater or less degree.

(ii) **Idiotoxins.** We saw in the first chapter that the grass pollen idiotoxin was essential to any attack of hay-fever. A similar specific poison or idiotoxin can be demonstrated in most of the toxic idiopathies and can be presumed in all ; it is, in fact, an essential part of their machinery of causation. This is harder to prove in some cases than in others, and with bacterial idiotoxins it is usually only to be inferred. We shall be discussing in the next chapter the machinery of causation which is common to all the toxic idiopathies, including, of course, the always necessary idiotoxin and its matching idioceptor. An idiotoxin is implicit in *our* definition of hay-fever : it is also, I think, implicit in any definition of Allergy II. For our present purposes this curious feature of a specific poison may very well serve as one more link—and a powerful one—binding all these disorders together.

(iii) **Heredity.** The family histories form an obvious link between the various toxic idiopathies. Consider the heredity chart shown in the last chapter and the charts shown here and elsewhere in the book (see pp. 12, 28 to 29).

The earlier examples show a preponderance of hay-fever cases, but that is because hay-fever was being exclusively looked for at that time (1911–14) ; therefore horse para-hayfever and asthma when they do appear were only found, as it were, by chance. Directly any other toxic idiopathies were looked for, they were found in plenty.

The charts showing these mixed assortments of the toxic idiopathies which the family histories disclose were none of them collected in recent years because, as soon as this link-up was firmly established in my mind, and after a few such charts had been published in demonstration, it seemed a waste of time to look for more. It is true that these few I show are convincing to see, but there are difficulties in

collecting them for most people nowadays ; the families are too small, and people's knowledge of even fairly close relatives (such as great-uncles and great-aunts) is too slight. Also families are widely scattered. Only a few people have the knowledge

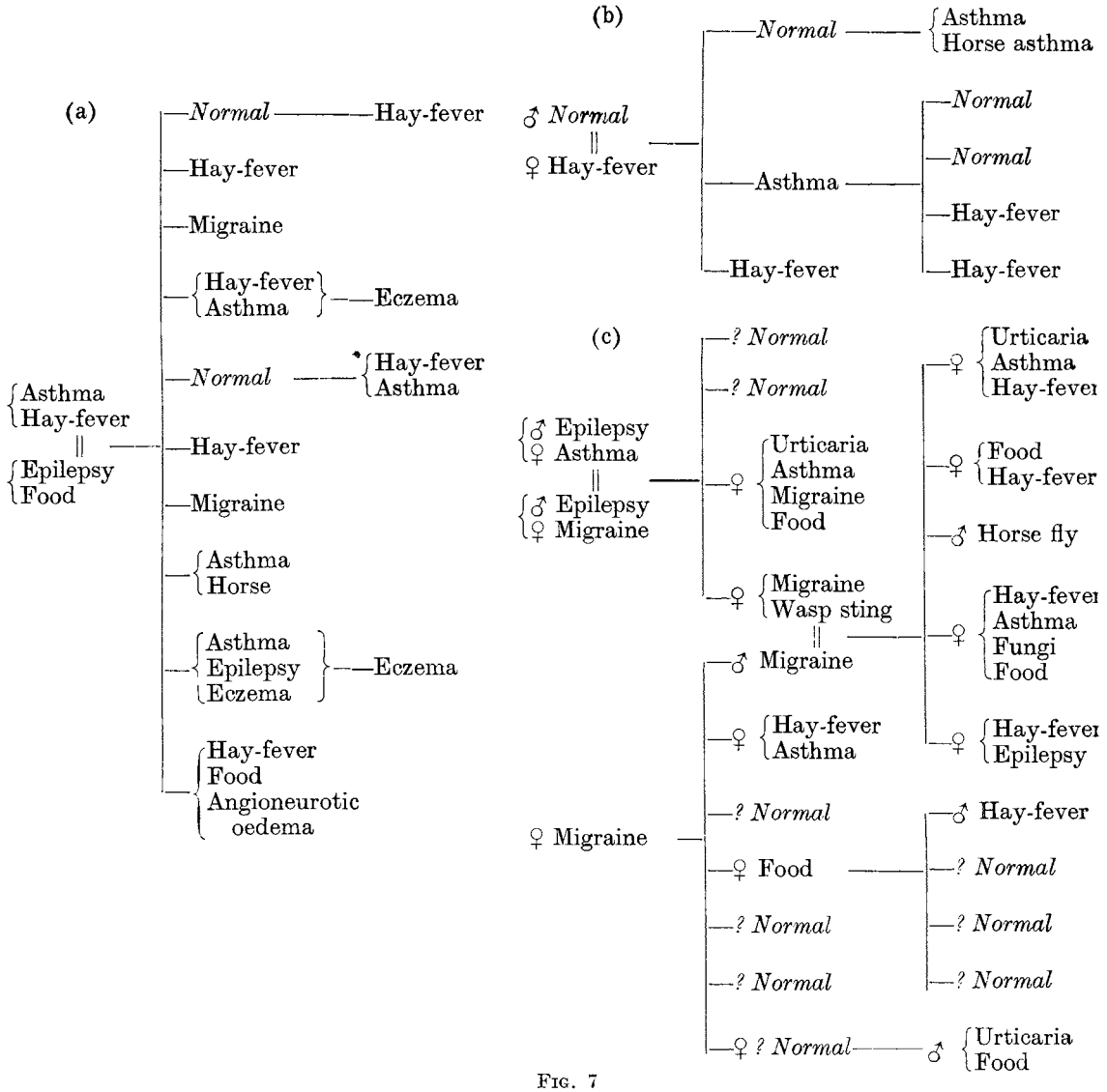


FIG. 7

Three heredity charts. They show that it is not one toxic idiopathy which is inherited but a variety of them.

necessary, or will take the trouble, to collect the full details. However, one's belief in the hereditary distribution of all these disorders mixed together is constantly being stiffened in the process of taking family histories for diagnostic purposes : enough ' tainted ' blood relations get disclosed (each with one or another of the toxic

idiopathies to their name) to make an enquirer certain of the hereditary link between them.

Looking at these charts again in the light of present knowledge it is clear that many of the toxic idiopathies are badly under-represented (e.g. eczema, ichthyosis,

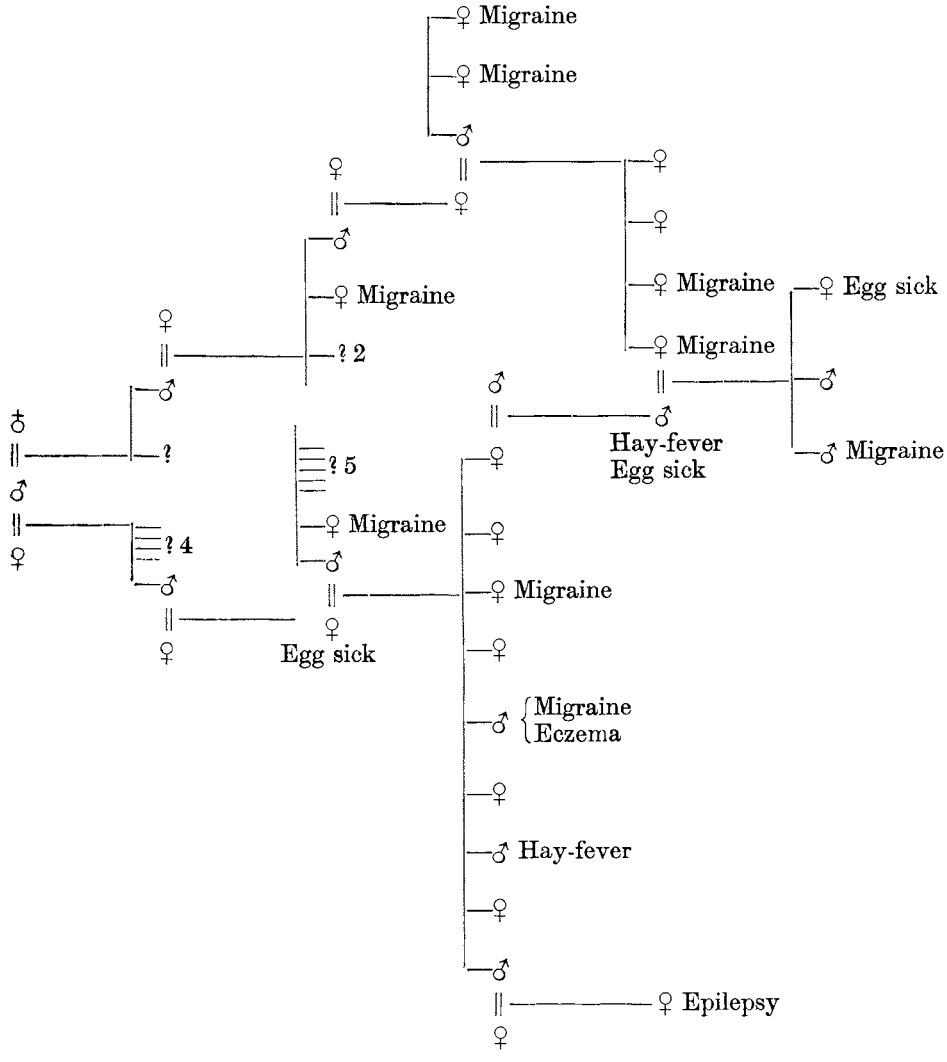


FIG. 8

This chart shows migraine and to a lesser degree egg-sickness dominating the inherited toxic idiopathies.

or albuminuria) simply because an insufficient interest was taken in them by patient and doctor.

Then again, people are reluctant to mention such supposedly shameful diseases as epilepsy.

Once, on taking the family history, I said to my informant: 'You and your family have produced a long list of these strange disorders, but surely with that fine array of them you must have had at least one case of epilepsy amongst you.' I got the answer: 'Yes, I'm afraid that Mother has it; but we are trying to keep that dark.'

Migraine, which is the sensory form of epilepsy, occurs with significant frequency even in these imperfect charts, but its authentic relationship to the other toxic idiopathies is doubted because migraine may often be legitimately attributed to something other than 'allergy', e.g. to strained eyesight.

Here, in Fig. 8, is a migraine-dominated chart which is rather impressive.

Urticaria, too, is doubted because it may be 'caused', e.g. by a hot bath or by ingesting a vegetable acid which would decalcify the blood (see Chapter VII). That is illogical; to think in that way is to fall a victim to the 'single cause' fallacy—which we shall be dealing with in the next chapter.

The allergic dermatites are clearly placed in our group by these heredity charts; with regard to bites and stings perhaps it should be explained that an allergic person sensitive, e.g. to a wasp's sting, will not have a temporary and local inconvenience like the rest of us, but will bloat all over the body and may easily be out of action for a week or more after a sting. More will have to be said about that in Chapter IV.

All things considered, these charts do form a powerful link between all our list of the toxic idiopathies. I think heredity is the most cogent argument to most people who work at the subject, but I would say that there were even stronger arguments to come.

(iv) **Mutation.** We have seen when looking at those heredity charts on p. 28 that different examples of the toxic idiopathies may be scattered about apparently promiscuously in the same family; we can also see that one member of that family may be credited with several of these diseases. They may be suffered simultaneously, but often they are not. Thus a child may show eczema at birth or soon after, urticaria may come on at weaning or at teething time, and later on asthma may begin either gradually or suddenly, and frequently as the asthma comes on the eczema goes off. Then the asthma may fade out as some of the causal factors which precipitated it pass away; later on urticaria or migraine may chance to come, then perhaps bronchial asthma or eczema in old age.

The sequence eczema-to-asthma is particularly common; I should say that with asthmatic children (accompanied by people who know the facts) about half are found after careful questioning to have had eczema previously. This skin trouble is by no means always *called* eczema by the tactful practitioner. It is amusing sometimes to see the evasive euphemisms used when the fond young mother demands a diagnosis for this blemish on her child. Eczema is apparently a shameful-sounding word, and I have inadvertently made mothers burst into tears by using it. Parents frequently relate that some upper air-passage infection—usually whooping-cough but often measles, pneumonia, or influenza—has 'changed' eczema into asthma; I believe they are right about this, as I hope to explain in Chapter VIII when speaking of the trauma factor of causation. The previous condition need not be eczema: angioneurotic oedema, migraine or hay-fever can be changed in this

way. I have known two cases where a previously existing and quite authentic epilepsy had been ' changed ' into asthma by whooping-cough : according to my way of looking at it, this is quite a reasonable result if epilepsy is a toxic idiopathy ; but it is quite illogical if it is not.

I have occasionally noticed that a long-standing and apparently fixed toxic idiopathy may, under successful treatment, be changed to some other form ; thus a case of asthma may be changed to one of eczema or migraine. If eczema may change to asthma because of damage to the breathing apparatus, then surely it is reasonable to suggest that if the damage is repaired the oedema tendency may find another vent (i.e. other than the asthma), perhaps only returning to some previous toxic idiopathy like the eczema or epilepsy cited above. This substitute idiopathy is seldom as severe as the one for which treatment has been given, but I have known cases where the patient has felt he was out of the frying-pan into the fire owing to treatment.

The changes from one toxic idiopathy to another may be spontaneous, sudden and frequent ; when they occur in this way they seem very convincing in showing their close relationships to one another.

I had a patient under observation for many years who used at one time to change her manifestations almost every week. One week she might have rather severe asthma ; next week this might stop, and she would have what she called ' the lumps ', i.e. angioneurotic oedema ; then ' the lumps ' would go, and she might get really bad migraine. Migraine she used to declare was worst of all, and that she looked forward to having something different on the following week. Occasionally they would all be absent ; rarely two or more might occur together.

Sometimes she had a swollen uncomfortable feeling in the abdomen which she called ' indigestion ', but she (and I) felt quite sure it was one of the series : an abdominal swelling was sometimes palpable, and I made the guess that this was something like an angioneurotic oedema occurring in the intestinal wall. In confirmation of this, surgeons have told me that they have come across something of the kind at operations. I have called this condition Idiotoxic Enteritis in the list on p. 26.

(v) **Synchronisation.** Where allergic attacks are coming and going, there is often marked synchronisation between two toxic idiopathies. Thus, migraine and asthma often occur together : ' I only get migraine when I have asthma ; at other times I'm quite free.' Also the combination of angioneurotic oedema and migraine is not uncommon. A patient who was very prone to angioneurotic oedema, only got migraine when the swellings occurred in the scalp over the cranial bones. One surmised that a similar serous leaking causing the migraine must be occurring on the inner side of the bones. An angioneurotic swelling of this patient caused by trauma is described in some detail on p. 130.

Good evidence that certain disorders are in our group is afforded by the surprising frequency with which they appear unsuspected amongst patients seeking advice for some well-recognised toxic idiopathy such as asthma or one of the spasmodic urticarial swellings. I find, for example, that ichthyosis is to be found with relative frequency in a large allergy clinic ; I have seen as many as three well-marked ichthyotics in one session who had all come to me for some quite different toxic idiopathy. I used to speak of an ' asthma skin ', meaning thereby a rather

dry, rough, powdery skin, but it is easy to see all stages between this rough skin up to a covering of ' scales ' almost like those of a fish or crocodile. I find my various dermatological colleagues express polite incredulity at my frequent cases showing ichthyosis, till I send them a sample or two to see for themselves.

The same is true of dermographia and allergic albuminuria ; the explanation is that these conditions seldom drive the patient to the doctor, and so they seldom get seen save as an accompaniment of a more alarming toxic idiopathy.

(vi) **Summations.** One toxic idiopathy can add itself to another (or to several) and thus cause one strongish attack. Thus, as we saw in Chapter I, a rather weak hay-fever tendency (as disclosed by a small skin reaction to grass pollen) when superimposed at midsummer on a perennial asthma or eczema frequently shows itself, not as hay-fever at all, but as an exacerbation of the asthma or eczema : this we called cryptic hay-fever in the last chapter because, though the increase of symptoms was almost certainly due to the grass pollen cloud, there were none of the ordinary signs of hay-fever. We come across this summation phenomenon in several combinations : thus a man with a mild horse para-hayfever all the year round and sensitive to grass pollen when it is about, i.e. from mid-May to mid-July, may be able to ride in comparative comfort during the rest of the year, but in those two midsummer months if he is so rash as to ride he may get either asthma or hay-fever—possibly he may get a third trouble, e.g. migraine or urticaria, or even albuminuria.

Beside this phenomenon of addition we can have subtraction forming a link between two toxic idiopathies.

The way in which successful treatment for chronic bronchitis took the sting out of a cat para-hayfever in an old lady subject to both these disorders is told on pp. 214 to 215 when discussing the bacterial factor of causation.

Subtraction can, of course, work either way : with two toxic idiopathies A and B it may be possible either to remove A and so help B, or remove B and so help A. In either case a ' total cure ' is a matter of reducing the total asthmogenic tendency below the level of manifestation.

A patient with bronchial asthma (i.e. with bacteria for the idiotoxin as I believe) was also sensitive to horses. I tried, but failed, to remove the bronchitic factor with an autogenous bacterial vaccine, so switched over to the more certain thoroughgoing desensitisation against horse substance. After this horse asthmogenic factor had been removed the ' bronchitis ' cleared up wonderfully and all the wheezing, dyspnoea and asthma stopped.

Frequently the subtraction phenomenon is seen when treatment for a little mild hay-fever is rendered unnecessary by bacterial treatment for bronchial asthma given throughout the preceding winter. An example of this is related in the anecdote on p. 215 about " the boy X—— Y—— ".

All this has a bearing on treatment, as we shall see when we come to it ; but here the argument is that you cannot add or subtract things in a different category : you cannot add up a beef-steak and the Mediterranean sea and make a useful, intelligible, total thing. Therefore bronchial asthma, hay-fever, cat asthma, etc., must all be in the same group.

(vii) **Metastatic Distribution.** In the first chapter we saw that the chief symptoms of hay-fever occur in those places of the body where the airborne idiotoxin makes contact with the living tissues of the victim, and that is chiefly in the eyes and nose : this botanical and physiological accident accounts for the ' format ' of classical hay-fever.

Experimental testing with grass pollen or its extract at other areas shows that these places can be affected as well. Thus, if put locally into the depths of the skin we get local urticaria as in a skin test ; if swallowed with the food or drink then vomiting and diarrhoea result, and presumably some oedema of the gut wall ; if placed up the rectum, then tenesmus and pruritis ani follow.

What happens, we may ask, if grass pollen extract is injected into, or gets into, the blood stream and thus is broadcast over the whole body? In the normal, i.e. non-hayfever person, nothing at all will happen with doses of, say, 50,000 units of grass pollen ; in the hay-fever subject something very serious indeed would happen with such a dose if put straight into the blood stream of an untreated case : presumably death by asphyxiation due to oedema of the glottis, though there would also be a general serous leakage into all the tissues.

Naturally I have not tried this experiment ; but in the course of the last thirty-five years I have given thousands of courses of subcutaneous injections of the pollen extract, and this extract slowly seeps through into the blood stream via the lymphatics. Occasionally the dose proves too big for that particular patient at that particular moment (see pp. 81 to 85) and there is a more or less sharp idiotoxic response, i.e. there occurs what is generally called a ' reaction ' from a dose. What form does this response to the blood-borne idiotoxin take? Practically any of our listed toxic idiopathies may occur, except those which take some time to develop like ichthyosis or eczema, but even a pre-existing patch of eczema may flare up. Classical hay-fever is not very likely to result unless the patient is having an attack of hay-fever at the time—and that is not likely because such doses should be given prophylactically before the season begins. The most common reaction resulting from an overdose of any idiotoxin—not only the pollen of hay-fever—is urticaria to a greater or less degree ; this may, in a severe reaction, go on to an angioneurotic oedema—especially in persons accustomed to these swellings. A migraine headache is common too, and asthma may occur where there is a tendency to asthma already. Paroxysmal hydrarthrosis may come if any joint has been previously twisted or damaged. Twice I have seen epileptic fits follow close on the heels of a dose of grass pollen—but these two were people given to such fits.

It was in 1913, I think, that I first found albuminuria occurring as the result of a reaction to a dose ; by inference from this discovery it was looked for and often found in the naturally occurring asthmas, urticarias, etc., of my allergy clinic as above related (see p. 26). Albuminuria can be found in most severe reactions from doses injected, and any idiotoxin can cause it.

What causes the trouble to be precipitated in one form rather than another in these artificial and metastatic toxic idiopathies we shall try to establish in Chapter VIII ; it is the same factor which decides which of the natural diseases shall occur.

We have seen that there is plenty of evidence of close relationship between the toxic idiopathies ; this evidence is of several kinds and is cumulative. Each undoubted link tends to stiffen the whole : they reinforce one another as the strands of fibrin stiffen a blood-clot.

Doubtful Cases

When a list of the toxic idiopathies was given above on p. 26 it was admitted that there were doubtful cases—both in and out of that list.

It is at present not possible to make a final list : in the past I have unexpectedly come across fresh examples from time to time such as the ' allergic albuminuria ' mentioned above. We may fully expect to find in the future that, when we know more than we do now, other disorders (and perhaps some surprising ones) will be added to our list of demonstrable toxic idiopathies. New examples are likely to be discovered below the skin, and so out of sight : regard the ten idiotoxic dermatites in the list on p. 26, and reflect that if we couldn't actually see them on the skin we might be ascribing the disturbances they caused to rheumatism, or to indigestion, to ' feeling run-down ', or to goodness knows what. (See the last paragraph of the anecdote told on p. 31.)

On the other hand it may be objected that I ought not to claim, for example, *all* urticaria, *all* migraine, or *all* epilepsy as representing toxic idiopathies. Perhaps not ; but in that event we should, if possible, re-classify these clinical entities into the idiotoxic and non-idiotoxic moieties, and give them different clinical names.

This re-classification has already taken place, for example, with asthma which used to be muddled up with pure renal and cardiac symptoms—but is so no longer we may hope. At the beginning of the first chapter it was necessary to be a little forthright with the somewhat dubious clinical entity of ' hay-fever ' itself.

Is migraine a symptom or a ' clinical entity ' ? Like that outlaw, rheumatism, we can regard it as either. There is no doubt that some cases of migraine are true examples of a toxic idiopathy ; witness the migraine of the boy shown at the right of the heredity chart shown on p. 29 ; on the other hand, some migraines seem not to be idiotoxic phenomena at all, and such cases should probably be re-classified more in accordance with the causal mechanism.

Epilepsy is another disease which may need a fresh classification ; some examples of it are undoubted toxic idiopathies ; witness the cases of it which are turned into asthma by the intervention of whooping-cough. Whether they all are toxic idiopathies depends on the homogeneity of ' epilepsy '.

Much the same may be said of the ' allergic dermatites ' ; many cases of eczema or of urticaria must remain on the border line till we can classify more satisfactorily according to the causal mechanisms.

How necessary it is to base the relationship of the toxic idiopathies on their causes rather than on the clinical symptoms was impressed on me at the first meeting of the London Asthma Research Club (of which I was the incautious progenitor). I thought a speaker was going rather far in giving too wide a meaning to the term ' asthma ', but when challenged he stoutly maintained that the short gasp that a man involuntarily gives on

inhaling the carbon dioxide given off by a whisky and soda *is* asthma ; presumably he was relying on the Greek word ' asthma ' which means a short-drawn or panting breath.

I rejoined from the Chair that we might as well label as asthma the short gasp one gives on jumping into an icy-cold bath, and got the reply that *that* was asthma too!

The matter was put to the vote and I lost the point on a show of hands ; though I would not guarantee that everyone was very serious in the voting, for it was an after-dinner occasion.

For all their diverse appearances the toxic idiopathies, take them all in all, seem to me a singularly closely knit group of disorders.

CHAPTER III

CAUSATION IN GENERAL, AND THE FALLACY OF THE SINGLE CAUSE. LIST OF OUR SEVEN SELECTED CAUSES OF THE TOXIC IDIOPATHIES. CAUSE A: HEREDITY

In the first chapter the disease of hay-fever itself was defined as being produced, in a select few of the population only, by the action of grass-pollen. This pollen was called the hay-fever ' idiotoxin ', i.e. the specific poison for those who suffer from hay-fever ; and our criterion for the disease was the way in which those few specifically sensitive persons reacted to this pollen when it was applied to their living tissues either by nature or by the doctor.

In the second chapter hay-fever was described as being the archetype of a large group of disorders, apparently widely differing, but in reality all knit closely together. These various disorders we called the Toxic Idiopathies (see p. 21). Reasons were given for believing the toxic idiopathies to be so closely related that one may think of them either as being different diseases, e.g. hay-fever, asthma, epilepsy, etc., or as being the local symptoms of an underlying allergic state.

In this chapter, and the six which immediately follow it, we will discuss those parts of this mechanism which are the more important to us because in our practice they have been found treatable, and have been treated.

Heredity, though hardly ' treatable ', is a natural concern for patients and their friends, and advice has to be given on the point ; it will be the first cause discussed, and at the end of this chapter.

Single Cause Fallacy. I speak deliberately of the ' causal mechanism ' and not of ' the cause '. Much medical controversy and misunderstanding is based on what may be called the Fallacy of the Single Cause. There are, of course, innumerable causal factors, of which several will be important to us—because they are readily treatable, as said above.

A doctor is subscribing to the single cause fallacy if he thinks that a disease can only be treated along one particular line ; and he is making the same mistake if he thinks that his success along this line rules out the possibility of success along any other line.

That seems a truism, stated thus baldly, if we grant that the argument from a single cause is a fallacy ; but in practice I find that doctors are sometimes jealous concerning their own treatments to the exclusion of the ' nostrums ' of their colleagues. That prevents a clear understanding of the disease in question, and may deprive it of useful treatment. The point has real importance for us ; so, before we start on the causes of hay-fever and the associated disorders, it is worth our while to consider the theory of causation systematically.

Theory of Causation. The late Professor R. G. Collingwood, in his essay on Metaphysics published in 1940, says that the word ' Cause ' in modern everyday language has at least three meanings, and possibly more.

In Sense I the ' Cause ' really equals the ' Motive ' : that seems to be a use of the word familiar to detective-story readers, and does not concern us here.

Furthermore, Sense III of the word ' Cause ' belongs, not to *practical*, but to *theoretical* science. It is J. S. Mills' ' real cause ' ; it embraces the infinite number of conditions—*conditiones sine quibus non*—necessarily antecedent to any event. One gathers that ' Cause ' (in Sense III) is philosophically tiresome and intractable ; mercifully it needn't detain us.

The Doctor's ' Cause '. Collingwood's ' Cause ' (in Sense II), however falls pat to our needs ; it is, he says, employed especially by doctors and engineers and by other exponents of a practical science. In this sense, that which is ' caused ' is an event in nature which we may hope to produce (or to prevent) by producing (or preventing) the cause. Thus, as he says, the cause of an event becomes the handle or switch by which human beings can manipulate this event, and the question : what is the cause of Y, really means how would you try to produce or prevent Y at will ; clearly we doctors would say : how should we ' treat ' Y. He then points out that, for any given trouble, different men might very well have different methods of treatment ; and, if so, different men would find different ' causes ' for the same event.

Relativity of Causes. One of Collingwood's illustrations of this point runs as follows :

> A car skids at a corner and turns over. The driver's reflection should be that, for the given conditions of the road and of his car, he was driving too carelessly (because *that* is the part *he* can alter). The County Surveyor notes the defects in the surface and cambering of the road (because that is *his* job). The car manufacturer's comment should be that the car's centre of gravity was not low enough, or its wheelbase insufficiently wide, to face the severe test of careless driving and of bad road construction.
>
> Thus, for each of these three men, there is a different cause which concerns him in particular.

In consequence of this, Collingwood enunciates a principle which he calls the Relativity of Causes.

Suppose of three persons, A can produce an event by method α, B can produce it by β, and C by method γ. Then, if each asks what causes this event, each will have to give a different answer. For A, α is the cause ; for B, β ; and for C, γ. The principle is stated thus : *for any given person the cause in Sense II of a given thing is that one of its conditions which he is able to produce or prevent.*

Dr. Y-Z's Difficulties. Collingwood doesn't ask what would happen if that experienced practitioner Dr. Y-Z was as familiar as he ought to be with all three methods of treatment α, β and γ ; for Dr. Y-Z there would be at least three causes in Sense II ; that, it seems to me, is the position we are in, or should be in, with regard to asthma, hay-fever and the like.

If poor asthmatic Miss X must be treated by a one-sided specialist—if Dr. A, the ' Allergist ', is to do his best about his beloved dust and feather pillows, if B, the Bacteriologist, snug in his laboratory, is to hunt for a green-type streptococcus maybe, if the dashing Mr. C is to give full throttle to his surgical skill, or if again Professor D from Vienna wishes to confine all treatment to winkling out complexes and inhibitions—it seems to me the poor lady will have less chance of avoiding the attacks and so becoming a normal woman than she would have if Dr. Y-Z knew enough of causes α, β, γ, and δ, to direct affairs himself, getting what help he required from the various specialists.

It is hoped that this book will be a contribution towards providing the cautious but sound Dr. Y-Z with some of the requisite knowledge. Then I think specialists will find that they must give up cherishing the Single Cause Fallacy.

Select Your Causal Switches. When Professor Collingwood likens our individual causes for a disease to handles or switches manipulable by way of treatment, he drops a valuable hint : that we shouldn't waste time on all those which are for us not so manipulable.

What, then, is to be our now mercifully shortened list of Causes? To my mind we can usefully name seven ; and as I've said above, it is to be the business of this and the succeeding six chapters to take these ' handles for manipulation ' one by one and see what treatment we may devise for the patient's advantage ; and any one or more of these manipulable handles will be at our disposal for any given case

This list of causes is only *my* list, i.e. they are what I have been working against during the last thirty years or so, and have been using as a guide to my treatment in allergy clinics. The list does not, of course, include all the causes of trouble that have been authoritatively put forward.

Rejected Causes. For example, histamine may, and presumably does, play a part in the production of the toxic idiopathies ; but I know little of this at first hand save perhaps in dust, and have no experience of any countervailing treatment in spite of the so much advertised anti-histamines. Therefore I leave it out of *my* argument.

The integrity of the nerve arc which supplies the point affected by the idiotoxin must be important, as Sir Thomas Lewis has shown ; but I have not tried to manipulate *this* ' handle '.

People who have a hankering after magical results invoke Internal Secretions or Vitamins : but these things don't concern us here, for I am *allergic III* to magic, and haven't worked along these lines. No doubt, if we knew all, there would be dozens of other conditions on which we might base treatment ; and so render them Causes (in Sense II).

Meantime, consideration of the seven in our list will give us ample occupation and, as I hope to show, may afford respectable results by treatment.

Here are my seven manipulable causes :

In this Chapter	(A) HEREDITY	This, as I have said, is hardly treatable, save perhaps prophylactically.
In Chapter IV	(B) IDIOTOXIN	The specific poison, e.g. grass-pollen in hay-fever.
In Chapter V	(C) IDIOCEPTOR	The sensitiveness which selects the patient to be the sufferer from that idiotoxin.

In Chapter VII	(D) SEROUS LEAK	Conditions which govern the rate of escape of fluids from the blood-vessels.
In Chapter VIII	(E) TRAUMA	The localising factor may determine whether an attack shall occur or not; and it may determine of what nature it should be, e.g. eczema *or* asthma.
In Chapter IX	(F) EMOTIONS	Always play a part, but *only* a part, in causation.
In Chapter XII	(G) BACTERIAL	Causation acts in various ways, such as B, D, E and F above.

From what has been said on the fallacy of ' the single cause idea ', the relativity of causes and so forth, it should be quite clear that none of these seven causes can ever operate alone. I would go further and guess that all seven are necessarily involved in every case, to a greater or less extent.

Sometimes one causal factor is more conspicuous—and especially to the people who work along that line ; sometimes, and perhaps to other people, another of the causes seems most obvious. This, however, never justifies us in classifying the various attacks of a toxic idiopathy as ' Emotional ', ' Allergic ', ' Bacterial ' and so forth, because all these elements may come in always.

A : HEREDITY

There is usually, nowadays, an acceptance of the hereditary principle in allergic disorders in the minds of our patients and of their accompanying friends and relatives ; there can in fact be no doubt of the truth of it, and of the important role heredity plays in determining the incidence of the toxic idiopathies in particular people.

It is, of course, not any particular one of these allergic disorders which is likely to be inherited, but the chance of any of them, as we saw when discussing their close relationships in Chapter II (p. 20). There is, however, sometimes a noticeable preponderance of one particular type of disorder to be seen in a family tree : sometimes epilepsy occurs with surprising frequency in allergically tainted families ; or it may be migraine or asthma ; I have found undue reactions to stings and bites of insects to run in families. In the heredity chart on p. 12, hay-fever clearly predominates, but probably not to the extent that is shown because at the time hay-fever was monopolising the interest of both patients and questioner to the exclusion of other toxic idiopathies. I am sometimes asked if asthma can be ' caught ' by one person from another ; properly speaking this is of course impossible, but the sore throat or the bronchitis which may determine an asthma attack is clearly infectious, and it may be something like that which makes asthma run in families : the asthma may be the results of trauma produced by the infection—Chapter VIII.

What is inherited? It must be the tendency to develop allergic sensitiveness ; not the sensitiveness itself, because this doesn't necessarily develop till some time after birth, and may not develop till extreme old age (see p. 40). If the sensitiveness to a particular protein may develop late in a long life, it is clear that

the person might have died from natural causes before the allergy showed itself. From this again it is clear that more of the people in tainted families, and I suspect we should say *many* more people, must have inherited the tendency to allergy than ever discover in a longish life that they have this lurking disability ; perfectly normal people belonging to these allergic families must be candidates for, and in danger of, one or more of the toxic idiopathies, but have not experienced it yet, and of course they may never do so.

Many years ago I had as hay-fever patients three unmarried sisters, and I treated them successfully for it. Their father, a retired lawyer of seventy-three, used to laugh at them about it all : ' Silly girls! They've heard that my old father was supposed to have had hay-fever, so they imagined that they had it too. If they inherited it from him it must have been through *me* ; fortunately *I* have a well-balanced nervous system, so I'm not troubled with these fancies.'

At the age of seventy-four he got hay-fever himself, and badly : his three daughters didn't fail to point the moral to him.

Note that, if the lawyer had died at the allotted span of seventy years, hay-fever would have been said to have ' skipped a generation '.

If we could put into our heredity charts all those who have inherited the seeds of possible future trouble, no doubt these charts would be even more impressive.

What causes these seeds of trouble to germinate? What sets them going—if they are set going? I imagine that a certain asthmogenic pressure is needed to precipitate an attack ; there is a ' threshold ' (to use the customary metaphor) which has to be overpassed. A previously sub-threshold state may be put across the border line by the addition of any asthmogenic factor. To judge by the stories patients tell of the first onset of their own particular toxic idiopathies, it is usually some emotional shock (see Chapter IX) or a bacterial infection (see Chapter XII) which seems to them to be the starting point, and they are probably right about it.

If this is so can we do anything to prevent any toxic idiopathy from ever showing itself? Probably we, or it may be the parents, can do a good deal, but it has got to be done intelligently and unobtrusively as told in Chapter X. Even a visit of a child to the doctor or to a health centre *has* its bad side—as tending to hospitalise.

Patients, and more especially the parents of patients, often ask if the asthmatic tendency can be eradicated for ever. I fear that, till we know more about it, the answer should be, ' No : that tendency is inherited and must remain all his life ; but that is no reason why he should have any manifestations of the trouble.' Attacks will only come if a number of prerequisites are complied with, i.e. the causes we are going to discuss in the next six chapters.

As we shall see in Chapter X, any ostentatious display of extra care, any extra yearning or fuss over a child, any appearance of wrapping it up in cotton wool, keeping it from school or allowing it to consider itself abnormal and needing special care, will itself be strongly ' asthmogenic ', i.e. is likely to defeat its object, and make the emergence of a sensitiveness more likely than ever. Asthmatic parents often fall into this pitfall if they have been panicked by the thoughts of heredity ; if they have any family at all they are likely to limit it to only one, and they are, of course, especially likely to make a pet of, and so to spoil, that only child. The truth

is that if they try to save their children too hard they are increasing the danger of losing them. Here ' happy-go-lucky ' is a better motto than ' leaving-no-stone-unturned ', but the *appearance* of happy-go-lucky, combined with quietly turning essential stones is better still.

Patients suffering from some toxic idiopathy and even normal members of a ' tainted ' family often ask if they have any right to have children. There is sometimes, of course, an attempt to get a doctor's backing for refusing to have them. Whether this is so or not, I think doctors should be very chary of giving any such advice. The occurrence of allergic manifestations in children is often avoidable by sound upbringing and seeing to it that for the most part children associate freely with other children and don't see too much of grown-ups. They should be told that by bringing up their children intelligently they will markedly decrease the chance of any trouble at all, and that the toxic idiopathies in children generally suggest a foolish upbringing by over anxious parents (see pp. 144 to 159).

The more immediate danger incurred by married people determining to remain childless is that this voluntary sterilisation from a possibly mistaken sense of duty has a very bad effect on their own emotional background. It marks them off from mankind as lepers if they are told that they have no right to reproduce their own kind. I know of many instances where a woman seems to have had her angioneurotic oedema, migraine, asthma or whatever it may be, augmented or even created by refusal of parentage, and cured by the acceptance of it. It is common for a man to deplore the fact that his wife refuses to have children because he or she has asthma or because it is ' in the family ', and suggest that this dereliction of duty may be at the root of her trouble. One hears heart-rending stories of the effects of this supposed, or reputed, abnegation.

My advice usually is : beget your children, don't stop at one, and when they arrive don't make fools of them by yearning over them excessively. After all, stupidity, bad temper and laziness are also handed down hereditarily, and will probably cause more unhappiness than a toxic idiopathy will ; yet people don't refrain from parenthood because they are stupid or because their tempers are queer.

The defeatist policy of avoiding marriage or parenthood on emotional grounds causes at least as much asthma as it saves.

CHAPTER IV

THE IDIOTOXIN CAUSE OF TOXIC IDIOPATHIES AND HOW IT MAY BE DEALT WITH

Interaction between idiotoxin and idioceptor is essential to any toxic idiopathy which they may jointly cause in the patient. To stop this interaction we may abolish the idiotoxin, and then the idioceptor won't matter ; that is what this chapter is about. In the next chapter we discuss the nullifying of idioceptor, when the idiotoxin won't hurt the patient though it may be in contact with him.

The idiotoxin of hay-fever (i.e. the pollen from the grasses) is, by the very definition of hay-fever, an essential part of its causal machinery. I said in Chapter I that :

> Hay-fever is the clinical resultant of the interaction between on the one hand the pollen of the grasses, and on the other hand the living tissues of those people who are sensitive to it.

Or, we can put it negatively, thus :

> If the trouble in question is *not* caused by grass pollen it can't be hay-fever.

We can say, then, that the grass pollen idiotoxin is a *sine qua non* of hay-fever ; but if hay-fever is the exemplar of all the toxic idiopathies (asthma, eczema, migraine and the rest of them listed in Chapter II), and if these disorders have a similar causal machinery, we must suppose that the grass pollen of hay-fever has its homologue (i.e. some kind of idiotoxin) in every other toxic idiopathy. Are we justified in making this generalisation? I think it is reasonable to make it.

Often, by direct experiment, it is as easy to prove the specific idiotoxin for a given toxic idiopathy as to prove the causal relationship between grass pollen and hay-fever ; all the para-hayfevers and many instances of asthma are cases in point. Sometimes the idiotoxin provoking some particular toxic idiopathy is not so easy to identify, but it can reasonably be inferred. Sometimes, however, we cannot name, and perhaps are hardly able to guess at, the nature of the idiotoxin which must be operating if our premises are correct ; in such cases we are, I hold, entitled to assume its existence because there is an idiopathic response in the form of asthma, urticaria, etc. ; still more perhaps we may assume it because we can find evidence of some other parts of the common machinery of the toxic idiopathies listed on p. 26 in Chapter III—e.g. heredity.

I have spoken above of some ' foreign protein substance ' constituting the idiotoxin ; this needs a word or two of caution. The easily provable idiotoxins are all protein in structure (e.g. grass pollen for hay-fever) and that is only what we should expect since the idiopathic response is clearly an immunological phenomenon. But it can be shown that people with the requisite diathesis may show sensitiveness not

only to undoubted protein idiotoxins, but also to substances like aspirin which are very complicated chemical substances no doubt, but very far away from the protein molecule. Patients may even show sensitiveness to such chemically simple substances as potassium iodide or formalin.

The explanation of this anomaly, which will be discussed again in Chapter XIII, p. 263, when dealing with patch tests, seems to be either that the patient's own tissues are sufficiently denatured to be acting as a foreign protein on him, or else that these things cause damage to his tissues, a fact which we must deal with in Chapter VIII when dealing with the effect of traumata. The occasional idiotoxic effect of accidental bruising or scorching of the tissues of the patient would support either of these explanations.

The effect of bacterial endo- and exo-toxins in provoking attacks, say of asthma, are somewhat anomalous, too, as seen from the idiotoxin angle ; they are dealt with in Chapter XII.

Lastly there is a discussion of bogus idiotoxins (including dust) at the end of Chapter XIV.

I think we can sum up the idiotoxin question by saying roundly that there must be one or more idiotoxins operating to produce any of the toxic idiopathies, though admittedly it may hamper treatment if we are not able to name them. As said in the last chapter it very much assists treatment if we can take hold of a ' handle of causation ' for curative manipulation, and hampers it if we can't ; a known idiotoxin is such a handle.

The Pioneer Idiotoxin has been grass pollen. There is to my mind a close parallel between believing that all infections must be transmitted by bacteria or viruses and believing that all toxic idiopathies are ' caused ' by some protein idiotoxin—though in neither case need we be so simple-minded as to suppose that either the bacilli or the idiotoxins can be the one and only causal factor.

Almost exactly a hundred years ago the main difficulty in accepting the queer notion that living bacteria are the cause of all infection was largely got over by proving the first case. This was then being fought out over the anthrax infection in sheep. As soon as it was proved satisfactorily that B. anthracis *was* the sole method of transmission from sheep to sheep, and that the disease was the interaction between the efforts of the sheep and of the anthrax bacillus to survive, it was comparatively easy to believe that other microbes, e.g. B. typhosus, the tubercle bacillus, the staphylococcus, could and did produce their own particular lesions.

When that once novel idea concerning anthrax had gained general credence it could be assumed that, even if it was impossible to demonstrate a causal microbe for an infection, one must surely exist.

When on this assumption the great Pasteur was challenged to produce a microbe for rabies, he admitted that he could not find one ; but he said that, as the disease was clearly infective, he could only suppose that the necessary microbe was too small to be seen by the microscopes of that day. Thus he was anticipating our present-day knowledge of ultra-microscopic viruses by two generations of research workers.

Blackley's Work. In this analogy our 'anthrax bacillus' is clearly grass pollen; and though nowadays there can hardly be two minds about the causal role of this idiotoxin for hay-fever, indeed it is embedded in the very definition of that disorder, yet it will be worth while to show on what firm ground we stand in the proving of this First Case amongst the idiotoxins. Blackley, a Manchester practitioner, achieved this in his great monograph on the subject published in 1873.

In the late sixties and early seventies of last century, after cutting his way through a tangle of popular misconceptions concerning hay-fever, Blackley established two points on the causal role of grass pollen. By ingenious experiment, since often repeated, he proved (*a*) that the grass pollen was present in the atmosphere just when patients were experiencing the hay-fever symptoms. He also proved (*b*) that at any time of year he could reproduce those symptoms experimentally in these patients by permitting some previously collected grass pollen to gain access to their conjunctiva and internal nares. So far as he knew, that was true of no other substance but grass pollen; nor, of course, can we think to-day of any substance other than grass pollen of which it would be true.

Blackley was lucky in this respect, however, that the cases on which he experimented—and that was usually on himself—were true and uncomplicated cases of hay-fever, and were not sensitive to other idiotoxins, e.g. to cat scurf, or to egg white.

Nowadays we make Blackley's two points whenever we diagnose a case of hay-fever correctly: thus we ask (*a*) are the patient's symptoms confined to the time when grass pollen is likely to be in the atmosphere (about the 20th May to mid-July in the South of England)? (*b*) Does the patient at all times of the year give the orthodox diagnostic skin response to grass pollen extract? Thousands of true hay-fever cases give a positive answer to both questions every year, so hay-fever diagnosis is made, and the causal role of its idiotoxin confirmed.

Thus grass pollen has been handsomely vindicated by Blackley as the idiotoxin for hay-fever—also by everybody else who has tried it after him: that is the proof of the difficult *First* Case. But hay-fever, as we know now, is only the archetype of the other toxic idiopathies, all of which possess fundamentally the same structure as hay-fever.

Some of them, the para-hayfevers, are so similar to our archetype that the incautious investigator sometimes muddles them up by calling them all 'hay-fever' as was shown in Chapter I. With these para-hayfevers, as soon as we suspect the specific idiotoxin, it is easy to prove or disprove its complicity as Blackley did for hay-fever. We must (*a*) be able to synchronise the patient's symptoms with the possible presence of the suspect substance, and the cessation of symptoms with its removal; further, (*b*) we must be able to prove its pathological action in our patient by an experimental application of it, e.g. by a prick test through a suitable extract of it.

Beyond the para-hayfevers the idiotoxin cause can be proved in so many of the toxic idiopathies that it seems reasonable to assume it in all, just as we now assume some microbic action in all infections. And if one particular idiotoxin is essential to an allergic manifestation, its removal or avoidance must stop an attack.

Clearly, if a patient is being affected by idiotoxins A, B, C and D, we can't expect a total cessation of all symptoms if we remove only one of them from the patient.

Air Filtration. These pollen grains are of some considerable size : they are two or three times broader than a red blood corpuscle. It is therefore quite practicable to filter the atmosphere clear of them in an air-conditioned room similar to Professor Sturm van Loeuwen's well known 'asthma chambers'. These would, of course, filter out any particulate matter from the atmosphere, and would be a 'cure' for any hay-fever patient so long as he remains in the air-conditioned chamber.

The people with extremely bad hay-fever whom I used to meet in the early days, had often been trying something of the sort, e.g. wet muslin stretched across their open bedroom windows. I have known people to attempt to strain out the pollen grains by putting cotton wool plugs in their nostrils ; anyone who tries this on a hot day will soon realise how much the hay-fever patient must dread the symptoms if they are willing to put up with this semi-suffocation.

Gas masks would be very much better than this. Indeed, their use is said to be very agreeable—by the people who have invented them. By their use, the hay-fever patient might reasonably hope to make hay in June ; but the wearing of masks is not particularly comfortable on a hot day, and must necessarily curtail social amenities.

As has already been said, the eyes usually cause more trouble to the hay-fever sufferer than the nose and throat ; it is a common practice for sufferers to wear tinted glasses with side shields of gauze to protect their eyes from both the stimulating light and also from the arrival of chance pollen grains from the atmosphere ; but if they are to do this with complete success the spectacles must be made to fit snugly to the skin of the face round the external angle of the two eyes by means of an edging of velvet or similar material.

Hay-fever subjects in their desperation sometimes fix watch-glasses over their two eyes with adhesive strapping. Here again a simple experiment on oneself at midsummer soon shows the plight which the hay-fever person must be in if he will willingly submit to such treatment. Any well-constructed gas-mask would embody effective goggles, and so keep the conjunctival sacs pollen-free ; the goggles should be made with slightly tinted glasses in order to cut out the over-stimulating light of the sun also.

Pollen Avoidance. It is obvious that distance from the hayfields will diminish the number of pollen grains in the atmosphere, and that the centre of a big city such as London or Manchester, as Blackley found many years ago, will diminish hay-fever symptoms by diminishing the pollen. Even when no special precautions are taken, a quiet room with the windows closed will give comparative freedom.

Heavy rain will wash the pollen out of the atmosphere if that rain extends for some considerable distance upwind, say thirty miles or so : but it must be remembered that the pollen which affects the patient may fly for some very considerable distance. Blackley proved by ingenious kite experiments that in ordinary weather with a light breeze pollen may travel for great distances, and that pollen from the grasses of Norway could be carried right across the sea to Yorkshire on an easterly gale—a distance of three or four hundred miles ; therefore, the freedom derived from local summer showers may be very transient.

The seaside has long been considered beneficial to the hay-fever subject, who, however, is sometimes disgusted to find that the sea doesn't always live up to this reputation. Before Blackley published his monograph, this amelioration of symptoms at the seaside was ascribed to the bracing atmosphere, also to the ozone or the iodine of the seashore ; but, once the role of the grass pollen was fully established, it became clear that the direction of the wind in-shore or off-shore must be the determining factor. If, as so often happens at midsummer, there are light airs blowing first off-shore and then back again, the cloud of pollen over the land will be smudged a little out to sea and the refugee on the coast will be very little better off than when he was twenty or thirty miles inland. All this seems very obvious, but even nowadays patients often complain that, to their surprise and disgust, they found themselves no better off at the seaside than at homes inland.

High altitudes offer relief from the pollen cloud just as we have seen that big towns or the seaside may do. People usually ascribe this to the exhilaration of the height and to the ' freshness ' of the atmosphere ; I have always thought that the eleutherotropic feeling we all of us experience at the top of high mountains must be due to the scarcity of oxygen which benumbs our critical faculties, and so we feel ' good ' and noble. However that may be, people do seem to get less hay-fever when living on mountains and high hills.

An extremely sensitive hay-fever case told me once that he could easily watch people making hay on a high alpine pasture, and without any symptoms if only he took the simple precaution of keeping a bit up-wind of the haymaking.

I think this freedom is due to the lack of buoyancy in the attenuated air at such heights ; this air allows the pollen to fall to the ground by gravity in a few yards instead of flying for many miles. I have also heard it suggested that there is an electrical action on these mountains which precipitates all small particles such as pollen. The fact remains that very high ground is beneficial to the patient.

No doubt the feeling ' good ' above mentioned, i.e. the conscience having a brief holiday, will play its psychological part, and also the feeling of being clear of ordinary domestic and business cares ; I suspect that the absence of pollen from the atmosphere is the more important of the two.

The well-to-do sufferer who is free to travel can fly from his enemy during the critical months, going further North or further South according to the prevalence of the pollen cloud. The hay-fever season, which as we have seen stretches roughly from the 20th of May to the 15th of July in the South of England, will be a week or two later in the North of England and possibly three or four weeks later in the North of Scotland, while in France the season will be considerably earlier than in England, and in Italy and Spain much earlier, for example in April in Italy. Also in these hotter countries the season will be much shorter because of the withering of the grass in the semi-tropical sun. In the actual tropics, the hay-fever season depends chiefly on the incidence of the rains, and we often get, as, for example, in certain places in Africa, two hay-fever seasons because there are two rains ; but in these hot countries the pollen cloud and therefore the incidence of hay-fever symptoms is generally less and of shorter duration than in such grassy lands as England, Ireland or Norway.

I noted in my hay-fever clinics during the 1939–45 War that Canadian and U.S.A. men and women serving in England usually complained that they had had what they thought was baddish hay-fever for many years on the other side of the Atlantic, but their symptoms had never been so bad as what they had recently experienced from it in England. Perhaps that depended on where they had been living in North America, and may not have been everybody's experience.

Extreme anxiety to avoid the pollen may dictate that the sufferer will cross the equator travelling south as the northern summer approaches, and returning northwards again in July or August ; thus he will live in continual autumn or winter. I have known several hay-fever patients who have done this for years in succession in a desperate attempt to escape their plague.

Pollen Refugee. One of these wanderers told me that he had as usual taken a slow-going cargo ship to South America to avoid the hay-fever, but had perforce to return to England earlier than he wished, or thought at all safe. While he believed he was still well out in the Atlantic he felt the dread symptoms, and said to the Captain, ' But for the fact that I have a thousand miles of good sea water between me and the nearest hayfield, I could swear I have an attack of hay-fever just beginning.' The Captain said, ' We are much further on our way than you suppose, we are well into the Channel and the south coast of Devonshire is about thirty miles upwind of us. Your pollen must be coming in over the port rail.'

This policy of avoidance of the pollen can only be a partial success, or if it is to be a complete success it can only be at the expense of disorganising the life of the patient ; only the well-to-do and leisurely could resort to it, and to live subservient to the pollen is a defeatist attitude. All the same, by taking thought it may be possible to reduce the amount of pollen to which the hay-fever patient is likely to be subjected.

Avoidance of other Idiotoxins. Clearly it would be quite impracticable to work through all the other possible idiotoxins, thousands in number doubtless, on the same scale as we have employed in the avoidance of grass pollen for hay-fever. However, some idiotoxins more important than the others may well be considered here in a little detail.

The animal scurfs form rather a group apart and they are notoriously frequent causes of para-hayfevers, asthma, allergic dermatitis, etc.

The *horse*, and the equidae in general, are the scurfiest animals that anyone is likely to meet in ordinary civilised surroundings—i.e. the skin scales are being shed off these animals in great abundance. It was only natural, therefore, that the first cases of para-hayfever to be recorded in medical literature were derived from a horse scurf idiotoxin. For the normal Englishman, whether countryman or town-dweller, horses are diminishing in importance, but they can still cause plenty of trouble. If a patient derives most of his illness from horse scurf and other horse substances he will naturally avoid horses if he can, but it is sometimes forgotten that it is the horse emanation and not the sight of the animal which does the harm. Many horse-asthmatics find it tolerable to be riding a well-groomed horse, but find it much more disturbing to drive behind this horse in an open carriage or cart ; the particles from the horse's skin fly back and are caught in eyes, nose and throat.

Worst of all for the horse-sensitive person is to groom a horse or go into a stable while the grooming is going on. For small boys and girls in a town, I find the milk roundsman's pony is usually a personal friend and therefore a frequent cause of trouble for young horse-asthmatics.

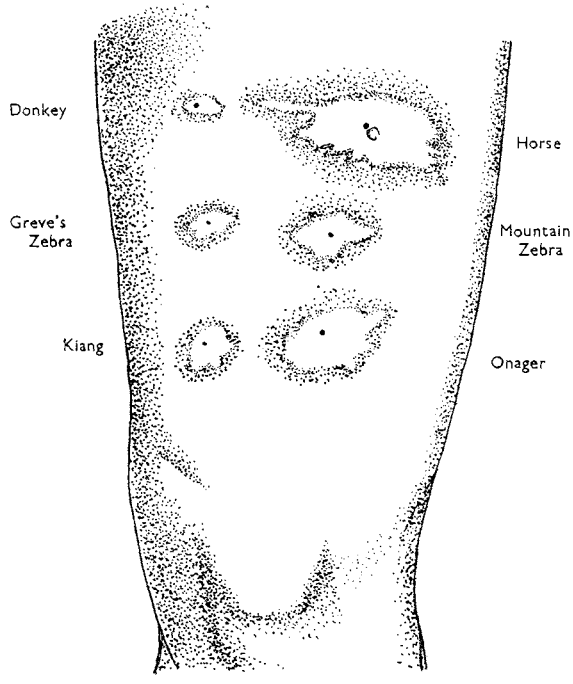


FIG. 9

A horse-sensitive patient tested not only with horse scurf extract but also with similar extracts from scurf of the other Equidae. As is usual the weal response is strongest to the horse scurf, and weakest to the donkey scurf. But then, of course, these patients come for treatment because of their *horse* sensitivity.

As I've suggested above, other members of the equidae produce a similar idiotoxin, that is to say if you are sensitive to horses you will also be sensitive to donkeys, zebras, onegas, kiangs, etc.

Zebra Sheds. The other day a mother brought her small son to me with an asthma which, so she announced, had been derived from giraffes in the London Zoo. I hadn't got any giraffe scurf by me but I tested him with a variety of animal scurfs and found that he was very strongly sensitive to horse scurf and, so far as I could tell, to horse scurf only. I then remembered that the giraffe sheds at the London Zoo may be approached via the Zebra yards ; and on questioning I found that the boy had spent some ten minutes or so in talking to and feeding the zebras, and had got his attack of asthma *immediately* on leaving them and entering the giraffe shed. As the attacks usually take from five to ten minutes to develop, it was clearly the zebras which had caused the trouble and not the giraffes.

Horse-hair stuffing of chairs, mattresses, etc., unless the hair has been very carefully cleaned, will convey the horse scurf.

Armchairs. A pleasant young woman whose trouble had only recently started proved to be very sensitive to horse scurf. ' It is most provoking ; I get a bad go of asthma and hay-fever every evening just ten minutes before my husband comes home, and he says I look an awful sight.' ' Well, what do you do during the twenty minutes before he gets back? ' ' Oh, I tidy up a bit, perhaps have a hasty look in the oven to see how supper's cooking, slip into a clean dress, then sit down to read in a *very* comfortable armchair. After a minute or two it all starts.' ' What about that armchair? Is it new? ' ' Yes, we've only just got them, two beauties—but rather expensive.'

They proved to be horsehair-stuffed. The patient was offered Thoroughgoing Desensitisation (described in Chapter V), but she preferred to change the chairs, and that stopped her trouble—for the time being at any rate.

I have known some half-a-dozen cases where boxing gloves stuffed with horse-hair have produced acute attacks of asthma and rhinorrhoea when these same boxing gloves have landed on the nose of a house-sensitive person.

The Boxing Cadet. When I said this to a young Naval Cadet who was very horse-sensitive, his Admiral father who had come with him suddenly snapped his fingers and exclaimed, ' We've got it! You remember we had to break off that little sparring match after I had landed twice on your nose. That was your very first attack of asthma you remember.' ' Yes! ' said the boy with feeling, ' and because my eyes watered you said I couldn't take it! '

(Possibly we have here also a trauma effect : Chapter VIII.)

The clothes of anybody who has groomed a horse recently are usually heavily impregnated with horse scurf, and if a horse-sensitive person sits next to somebody wearing such clothes, let us say in an omnibus or train, they are liable to have an unexplained attack.

Horse manure will also spread the horse emanations ; so also will the trousers or boots of anybody who has been digging horse manure into a garden. A heavily manured nursery-garden up-wind, or bedroom window-boxes with manured plants in them can often be detected as the nightly nexus between horses and the horse-asthmatic patient. The little boy collecting horse-droppings from the country lane to increase the fertility of his daddy's garden suffers for it if he is horse-sensitive.

My colleague, Dr. Harkins, has found that in the manufacture of rough felt (e.g. as used for under-carpets) the hair of horses and cattle is habitually employed, and often in an unwashed condition. Extracts made from samples of these under-felts brought to us by horse-sensitive patients have several times given positive responses when tried on their skin. Horse scurf from such material would also find its way into the much dreaded ' house-dust '.

Horse emanations are evidently harder to avoid than one would have supposed.

Cats. Of the other domestic animals cats perhaps cause the most trouble. Here again it is the cat scurf which causes the trouble and not so much the seeing of the animal as some seem to suppose, or even the legal ownership of it. Where a cat has slept, e.g. on the hearthrug or in some comfortable armchair, there will certainly be plenty of cat scurf. They lick themselves clean, but not the places they have slept in.

Cats are particularly elusive animals and don't always advertise their visits.

The Wayward Cat. A little boy aged five with a tendency to bronchitic asthma in the winter had occasional unexplained bouts of asthma at all times of the year. Testing the skin with a considerable number of foreign proteins, a strong response to cat's scurf was obtained, and also a corroborating small response to my animal group pathophane, which of course contains cat. Though I didn't imagine that cat scurf could be the main cause of the little boy's troubles I warned the mother that, without making too much fuss about it, she should try to keep the child clear from cats. She told me rather haughtily that she had always taken good care that cats never came anywhere near her little boy—I might be quite sure of *that*, she said. I felt abashed ; but, in the pause that ensued, the child threw me a lifeline by saying, ' Well, Mummy, one was sleeping on my pillow all last night. It came in through the window.'

Just as the horse-asthmatic is sensitive, more or less, to the other members of the equidae, so a person sensitive to cats is sensitive to other branches of the cat family, for example to lions and tigers ; the ' Small Cat House ' at the London Zoo is particularly fatal for them.

Dogs. Much the same tale might be told about dog scurf, and a person who is sensitive to a foxhound will be sensitive also to a Pekinese or to an Alsatian, let us say. Furthermore, people who are sensitive to dog scurf are sensitive more or less to foxes, wolves, etc., and are usually a little sensitive to hyenas even. Mothers often argue that a child can't be sensitive to dogs (or, as the case may be, cats) because the asthma occurred long before the animal came to the house. That is the ' single cause fallacy ' again, of course.

I needn't work my way through the whole animal kingdom. Enough has been said to show the sort of hints that should be given to patients or their friends when avoidance is being attempted. Once again, it must not be assumed that because a positive skin test has been discovered, say, to dog scurf, that therefore the dog is the cause of all, or even any considerable part, of the patient's troubles, or that if such scurf is successfully avoided all the troubles must cease. That would only be true when this dog scurf is responsible for one hundred per cent. of the patient's troubles, and that is very rarely the case.

The indiscriminate abolishing of all animals in the home directly anybody in it is suspected to have a little asthma or other toxic idiopathy is a thoroughly bad policy. It is teaching people to think of themselves as abnormal, and if they are not sufficiently critical of their elders' lack of logic it may cause them to live a life of apprehension ; in all probability the animals that have been abolished are perfectly harmless. When a child has been needlessly and fussily deprived of some pet animal by the too solicitous mother, it is sometimes wise to prescribe a cat or puppy according to the child's taste, in order to get the obsession of abnormality well out of the mother's head. But that is psychological treatment : Chapter IX.

Before leaving this question of animal scurfs it should be mentioned that furriers and people who deal with cured furs, making them up into clothing, are very often frightened that this may be the cause of asthma. I've often been asked to make special extracts for testing patients from specimens of skins that people have brought to me. Usually these ' autogenous ' extracts give a poor or negative result when tested on the skin, and I suspect that carefully prepared furs have usually very little scurf on them. I believe that the trouble they cause really comes from

the mechanical irritation of short hairs that may break off, rather than from the response to the animal's scurf acting as an idiotoxin ; as we shall see later in Chapter VIII this question of mechanical irritation, rather than chemical reaction, runs all the way through the problem of the airborne idiotoxins.

Food Idiotoxins. As we shall be seeing later on in this chapter, alleged food idiosyncrasies are not always what they seem, i.e. are not always a question of allergy. True allergic poisonings by food are chiefly to be found in children and more especially in small babies. Probably the strong-minded mother who bids her child not to be dainty or wasteful is often desensitising this child to a slight allergic idiosyncrasy by making it clean up its plate. Everyone, I suppose, has noticed how chary most children are of tasting any unaccustomed food, especially away from their own home ; clearly this may be an inherited instinct of self-preservation in avoidance of poisoning, but amongst people with an allergic diathesis there is often an uncomfortable sensitiveness to several foods. A grown-up patient with a well marked idiosyncrasy to some particular food will usually tell you all about it before you can get him or her seated in the consulting room chair, but about their children the mothers are not so certain because of this instinctive faddiness ; often, of course, the strong likes and dislikes are not allergic at all, and none of them should be taken as genuine food allergies unless backed by a reliable skin response to the specific protein food in question.

Genuine Food Idiosyncrasies. If any food is detected as producing a genuine toxic idiopathy, then avoidance of that food is an obvious method of treatment, and will be the plan usually adopted at least for the time being or until some other method of dealing with the trouble can be arranged. This avoidance may be difficult or a nuisance. Certain basic foods, e.g. eggs or milk, wheat or barley, beef or mutton, are always cropping up in the dietary and they often appear on the table as an admixture in the cookery, quite unsuspected by the non-cooking person. Beside this there are the foods sold in bottles, tins or cartons under fancy names like ' Tono-Bungay ' : many housekeeping mothers nowadays just don't know what the food really is that they are giving their household, and this may make it hard to trace exactly what it is that is causing the trouble in one of these proprietary foods.

Marmite Lady. A woman came to see me who had had a violent crop of nettlerash ' from eating Marmite '. She had been told by a vegetarian friend ' it can't possibly hurt you, dear, as there is no meat in it whatsoever'.

By a simple test on the skin I found that Marmite itself did produce the urticaria ; and on asking the Marmite authorities I was very courteously told just what was in their product . . . all quite harmless, of course, and, as the vegetarian said, containing no meat. Of these ingredients I found it was the yeast put in to supply vitamin which was causing the trouble in this person of abnormal sensitivity.

Idiosyncrasies confirmed by positive skin tests, and of some considerable intensity, are most frequently to be found with the very common nursery foods, and of these I suppose that milk, eggs, fish, oatmeal (in that order of precedence) are the most common.

A sensitivity to cow's milk is naturally the most serious problem with very young children. Human milk is the best, but asses and mare's milk or goat's milk

can usually be substituted for cow's milk if obtainable—and if the patient is demonstrably not sensitive to them.

The farmer supplying unusual milks often thinks the whole business is nonsense, and seems incapable of understanding that the fraud will be detected if he ekes out a scanty supply of goat's milk, let us say, with ordinary cow's milk. Denatured cow's milk is on the market, but in my experience this has not been so successful with children as the milk of an animal to which they are not sensitive, if you can find it.

Eggs are notoriously a frequent cause of trouble, and it is usually the white of the egg rather than the yolk to which the patient is susceptible, though not invariably. Also I have known patients who by prick-testing were sensitive to hens eggs, but not to ducks eggs and vice versa. As a rule, however, if they are sensitive to one kind of egg they are more or less sensitive to others.

Patients often say that the harder the egg is cooked the less it upsets them ; that is, of course, a denaturing of the egg protein by heat.

' Flat Eggs.' A five-year-old boy gave a small wealing on being tested with white of egg, and the mother said she didn't think he really liked eggs. ' Do you, or don't you, like eggs, Brian? ' ' Well, I only *really* like them when they are very flat.' The mother interpreted, ' He likes them, doctor, if I fry them very hard and then I turn them and fry them very hard again.'

The boy was treated along other lines ; but the mother was directed to keep him off all egg food till the asthma was more under control, then to return to it cautiously ; above all she was not to make it a matter of comment in the home.

Mothers are sometimes surprised that eggs in cookery, for example in a cake, have the same effect as that produced by an egg eaten knowingly as an egg. I have heard them say, ' Well, that *is* surprising, and of course it accounts for his not being able to eat sponge cake.' Also they have sometimes to be warned that dried egg is just the same chemical material as egg in the shell.

For another story of egg sensitiveness, see p. 247.

Fish. An idiosyncrasy to fish is also very common, and perhaps deserves special mention. Parents, after they have been shown a positive skin response to fish myoplasm on their children, will declare that they know all about that, and have carefully avoided giving the children any fish ; but they are surprised to hear the fish oils given as a tonic will have the same effect. They take this news especially hardly when the fish oil has not only been prescribed by some medical man, but it is also disguised under some proprietary name which doesn't suggest fish to them or to anybody.

They are sometimes surprised to hear that lobsters, shrimps, oysters, etc., are *not* fish, and that these things have their own special idiotoxins, related (some of them) more to the insect world than to fish.

I should say that shell-fish in general, though they are usually classed in the public mind as detrimental in character, chiefly perhaps because they are not so often seen in the nursery, are really much less often allergically harmful than real fish. On the other hand, though a patient may not be specifically sensitive to them, they act as lymphagogues and so may tend to produce oedema in the patient.

Oatmeal is popularly supposed to be ' heating to the blood ', which I suppose means inclined to give people urticaria ; judging by the response to skin tests it must be considered a fairly frequent cause of trouble.

Before giving advice about any particular food it is highly desirable to make up one's mind as to the way in which that food is prejudicial to the patient. Food, in addition to the allergic sensitivity to it, may upset people in very many and unsuspected ways, and these all tend to get muddled up together. (i) There is indigestibility and biliousness. (ii) Then people have fads and fancies about food. For example, some people loathe artichokes or parsnips. A child dislikes fish because of the bones ; he may avoid meat because it may need hard biting of tough meat, and his gums are sore. It is useful to ask if a child likes gravy (i.e. extract of meat) or if he likes mince meat. Very often the mother who has just declared that the child won't touch any meat will at once say ' Oh, he *loves* mince '. (iii) Food may upset by bulk and weight in the stomach. As we all know, a distended stomach sends distress signals via the vagus to the central nervous system after any heavy meal and we feel uncomfortable ; but, if the person is of an allergic diathesis and is at the moment trembling on the brink of an attack of asthma, those signals of distress will quite easily precipitate the attack of asthma. Such patients frequently say ' If I am well I can eat anything, but if I am inclined to be asthmatic any food upsets me no matter what it is '. With such people there is very rarely a definite skin response to any food prick-test. (iv) Decalcification by some vegetable acids such as rhubarb or strawberries may easily precipitate an attack of urticaria, hay-fever, etc., which might otherwise have been avoided (see Chapter VII). (v) Perhaps, however, suggestion plays almost the biggest part in producing a toxic idiopathy from food. When a patient's mind has become suspicious of any particular food, whether on the advice of some cautious relative, or some fussy interfering neighbour, or sometimes even by a doctor, that is the time to clarify the situation by some careful skin testing.

Tomato Obsession. I had a patient once who told me that she had become so obsessed with the idea of trouble from eating tomatoes that she had on several occasions derived a sharp attack of asthma from suddenly seeing an advertisement on the hoardings picturing a gigantic but realistic tomato—her established *bête noir*.

I found that this woman was quite insensitive on the skin to tomato proteins. Possibly on one or two occasions the acidity of the tomato juice may have precipitated an attack when she was hovering on the verge of one from other causes, but the effect of suggestion is more likely. Anyway, the completely negative skin test removed all fear of this, for her, quite harmless fruit ; she was thereafter able to see or even eat them with impunity.

Too Careful! On the whole I should say that people do almost as much harm to themselves by unnecessarily avoiding something which is harmless as they are likely to do by failing to avoid something which is really harmful. The can't-be-too-careful school of thought is wrong, and nowhere more wrong than in the matter of food.

In the course of years, simple-minded people who believe all that is suggested to them, often collect such a list of foods that are taboo that very little is left for them to live on ; when a patient produces such an interminable list of his supposed

specific poisons, it is almost a safe bet that he is not sensitive to any of them. After I have proved this by copious negative skin tests, I have sometimes sent the patient off to a restaurant with instructions to eat as complete a collection of these supposedly dangerous foods as they can manage to cram into the menu—that was in pre-rationing years of course. They came back rubbing their hands, saying that they felt all the better for this adventure. But it wasn't eating the food which had done them good, it was clearing from their minds all the fuss and bother, guilty feeling or wistful regret, that they had been experiencing over long years while looking at a well-filled plate ; in fact, the effect here is psychological, but none the less worth having for all that.

Dubious Idiotoxins. There are some allergic bogies about ; and the chief of them is perhaps ' house-dust '. I believe that for the most part these substances don't cause much trouble by idiotoxic action but—as we found was often the case with foods—they produce their effect in some other way, e.g. by chemical or mechanical trauma on an already very sensitive and inflamed mucous membrane, or perhaps they bring on attacks by mere suggestion from kind friends—including the doctors. As bogies, beside leading to much futile treatment, are particularly undesirable for the patient with an allergic diathesis, let us take them in some detail. The best thing to do with a bogy, if it really is a bogy, is not to avoid it but to walk through it.

Furriers Asthma. I have spoken above, when dealing with animal scurfs, of furs and pelts irritating mechanically the inflamed and oedematous upper air passages. The scurf of the animals from which the pelts came will, of course, act as an idiotoxin on anyone who happens to be sensitive to that animal, but hairs clean of scurf can have little or no chemical action. They may, however, be intensely irritating if short hairs are inspired into the nose or throat. Treatment must be by avoidance and the clearing up of that throat.

Feathers. These again consist of keratin which is, chemically speaking, a similarly inactive material. Horn cups do not spoil the flavour of wines, and that is a very delicate test of chemical contamination. The curious thing is that ' feathers ' (after perhaps 'the pollens ') are the material most often accused of being peccant idiotoxins. I make it a routine practice, therefore, to test patients with the best extract I can make from feather fluff, or better still from the skin scales of hens. I will give some practical details about this extract in Chapter XIV.

I find that most patients (though not all) give no response whatsoever to my feather pathophane, not even those who have been pronounced allergically sensitive to it. I therefore tell the patients who have failed to react to it, or their mothers, not to bother about feathers in the pillows, etc., because such bothering will do more harm than good in all probability. When I say that, I am frequently told by the mothers that they have long since put the feather pillows back again in spite of their doctor's orders, and have found that it made no difference.

There does remain, however, the mechanical action of the feather fluff, and this is difficult to avoid.

If, in the early morning, there is a beam of strong sunlight passing through a dark room, that beam will be invisible so long as there are no particles of solid matter floating in it. If, then, a pillow just under the beam is patted with the hand, that beam of light, hitherto almost invisible, will at once become milky white by reason of the millions of minute particles of featherets which have shot out of the pillow into the sunlight. If the pillow is kept quite still, then the milky beam will gradually become invisible again as the particles fall out of it onto the floor or bed.

Therefore the directions often given for avoiding these particles of feather fluff from pillows are ludicrously inadequate. The ' feather sensitive ' person is often told to lay a piece of jaconet over the pillow before he puts his head on it. So long as the jaconet covering allows air to pass out of the pillow it will carry particles of feather into the room ; if the jaconet covering is made airtight—which is a difficult thing to do—then you have made an air cushion. It would be better to buy an air cushion straight away, and there are some few feather sensitive people for whom this is advisable. It must be remembered that feather quilts or other feather pillows in the same room must be left undisturbed if the air is to be kept clear of particles therefrom.

I remember the case of a rather neurasthenic woman who told me she had been cured of her asthma after she had abolished all feather pillows from her bedroom. She was at the moment lolling against a sofa cushion from which a white feather or two could be seen protruding slightly. That room had a dozen cushions in it, and on inspection they were all found to contain feathers.

' Oh dear,' said she, ' I do hope I shan't get asthma again now! ' ' Well, if you do, it will be clearly the *idea* of the feathers, not the feathers themselves, which does the harm.'

She was able to walk through that bogy.

Sheep's Wool is often accused of causing a rash on the skin, or causing asthma if there are blankets in the bedroom. Here again the wool particles are often, but not always, wrongly accused of acting as an idiotoxin. I find most toxic idiopathy cases are insensitive to my wool pathophane ; but as people are not so often told to eliminate all woollen articles including blankets from the bedroom, I don't find it so necessary to make routine tests. Yet if the beam of sunlight test as suggested for feather pillows is applied to blankets, they will be found to set free plenty of particles too.

Here again the trouble they cause must usually be mechanical. Fortunately they are not very much talked about by doctors—probably from the difficulty of doing without blankets.

Dust from a chaff-cutting machine is very often implicated by farmers, farm labourers and land girls : the cloud of dust which inevitably rises from the machine gives them asthma immediately, they say. What is chaff? Chaff consists of a collection of grain husks, the bracts of the grass flowers, and the finely chopped particles of hay or straw. The chaff dust is made up of microscopic particles of this collection, mostly nasty spiky little bits of dried-up grass or straw, and anybody who has seen a chaff-cutter at work in the evening sunlight will remember the milky-white cloud of particles surrounding the machine. These are well calculated to tickle any nose or throat, and especially the oedematous air passages of the patient

who is trembling on the brink of a rhinorrhoea or an asthma. When the sufferer sends to the laboratory a little of that dust for extraction, I have found over and over again that the extract is quite inert on their skins. The asthma they get from it is not derived from a specific sensitivity to the protein of the dust, but to those jagged particles of hay or straw which act as traumata. Avoidance of chaff, while the throat is being cleared up bacteriologically, is the best line of treatment.

House-dust. Dust out of doors consists chiefly of finely powdered particles of earth or other matter, except in special circumstances, as, for example, near a chaff-cutting machine.

Indoors, save for the mud off boots, the earthy matter is replaced by fluff, hairs from carpets and felt undercarpets, hangings, blankets, pillows, etc. Also house-dust includes the scurf from domestic animals including man, and such things as wheat flour in the kitchen. House-dust may include face powder—but this to a much less extent than is suggested in the literature. Naturally anything, e.g. grass pollen in due season, which may have blown in through the open window, may be found in house-dust.

This is evidently, as I have said, a most heterogenous collection which would differ from house to house, and also from month to month during the year. We cannot reasonably treat this as a fixed chemical substance or even a fixed mixture of substances which we can extract and file away as a test for sensitiveness to ' dust ' in general. Nor can we use such an extract as a standard idiotoxic vaccine for use with people who are believed to be dust-sensitive. To my mind that procedure is clearly ridiculous, though I find it is being done every day.

Music-hall Dust. A year or two ago at the Asthma Research Club of London, I asked a prominent member who was very dust-minded where he got his material from for making the pathophanes and idiotoxic vaccines for ' dust ' in general.

He told me he got it from a well-known music-hall, which he named ; when I asked him why he got it from there of all places, he said, ' It's splendid stuff, Freeman, everybody reacts to it ! ' He was an honest simple-minded man, and I am sure he had no idea he was being somewhat illogical.

Dust is claimed by many people to be a cause of their asthma, etc., and therefore they are said to be dust-sensitive. Many of them bring letters from doctors stating this sensitivity. Usually it isn't even their own house-dust that is accused, but dust in general, which is, as I have said, an entirely fortuitous collection of fine particulate matter—some of it organic and some of it not.

I don't usually pay much attention to this dust story, except, of course, to tell people to avoid dust if they find it gives them trouble ; but I do look for the conditions which are making the irritation from dust so offensive to them in particular.

' Autogenous ' Dust. If patients or their doctors are very persistent, I sometimes get them to send me some of the dust, carefully collected in a vacuum cleaner, from the room which seems to cause them trouble. An extract is then made from it, as described in Chapter XIV, and this is carefully tried on the skin of the patient by the customary prick test ; it is necessary also to make a similar test on one or two normal people. As a rule both patient and normal controls give a positive

wealing, and frequently the normal people react more strongly than do the patients who are thought to be allergic to that dust. When this happens there is no evidence whatever, of course, of any specific sensitisation.

In point of fact, dust usually contains poisonous material, possibly some substance ' of a histamine-like nature ', which will poison (and so irritate and damage) all skins and all mucous membranes. That of course does not preclude any dust from being used as a general, that is to say non-specific, antigen ; I feel strongly, however, that if we are to embark on dubious and discredited non-specific immunisation, it would be far preferable to make an antigen from known substances, e.g. peptone, cow's milk, typhoid vaccine, or ' whole blood ', rather than make a random use of that heterogenous and often poisonous substance ' dust '. I may add for reassurance of the timid that the doses of dust extract now being prescribed for treatment are not likely to do any noticeable harm—or, of course, much good either.

Proteins in the Dust. On the rare occasions when there is a real specific response to a dust extract pathophane, that is to say when the supposedly sensitive person gives a definite response on the skin to the prick test, but normal people give a much smaller response, then we must suppose that the patient is sensitive to some particular ingredient in the dust. It might, for example, be due to some animal scurf which clearly should itself be detected by the appropriate specific skin tests.

Dog-dust Girl. I remember the case of a young girl who was sent to me certified as allergic to dust. I naturally asked her the dust of what room in particular she thought caused her trouble. She said the dust of the office of her father, a land agent, for whom she had been acting as secretary. When I asked her to describe the room, she said, ' Well, the first thing that happens when you go into it is that you trip over a dog or two : there are usually five of them lying on the floor.'

A fairly comprehensive series of skin testings showed that she responded to dog extract, and apparently to that only. A thoroughgoing desensitisation course to dog scurf ended this girl's alleged sensitiveness to ' dust '.

Mould Spores. The occasion when a ' dust ' extract is best worth using as a pathophane, and perhaps subsequently as a vaccine, is when there is good reason to suppose that the dust complained of contains a large number of mould spores such as would be derived from moulds growing on the walls of damp cellars, in damp cupboards, or on the adhesive paste behind damp wall-paper.

I have several times also found a thick felt-work of mould mycelium underneath a linoleum or cork floor-covering. As with the wall-paper, it is the adhesive paste (used to fix the covering down on the floor-boards) which offers food to the moulds ; a daily scrubbing by a conscientious housewife will keep this paste wet enough for mould growth.

Dry rot in the rafters and joists also provides plenty of mould spores in house-dust.

A toxic idiopathy derived from mould spores is usually to be suspected by the sharp localisation of the trouble. It arrives only in one or two damp houses ; very often trouble is experienced only in one particular room of a house, and there is immediate relief on going into another room.

Usually, of course, such very sharply localised trouble is dealt with by avoiding the locality, and by attacking the other causes of the toxic idiopathy as in the case of the airman in the anecdote given below.

R.A.F. and Dry-rot. Between the wars I was asked by an R.A.F. high-up to pay special attention to the asthma of a young man whose brains were urgently needed for the then rapidly developing Air Force. He had suffered from fulminating asthma on three occasions when sleeping for the first time in picturesque, but damp and mouldy cottages ; he had a considerable amount of residual asthma at other times too.

Culture plates were exposed many times in all three of these cottages, and cultures were made from the multiplicity of different moulds which grew thereon ; but I could never determine which of these different moulds was causing my patient's trouble in that particular house.

Meantime, the legs of a heavy grand-piano gave a strong hint one night by disappearing through the floor-boards of the worst of the cottages. The place was riddled with dry-rot ; but try as I would, I never managed to get the spores of that dry-rot for his treatment, or even for his specific diagnosis.

Meanwhile, the patient was able to keep at work by avoiding those damp houses and having the other causes of his asthma attended to.

Tomato Moulds. When tomatoes are being grown in large quantities in humid hothouses, moulds develop on their leaves, and this to such an extent that the asthma they cause amounts almost to an occupational ailment among the growers and pickers. The patients, usually professional tomato growers, naturally accuse the tomatoes and especially their pollens, but there is little doubt that it is the parasitic moulds which are usually at fault.

Unfortunately, the mycological story is complicated and it is not easy to grow and collect spores from the peccant moulds.

It would be preferable to make specific mould spore extracts for idiotoxic vaccines for a desensitisation course ; meantime a careful collection of dust from these houses does offer a substitute vaccine, though not a very satisfactory one. To date I have found the best treatment is avoidance of the mould spores, i.e. change of occupation, combined with treatment of adjuvant factors, e.g. gut sepsis.

Out-of-door Moulds. Out-of-doors, the damp and rotting leaves of the trees are supposed to be the main source of mould spores, but they don't, in my experience, give anything like so much trouble as the moulds growing in confined spaces. I have never yet been able to make a positive idiotoxin diagnosis in such a case ; treatment has been by dealing with adjuvant factors—and avoidance.

' Proteose ' (so called) should, I think, follow ' dust ' as an allergic bogy ; it was a heterogeneous mixture of substances which had little to do with the proteoses as the chemists know them. ' Proteose ' was never a clearly defined chemical entity. It was what came down when the patient's urine was treated with ether and alcohol —(but the methods for gaining it from the urine varied from time to time). It was put forward as a personal idiotoxin for the desensitising of any case suffering from the allergic disorders ; even nowadays I get patients announcing with pride that they have been treated with doses of proteose by their practitioners.

The idea was that the kidneys of a patient suffering from asthma, hay-fever, or apparently any of the toxic idiopathies, would naturally try to excrete from the blood-stream the peccant material ; this, when gained from the urine, could be re-dissolved and used as a desensitising agent—could, in fact, be treated as the specific idiotoxin for that patient.

The theory depended on the specific reaction of the patients' skins to their own proteose, and this was declared to be very exact. Unfortunately this was not the case ; the error resulted from a faulty reading of skin responses, combined with a bad technique for producing them.

' Proteose ', as one might expect from its origin, is universally slightly toxic ; it gives no more reaction on the patients' skin than it will give on any other allergic patient, or on any normal person either.

These results are clearly very similar to the results obtained by skin testing with most dust extracts.

CHAPTER V

IDIOCEPTOR: THE ALLERGIC SENSITIVITY AND HOW WE MAY DEAL WITH IT

The idiotoxin, the second item in our short list of causal factors, we dealt with in the last chapter.

The idioceptor is that specific quality or attribute in his living tissues which makes the patient sensitive to the idiotoxin; this we are to discuss now.

The Idiotoxin-Idioceptor Couple. An idioceptor is specific for its idiotoxin, and vice versa. 'Idiotoxin' and 'Idioceptor' are therefore correlative terms. They have a reciprocal relation, such that one is complementary to the other: they are reciprocants. Just as we saw in Chapter IV that by removing or altering the idiotoxin we could make the idioceptor harmless, so if we can remove or nullify the idioceptor of a patient, we shall make the idiotoxin to which he has previously been sensitive become of no account to him.

This specificity of idioceptor for idiotoxin is not absolute. Its limitations are discussed in Chapter XV.

Proof of Idioceptor. Let us look more closely at the idioceptor before we discuss how we may deal with it, and so produce what the patient would call 'a cure'. There must be something which differentiates, for example, the hay-fever subject from the non-hay-fever subject; we can prove that this something resides, in part at least, in the blood. If a blood transfusion is given from a sensitive to a normal person, that normal man becomes slightly and temporarily sensitive to the specific idiotoxin; this can be proved by skin tests, or by the more sensitive eye tests, etc.

If the serum from a sensitive person is injected into the skin of a normal person, then the piece of skin so treated becomes quite strongly sensitive and remains so for a considerable time. This, the Prausnitz reaction, is described in Chapter XIII, p. 241; in fact, that chapter is devoted to a fuller discussion of the idioceptor and its ways.

It is clear from this Prausnitz reaction that the attribute or quality which I have called the idioceptor resides, in part at least, in the plasma or serum. Though we cannot, in our present state of knowledge, isolate an idioceptor, or even know if it is isolatable, we can surely detect its presence by its action when it meets its reciprocant, i.e. the specific idiotoxin.

Furthermore, if we take some serum from an allergically sensitive person and blend this with an extract containing its reciprocant idiotoxin, we shall have an oedema-producing poison mixture, which will produce its effects in a few minutes into whomsoever it goes—whether specifically sensitive or not. All this, as I have said, is discussed much more fully in Chapter XIII.

If our hypothetical idioceptor is capable of producing its effect after being injected into any normal person from a syringe, it must be something physical, and something which we may hope to combat ; it is not a ghost of our imagining.

As Professor Collingwood might say (see p. 38), it is a causal handle which we may hope to manipulate to the patient's advantage, and this chapter and the next are to be devoted to its manipulation.

Exclusive Diagnosis Necessary. The idiotoxin and idioceptor are mutually specific each for each ; therefore, to attain success in treatment of the idioceptor by any immunological or phylactic method, we must once again make, not only the true, but also to some extent the exclusive diagnosis of our patient's peccant idiotoxin.

If half or three-quarters of a hay-fever patient's troubles are not due to grass pollen but to quite different idiotoxins, or to some intercurrent infections (pp. 222 to 227), then, as might be expected, we can obtain only a partial success at best by the most careful specific treatments. It is even possible that, after a technically successful desensitisation course to the selected peccant idiotoxin, no improvement will be experienced or admitted by the patient.

There is another cogent reason for this exclusive diagnosis. When we come in this chapter (pp. 82 to 83) to discuss the possibility of unpleasant reactions which may follow some of our desensitising doses, we shall find it is just these accessory and intercurrent factors which are often responsible for the trouble during the course of treatment. For simple and easy desensitisation we need, therefore, a well isolated idiotoxin-idioceptor complex, and an exact and exclusive diagnosis of it.

Hay-fever the Test Case. Fortunately, true hay-fever cases usually afford a sufficiently clear and exclusive diagnosis, and it is for this reason that hay-fever has been such an excellent exemplar for all our desensitisation treatment. Fortunately, also, that great pioneer Blackley was a true and uncomplicated case of this, our key disease to the toxic idiopathies. If he had been, in addition, cat-sensitive, or a chronic bronchitic, he would have had much more difficulty in proving the case against grass pollen for hay-fever when experimenting on himself in the late sixties of the last century.

Blackley. In trying to trace the beginnings of immunity work against hay-fever, I find that in the *British Medical Journal* of March 26th, 1898, there is a letter from Blackley who had published his monograph on hay-fever twenty-five years previously. In this letter, after answering some criticism, he goes on to say :

> In addition to the facts given above, I have for the last fifteen years been engaged on a set of experiments on the subcutaneous injection of the active portion of the pollen grain. These have given much the same results in the matter of dose and in the matter of symptoms as my other experiments. These have not yet been published, but I hope soon to do so.

So far as I know, this work has never been published : Blackley died in 1901.

It is difficult to say whether the experiments referred to here were really in the nature of diagnostic tests by intradermal injection (i.e. were an experimental

but localised production of the disease in hay-fever subjects (see p. 254), or whether he was actually attempting desensitisation. Even in 1898 he would have been rather in advance of his time if he were embarking on active immunity, though Wright was then working at prophylactic typhoid vaccines ; but Blackley in the passage quoted says that he had been working at this subject for fifteen years, i.e. since 1883, which would have been still more in advance of his times. So it is more probable that these researches were not on active immunisation but were simply experimental productions of localised hay-fever, i.e. were in the nature of diagnostic tests.

Passive Immunity. In 1903, i.e. about five years before Noon and I started work on hay-fever, Dunbar of Hamburg was working at this subject ; at that time and place passive immunity by antitoxins and serum-therapy in general was more esteemed than active immunity. So Dunbar naturally attempted to make an antitoxin to hay-fever by inoculating a grass-pollen vaccine into horses or other animals ; the resulting serum, or the dried powder made therefrom, he called Pollantin ; and this was applied to the inflamed eyes and noses of the hay-fever patients.

One heard various reports of its efficacy : ' it seemed to do some good in certain cases '—a result attained rather readily when any scientific innovation is applied to despairing and perhaps hysterical patients. It was frequently reported that the application of Pollantin for the second or third year was even less successful, and some disappointed patients said it seemed to make them worse than ever. We may, perhaps guess now that, in the passage of time, this novelty in treatment had merely lost its emotional ' kick ' ; but Dunbar ascribed its comparative failure in succeeding years to a horse-serum anaphylaxy. Anaphylaxy was then very much in the immunological limelight ; we didn't then know its very strict limitations when applied to man. I believe Pollantin is seldom or never used nowadays.

Dunbar seems to have missed the point that hay-fever was a constitutional and not a local disease, but his work does illustrate the fact that in 1903 hay-fever was recognised as being, in part at least, an immunological problem.

Earlier Active Immunity. When in 1907 my boyhood's friend Leonard Noon posed the problem of hay-fever treatment to the people then working in Sir Almroth Wright's laboratory, it was only natural that we should all jump to the idea of active immunisation with injections of the ' peccant ' material, i.e. the use of a grass-pollen vaccine. About ten years previously, Wright had launched on the world his successful prophylactic inoculations for typhoid fever, and in 1908 we were all hard at work making the recently formed ' Inoculation Department ' of St. Mary's Hospital into a centre for Active Immunity.

Noon, with his botanist sister, Dorothy Noon, devised methods for collecting grass pollen which still hold the field ; they are at any rate still used by me (Chapter XIV). Noon began experiments with this pollen on friends with hay-fever, and made tentative casts over the work described by Blackley in 1872.

In June 1911, Noon, after some experiments, wrote from what proved to be his death-bed his ' preliminary ' and, alas, also final paper on hay-fever ; in this he discussed the practicability of immunisation against the effects of grass pollen in

hay-fever subjects. He showed the effect of small doses of a pollen extract by making a series of tests of sensitivity on the eyeballs of the patients with this same extract, but in extreme dilution : he was thus observing the dictum of our chief, Almroth Wright, that to have certitude of success in attempts at immunisation one should if possible control the effects of the inoculation doses by means of laboratory tests.

In September of the same year I published the ' clinical results ' of some twenty cases I had been inoculating from about a year previously with a grass pollen vaccine. As now I read again this old paper of mine, I realise with a sigh how unnecessarily difficult I made that first test : more than half of the cases were inappropriate, or had unsuspected pitfalls which anyone might now avoid. However, the benefits received by those patients (as tested by the hay-fever season of 1911) were not too discouraging ; the most encouraging part to *me* was that after any considerable amount of treatment an increased resistance on the part of the patients to the effects of the pollen could be demonstrated by our experimental eye tests. To this day, skin tests controlling the effects of treatment remain the basis of all our prophylactic desensitisation—as will be described on pp. 73 to 76 of this chapter.

Later Results. I will not indulge in what is derisively called a ' total recall ' of desensitisation treatment in those thirty-five succeeding years ; the present highly successful, though perhaps tiresomely lengthy, prophylactic treatment for hay-fever, etc., has been gradually built up year by year from that not so very auspicious start in 1910 and 1911 by steady improvement of technique and method. For the last twelve years or so I have not thought it necessary to make any material alteration ; and nowadays, if we are careful in choosing only suitable cases by making the necessary *exclusive* diagnosis, and if we accomplish the full treatment by methods to be described later, we can claim perhaps a 95% success in prophylactic treatment when our work is subsequently tested by the yearly recurring grass pollen cloud at midsummer. I think that if we were clever enough or careful enough to make the exclusive diagnosis of hay-fever even more exactly, and to detect still more of the handicaps to prophylactic treatment, or were more ruthless in refusing to treat the more complicated cases, we might cut down very considerably, or even completely, on that remaining 5% of comparative failures.

If **Phylaxy**, which should we choose : *pro*-phylaxy or *hama*-phylaxy (i.e. to indicate synchronous therapeutics? Without question, for any immunological treatment, we should choose prophylaxy if possible ; and it is strictly for prophylaxy that we can claim results anything like what are given in the paragraph above.

Perhaps our original idea, and certainly what our patients expected of us, was that we should wait till they had developed hay-fever, and then treat it by an injection of grass pollen extract. Such therapeusis or, as I should prefer to call it, hamaphylaxy (not *ana*phylaxy) has unfortunately only limited use. It is as if we waited till our soldiers were ill in hospital with typhoid fever before we thought to give them a dose of typhoid vaccine.

Although we knew from our experience with typhoid inoculation that prophylaxis was far easier than hamaphylaxis, yet I found it difficult to avoid getting the

two types of treatment muddled together in that first essay at immunisation in 1911 which I have mentioned four paragraphs ago. Our profession demands that we take on all comers when they come to us for help.

Surreptitious Prophylaxis. Even now patients will come to us in June with eyes red and streaming from hay-fever, and ask us to give immediate help ; so I should first perhaps discuss how we might deal with such cases before proceeding to the more satisfactory Prophylactic Thoroughgoing Desensitisation (' P.T.D. ') treatment. When the patients are already experiencing hay-fever symptoms, such thoroughgoing treatment cannot of course be given till before the hay-fever season of the *next* year ; that may seem an unconscionable postponement to the suffering patient.

The first consideration is whether or not we can give something approximating to prophylaxis even after the grass pollen cloud has arrived to plague the hay-fever patient ; and the first step is for the patient to try to avoid the grass pollen as much as possible, as was described in the last chapter (p. 45). If we can successfully dodge the peccant idiotoxin—even grass pollen in June—with any considerable success, we can then, if we wish, make some attempt at a prophylactic treatment.

The pollen cloud is never continuous from mid-May to mid-July. There are pauses, for example, due to cold and wet weather in June ; but, as explained in the last chapter, it is difficult to escape from grass pollen in an English midsummer for long enough to do much with treatment unless the patient chooses to live in an air-conditioned and air-filtered room for some considerable time—such a room as one of Professor Storm van Leeuwen's ' asthma chambers ', which he instituted at Leyden. But, if it can be done, then something useful might be accomplished for the patient by, for instance, ' Rush Inoculation,' which will be discussed on p. 86.

Generally speaking, and except in special circumstances, such surreptitious prophylaxis during the actual hay-fever season is rather a hit-or-miss business.

Hamaphylaxy. Failing the above expedients for surreptitious or partial prophylaxis, can we do anything at all for hay-fever by inoculating a pollen vaccine when the disease is actually occurring? Yes, something perhaps : and though we can't make a clean sweep of it, as we should do with proper prophylactic treatment, we do occasionally seem to achieve such a striking success as to make the patient doubtful of the need for proper prophylaxis in the succeeding year. As we may guess, suggestion plays its part here ; but that won't account for all the benefit from these occasional small doses given during the grass pollen season.

Accurate and exclusive diagnosis is of course necessary even for this hamaphylactic treatment with a pollen vaccine ; but here we have the patient before us with the symptoms still on him, or at least with very recent and vivid experience of them. Even so, one should check the patient's statements by a skin testing with grass pollen and with other pathophanes (see pp. 15 and 92) but this testing is not so necessary as it will be for prophylaxis.

The difficulty is, of course, that when we are in the thick of the pollen cloud—say in June in England—we never know how much of it may, even then, be in close contact with our patient's tissues in the upper air passages or eyes, and also how

much of the pollen grains may be landing on those and other areas during the next few hours. Also we don't know if some trace of the idiotoxin may not have been extracted from previous chance grains, and be even at that moment circulating in the patient's blood-stream (see p. 250). We must, therefore, confine ourselves to small amounts of pollen extract—in fact, to doses round about the initial dose which we give for that lengthy prophylactic (' P.T.D.') course which will be described later.

So the strength of the doses should range around 40 or 50 Noon-units of grass pollen—a *unit* being the amount of extract to be obtained from a millionth of a gram of grass pollen (see p. 278). The hamaphylactic doses should range from 10 such units up to perhaps 100 ; the larger amounts are given when the patient is judged to be free of symptoms and the fear of them, and when the locality and the weather make the air likely to be pollen free. On the other hand, the doses should be cut down towards the vanishing point if the patient experiences any slight symptoms, or when the weather and the locality threaten him with the pollen.

It is a good plan to add a minim or two of the 1:1,000 adrenalin to the minute dose of pollen extract in the syringe, particularly of course when the patient is experiencing or is fearing any symptoms of hay-fever ; sometimes, naturally, it is better to give adrenalin alone and postpone the pollen dose.

Spacing. The length of the interval between these hamaphylactic doses depends on the amount given previously : a minute dose of 10 or 20 units could be repeated, say, within 48 hours ; a larger dose of 100 units in perhaps a week. Also these re-inoculations, and their frequency, must depend on the weather and the physical circumstances of the patient at that time : one must be an opportunist.

The Hay-fever Pattern of hamaphylactic treatment can be copied with any toxic idiopathy, and perhaps particularly with the para-hayfevers. Always there is the attempt to give the little doses when the idiotoxin is not threatening for the moment ; but there may be small opportunity to pile up the dose higher and higher, as in true P.T.D. treatment, because the intervals between the attacks are not long enough.

I am reminded of a strongly horse-sensitive woman patient who thought it was necessary for any really serious person to go fox-hunting at least twice weekly in due season. In the intervals I was allowed to do the best I could by hamaphylactic doses—which wasn't much. She told me that the sneezing and eye-running which she suffered at this, her life's work, didn't make for comfort or for hard riding.

This futile business went on for a long time ; then she came one day saying that luckily she had broken her arm by a bad fall in the hunting field, adding that now I could take my time over the treatment.

The orthopaedic surgeon co-operated nobly by spinning out his manipulations in a London nursing home, and we were able to switch over to prophylactic treatment with the usual good results. Then she could hunt in comfort.

In general, we must admit that hamaphylactic desensitisation is a poor substitute for proper prophylactic thoroughgoing desensitisation, for which it may be regarded perhaps as merely marking time.

Palliatives are clearly called for when venturing on this uncertain business of hamaphylaxy. They can do a great deal to restore that morale of the patient which is so important in the toxic idiopathies ; used in this way, they can give the little doses their chance. I hesitate to give details of such palliatives because any experienced practitioner will have his own ideas about them ; however, this seems to be the place to give them, if at all. I will put down what I have used on patients in my efforts to curtail symptoms, while giving the minute therapeutic doses of grass pollen—themselves possibly rather of the nature of a placebo.

Eye Lotions. For all exclusively airborne idiotoxins, such as grass pollen in hay-fever, the eye symptoms are usually the worst, as explained in Chapter I. These can be mitigated, or even ended, by the simple business of washing the conjunctival sac free of pollen grains with lukewarm normal saline ; the same holds good, of course, in any para-hayfever, whatever the airborne toxin may be. It increases the comfort and confidence of the patient if a little adrenalin is added to this very simple eye lotion. I prescribe it of double strength, thus :

Sodium Chloride 2% in water - - - -	9 parts
Adrenalin 1 : 1,000 - - - - - -	1 part

Patients are told to add to it in the eye-cup an equal bulk of warm water from the tap ; this gives the patient a lukewarm wash of the same osmotic strength as the tears. This should be used very freely whenever any pollen grains are worrying the patient's eyes.

Patients tell me that they have tried washing out the nose and throat with this lukewarm lotion and have derived considerable help from it. I suspect that repeated washings of the eyeballs must introduce some of the lotion to the nasal cavity via the lachrymal ducts : this itself would help to keep those ducts patent, and so keep the tears from spilling over the cheek. Unwanted tears have a bad psychological effect on the patient.

Nebulisers can perhaps be more easily used to distribute palliatives to the upper air passages ; they are convenient, especially at night, for the patients old or young to manipulate themselves, when they have enough self-respect and self-control to refrain from disturbing the rest of the household. Plenty of proprietary drugs are on the market for ammunition in these nebulisers ; I have used adrenalin for two generations, and I still think that, in the 1 : 100 strength, it is the best of them all.

Adrenalin is of course usually applied, not locally in washes or from nebulisers, but by parenteral injection. It is less upsetting to the internal secretions to give a number of *small* injections of adrenalin when these become actually needed, and to stop them when they are not needed, rather than to give one good ' shot ' of adrenalin as the practitioner is almost forced to do when called to a household with all the inmates in a state of panic in the middle of the night. I advocate small injections every 15 minutes for as long as may be needed.

Domestic Inoculation. For these repeated doses of adrenalin it is best, indeed it is almost obligatory, to teach the patient to inoculate him- or herself. If the patient is instructed to self-inoculate with adrenalin (using either the 1 : 1,000

FIG. 10

A box containing equipment for domestic self-inoculation with adrenalin. The technique to be taught the patient is the same as that taught for the P.T.D. course of treatment in Chapter VI.

NOTE: The 'nick,' referred to in the directions pasted into the box are each of 1/20th of a C.C.,—i.e. it is an 'insulin' syringe in this case.

strength, or the milder and therefore perhaps safer 1 : 5,000 strength as issued in the P.T.D. set for hay-fever) it is essential to teach some very simple and safe technique for it. The method of self-inoculation with pollen extract described very fully in the next chapter has been found easy to teach, and should provide a good basis for the instruction of the patient when inoculating these small doses of adrenalin ; it is, however, necessary, even in this simple business of adrenalin inoculation, that someone who really knows how to carry out this domestic technique quickly and efficiently should give the patients personal tuition and verify that the patient has confidence about it. The ' district nurse ' is usually too tradition-ridden to teach such a simple technique with facility.

If the self-inoculation of adrenalin is all that is to be taught, I issue a little box (as shown in Fig. 9) which contains all that is necessary for carrying out the technique in the way described in Chapter VI.

Ephedrine, taken by the mouth, is recommended by some because it avoids the really very slight difficulty of inoculation. The trouble is that the effect comes on slowly, so that people have time to get panicky ; and of course the effect can't be switched off when no longer required. I seldom prescribe ephedrine.

Benadryl is highly spoken of as an adjunct to or substitute for manipulation of the idioceptor by inoculation treatment. It doesn't suit some people, very often making them dopey and queer. It cannot be given in conjunction with narcotics such as luminal : if an attempt to do so is made, the patient experiences very unfortunate symptoms. Here I speak more from the experience of my colleagues in the Allergy Clinics, for I rarely prescribe benadryl myself.

Any soporific will damp down the emotions and restore the morale of the patient, and I see no reason, for example, why luminal should be more appropriate for epilepsy than for hay-fever ; it would be given for the same reason in both cases. But there is a danger in using powerful drugs like morphia to cut short an acute attack, say of asthma ; the patient may give up the struggle to breathe, and so asphyxiate himself.

Desensitisation—' P.T.D.'

To be really effective, a desensitising treatment, with its 50 or so doses, must be a ' Prophylactic Thoroughgoing Desensitisation ' : and this mouthful of polysyllables has long ago been shortened to ' P.T.D.' in our laboratory jargon, and on the case notes. To save our time, I have taken leave to call it P.T.D. here also.

I find that patients, and practitioners too, make a quick jump from ' Is this desensitisation treatment any good at all? ' to ' Is this treatment always completely successful? ' When the first question has been answered with an emphatic ' Yes ', and the second with the more cautious ' Yes, certainly, when the attendant circumstances are not too difficult ', we are often posed with the third—' Can't it be done without taking any trouble about it? ' and to that the answer is ' No '. Trouble is necessary to mitigate or prevent many unpleasant circumstances for the patient.

The chief difficulty is the large number of injection doses which should be given to attain 100% success without undue discomfort or even risk. The two or

three biggish doses which are given as prophylactic treatment for typhoid fever are quite inappropriate for hay-fever, which needs several dozen doses of gradually increasing sizes if the work is to be well done ; in my routine course of treatment, I regularly prescribe 54 subcutaneous inoculations carefully graded from 40 up to 100,000 Noon-units per dose. There is, of course, no special point in the number 54 ; I am recording what I have myself found to be the most convenient routine.

As with typhoid, we must take time by the forelock ; to stave off hay-fever at midsummer all these doses must be given prophylactically in early spring, i.e. just before the hay-fever season, yet at a time when there is little or no grass pollen about in that particular place.

Hay-fever is a model for similar treatment against innumerable idiotoxins ; it can be followed exactly, even to the number of units in the long series of doses which must be given for each complete desensitisation course. These idiotoxins can be any airborne protein matter, such as the pollens of the low-grade compositae, or of the trees which are wind-pollinated, or animal scurfs, or protein foods—in fact all the substances mentioned in Chapter XIV, and many more.

Mixed Idiotoxins. It is possible, and on occasion it has its advantages, to combine several idiotoxins for desensitisation in one course of P.T.D. treatment ; but the need for each addition should be as carefully considered, as was necessary for deciding on P.T.D. treatment of hay-fever itself. We have found that the commonest useful addition to grass pollen has been the pollen of the compositae as mentioned above, for these often prolong the true hay-fever symptoms till far beyond their proper end-point in mid-July. But any combination is possible ; thus, horse scurf or cat scurf can be combined with grass pollen if it is certain that this addition is really worth while. Horse scurf with egg protein : egg with fish : cow's milk with cat's scurf and grass pollen—in short, any combination can be made.

These extensive admixtures are, however, generally to be deprecated ; they can so easily become a ' polyvalent vaccine '—an omnium-gatherum carelessly thrown together to avoid the trouble of making a discriminating diagnosis. The novice at P.T.D. should undertake a singleton to begin with.

With all these variations of idiotoxin, there are according to our practice the same 54 doses to be used, the same rise in units for each succeeding dose, and the same precautions to be taken in giving them ; and there are also the same difficulties to be overcome.

For any course of treatment, either prophylactic or hamaphylactic, for any single idiotoxin, or for any combination of them, the model of hay-fever desensitisation is so closely followed that they need no separate description of method beyond what will be given for hay-fever in this and the next chapter.

Why 54 Doses? I have said above that I usually prescribe 54 gradually increasing doses for a thoroughgoing desensitisation. Our routine course for self-inoculation P.T.D. has necessarily become stereotyped (see next chapter) and it usually begins with a dose of 40 Noon-units and ends with a dose of 100,000 units. That 40-unit dose is about the biggest that can be given easily to start with in an

uncomplicated case; and the 100,000 units has been found to be usually a satisfactory ending. The tiresomely large number of doses in between these two extremes is necessary to make the passage from one to the other easy or even tolerable for the patient.

40	units
60	,,
80	,,
100	,,
120	,,
140	,,
160	,,
180	,,
200	,,
230	,,
265	,,
300	,,
345	,,
400	,,
460	,,
530	,,
610	,,
700	,,
800	,,
920	,,
1,060	,,
1,220	,,
1,400	,,
1,600	,,
1,840	,,
2,100	,,
2,400	,,
2,750	,,
3,150	,,
3,600	,,
4,100	,,
4,700	,,
5,400	,,
6,200	,,
7,100	,,
8,100	,,
9,300	,,
10,700	,,
12,300	,,
14,200	,,
16,300	,,
18,800	,,
21,600	,,
24,800	,,
28,500	,,
32,800	,,
37,800	,,
43,500	,,
50,000	,,
57,000	,,
66,000	,,
76,000	,,
87,000	,,
100,000	,,

FIG. 11

List of doses suitable for a P.T.D. Treatment.

The aim must be that no step upwards in dosage from 40 to 100,000 units is more difficult to take than another. It soon became clear that we should not increase by the addition of so many units each time; for if we didn't have much too big and difficult steps at the beginning of the series, we should certainly have much too easy steps at the end of it: and this would mean, of course, a longer series of doses than the 54.

A little reflection convinced me that we must for the most part increase by a *multiple* of the previous dose, and not by an addition to it. This has the effect of seeming to increase very slowly at the beginning, but to be increasing very fast indeed towards the end.

Clearly, the bigger the percentage increase, the fewer intervening doses need to be given to get from 40 to 100,000; whereas, if we made the climb too easy by a smaller multiple, the doses become more and more numerous. After many years of testing, I found that a 20% rise at each inoculation was too abrupt, and made the occurrences of 'reactions' too frequent; on the other hand, a 10% rise was too little, and was made at the cost of increasing the number of doses unnecessarily.

15% Rule. I find that 15% makes a good compromise, and, after several years of trying a little above and a little below this multiple, I am now keeping to just that percentage of increase. I find, for example, that it is no easier to increase from 200 to 230 units (an addition of 30 units) than it is to increase from the 20,000 to 23,000 (an increase of 3,000 units).

However, this 15% increase of dose is generally too cautious below the 200-unit dose level, where we are, so to speak, finding the range. Therefore, to save time, I make a bigger, but a rapidly falling, percentage increase from the first dose of 40 units to the 200-unit dose. This is achieved by the simple process of *adding* 20 units to the preceding dose at each re-inoculation. Thus the first nine doses are 40, 60, 80, 100, etc., up to 200 units; and the percentage increases are therefore 50%, 33%, 25%, 20%, 17%, etc.

In Fig. 11 is given the full list of 54 doses which comprise my routine P.T.D. course. These exact doses are not of course a law of nature; they are merely a compromise between the steady 15% rise from the 200-unit dose and a relatively round number of units: it is not practicable to measure out the volume

of the inoculum to three places of decimals, though with suitable syringes it is easy to manage two.

There is no particular reason why the first dose shouldn't be of 30 or of 50 units, and I frequently make it as low as 20 in sensitive and complicated cases ; but in general it will be found that a 40-unit dose makes a safe beginning.

The medical officer, in directing the P.T.D. course, may prefer to arrange his own doses ; but I urge on his attention the necessity of a *percentage* increase, and preferably, I think, of 15% rises.

Excelsior! As I have said, I aim at a top dose of 100,000 units. This is usually all that is needed for even the most difficult case ; but some of my patients have been so impressed at the ease with which they can take the highest numbers ' in spite of the number of noughts ' that they have been seized with the excelsior spirit and have wished to see how far they could go ; and, when I could spare the necessary pollen extract, I haven't thought it necessary to stop them. The top figure to date is, I believe, 700,000—i.e. 7 c.c. of the mother extract, which amount has to be put in subcutaneously out of a 10 c.c. hypodermic syringe. It is noteworthy that the patient experienced no more disturbance from this comparatively vast dose than he got lower down the scale with doses of 50 or 100 units ; in fact, to use his own phrase : ' it might be so much ditch-water.' Needless to say, he gets at this stage no symptoms of hay-fever whatsoever, even though walking through pollinating grass in the middle of June.

' Ditch-water? ' That mother extract of 100,000 units per c.c. is certainly not so much ditch-water—save of course to the non-hayfever person or to the efficiently desensitised patient. Those who try taking, as an experiment, more than they are advised to take soon find this out.

A very learned though rash scientist at Oxford University tried, without my knowledge, to see if I had been exaggerating the perils of an over-dose. So he took 20,000 units when he had been directed to take only 40 as an initial dose.

Within 8 minutes he wished he hadn't ; his wife was telephoning in all directions for doctors, for cylinders of oxygen, and all the paraphernalia for treating shock.

The rash man did not suffer anything very serious or prolonged, and lived to reap the benefit of the more orthodox desensitising doses.

Subcutaneous Inoculation. Perhaps it is unnecessary to say at this time of day that doses must be given by inoculation. As had been found with typhoid vaccine, oral administration of the pollen extract is far too random in its effects. Inunction of the pollen grains or extract made therefrom is clearly a difficult business to measure with accuracy, and is found to be very ineffective.

We inoculate, and teach the patients to inoculate themselves, *subcutaneously* ; doctors sometimes tell me that they find intramuscular doses are less uncomfortable afterwards. I don't myself advise intramuscular doses for the self-inoculator because of the danger of the needle breaking off at the haft, and thereafter being recovered only with difficulty. With clumsy subcutaneous dosing the needles also sometimes break off, but in my experience they are always easy to pull out from the skin.

Not into Veins. Intra*venous* injections must always be carefully avoided or the patient will get unpleasant shocks. It is equally important to see that an intravenous injection is not given, without intending it, by puncturing accidentally a vein with the hypodermic needle. This last possibility will be discussed in detail when teaching self-inoculation to the patient in the next chapter.

Concentration Effect. I said above that a steady increase of the dose by 15% provides a course of inoculation where every step can be made with equal ease. This is roughly true, but it is only true if all the doses are given in the same way. It is obvious from what I have said in the last paragraph that if some doses were given subcutaneously and some intravenously, although we were keeping strictly to the 15% increase rule, the patient would receive a series of shocks whenever the whole dose got into the blood stream in a few seconds instead of in a few minutes.

If a dose is given in a more concentrated form, we may surmise that it does get into the blood-stream rather more quickly than precisely the same dose given in a larger quantity of fluid. If the patient is only just tolerating the 15% rise, he is likely to notice some disturbance every time he takes the first dose out of a new and more concentrated bottle. He may even be led to suspect that there is something wrong with that new bottle.

This increased chance of a reaction on changing from bottle to bottle can be guarded against if it is found necessary, though as a rule patients don't notice this slight extra jolt in passing to a stronger bottle from a weaker, and then no precautions need be taken.

The same *nominal* dose can of course be given twice over—once as the last dose from the weaker, and once as the first dose from the stronger bottle. To give an example : the 920 units dose given by inoculating 0·92 c.c. of the 1,000 per c.c. strength (as on the card) can be repeated with another 920 units dose given by inoculating 0·46 c.c. of the 2,000 per c.c. strength, before passing on to the next dose on the card, i.e. 1,060 units.

The Captious Critic complains that this jolt should be provided against by prescribing a slightly smaller rise than the routine 15% when proceeding to a stronger bottle. That is true : I commend the idea to anyone making up his own list of steadily increasing doses, though it would entail a slight increase in their numbers.

Intervals. The length of the intervals between the doses is very much at the discretion of the inoculator, and there is no particular reason (save that of convenience) why the intervals should be of equal length : they can be made to fit in with varying circumstances, as will be explained later. The extremes for these intervals may perhaps be taken as a fortnight for the longest, and two or three hours for the shortest. We may have need to make use of one or other extreme on occasion (see pp. 115 and 87), but my usual interval between doses is either a day or two days.

If there is hurry to get the course of doses completed before the hay-fever season, it is quite possible to give two doses a day : but this is usually as much as

a patient will care for. Three doses a day, at morning, noon and night, are scarcely practicable while the patient is doing a full day's work. When the receiving of three inoculations is the total work of the day, this state of things is of course approximating to rush treatment. It may be done, however, with the patient still hard at work, but only if he is sufficiently determined about it.

I remember a Swedish engineering student, a young man of about 19, who presented himself for prophylactic treatment for hay-fever on the 2nd of May, i.e. much too late for any leisurely and comfortable treatment. He gave a really big skin response to my prick test with grass pollen, and the evidence suggested that, though his was a serious case, he had no other sensitisation ; i.e. he was highly suitable for prophylactic treatment, save for the lack of time.

I explained that he would have to take 3 doses a day. He decided to do all this, and carried it out triumphantly.

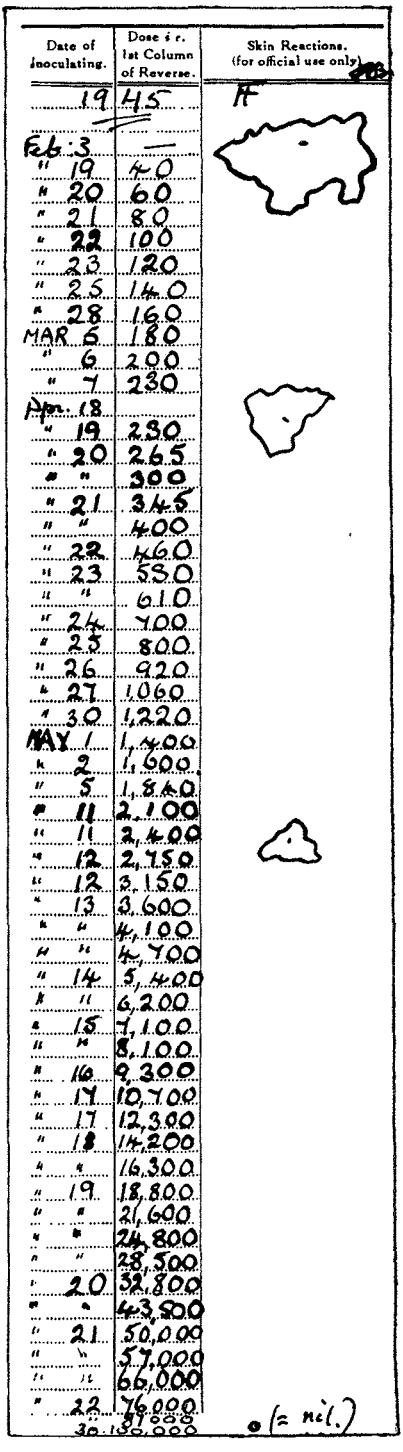
Towards the end of June, I got a letter from Sweden saying that he had had no symptoms whatever, and that his relatives had come from far and near to marvel at him, for his previous hay-fever proclivities had become a by-word in the family.

Control by Skin Tests. I said earlier on in this chapter (see p. 63) that Noon started his immunisation work on hay-fever by noting the conjunctival reaction to graded pollen extracts on the eyeball before and after treatment. He used this to help him select a useful, or at least harmless, dose. I followed him in that plan, and when the more convenient skin tests were adopted, I substituted these for the eye tests.

Whether a patient is being inoculated professionally, or is self-inoculating, or (being incompetent) is being inoculated by a relative at home, it is desirable that the process of desensitisation should be watched by the doctor, and, if necessary, controlled from time to time by observing the skin response to a prick test. The exact times for this supervision are not vital, but the following is our usual routine : in addition to tests for diagnosis made before the treatment is begun, the patients should have tests made at about one-third of the way through, two-thirds of the way through, and within a dose or two of the end of the course of P.T.D. doses as shown in Fig. 12.

Though not so necessary, it is satisfactory both to patient and to doctor to make a final test. There should then be showing only the faintest response (or perhaps only the prick mark) as in Fig. 12 here, and Figs. 13 and 14 below.

These tests will present no difficulty if the doctor himself is inoculating the patient ; but, as will be explained later, the vast bulk of my patients on P.T.D. treatment for hay-fever, etc., are being inoculated at home—either by themselves or a relative. They sometimes make difficulties about coming back for testing, especially of course if they come from a distance. It really is useful to check up on the progress of desensitisation with every case, and especially during the first year of treatment when the patient is a novice at the art of self-inoculation. This not only enables the supervising doctor to modify the dosage as may seem necessary from the tests, but he will probably find it necessary to re-arrange the rate of inoculation, quickening it up a little or slowing it down, so that the patient may reach the end of the course and be in a desensitised state (as judged by the skin tests) by about the 20th May—or whenever the danger period may begin in that particular neighbourhood.



Date of Inoculating.	Dose c.c. 1st Column of Reverse.	Skin Reactions. (for official use only)
1945		
Feb 3	—	
" 19	40	
" 20	60	
" 21	80	
" 22	100	
" 23	120	
" 25	140	
" 28	160	
MAR 6	180	
" 6	200	
" 7	230	
Apr. 18		
" 19	250	
" 20	265	
" "	300	
" 21	345	
" "	400	
" 22	460	
" 23	550	
" "	610	
" 24	700	
" 25	800	
" 26	920	
" 27	1060	
" 30	1,220	
MAY 1	1,400	
" 2	1,600	
" 5	1,840	
" 11	2,100	
" 11	2,400	
" 12	2,750	
" 12	3,150	
" 13	3,600	
" "	4,100	
" "	4,700	
" 14	5,400	
" "	6,200	
" 15	7,100	
" "	8,100	
" 16	9,300	
" 17	10,700	
" 17	12,300	
" 18	14,200	
" "	16,300	
" 19	18,800	
" "	21,600	
" "	24,800	
" "	28,500	
" 20	32,800	
" "	43,200	
" 21	50,000	
" "	57,000	
" "	66,000	
" 22	76,000	
30	87,000	
	100,000	(= nil.)

Date of Inoculating.	Dose c.c. 1st Column of Reverse.	Skin Reactions. (for official use only)
1946		
Feb 18	40 Units	
19	60 Units	
20	80 Units	
21	100 Units	
22	120 Units	
24	140 Units	
25	160 Units	
26	180 Units	
27	200 Units	
28	230 Units	
Mar 2	265 Units	
3	300 Units	
4	345 Units	
5	400 Units	
7	460 Units	
8	530 Units	
9	610 Units	
11	700 Units	
12	800 Units	
13	920 Units	
14	1060 Units	
16	1220 Units	
18	1400 Units	
20	1600 Units	
22	1840 Units	
24	2100 Units	
25	2400 Units	
27	2750 Units	
29	3150 Units	
31	3600 Units	
Apr. 1	4100 Units	
3	4700 Units	
4	5400 Units	
5	6200 Units	
7	7100 Units	
9	8100 Units	
11	9300 Units	
13	10700 Units	
14	12,300 Units	
16	14,200 Units	
24	16,300 Units	
26	18,800 Units	
28	21,600 Units	
30	24,800 Units	
MAY 2	28,500 Units	
4	32,800 Units	
6	37,800 Units	
7	43,500 Units	
8	50,000 Units	
9	57,000 Units	
13	66,000 Units	
14	76,000 Units	
15	87,000 Units	
16	100,000 Units	

FIG. 12

Two Dose Cards as filled in by the patients. They show also the diminishing weal outlines—put in by the doctor. These gradually get smaller and smaller as the treatment proceeds. The cards here are reproduced threequarters of the natural size.

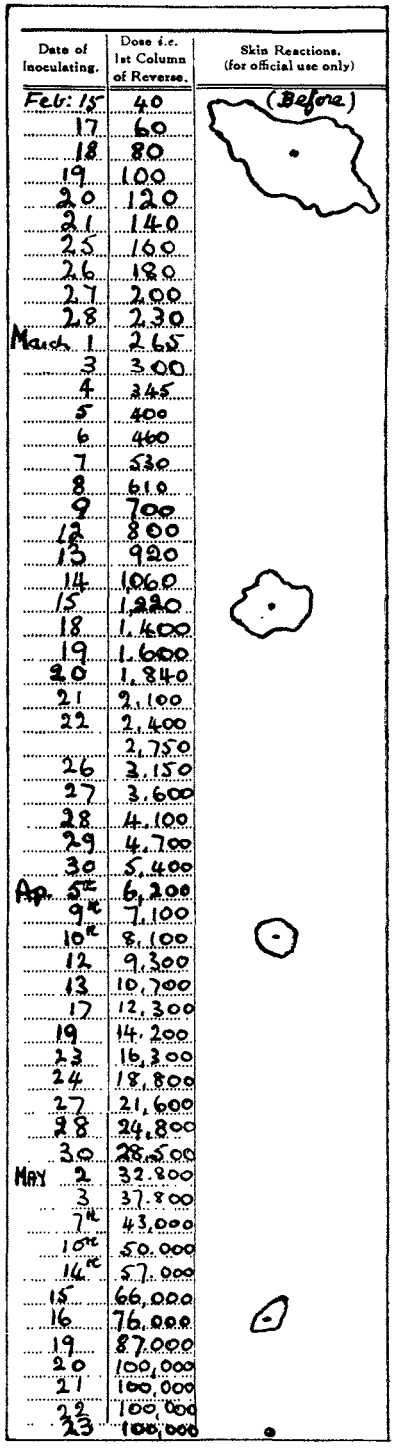
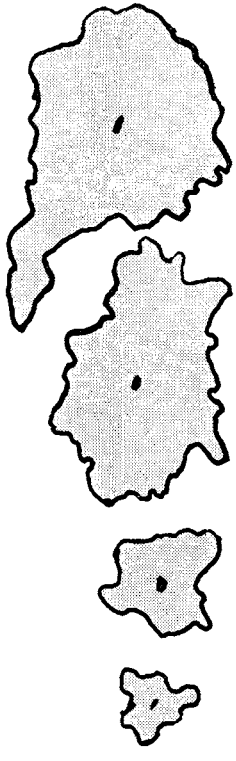


FIG. 13

A Dose Card which, like the cards in Fig. 12, is threequarters of the natural size. Note that here three extra doses of 100,000 units had to be given at the end before the wealing was reduced to zero.

For convenience of reference, and also for psychological reasons, the doctor should trace the silhouette of the weal on the dose card, placing it opposite the dose which has recently been given, as shown in Fig. 12 above. It greatly encourages the patient to persevere in the treatment if he can see for himself that he is steadily becoming less sensitive to his enemy, the pollen, as the treatment proceeds.



March 16. This, the diagnostic weal, was of course taken before any treatment was begun.

April 10. This silhouette of a weal was taken after the 24th dose (i.e. one of 1,220 units) had been given.

May 9. This silhouette was taken after the 52nd dose (i.e. one of 43,000 units) had been given.

May 15. The 58th dose had by then reached the usual top figure of 100,000 units; yet the weal was not small enough for safety.

So a further seven doses of 100,000 units each were ordered, and after that another skin test was made.

May 24. This last small wealing was then recorded. and treatment was stopped for that year.

FIG. 14

I have said above that the doctor may wish to modify the dosage shown in Fig. 11 (p. 70) because of abnormal skin-test wealings. Fig. 13 shows a card where three extra doses of 100,000 units were given at the end because the most recent skin-testing still showed, perhaps, too large a weal for safety.

Fig. 14 shows at life size the wealing in a very severe case of hay-fever. Here a still more abnormal dosage was employed: the first dose was one of

10 units only, in place of the usual 40; and the second dose was still only of 20 units.

At the other end of treatment, when the usual top dose of 100,000 units had been reached on May 15th, the wealing was still so big that seven more doses of 100,000 were ordered—with the resulting weal shown on May 24th. Treatment was then discontinued and the patient passed a perfectly satisfactory summer without hay-fever symptoms.

As with other biological processes, different individuals do not always respond to treatment with quite the same regularity, though there is, I find, surprisingly little difference between the best and the worst in the response to P.T.D.

Prick Test Vagaries. As hinted above, the diminution of the prick test weal on the patient may not go quite regularly during the course of the treatment, so the supervising doctor should perhaps here be warned that he and the patient may sometimes have to take these weal silhouettes with just a grain of salt, for the weals don't only measure the functioning of the idioceptor (see p. 260).

We shall have to return to the causes of the irregular wealing when discussing the occurrence of ' Reactions ' to inoculation doses on p. 81, but it may here be said that the wealing from a prick test with grass pollen is in reality a miniature attack of hay-fever, arranged to take place in a conveniently observable spot, and to a safely small extent; this ' attack ' will be subject to all the causal factors which govern the attacks of the naturally occurring disease.

The effect of the idiotoxin-idioceptor couple is, maybe, the most important factor concerned, and it is certainly what interests us when making these tests, but all the other factors causing hay-fever (as listed in Chapter III) will be operating too; we shall only get an approximately exact measure of the idioceptor if all the other factors affecting the wealing keep more or less constant. So, on occasion, the weal silhouette may be bigger or smaller than would be expected from the progress of the P.T.D. treatment; we must not be too discouraged if the silhouette of a weal is temporarily larger than we thought it should be by now, nor too much elated if it shows a more rapid diminution than was expected. These aberrations are probably transient—as the next skin testing may show.

If the patient shows a persistently quick or persistently slow response to the P.T.D. treatment, then, under the guidance of these revealing skin tests, the doctor in charge can alter the dosings accordingly, as has been said above (Figs. 13 and 14). This is seldom necessary.

When the idioceptor has been dealt with quite successfully, whatever the perturbation of the patient or the state of the blood calcium, etc., may be, he will be able to produce no positive wealing on his skin, and no hay-fever from the grass pollen clouds. In fact, he can go and make hay.

Short of this complete abolition of the wealing, any considerable diminution of it should mean a corresponding amelioration of symptoms—if allowance is made for the vagaries of the silhouette above mentioned.

Need for Repeat Courses. Do the effects of one P.T.D. treatment last for ever? Unfortunately they generally do not, but patients differ very considerably in the number of years for which it will be advisable to go on with them. Even though

the diagnostic weal which prompted us to the P.T.D. treatment has been reduced to zero at the end of May, and though the patient has escaped all symptoms at mid-summer through this treatment, yet the weal will be showing again next spring in

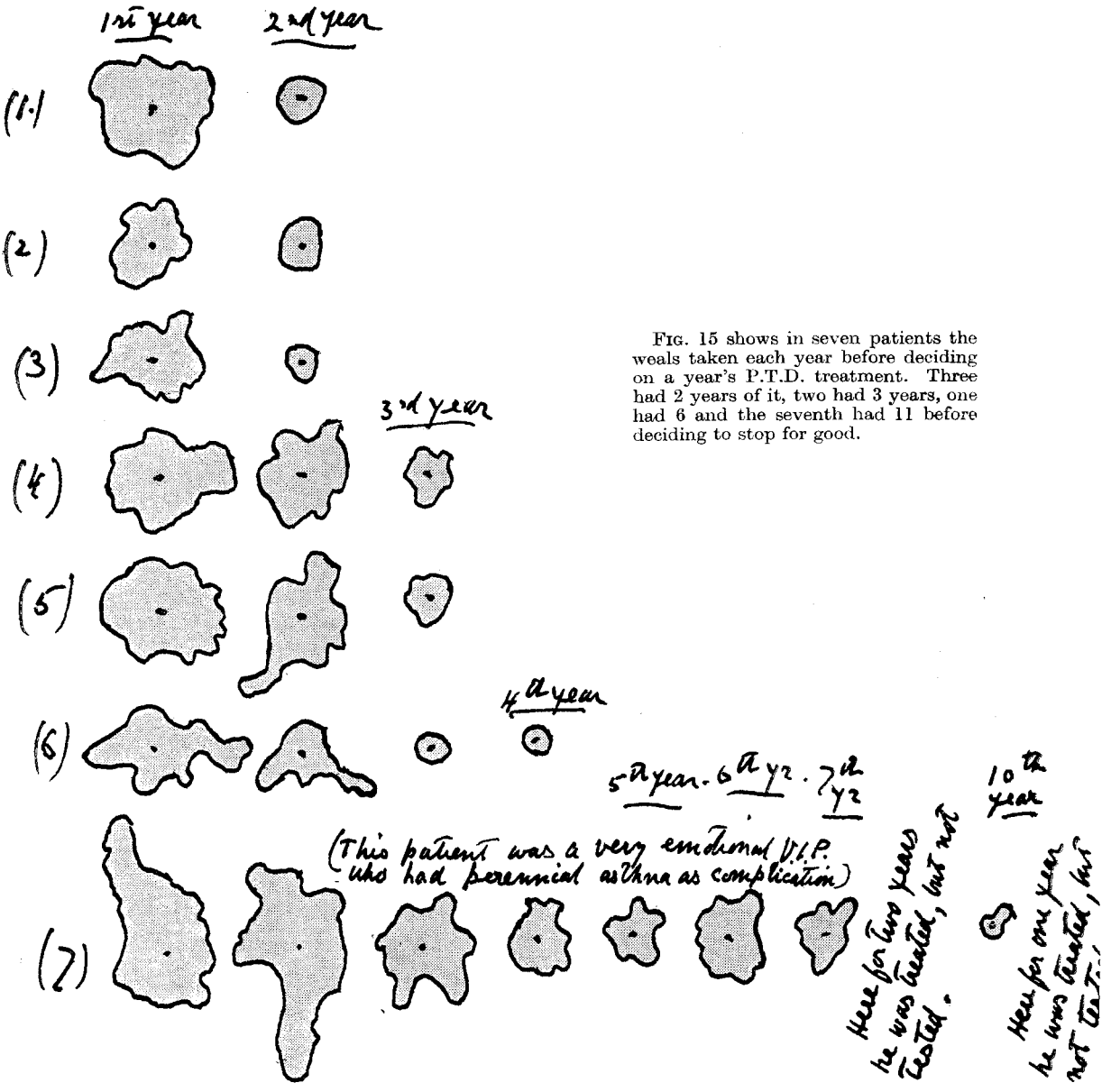


FIG. 15 shows in seven patients the weals taken each year before deciding on a year's P.T.D. treatment. Three had 2 years of it, two had 3 years, one had 6 and the seventh had 11 before deciding to stop for good.

all probability—though not so large as it had been before the treatment. It is probably true to say that, after one year's full treatment, the attacks of hay-fever will never be so acute again ; but they will usually return sufficiently to make the patient regret he was not treated the second year.

My advice to patients when I say good-bye after treatment is that they come the succeeding February to recount their experiences during the past hay-fever season, and to have another skin test made to decide whether another course is necessary. If any considerable skin response *has* returned, then I advise them to be treated once more.

Sometimes, but not often, one year of treatment is enough to stop hay-fever for ever ; more usually, it is necessary to go through the whole course for two, three or four years. If patients decide to stop treatment because the reaction of the skin has now grown quite small, they very frequently have that year clear of the trouble, but on the second or third year, symptoms begin to come back, and they will decide to have another thoroughgoing course.

I have one or two patients with whom, though they have had treatment for very many years, and though each treatment is perfectly successful, it seems im-

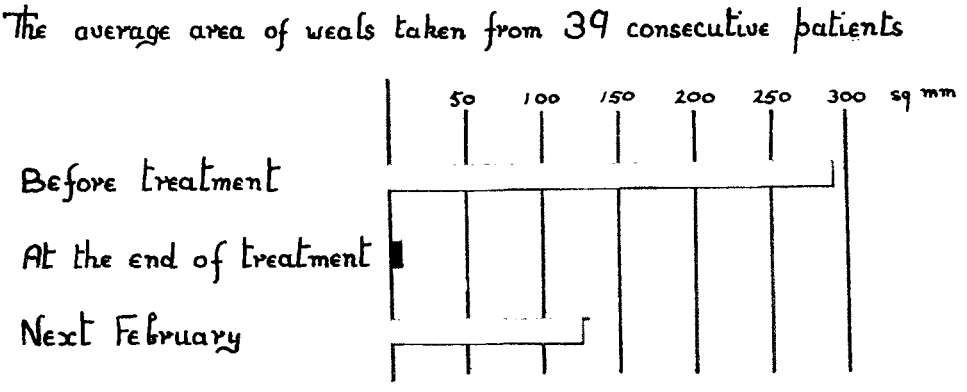


FIG. 16

possible to stop the treatment without a return of hay-fever. One such patient has twice tried to do without it, and each time has regretted it ; now she declares she will never stop again.

I should say that three years is about the average time over which treatment must be given each spring ; but, as I have shown, patients vary about this.

I think these seven cases shown in Fig. 15 afford a very fair sample of the way sensitivity is gradually worn down by successive years of P.T.D. treatment. Perhaps more convincing because they are necessarily unselected, are a chance collection of 39 hay-fever cases who all came back together in the early spring to be told if they needed a fresh treatment or not. I was one of three doctors seeing such cases at the hospital that afternoon, and these 39 chanced to come to me : I may add that this is the only time we have made such a recording. See Fig. 16 above.

It is unnecessary to give all these 39 records, but I have calculated the *average* area of (*a*) all the weals before treatment in the previous year, (*b*) the size of the weals just at the end of the P.T.D. treatment, and (*c*) the amount to which the sensitivity had returned at the time of their return to us in February. The weals after the P.T.D. course, i.e. (*b*), would be much smaller if 9 of these patients hadn't

failed to come to be tested at the end of the course. Their last available tests have been included perforce in the average.

Easy Repeat Courses. As a matter of experience, there is little difficulty in getting the patients to have another year's treatment ; the difficulty is all the other way. If I think that pollen may be in short supply before the end of the inoculation season, I sometimes try to persuade the patients to go without the treatment that particular year ; but in nine cases out of ten they reply that, if there is *any* danger of any symptoms whatever, they would rather go through the treatment because the bother of it is so very slight—once they have grown accustomed to the business. ' It is as difficult and as painful as cleaning your teeth, and takes a much shorter time.'

An old hand at the business said to me the other day that her main trouble was that she could not for the life of her remember as she was jumping into bed whether she'd given the dose or not, because the affair nowadays made so very little impression upon her.

If she had entered up the doses promptly on the dose-card, as she had been instructed, she would have had written evidence as to whether or not she had given the dose that evening.

Maintaining Doses. We have seen that the effect of a P.T.D. treatment for hay-fever tends to pass away during the winter, and the sensitiveness begins to be considerable again by the time of the hay-fever season for the succeeding year—unless treatment is repeated. This is true of P.T.D. treatments against other idiotoxins—against, let us say, dog scurf in the kennel or wheat flour in a bakery ; the effect of a desensitising treatment tends to pass off, though, as with hay-fever, the old attacks of rhinorrhoea, or asthma, or whatever they may be, will always have lost their edge, and be more tolerable after only one treatment.

The fox-hunting people are perhaps the most insistent about being 'cured ' quickly and permanently. With them I have found the result of successful P.T.D. treatment may remain excellent for a while after all treatment has stopped, but the time of freedom from all symptoms varies greatly with different horse-asthmatic patients. It may vary perhaps from three to eighteen months, with nine months as the average time ; then symptoms may begin to recur.

A plan of maintaining doses was adopted so that treated patients could keep sufficiently desensitised to enable them to handle horses with impunity all the year round. It was soon found sufficient to repeat the maximum dose of 100,000 units first every fortnight, then later on every three weeks or a month, gradually extending the interval till that ' interval ' became a permanent stoppage. If the patient began to have reactions from the maintaining doses, that was a sign that the interval had grown too long, and would have to be shortened again. This plan works very well for people who have to be constantly in danger of meeting the peccant idiotoxin—e.g. horse scurf for riders, horse breeders and spreaders of ' muck ' in the fields.

Maintenance in Hay-fever. I had hoped that these maintaining doses would work well with hay-fever patients also. The plan would be of course to give a full dose as the pollen cloud passed away in July, and then to keep on with these maintaining doses right through the winter till the succeeding season.

In practice, this has not worked so well as I hoped ; though some patients prefer it to going through a course of treatment each year, I find that most patients are, not unnaturally, somewhat bored at the end of the P.T.D. course ; they want to forget all about it and have a holiday. So they are reluctant to give that important first maintaining dose in July, and postpone it until they have already lost a good deal of the previous desensitisation ; also they tend to forget all about giving the doses at Christmas time. Whenever such a lapse has occurred, they may have to go back to much smaller doses, and then work their way back to the full dose of 100,000 units.

There is another difficulty. All this time, the grass pollen extract is slowly losing potency (see p. 279), and there comes a time when the ' Maintainer ' will have to switch to the new season's pollen ; in fact, he should do this as soon as he can get the new brew—at the earliest, probably in October. He will then find that he can't tolerate the full 100,000 units of the stronger supply without discomfort, and may have to go back to, say, 10,000 units, or even a 1,000, and work up again to the full strength.

All this is very irksome, and I find that, after trying the maintaining doses, most patients prefer to start the P.T.D. course afresh each February, till these yearly repetitions of courses are no longer needed.

Even for hay-fever, these maintaining doses should be kept in mind as a possibility, and may prove very useful. For example, a man protected against the English hay-fever season may be travelling in the early English autumn to South Africa or Australia, where he will be just in time for another hay-fever season in the southern hemisphere. Such a person would certainly be wise to keep up his defences by four or five maintaining doses.

' Reactions ' to Doses. Reaction is an overworn term nowadays : in Bloomsbury slang it means little more than the effect or influence of almost any occurrence, while in political circles it means an opinion less left-wing than your own. But in medical circles, and when taken in connexion with any therapeutic inoculation, the word is taken to mean an immediate and unpleasant sequel to the inoculation.

' Reaction ' is also the name used very generally to denote the urticarial wealing of the skin which, with a specifically sensitive patient, follows a diagnostic prick test. I have found it convenient to call these minute diagnostic reactions by the name of ' response ' or ' positive results ' in order to avoid that ambiguous word ' reaction ' ; as I have pointed out, these intentionally provoked diagnostic wealings are of the same nature as an unintended reaction from an indiscreet dose, and indeed are of the same nature as the allergic disorders themselves.

It was said above that it was the first inoculation dose of all which was the most likely to have this unpleasant sequel because that particular patient's proclivities concerning the doses were then comparatively unknown. But such a reaction should not occur if due care is taken to see that the patient is suitable at the time for the commencement of inoculation treatment. (See the story of ' The Britannia Girl ' on p. 212.) As the course of P.T.D. doses proceeds, the reactions should become rarer still, because by then we know more of the proclivities and disabilities

of the patient. As has been said, the scheme of dosage based on the 15% increase seems to spread the desensitisation load very fairly and, bar accidents, there is no more risk of an unpleasant reaction at one point than at any other.

A slight exception to this occurring when the patient proceeds from a weaker to a stronger bottle of the extract was noted and explained on p. 72 under the heading ' Concentration Effect '.

On the second year's treatment for hay-fever, and still more on the third, etc., unpleasant reactions become less and less likely, and should certainly be easily avoidable.

What are these Reactions like? Briefly, they may be any form of toxic idiopathy, as was said on p. 33. Perhaps urticaria is the commonest form of reaction to an injudicious dose, or urticaria and angioneurotic oedema ; after that, perhaps migraine and asthma ; or perhaps paroxysmal hydrarthrosis if the joint has been recently damaged. A temporary albuminuria is very common, but as it causes no discomfort, it is seldom detected.

In general, it may be said that these unwanted reactions are more likely to occur in a part of the patient's body which has been somewhat damaged, and thereby been made liable to attack by this trauma. As related in Chapter II, I have twice seen an attack of epilepsy follow immediately after an inoculation dose, but here again, these attacks occur in people with presumably damaged central nervous systems ; these two patients had both of them had attacks of Petit Mal previously in their brief medical history.

All this seems to add up to a rather sinister collection of mishaps which might follow the injudicious dose ; but these reactions are the exception, particularly if due care is taken to see that the case is suitable for P.T.D. treatment, as will be described later.

Normally, the patient goes right through the season without any such unpleasant occurrences ; and in the second or third year of treatment, this absence of reactions may almost be taken as a matter of course.

For psychological reasons, the very occurrence of a reaction makes further reactions more likely to occur, as we shall be explaining in the next section.

Why do Reactions ever occur? It may be asked why, if a patient is working meticulously through such a lengthy course as this of 54 inoculations, he should be in any danger at all of unpleasant reactions from doses. The testy patient might say after a crop of nettlerash, ' Surely a course might have been designed which could eliminate all possibility of unpleasant consequences from the injections? '

Unfortunately, no stereotyped list of doses can possibly be reaction-proof because the patient himself may change from day to day, and so may his circumstances ; he may not be able to receive a dose on Tuesday that he could have taken quite easily on Monday.

Similarity of Diagnostic and Dose Reactions. Before we consider what these changes are which might make the patient more liable to unpleasant reactions, let me remind the reader that these reaction symptoms mentioned above are themselves

' hay-fever ' just like the effects of a diagnostic prick test which were discussed a page or two back.

By our definition on p. 1 :

' Hay-fever is what happens to people who are sensitive to grass pollen when this pollen meets their living tissues.'

Therefore these reactions to doses will be governed not only by the idiotoxin-idioceptor couple, but also—as was the case with the skin tests—by the other causal factors mentioned in Chapter III.

With regard to this similarity between a diagnostic test for hay-fever and a therapeutic inoculation of a pollen vaccine, it is clear that the prick test into the skin is in fact an intradermal inoculation ; I have calculated (see p. 257) that it amounts to the intradermal injection of about 40 units of idiotoxin. This may even on rare occasions produce a temporary generalised reaction of urticaria, angioneurotic oedema, asthma, etc. I have twice seen this happen from a routine diagnostic test in a very septic patient. Moral : be careful how you skin-test very septic patients ; but usually the oedema is confined to the skin of course.

As a skin test really amounts to an inoculation dose of 40 units or thereabouts, it is sometimes rash to give the initial dose immediately after the diagnostic prick test ; this may to some extent account for the comparative frequency of unpleasant reactions after the first dose.

Reaction Producers. What, then, is liable to provoke a reaction? What should warn us to go very carefully with the routine P.T.D. doses? With the exception of ' (A) Heredity ' which obviously we can't influence after conception, and of ' (C) Idioceptor ', the vagaries of which we are trying to cope with in this chapter by P.T.D. treatment, we shall have on our hands at least the other five causal factors listed on pp. 38 and 39 :

These five are :

(B) Idiotoxin—which we discussed in Chapter IV.
(D) Serous Leak—which we must deal with in Chapter VII.
(E) Trauma factor—Chapter VIII.
(F) Emotions—Chapters IX, X and XI.
(G) Bacterial Toxaemia—Chapter XII.

Let me here illustrate as briefly as may be how each of these five causal factors can influence the chances of an unpleasant reaction to an inoculatory dose. (And, incidentally, how they must influence, too, the size of a wealing produced by a prick test ; for this, as we have seen, is an intradermal injection of a minute quantity of an idiotoxin.

(B) **Idiotoxin.** If the inoculum varies in the value of its units from dose to dose, this will clearly upset the series of tolerable 15% rises. For example, in a P.T.D. course for hay-fever, if we switch from the deteriorated extract used the year before, to the fresher and stronger extract of the current year, we shall then be inoculating stronger ' units ', and in place of the 15% rise, we may really be increasing by 30% or more.

We may run into the same dangers of an excessive rise in dosage if in the middle of the course we switch from one extraction to a stronger extraction. The units must, of course, all have the same value throughout one P.T.D. treatment: we had better keep to the same brew.

(D) **Serous Leak** will be dealt with more fully in Chapter VII, but it is clear that the greater the fluidity of the plasma, the more easily will this leak out from the capillaries, and so produce the reaction oedema. It is therefore unwise to inoculate immediately after ingesting any decalcifying food, such as large helpings of rhubarb tart, or of acid fruits or wines.

The corrective is of course calcium, taken half-an-hour before the inoculation, or, best of all, a good drink of milk at that time—as will be described among the technical details of self-inoculation in Chapter VI.

(E) **Trauma** does not, I think, come in very much as a reaction-enhancer; but it does help to determine where the reaction can most easily manifest itself, and in this way a reaction may be produced which might otherwise have escaped. Oedema tends to occur at the most damaged area, e.g. migraine or epilepsy will presumably occur only when there is some trauma to locate them in the C.N.S.; asthma is likely to happen if the upper air passages have recently been injured by whooping-cough or poison gas, etc.

(G) **Bacterial Toxaemia** is perhaps the greatest bugbear of the doctor in deciding on any P.T.D. hay-fever course. Any considerable intercurrent infection will increase the chance of reactions from the doses.

Every year one is on the horns of a dilemma with chronically infected hay-fever cases. A patient with chronic bronchitis and a persistent cough who has perhaps sporadic asthma all the year and marked hay-fever from mid-May to mid-July asks in late February, quite correctly, for hay-fever prophylactic treatment. Should reactions from the chronic infection be risked while proceeding with the P.T.D. course; should one try to cope with the infection, and thereby postpone the hay-fever treatment for another year? Every case must be decided individually: the 'Britannia Girl' anecdote, above referred to, relates how one such problem was, in the end, successfully dealt with.

In addition to these chronic infections which may perhaps be arranged for beforehand, there are the sudden and acute infections, such as an influenza epidemic, which take us by surprise; for these we have to keep a wary eye open or reactions may result during the infection.

Gut infections in particular, because the toxaemia may come on very suddenly, seem to have a peculiarly disturbing effect on the P.T.D. course of inoculations. Urticaria is very commonly ascribed to 'fish poisoning' by the doctor in charge with the vague idea that the patient is sensitive to fish myoplasm. In the majority of such cases it is not the fish but the bacterial taint in the fish which is at fault: the fish is 'bad'—is infected with bacteria and infested with their toxins.

Six 'Rush' Ladies. I once had a good demonstration of the possible reaction effects of this so-called fish poisoning. I had at the time six patients undergoing a course of rush

treatment for hay-fever in a nearby nursing home (see pp. 86 and 87); they were all at different stages of treatment because they had come in haphazard on different days, and the nurses were giving the doses every two or three hours during the day.

On paying my customary visit to see how they were getting on, and to check-up with the daily skin testing, the first patient I saw exclaimed: 'Just look what you've done!' and she displayed nettle-rash all over her chest and stomach. She naturally insisted that it was the fault of the dose, because the trouble had begun within seven minutes of the nurse's puncture. I could hardly gainsay that, but I suggested that there must have been a change in *her* to make an otherwise harmless dose so unpleasant. She admitted to some diarrhoea, but reiterated that it must be *my* fault because of the prompt arrival of the nettle-rash after the dose.

Of the remaining five patients under rush treatment, four of them showed for the first time in that treatment allergic reactions from their doses, the fifth patient was apparently unaffected. Two of them had asthma and urticaria, one had urticaria and migraine, and the fourth had asthma alone.

Turning to the Matron of the Nursing Home, I asked her privately what had been happening; she confessed that the fish on which she had fed her patients and nursing staff the day previously had been badly tainted, with the result that nearly all the patients, and also the nursing staff, were down with diarrhoea of varying intensity, and some had diarrhoea and vomiting.

If I had briefed my nurses rather better they would have known that, in the crisis, they ought to have withheld all doses till their patients recovered normal health.

None of these six patients was sensitive to fish as shown either by her past history or by failure to respond to the pathophane of fish myoplasm.

(F) **Emotions and Moods** also play an important part as reaction-producers. As stated in the next chapter, bacterial infections and strong emotions are the two causes of trouble which the self-inoculator is warned he must avoid in choosing the time for a re-inoculation in the P.T.D. course. Just as any strong emotion or sudden shock may provoke an attack of any toxic idiopathy (see pp. 140 to 143), so they may render a reaction from a dose the more likely; also, of course, they will tend to make a wealing from a prick test the bigger than it otherwise would be.

Quite obviously, too, fear of the needle, and fear of any reaction from the dose, will make that reaction the more likely of occurrence. Self-inoculators who sit with the needle of a charged syringe hovering an inch or two over their skin while they slowly screw up sufficient courage for the deed are asking for trouble; they would be well advised to postpone the dose till their feelings are more rational, e.g. after a thorough course of 'pin-cushion drill' (see the next chapter) or till next morning.

The local swellings at the site of the inoculation are also affected by all the causal factors which govern the real disease of hay-fever; but chiefly they seem to arrive after undue concern about their possible occurrence. The self-inoculator who keeps on peering at her leg to see if the inoculated area *is* going to swell will be all the more likely to get this local reaction. By the time the patient forgets to look for them, these local swellings usually fail to arrive.

Suitable Cases for any P.T.D. treatment must, it is clear from the foregoing, be selected with reasonable care—whether for the patient's comfort, the convenience of the doctor, or for the prestige and credit of P.T.D. Some cases will be so unsuitable that they should not be attempted; it would be foolish to give a hay-fever

P.T.D. if the pollen factor only accounts for perhaps 5% of the symptoms of which the patient is complaining—and only that little amount from mid-May to mid-July.

From such an impossible case there will be all degrees of suitability up to 100% hay-fever which may expect 100% relief of symptoms—for that season at any rate.

It is not necessary, or even possible, to avoid all danger of reaction difficulties ; to demand complete freedom from all possibility of them first, last, and all the time, will get us nowhere ; but we must avoid any serious unpleasantness. It is certainly necessary to know, in our patients' interests, that these dangers do exist, and to be on the look-out for them.

Those 54 Doses! What most people will be dubious about when the proposition is first made to them is the possibility of getting anyone to submit to receiving as many doses as may be necessary for a completely successful P.T.D. course. As the system developed, and as more and more doses became necessary, it became abundantly clear that the treatment could not be carried out by fifty or more professional consultations with the doctor—it would waste far too much of the patient's time, and of the doctor's too.

Two Successful Plans. I found two solutions of the difficulty. The first, and less successful, was to take the patient into hospital for a week and have the doses administered by a trained nurse as rapidly as possible, i.e. a dose every 2 or 3 hours during the day : this I call ' Rush Inoculation '. The second was to teach the patient to inoculate him- or herself on a carefully thought-out and stereotyped technique : this last has proved surprisingly easy and successful.

As the propriety of this self-inoculation is sometimes called in question, I will say a word or two in justification of it at the beginning of Chapter VI, and also defer to that chapter a meticulous exposition of this stereotyped home technique. It must be thoroughly understood that if the laity are to be trusted to inoculate themselves and carry out a lengthy and rather complicated system of dosage, we have got to be sure that the system is as easy and understandable as care can make it, also that these self-inoculating patients not only understand what they are doing and how to do it, but are themselves quite confident of their knowledge and skill ; the doctor in charge must make himself responsible for all that.

Rush Inoculation

Ordinarily, the patient will have to come into hospital for Rush Inoculation to be given by the nursing staff. I said several pages back that inoculations could be given as frequently as one every two or three hours. On occasion, they may be given every two hours, but an interval of three hours is usually short enough—indeed, this may have to be extended a little if the patient is having any trouble in tolerating the dosage.

By starting at five o'clock in the morning, and inoculating every three hours, we should arrive at the seventh dose by 11 p.m. By starting at the same time, and inoculating every two hours, we can get in ten doses in the same time. The average of eight doses a day for a week would be more than enough to complete the course of fifty-four doses.

Short Rushes. Of course a great deal less than a week's ' Rush Course ' will bestow a very considerable amount of desensitisation on the patient, and in fact a short ' rush course ' of two or three days is a handy way of quickening up the more leisurely method to be described in the next chapter. A patient who is starting, perhaps at the middle of April, would be able to make up for lost time if he had only three days in a hospital undergoing the first stage of a ' rush course '. At the other end of the story, a patient who has struggled through two-thirds of the more leisurely course of treatment, and yet fears he will be too late for the beginning of the season, can finish off a course with a short ' rush ' of a day or two.

This terminal short rush would be the more necessary where the controlling skin tests disclose to the doctor in charge that for some reason or another the patient is not desensitising as rapidly as he should do, as in Fig. 14, p. 76.

Technique for a ' Rush '. This may perhaps be left to the taste of the doctor in charge, who would, of course, have to instruct carefully the nursing staff on the way he wishes the business to be carried out. For psychological reasons, it is very necessary to make the technique employed seem to be as perfunctory as possible, and thereby as unemotional for the patient as can be managed. Also it is very necessary for the nursing staff to be extremely matter-of-fact, and to avoid any chaff about the patient's submissiveness to this ' terrible treatment ' : a bad nurse can easily frighten the patient out of hospital.

I shall have to give in the next chapter some details for making the self-inoculation course seem ordinary and unemotional to the patient. This is also desirable for the Rush Course patient, and is in line with the ' calculated neglect ' advocated in p. 154. In briefing the nurses for their Rush inoculations the doctor in charge should get them well away from the ' minor operation ' atmosphere which nurses sometimes hanker after.

Control by Tests. The Rush Course of P.T.D. should be controlled by several skin tests as shown in Figs. 12, 13 and 14, to see how the desensitisation is progressing from day to day. If possible, these tests should be made the first thing in the morning and before the first inoculation of the day. Indeed the whole affair is conducted on the same lines, save for the rapidity of re-inoculation, as for the self-inoculator's more leisurely course of treatment—which is to be described in Chapter VI.

CHAPTER VI

PROPHYLACTIC THOROUGHGOING DESENSITISATION (P.T.D.) BY SELF-INOCULATION. HOW THIS IS TAUGHT TO THE PATIENT

In Chapter V we were discussing in general terms how we could deal with the idioceptor, and thereby give help to the patient who is suffering from some toxic idiopathy.

In this chapter I am to describe, as clearly as I can, the exact technique to be employed by the self-inoculator in the P.T.D. treatment of his hay-fever; this treatment is equally useful when dealing with idiotoxins other than grass pollen, but a careful description of self-inoculation for hay-fever will serve for all of them.

The Propriety of Self-inoculation. As this chapter is to be devoted to teaching self-inoculation to the patient, and equipping him for the purpose, and as some doctors (and perhaps more parents of patients) protest against this procedure, something must be said in its justification. It is not, however, so necessary to justify self-inoculation after fifteen years trial of it, as it seemed to be to most of us before this method was begun.

The Need of It. As we saw in the last chapter, if any desensitisation is to be accomplished, it must be by means of injections—and, as I think, preferably by subcutaneous injections. Experience has shown us that, to attain anything like complete desensitisation, the dose of the pollen (or whatever it may be) in the inoculum must attain a size at the end of the process which is very large compared with the dose with which it is safe to begin. I aim at ending with a dose of 100,000 units, and usually begin with a dose of 40 units.

Experience has shown also that when the doses reach a certain size (200 units) it is not practicable to increase them by more than 15% of the previous dose—i.e. by multiplying the previous dose by 23 and dividing by 20.

We can afford to advance rather more rapidly than this at the beginning of the series of doses, but still we can only attain that maximum dose of 100,000 by giving 54 of them, or thereabouts.

Those 54 Doses would mean fifty-four visits to the doctor, or by the doctor to the patient, if the doses *must* be given professionally and not by the patient. This necessitates too great a tax on the time both of the patient and of his doctor—unless both of these people have a great deal more leisure than is customary nowadays.

The long and short of it is that if those 54 doses are to be got into the patient, they will have to be given in his home and unprofessionally.

I spent years trying to avoid that conclusion, but was unable to do so—save by the somewhat inconvenient device of Rush Inoculation (p. 86).

Even when there is a doctor living in a patient's own home, as is the case with the children or the wives of doctors, the troublesomeness is very considerable ; it generally proves too much for both inoculator and inoculatee, and a demand is made by both parties that self-inoculation should be taught.

Hospitalising. Even if the patient and his friends can afford the time to spend an hour or two every day for months on end in going to a doctor's consulting-room, this is not very good for the patient psychologically ; it leads to his thinking far too much about his ailments and his treatment, as compared with the effect of the two or three minutes which is all the time that his self-inoculation takes up when once the patient has got his hand in. I regard this avoidance of hospitalising as being of the greatest psychological importance when dealing with all the toxic idiopathies (Chapters IX, X and XI).

Positive Gain. I confess that when I began to teach self-inoculation I was only considering its convenience to patient and doctor ; since then, I have come to believe that the psychological discipline of learning to inoculate oneself quickly, correctly and painlessly in a minute or two, is of itself an advantage.

Most of the hay-fever children and young adults are being over-handled by their nearest and dearest ; almost deliberately they are being made to feel and act as though they were not their proper age. Consequently, it is very salutary that boys and girls in their teens should find that they can do quite easily something that their parents think they can't do—it helps to remove that inferiority complex which even the best parents seem so anxious to inculcate. This aspect of the problem will be dealt with at considerable length in Chapter X.

The Proof of the Pudding. Self-inoculation for hay-fever has been under trial now for many years. Perhaps the best evidence of its feasibility for hay-fever P.T.D. is afforded by the crowds that yearly throng our clinics each early spring, by the appreciative reports of practitioners, and by the remarks of the patients when they come to see us again next February—' Heavenly ' or ' Simply perfect ' are usual phrases.

Significant also is the very little difficulty in getting patients to go on with treatment when once they have had P.T.D. for one season ; indeed, there is usually more difficulty in getting patients to stop this treatment if they imagine there can be any chance of a return of symptoms.

The Insulin Example. When insulin treatment for diabetes first came into use there were doctors who wished to withhold treatment at each mealtime because of the impossibility of the patients giving the inoculation to themselves. Common sense won in the end of course ; nowadays, no one is surprised at seeing intelligent children inoculating themselves before a meal. What is sauce for the diabetic goose is sauce for the allergic gander.

An Aside to Colleagues. I have heard it whispered that some practitioners try to insist on an absurdly complicated technique for giving these insulin doses. May I suggest

that those who are doubtful about it should read the very simple, yet safe, methods about to be advocated in this chapter for hay-fever, etc.

Drug Fiends. Doctors have suggested to me that patients may become drug addicts as soon as they learn the extreme ease of inoculation. I don't think that this is a real danger, and certainly we have had no experience of it—complaints of any such a calamity would come back with extraordinary rapidity.

Practicability of Teaching? A doctor once said to me many years ago that there was not one patient in all his practice who was capable of self-inoculation. That probably was an over-statement, even for his own practice as it stood. I know it would be incorrect if he had previously made the attempt of teaching them.

Perhaps I had better say what our experience has been ; and, as the reader will find out if he manages to get to the end of this chapter, our teaching methods are thorough and detailed. All the minutiae to be given in this chapter need not of course be crammed down the throat of every self-inoculator : we needn't explain the difference between 0·1 and 1·0 to the mathematician, for example, nor to the physicist that air bubbles must float on the surface of water. On the other hand, I think we shouldn't grade any pupil as unteachable till we have expended as much patience and care in the teaching as this chapter suggests may possibly be necessary.

Naturally some people can learn much more easily than others, but I think this is largely due to psychological differences, and with children (who are usually the easiest to teach) to the attitude of the surrounding grown-ups—and that includes the doctor of course.

I began to teach self-inoculation gradually, and with carefully selected persons. As I had expected, there was no difficulty with the student type, for these people were accustomed to think and act for themselves.

As the method developed, I tried a wider range of patients. The surprise to me was the great ease and safety of teaching boys and girls in their early teens ; in fact, it may be said that boys and girls of twelve, thirteen and fourteen (unless they have been grossly overhandled by their parents and thereby not allowed to be their age) are the easiest to teach of all the population—so long as parents don't interfere. Indeed, in the case of exceptionally intelligent and self-restraining mothers, I found it quite practicable to teach a child of seven to carry out the whole business for him- or herself with ease, confidence and accuracy.

I suppose all doctors have discovered how much easier it is to get sensible behaviour out of a child in the absence of the average parent. In some cases, as in the anecdote below, the parents seem deliberately to inculcate inferiority, timidity and diffidence in their offspring. The children, if sufficient parental pressure of this kind is brought to bear on them, usually take the line of least resistance, and respond by becoming childish to suit their parents' taste for it.

I have before me the vision of a schoolboy of 12 in my consulting-room settling down (after he had been instructed) to the correct measuring of doses out of his inoculation set with the syringe. His behaviour was as deft and confident as that with which he would have put on his boots, or have taken his stance at cricket.

Soon, however, his two parents butted in :

' Darling, *do* be careful.'

' Dearest, I do think you're brave.'

' Darling, are you *sure* that's right? '

' Now, my boy, no mistakes, mind!—We can't have any mistakes in a thing like this! '

' Darling, you know how *clumsy* you are ; it would be too terrible if you broke the syringe.'

They went on in this way until at last the poor little wretch put down the syringe and began squirming in his chair, saying : ' Oo-er, I don't like doing it! '

At this point, of course, I turned the parents out of the room, and told the boy to get on with it and not play the fool. A look of cold determination came into his face, and he went through the whole business as accurately as, say, a watchmaker at his job. His difficulties had been entirely parent-produced.

Parents, on the contrary, usually think—and in fact sometimes insist—that they could do the work much better for the child than the child could do it for himself. If they do so insist, it is often a good plan to teach both together—when it usually becomes apparent to both adult and child that the child is the quicker and safer learner.

Mother, discussing self-inoculation by her ten-year-old daughter : ' Oh doctor, *she* could never do it by herself, she is a mere child! You must teach *me*.'

' Very well, then, we will teach you both,' said I.

Ten minutes later I heard the ' mere child ' say from a secluded corner of the waiting-hall where she was being taught : ' Oh *Mummy*! *Can't* you understand? Nurse has already told you all about that three times over! "

Then I heard the mother reply meekly : ' No, I'm afraid I can't, darling ; I think you *had* better do it for yourself after all.'

Good-looking young women are sometimes tiresome because they are accustomed to resolve their difficulties by a friendly display of teeth ; but that habit of mind can be got over by a little firm teaching.

Older women sometimes try to bluff it out, and guess the correct procedure without really understanding what they are doing ; in particular, it is sometimes difficult to get them to look at and read the label on the bottle they are handling. Here again, persistence should remove the difficulty.

Elderly people don't very often need a thoroughgoing desensitisation course—at any rate for hay-fever. If they do, and if they have become rather timid and helpless, they usually wish to be inoculated by the appointed daughter, or whoever it may be who is caring for them.

Altogether, there are very few people who cannot be taught safely the art of self-, or at least home-, inoculation if we bring sufficient experience and pertinacity to bear on the problem.

If there is any serious difficulty in teaching any patient, it is generally useful to change the teacher ; I firmly believe that the master's report on a school-boy which runs : ' He doesn't pay enough attention, and seems uninterested in his work ' should often be amended to : ' I must apologise for not holding this boy's attention and for not keeping him interested.'

Desiderata in Technique. Very possibly as good or better techniques for home-(or self-) inoculation can be devised ; in this chapter I must only explain what I

have actually done, and it seems to have worked remarkably well ; I offer it for your consideration—if you can't think of a better.

In teaching such a thing as self-inoculation to a large number of people the method to be employed by them cannot be extemporised ; it must be cut and dried, and must not vary with the mood of the teacher at the moment of teaching. As was said when discussing Rush Inoculation in the last chapter (p. 87), the technique should be as unimpressive as possible, and all suggestion of the ' minor operation ' atmosphere must be avoided. The method must also be quick and safe ; to obtain this, I found it was necessary to throw overboard several rather cherished medical customs and superstitions. By doing so, a technique was arrived at which is nearly as simple, safe and quick as cleaning your teeth.

The Doctor's Control. Doctors have asked me indignantly : ' Well, but where do *we* come in? ' I have been told by some publicity experts among the pharmaceutical chemists and wholesale drug houses that practitioners will never be content at seeing so little of their patients, and at being pushed so far out of the picture.

In reality, responsibility must still rest with the doctor, and I should say that what he has to do amounts to quite enough work. It is certainly enough for me.

Let me here detail what the doctor should do when he prescribes P.T.D. self-inoculation treatment for hay-fever.

(*a*) The doctor must be quite sure that a correct diagnosis has been made.

I have said elsewhere (p. 19) that every February, when candidates for our treatment of hay-fever begin to arrive at St. Mary's Hospital, we get some hundreds of cases which, on investigation, prove not to be hay-fever at all—though most of these cases come with a note from their practitioner giving this diagnosis in explicit terms. Here is such a letter sent with a patient who proved to have no hay-fever at all :

' This patient, Master ——, is a very severe case of typical hay-fever ; please see what you can do for the poor boy.

' I may add that all previous treatment has failed—including grass pollen inoculations.

' Do what you can for him, for he is a charming young fellow and has had a very distressing time all through the last few summers.'

Sometimes the practitioner who is sending the case will even emphasise the fact that the so-called hay-fever goes on all the year round, and is worse in winter time!

(*b*) The doctor in charge must see that, even if the case is one of undoubted hay-fever, it is suitable for P.T.D. treatment, i.e. is not encumbered by too many complicating circumstances—such as by a multiplicity of sensitisations or by chronic bacterial infections.

(*c*) The doctor in charge may have to deal with parents or relatives all apparently bent on trying to make self-inoculation more difficult for the patient, or at least to shake the patient's belief in his own capacity. The co-operation of the friends and relatives must be gained, or at least their good-will.

(*d*) Having decided upon this P.T.D. course, the doctor must first teach the patient the technique for the self-inoculation to be presently described (or some

equivalent for this) ; then he must see by a personal examination that the patient knows how to conduct the business perfectly, and is also confident that he can so perform it.

(*e*) The doctor should instruct the patient to revisit him two or three times at least during the course of treatment ; primarily, this is to make the requisite number of skin tests, which should show the diminishing size of all the weals as treatment proceeds (see p. 76, Fig. 14). He must make sure, too, that in other respects the treatment is going smoothly.

At these times the doctor will almost certainly have to re-arrange the intervals between the doses so that the highest dose may be reached just before the season is due to commence.

At these times, too, he may have to overhaul the patient's apparatus. For example, he may have to make sure that the hypodermic needle really *is* still quite sharp and that the patient's skin has not suddenly ' grown tough ' : he must make sure that the ' bluntness ' of the needle is not only psychological. If it is, he may have to institute ' pin-cushion drill ' to make further inoculations easy again.

(*f*) In addition to these regulation visits, the doctor in charge should, I think, tell the patient to come and see him if there are any unexpected difficulties or alarms.

All the above should give the doctor quite enough to do to satisfy the most conscientious man.

How to teach Prophylactic Thoroughgoing Desensitisation (or P.T.D.)

A Second Session. In hospital practice, I find it is generally necessary to have a first session for diagnosis and general consideration of the suitability of the case ; then a subsequent session in which to instruct, examine and equip the patient for self-inoculation. When the patient has come from a great distance, and must return that day, maybe special arrangements must be made ; but in general it wastes too much time to work that way. In private practice it is more often possible to manage everything at a single long session.

Preparatory Office Work. Someone between these two sessions should see to it that each patient's dose-card is made up with his name and address on it, and that the vaccine to be used by him has been recorded at the top of the front page as in Figure 14 below. Also on the reverse side of this (i.e. at the top of p. 2) the patient's diagnostic weal and the date of taking this should be recorded as in Fig. 15.

Technical Work. Between the diagnosing and teaching sessions the inoculation set must be got ready to put before the patient.

Somebody must see that it contains the desensitising vaccine which has been prescribed for him ; if the patient is to have grass pollen and aster pollen, for example, or grass pollen and horse scurf simultaneously, these special blends must be prepared and correctly diluted, and put into the vaccine bottles according to the instructions on the dose-card as in Fig. 17.

The Dribbling Syringe. A technician must see to it that the syringe is in proper working order, with a piston that moves without jerks, a sharp needle fitted on

firmly, etc. The syringe must not be a ' dribbler ', i.e. the plunger and contained vaccine must not shift up or down the barrel by gravity whenever the syringe is held vertically. It is embarrassing for the non-expert when inoculating himself to have to correct this fault by holding the syringe horizontally whenever the piston is left free to move in the barrel.

Sterilisation. Before the set is given to the patient the inside of the syringe and needle must be thoroughly sterilised of course. I very much prefer sterilising with oil at 130° C., according to the routine practice of Almroth Wright's laboratories ; the oil at this temperature is drawn up into the syringe and then blown out again two or three times, after which all the surplus oil is ejected. This not only

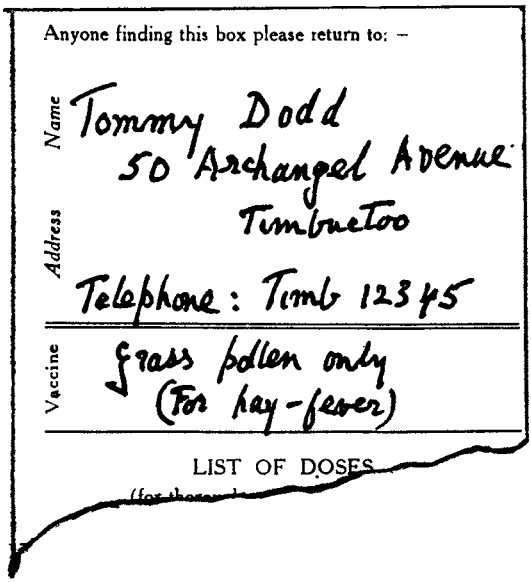


FIG. 17.

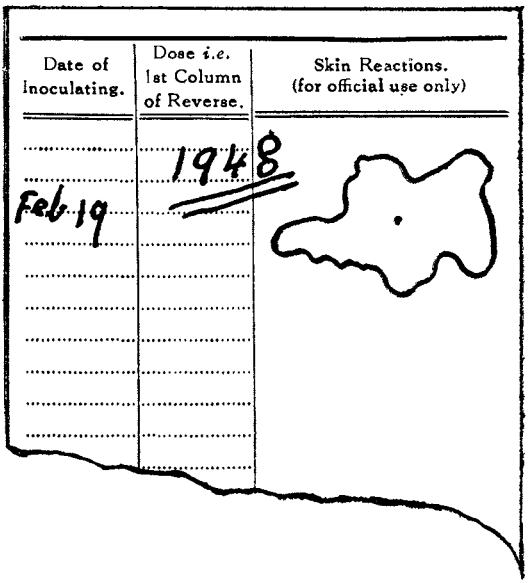


FIG. 18

The tops of the first two pages of the patient's dose card. These have been filled in from the case card, and ready for issue to him.

sterilises, but leaves the barrel and plunger nicely oiled. The sterile syringe is then inserted into its protective sheath and put into the inoculation set, as shown in Fig. 19.

A Negative Pressure. It is also embarrassing to the novice inoculator if a strong positive pressure in any of the bottles forces the contents into the syringe without the control of the self-inoculator. In general, it is better to have ordinary atmospheric pressure within the bottles, or if anything a *slightly* negative pressure.

This is particularly the case with the bottle of alcohol. If there is at all a strong positive pressure within this bottle, alcohol will force its way into the syringe during the act of sterilising the outside of the needle.

The obvious course is to aspirate some of the air from the bottle and thus reduce pressure. But it is tiresome to have to explain this to the patient ; therefore, do it for him beforehand.

In short, some competent person must look over the apparatus to make sure that everything that the patient will need is there and in good working order.

Sterilisation Bogies. We sometimes, but at very long intervals, see in the lay press hair-raising accounts of cases in court claiming damages when an abscess, or even a septicaemia, has resulted from a hypodermic injection. The well-known medical expert is questioned and cross-questioned by counsel as to the exact procedure necessary for sterilisation ; he is asked how far it is safe to carry the sterilised syringe with its hypodermic needle uncovered ; also he is asked what procedure is necessary to sterilise the skin *completely* before it is safe to puncture it. The nervous G.P. gives a shudder concerning the defendant, and murmurs to himself : ' There, but for the Grace of God, go I! '

I strongly suspect, however, that the accidental infection of the site of the puncture which brings the case into court has very little to do with methods of sterilisation of syringe or skin, but must have been brought about in one of three ways :

(*a*) by carelessly puncturing through a previously existing pustule on the skin of the patient ;

or (*b*) by injecting an already septic inoculum ;

or (*c*) and perhaps this is the most likely of all, by the ordinary human error of forgetting to sterilise, or perhaps even to wash out, the syringe after it had been used for evacuating an abscess on some other patient.

The steady use of it for a life-time has shown that Wright's method of sterilising with hot oil is safe and efficient. As for the skin, no sterilisation is necessary as will be explained later, nor is any method practicable by the customary procedures now in use.

The Inoculation Set. The box containing the necessary apparatus (and containing, of course, his personal dose-card) is placed before the patient as shown in Fig. 19 and the contents pointed out to him.

There are ten bottles in the box, each containing a different strength of vaccine ranging from 100 units per c.c. (which is the weakest) up to 100,000 units (which is the strongest). Behind these is a syringe in a glass casing.

To the right of the ten bottles of vaccine is a small flat bottle of clear glass containing surgical spirit and labelled ' Alcohol ' ; alongside this is a similar bottle (but of amber glass) containing a solution of adrenalin 1 : 5,000.

With all these is the dose-card, already inscribed with the patient's name etc. This, in Fig. 19, is of course lying on the open lid, and is showing the front page.

Pamphlet of Instructions. When this outfit is handed over to the self-inoculator on his departure from the teaching session about to be described, a pamphlet of 8 pages is also included ; this gives him some brief notes concerning the instruction he has just received. It will of course relieve him of the need for taking notes while the teacher is explaining things to him. The pamphlet is reprinted at the end of the book as an appendix (pp. 307 to 314).

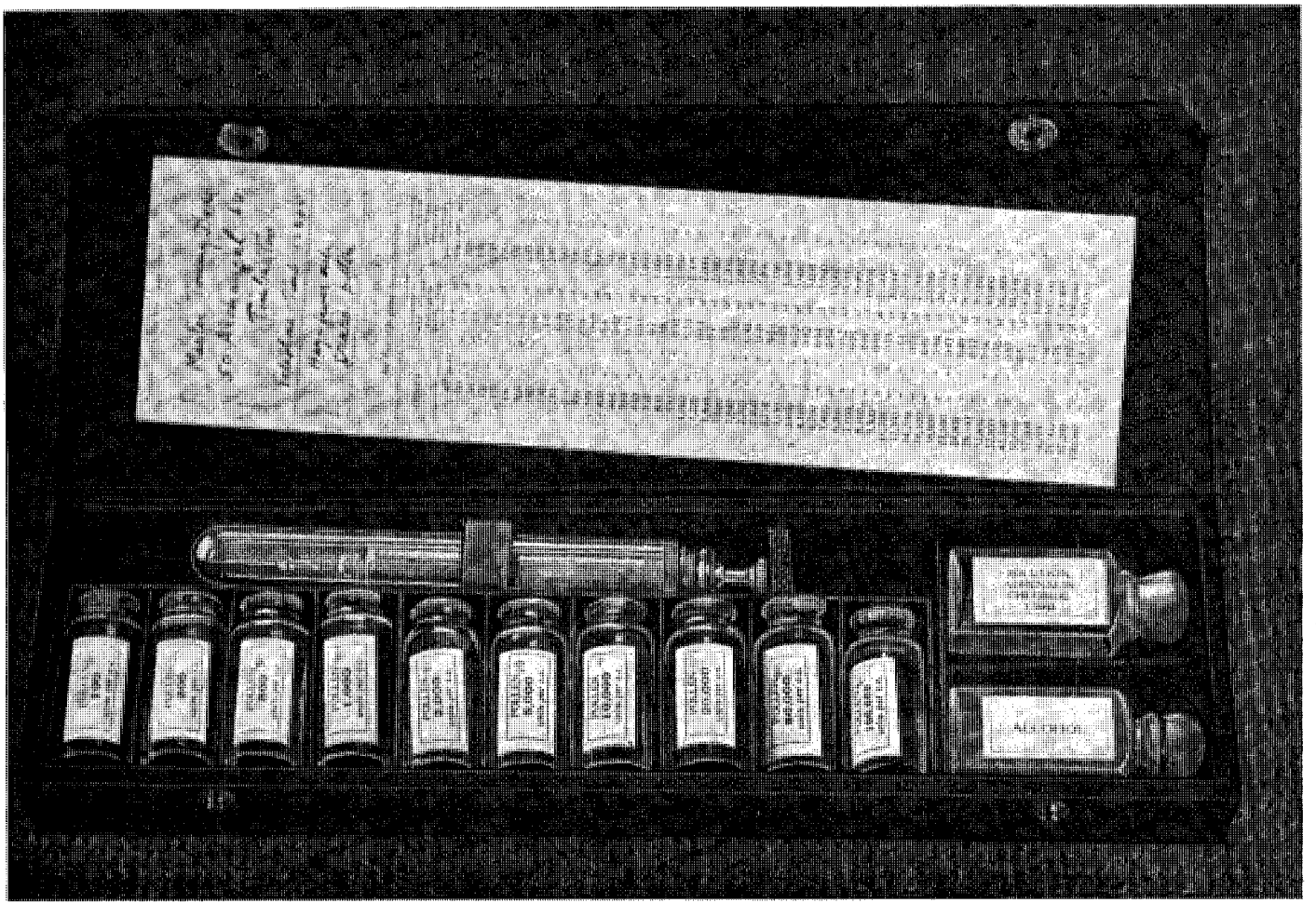


FIG. 19

The Inoculation Set. Reproduced half actual size. Contains the complete outfit for self-inoculation. The dose card is on the lid of the box. (The Instruction Pamphlet has been omitted.)

It is as well, I find, not to hand this pamphlet to the patient at the beginning of the teaching session with the set as shown in Fig. 19 ; if you do so he may start reading it, instead of listening to what is being taught him.

Examine the Syringe. The syringe should be lifted from the box in its sheathing test-tube by the little tab, as shown in Fig. 20.

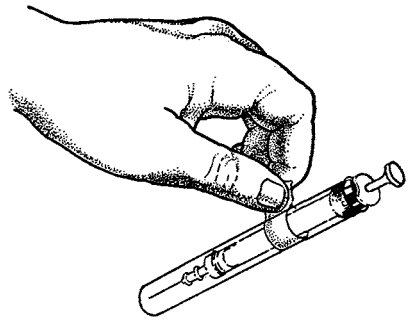


FIG. 20
Left hand lifting the syringe in glass tube out of box by its tab.

This syringe is fixed into its glass sheath by a piece of rubber tubing put round its barrel in such a way as to act as a cork in the mouth of the test-tube sheath. To remove the syringe safely from its sheath, hold this horizontally in the left hand and place the nails of the thumb and first two fingers of the right hand firmly on the protruding portion of this rubber cork (Fig. 21) ; then, if the sheath-tube is gently rotated by the fingers and thumb of the left hand, the syringe should come out easily, and without any damage to the fragile piston or to the delicate point of the hypodermic needle. This action should perhaps be demonstrated once or twice to the patient.

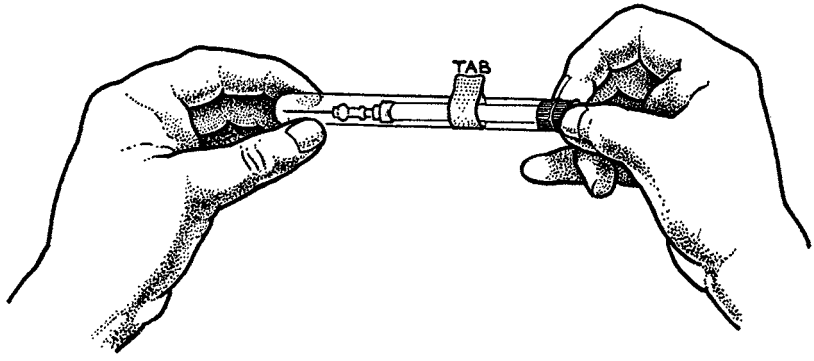


FIG. 21.—Withdrawing syringe from its glass protecting tube.

When the syringe is safely delivered from the sheathing-tube (Fig. 22) the patient should take it in his hand, see how the piston-rod moves the plunger up and down, and note the markings on the barrel of the syringe.

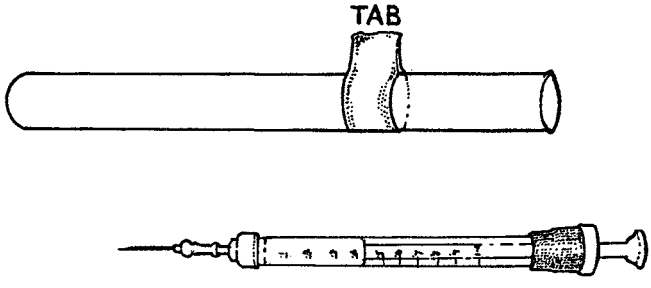


FIG. 22.—Syringe safely out of its sheath.

The Syringe Markings. As my pollen extracts—or the other extracts for that matter—are made up to contain so many units per c.c., the syringe must be a cubic centimetre syringe, and be divided at least into tenths of a c.c.

As during the course of treatment we shall have to work to two places of decimals very frequently, it is useful to have a very long syringe of the so-called ' tuberculin type ', i.e. one capable of being divided up into 100 divisions, by the sub-division of each tenth part of the cubic centimetre into further tenths, as in Fig. 23 below.

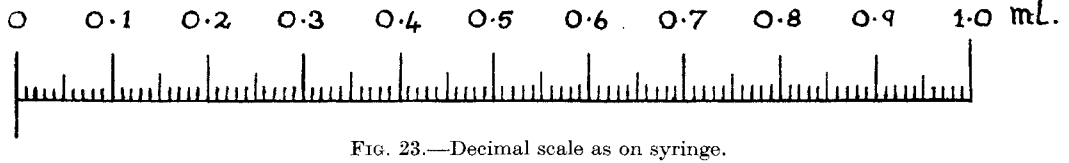


FIG. 23.—Decimal scale as on syringe.

Teaching the Measurements. To see that the patient does really understand these markings, he is instructed to put the plunger accurately at given points: first, let us say, at the 0·4 of a c.c. mark—which is the measurement for the first dose on the dose-card; then perhaps at 0·6 of a c.c.; and then at the full 1·0 c.c. mark, which is of course at the extreme limit of the scale. Patients may be a little hazy as to the difference between 0·1 and 1·0, and that must be explained clearly before they can get into the well-known ' Oh, all this mathematics is terrible ' state.

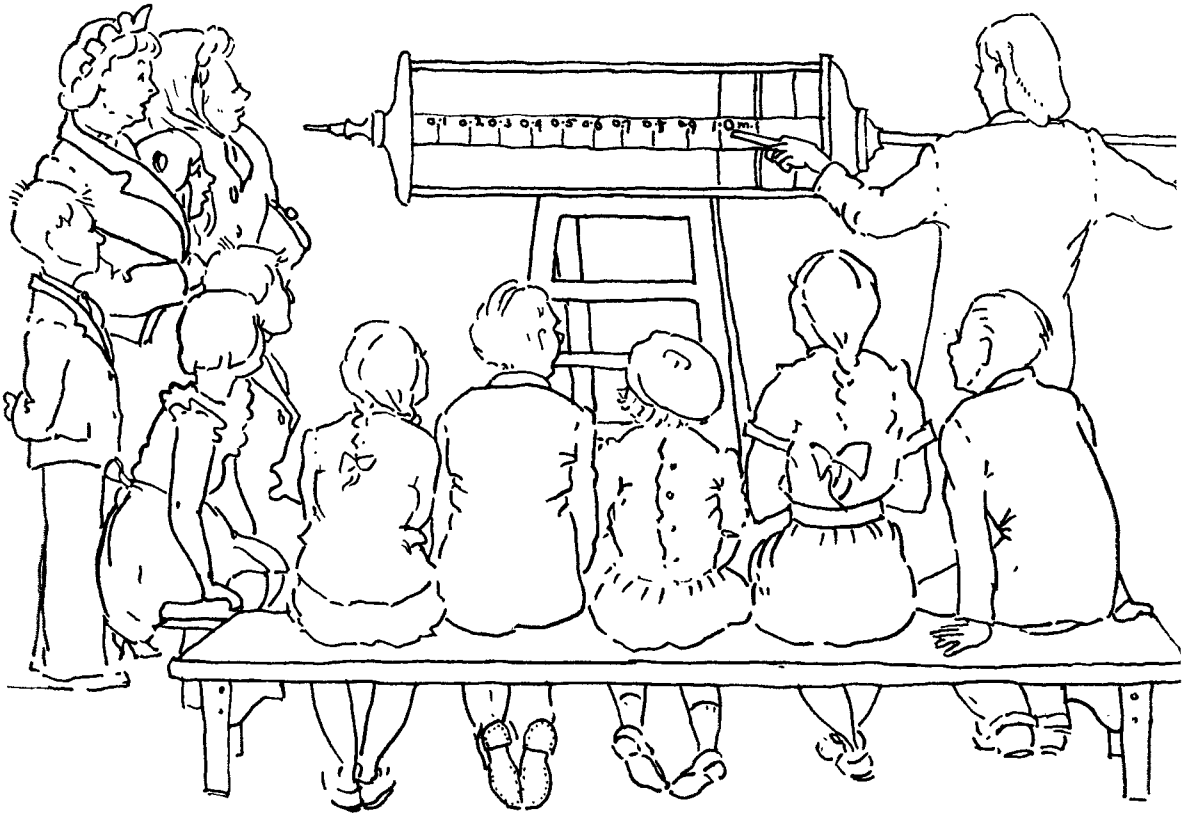


FIG. 24.—Nurse demonstrating pantomime syringe.

The next move is to explain the second place of decimals. The demonstrator might, for example, put the plunger of the syringe to the 0·46 mark (the amount taken for the tenth dose, i.e. 230 units). When the patient has had a good look at this position of the plunger and understands it, he may be invited to put the plunger at the 0·53 mark, i.e. the next amount on the list, or at the 0·92 mark, and so forth.

There is no practical difficulty in teaching small children, or even elderly adults, what the scale on the syringe really *does* mean. If we do have any difficulty with a big class, they are taught on a large ' Pantomime Syringe ' (see Fig. 24).

It is of course very easy for an instructor to show on this big syringe exactly where the piston should go to measure up to any required mark, and a crowd of pupils young and old can be taught all together. I find this pantomime syringe is seldom needed, however, and never when a single pupil is being instructed.

When the meaning of the marks on the patient's syringe has been thoroughly mastered, this syringe is replaced in its sheathing test-tube, and put back in its place in the box.

The Dose-card. The patient or patients are then instructed to take up their own dose-cards and look at the front page. The top portion, as we have seen in Fig. 17 above, gives the name of the individual patient, and also the nature of the vaccine to be employed. Here in Fig. 25 is the remainder of that front page ; this shows in three vertical columns (1) the dose to be inoculated, (2) the amount of fluid to be taken into the syringe to get that dose, (3) the bottle from which this inoculum should be taken.

(1) **The Dose.** In the left-hand column is the list of all the doses given in Noon-units—each unit representing a millionth of one gram of grass pollen. (This dose-list has been given already on p. 70 in Fig. II.)

These doses begin with forty of such units ; they end with a hundred thousand of them—which is two or three thousand times as much as the first dose. It is clear that it would be quite impossible to get all that long range of doses out of one single bottle : no syringe could compass it. Actually it is found convenient to have ten different strengths of the pollen extract, as said above.

(2) **The Volume** of the inoculum (i.e. the amount that must be taken into the syringe from that correct bottle in the right-hand column) is given in decimals of a c.c. in the *centre* column. Thus, to put the top line of the dose-card into words, we get a dose of 40 units by taking 0·4 of a c.c. from the 100 unit per c.c. bottle. And so for every dose, reading from left to right.

(3) **The Bottles.** The patient should next be told to look at the third or right-hand column. Here is indicated just which of the ten different strengths will best serve for the measurement of any particular dose. Thus the first dose of all (at the top of the left-hand column), i.e. 40 units, should come out of the 100 unit per c.c. bottle ; and so should the next three doses—each requiring a little more and a little more out of that bottle to gain the steadily increasing dose. But if an attempt were made to get the fifth dose from that same bottle (i.e. a dose of 120 units) the

esensitization).

DOSES	Amount to take into the syringe to get the dose	Bottle from which the dose is to be taken
40 units	= 0·4 of	100 units per c.c.
60 ,,	= 0·6 of	100 ,,
80 ,,	= 0·8 of	100 ,,
100 ,,	= 1·0 of	100 ,,
120 ,,	= 0·6 of	200 ,,
140 ,,	= 0·7 of	200 ,,
160 ,,	= 0·8 of	200 ,,
180 ,,	= 0·9 of	200 ,,
200 ,,	= 1·0 of	200 ,,
230 ,,	= 0·46 of	500 ,,
265 ,,	= 0·53 of	500 ,,
300 ,,	= 0·6 of	500 ,,
345 ,,	= 0·69 of	500 ,,
400 ,,	= 0·8 of	500 ,,
460 ,,	= 0·92 of	500 ,,
530 ,,	= 0·53 of	1,000 ,,
610 ,,	= 0·61 of	1,000 ,,
700 ,,	= 0·7 of	1,000 ,,
800 ,,	= 0·8 of	1,000 ,,
920 ,,	= 0·92 of	1,000 ,,
1,060 ,,	= 0·53 of	2,000 ,,
1,220 ,,	= 0·61 of	2,000 ,,
1,400 ,,	= 0·7 of	2,000 ,,
1,600 ,,	= 0·8 of	2,000 ,,
1,840 ,,	= 0·92 of	2,000 ,,
2,100 ,,	= 0·42 of	5,000 ,,
2,400 ,,	= 0·48 of	5,000 ,,
2,750 ,,	= 0·55 of	5,000 ,,
3,150 ,,	= 0·63 of	5,000 ,,
3,600 ,,	= 0·72 of	5,000 ,,
4,100 ,,	= 0·82 of	5,000 ,,
4,700 ,,	= 0·94 of	5,000 ,,
5,400 ,,	= 0·54 of	10,000 ,,
6,200 ,,	= 0·62 of	10,000 ,,
7,100 ,,	= 0·71 of	10,000 ,,
8,100 ,,	= 0·81 of	10,000 ,,
9,300 ,,	= 0·93 of	10,000 ,,
10,700 ,,	= 0·53 of	20,000 ,,
12,300 ,,	= 0·61 of	20,000 ,,
14,200 ,,	= 0·71 of	20,000 ,,
16,300 ,,	= 0·81 of	20,000 ,,
18,800 ,,	= 0·94 of	20,000 ,,
21,600 ,,	= 0·43 of	50,000 ,,
24,800 ,,	= 0·49 of	50,000 ,,
28,500 ,,	= 0·57 of	50,000 ,,
32,800 ,,	= 0·66 of	50,000 ,,
37,800 ,,	= 0·76 of	50,000 ,,
43,500 ,,	= 0·87 of	50,000 ,,
50,000 ,,	= 0·5 of	100,000 ,,
57,000 ,,	= 0·57 of	100,000 ,,
66,000 ,,	= 0·66 of	100,000 ,,
76,000 ,,	= 0·76 of	100,000 ,,
87,000 ,,	= 0·87 of	100,000 ,,
100,000 ,,	= 1·0 of	100,000 ,,

FIG. 25
Shows the lower three-quarters of the first page of the dose card, i.e. remainder of Fig. 17.

syringe would be overfilled ; so we pass to the next bottle (i.e. of 200 units per c.c.), and of course take a good deal less of the fluid.

It should here be pointed out to the patient that these ten different bottles shown in that third or right-hand column correspond with the ten different bottles of vaccine arranged along the length of the box of equipment (Fig. 19).

The Inside of the Card. Before putting the card down, the patient should perhaps look inside it ; here he will see of course his own diagnostic weal, together with the date on which the test was made (Fig. 18). He can be told that as the treatment progresses, so will that weal get smaller and smaller, till in the end it should be scarcely (if at all) distinguishable from the result round the control prick-test made through the normal saline.

The card should now be put back into the box preparatory to the teacher giving a demonstration of how to charge the syringe with the first dose, and so be ready for use.

Sterilising the Needle. The box is opened once more by the demonstrator and the dose-card put down on the table in front of him. Then the syringe in its sheathing tube is lifted out of the box by the little tab (as shown in Fig. 20) and the syringe itself is carefully removed (as shown in Fig. 21).

The demonstrator takes the little flat bottle of alcohol from the set ; then, holding this in his left hand, and holding the barrel of the syringe between the thumb and first two fingers of his right hand (as when throwing a dart), the needle of the syringe is thrust through the rubber cap of the alcohol bottle and into the alcohol within. Then the bottle of alcohol and syringe are placed on the table as in Fig. 26 below :

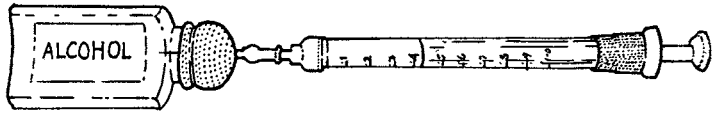


FIG. 26
Sterilising the *outside* of the hypodermic needle.

It is, of course, necessary to avoid blunting the hypodermic needle on the glass neck of the alcohol bottle, also to see that the needle is bathed in the alcohol, and is not merely resting in a bubble of air above it.

During the course of treatment the outside of the needle necessarily becomes contaminated with body-fluids and with bacteria ; so before each inoculation I insist on the needle being washed in alcohol.

The Dose to be aimed at. Leaving that needle sterilising in the alcohol, the demonstrator should go through the motions of looking within the dose-card to see what the *last* dose to be inoculated may have been. Of course, both the demonstrator and the pupil must know that no dose has yet been given ; but this look should be made none the less, otherwise it may be forgotten after treatment has commenced.

The demonstrator's audible comment should run : ' No dose has yet been given, so the " next " dose *must* be the first dose.' He then turns back to the first page of the dose-card (Fig. 25) and puts his finger under the first dose on the card, i.e. the 40 unit dose printed at the head of the left-hand column in Fig. 25.

The Correct Bottle. He then passes that forefinger horizontally to the top figure in the right-hand column, thereby finding that the correct bottle to be used should be the one containing 100 units in every cubic centimetre of its fluid : this is naturally the weakest strength provided in the set. The demonstrator and pupil should not arrive at the correct bottle for this reason, but ' because the card says so '.

This 100-unit bottle should then be lifted from the set, and the label on it put against the corresponding figure in the dose-card, while the demonstrator says aloud : ' One-0-0, One-0-0 : always see that the figure on the dose-card, and the figure on the label of the bottle, tally exactly ' ; make absolutely certain that you *have* got the right bottle out, as shown in Fig. 27.

It is difficult to get some patients to put the label on the bottle against the strength as indicated on the case card, and then to ' count the noughts '.

At this point I may explain that I have known two women who took the first dose out of the strongest bottle by mistake ; fortunately nothing very serious happened, but they were very sorry for themselves for several hours.

And I have known perhaps a dozen women when under tuition to take up the wrong bottle and go through the whole business of measuring out the dose, and even to do a ' careful ' check-up, without detecting their silly error. I may add that I have never known a man or a child of either sex to make this mistake ; it only happens when the ladies have reached what W. S. Gilbert called ' years of indiscretion '.

The Correct Amount. Then the forefinger of the left hand should be moved to the top figure of the central column and this running commentary made : ' 0 point four of a c.c. : that is the amount we must take from this bottle to get the correct dose because the card says so.'

Withdraw the Needle. By this time the outside of the needle on the syringe has been washing in the alcohol for more than the necessary half a minute, so the syringe and its needle may be withdrawn from the cap of the alcohol bottle which it has been puncturing. The alcohol bottle should be left where it lies on the table.

The demonstrator may then make the comment : ' This sterilisation of the outside of the needle is the only piece of sterilisation which takes place in the entire technique of self-inoculation. The *inside* of the needle, and the *inside* of the syringe, are sterile when you receive the set ; if the syringe and needle are used only as described in this technique, sterilisation other than of the outside of the needle as we have just performed it will be unnecessary.'

If any alcohol has inadvertently found its way into the syringe, this should be carefully ejected before the business of measuring out the dose is begun.

Draw in the Air. At this point the demonstrator may perhaps refresh his memory as to the bulk of the inoculum by looking once more at the top of the centre column, and he will once more murmur aloud : ' 0 point four of a c.c.'

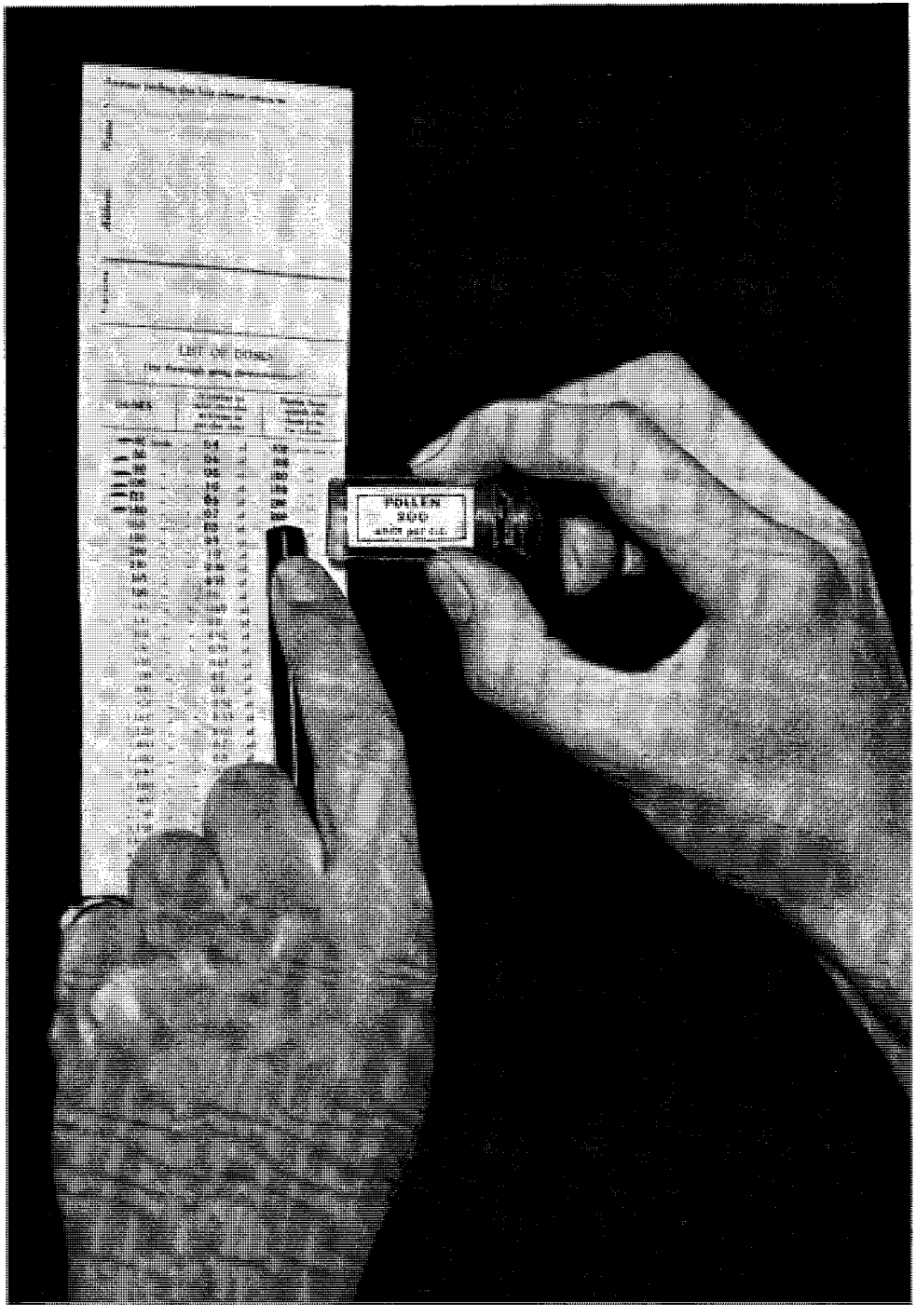


FIG. 27.—Shows the incident of making the figure on the label tally with the figure on the card while measuring out the 6th dose, i.e. of 140 units.

He then draws 0·4 c.c. of *air* into the syringe by withdrawing the plunger to the 0·4 mark. This is a little unexpected by the patient, but it should be insisted upon.

Such a procedure serves two purposes. If this amount of air is blown into the bottle every time before a similar quantity of vaccine is removed from it, the pressure in the bottle will be kept constant or nearly so.

Also, if this amount of air is injected through the needle into the bottle, any protective paraffin wax (which contains a small quantity of phenol for purposes of sterility) that may have got lodged in the lumen of the needle will be ejected, together with the air, like a cork out of a pop-gun. It is perhaps not necessary to give this explanation to the patients unless they are mechanically minded.

Blow out the Air. The syringe containing the 0·4 c.c. of air should be held by the barrel precisely as shown in Fig. 28 below, i.e. as if it were a dart to be thrown.

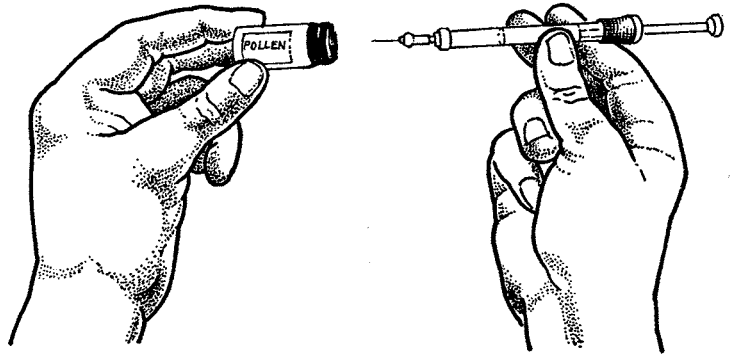


FIG. 28
Holding the syringe correctly.

The needle should then be thrust through the cap of the 100-unit bottle which has been resting meanwhile on the table in front of the demonstrator: now the plunger should be pushed home, thereby injecting the 0·4 c.c. of air into the bottle.

Take out the Dose. The bottle, now impaled on the needle of the syringe, should be held bottom upwards so that the big bubble of air in the bottle may be well away from the opening of the needle—as shown in Fig. 29 below.

It may perhaps be explained to the patient that if the needle-opening is in the bubble of air, then air and not vaccine will be sucked into the syringe.

The piston should now be withdrawn so that the vaccine is sucked into the barrel of the syringe. Usually at this point it is seen that there is a largish bubble of air in the syringe itself; this is actually the air that was contained in the nipple and butt of the needle. The bubble should be pointed out to the patient; it should be explained that, though that bubble would do no harm whatever to the patient if injected beneath his skin, yet it would clearly make an error of measurement, and therefore it must be got rid of by injecting it back into the bottle.

As bubbles will float on top of a watery fluid, it is clearly necessary to hold the syringe vertical when putting that bubble back into the bottle; but, as it is

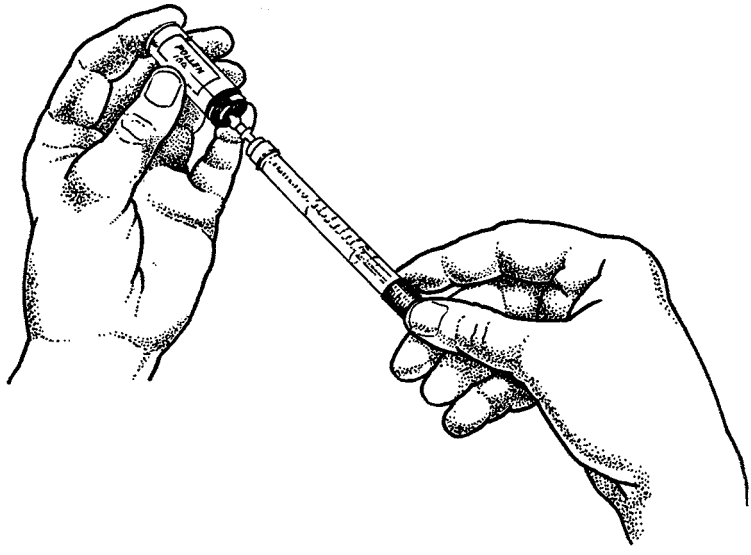


FIG. 29
Withdrawing vaccine from the bottle.

difficult to explain to patients what is meant by ' vertical ', I now instruct the patient after he has drawn a considerable quantity of fluid into the syringe (plus that bubble of course) to place the end of the piston-rod on the table with the syringe pointing straight upwards into the bottle as shown in Fig. 30. Then, with a brisk movement downwards of bottle plus syringe, that bubble can be shot up out of the syringe and back into the bottle.

All the intruding bubble may not be got rid of at the first attempt, but two or three downward movements, as in Fig. 30, will reduce the air in the syringe to negligible amounts. Then, so long as the air in the bottle is kept well away from the needle opening, the correct amount of fluid (i.e. up to the 0·4 mark) can be withdrawn at leisure.

When the exact amount has been withdrawn, the bottle should be *quickly* lifted off the needle and syringe, and the bottle from which the inoculum has been withdrawn should be placed on the table. It is necessary to emphasise that ' quickly ' because if the pressure in the bottle differs considerably from the pressure in the outside world, and if the plunger of the syringe runs too easily in the barrel, it is

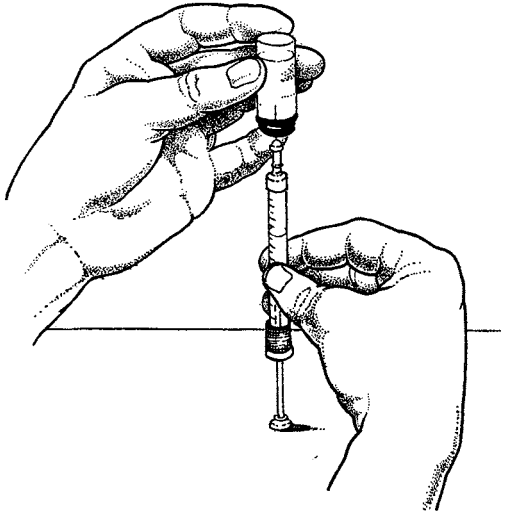


FIG. 30
Getting rid of the bubble in the syringe.

possible that this difference of pressure will either suck back fluid from the syringe, or will blow more vaccine into it than is required. It will falsify the measurement.

Not too little. If, because the light is bad or because the plunger is moving irregularly in the syringe, it is difficult to get the *exact* amount of fluid into the syringe, it is better to take too much rather than too little ; if too little has been taken, recourse must be had again to the vaccine bottle. But if a little too much has been taken, it is quite easy to correct this by holding the barrel of the syringe with the left hand, as shown in Fig. 31, and then gently rotating the piston-rod and plunger with the right forefinger and thumb while at the same time pressing the piston and plunger gently forward towards exactly the correct mark on the syringe. In this way it is quite easy to get rid of, say, one tenth of a drop, or less than 0·01 of a c.c. : see the drop on the end of the needle in Fig. 31.

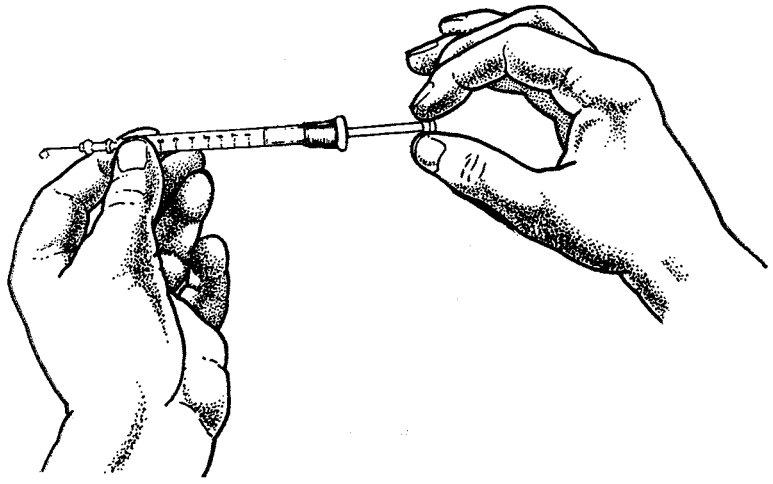


FIG. 31
Getting rid of excess of vaccine from the syringe.

When the exact amount of inoculum is in the syringe, place this on the table, together with the dose-card, the alcohol bottle, and the bottle of vaccine which has just been used.

The Check-up. It is necessary of course that mistakes should have been avoided in the measuring of the doses. It is also necessary that no suspicion of a mistake should cross the mind of the self-inoculator as he is putting the needle into himself through the skin ; his mind will eagerly seize this excuse for postponing the dreaded puncture. I therefore always insist on a check-up with the four objects which should now be lying on the table in front of the inoculator.

In checking up, I tell the patient never to rely on memory, but purely on the evidence of these four objects in front of him. I sometimes say : ' Imagine yourself to be an inspector (or a school-teacher) who has come in from outside after you have measured the dose into the syringe ; he wishes to make sure, by examining the evidence of the four objects left on the table, that no mistake has been made.'

Children have no difficulty about this conception, but the grown-ups sometimes don't find it easy. To quote my old chief Almroth Wright, who was fond of misquoting Scripture: 'Unless ye become as little children, ye shall not enter the Kingdom of *Science*!'

The running commentary of the check-up should be somewhat as follows:

(1) 'Did I sterilise the outside of the needle? Yes, obviously I did, because the alcohol bottle is out on the table; and, what is more, there is a damp spot on the rubber where the needle punctured it. So that is all right'—and I put the bottle away in the set.

(2) 'What dose should I have been aiming at?' Then looking on the inside of the dose-card:—'No dose recorded yet; so it *must* be the first one.' Then looking back to the top of the left-hand column of the first page of the dose-card:—'The card says that the first is a dose of 40 units, which *should* come from the 100-unit bottle.' Then picking up the vaccine bottle lying on the table, and approximating the label to the corresponding figure on the dose-card as in Fig. 27:—'One-0-0 and one-0-0 —it did come from that bottle; so that was correct, and I can now put the bottle away.'

(3) 'Did I take the correct quantity into the syringe? The top figure of the dose-card says 0·4, and the syringe is set at 0·4 also. Everything is obviously quite correct, and all that now remains is to inject the dose.'

The Patient tries. The teacher must not be content with a careful demonstration given by himself, and accompanied by his running commentary to the patient; so having put everything away again into the box, he should ask the pupil to change places with him and measure out a dose for him- or herself, using precisely the same technique as has just been demonstrated.

In order to make sure that the patient understands thoroughly, I often say: 'Imagine that, on looking on the inner side of the card, you see that you have already had the first dose of 40 units, therefore you are to measure the next one. That will be the second dose, one of 60 units of course.'

The average pupil will go through the whole business exactly; but the careless and unobservant may have to be prompted—particularly as to injecting into the bottle as much air as he will subsequently be taking of inoculum. Also the clumsy pupil may have trouble with his hands in getting that tiresome bubble in the syringe back into the bottle.

If the patient is too hazy about it all, then it may be necessary for the teacher to demonstrate the technique once or twice again in the same order as before, and using the briefest possible running commentary.

If there is any doubt whether the patient understands exactly, then it is a good plan to make him measure out a variety of different doses. For example, he can be told to imagine that doses up to the 200-unit dose have been recorded on the inner page of the dose-card; therefore the next dose to be measured by him is the one *after* 200 units—viz. 230.

The Patient checks too. I hold it necessary for the patient, always after each measurement of a dose, to check the accuracy of this work in the way in which

the teacher has done it—i.e. on the sole evidence of those four objects on the table before him.

Intelligent and humble-minded people will see the need for this, but occasionally the grown-ups (and particularly middle-aged women) become dignified and sulky about it, and say : ' But *of course* I remember what I have done, doctor! '

Sometimes, when patients have been repeatedly careless in carrying out technique, it is useful to allow the mistakes to pass without comment to see if they will be detected in the check-up. ' Now you have made one bad mistake in what you have done ; if you can't detect this in your check-up, you are certainly not safe to be entrusted with this job of self-inoculating.'

Loss of Temper. The moment the teacher realises that he is losing his patience with a pupil self-inoculator, *both* should take a rest. It is a good plan to hand that patient over to somebody else for a spell of teaching, though they must always come back to some doctor for a final demonstration of their competence.

In extreme cases it may be necessary to tell the patient to come back on another day for a second teaching session on the plea that they are too tired to go on. This should only happen once in a hundred cases, or perhaps less often than that.

Thus the doctor has the matter entirely in his own hands. He certainly should refuse to allow the patient to proceed to further stages of teaching unless he is convinced that the patient can manage the measurement safely, and until the patient is sure of this too. It is the doctor's responsibility.

The Charged Syringe. All this teaching of technique, though lengthy on paper, has so far taken us ten minutes perhaps with the patient ; and there is the syringe, ready charged with the correct dose of vaccine, on the table in front of the person who is to self-inoculate : no one concerned can have the slightest doubt that the measurement of that dose is accurate and according to the dose-card. All is now ready for the act of inoculating.

The Inoculation

As a rule I don't ask the patient to give himself the dose straight away, and in public. Sometimes they want to do this, but if they have quite recently been skin-tested (which is in itself a dose) it is better for them to give the first inoculation at home.

In place of this, I continue the demonstration as if on myself ; in fact I find it is often worth while to make a real puncture through the skin of my own leg to show them exactly how it is done and that the pain is negligible if the puncture is carried out correctly—though this isn't strictly necessary. In preparation for this, the syringe is emptied of its dose, and then recharged with normal saline measured meticulously to exactly the same volume as previously.

What Area of Skin? The self-inoculating patient always wants precise information about this ; he may be told that any area of skin will serve where it is possible to pick up a fold of skin between the forefinger and thumb of the left hand, while the right hand is free to wield the syringe. This of course rules out the skin of either

arm, and in practice means one of three areas, which I give in order of popularity : (1) the upper surface of the thigh, (2) the front of the abdomen, and (3) on the chest below the clavicle.

(1) The front of the thigh is the most obvious, and it is in plain sight as one sits on a chair or on the edge of a bed. It also has the advantage that in both sexes it is fairly easy to get at without undressing.

I usually pull up a trouser leg to well above the knee and demonstrate how to handle the skin of the thigh with my left hand ; and at the same time I show with my right forefinger (or with a pen or pencil) the direction which the hypodermic needle should take : see Fig. 32.

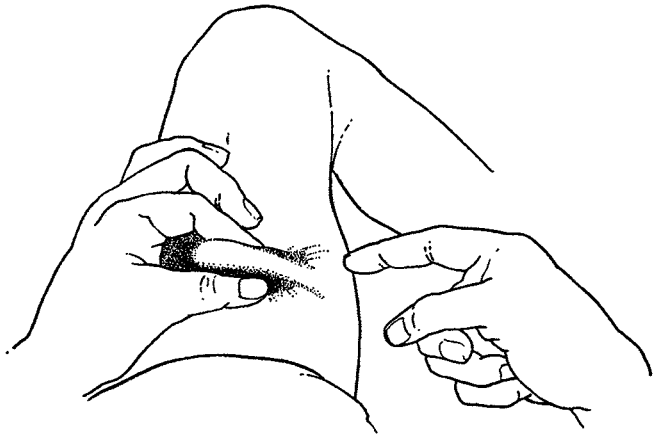


FIG. 32
The ' sausage-roll ' of skin into the end of which the needle should be thrust in the direction indicated by the finger.

(2) The skin over the abdomen is in most people much easier to pick up between finger and thumb ; it is quite as safe as the area on the front of the thigh, and is on the whole less obtrusive afterwards because there is more room for any slight oedema. It is wise to tell patients that they can only pick up skin and fat ; the belly wall is much too tight to be pulled up between thumb and fingers, so there is no danger of that.

The abdomen is, of course, an inconvenient site to get at in the day-time and in public, but it may be commended for home use as in Fig. 33, but note that the patient is depicted holding the syringe wrongly, i.e. *not* holding it ' like a dart '.

Women often have inhibitions about puncturing this area, but those who can be induced to try it usually approve of it.

(3) The chest under the clavicle is a good place, too, for subcutaneous inoculations, but the self-inoculator usually needs a looking-glass to see where he or she is puncturing.

Women are afraid of marking their chests with the punctures ; but, as they will be forbidden to use an antiseptic on the skin, prick marks won't be permanent. The chest is a good place on which to inoculate other people, but it is really not the easiest for the self-inoculator.

(4) and (5). The posterior portion of our anatomy is out of reach for the normal self-inoculator ; but it is very suitable if a mother or other relative is to inoculate some patient who is rendered incompetent by childhood or by old age.

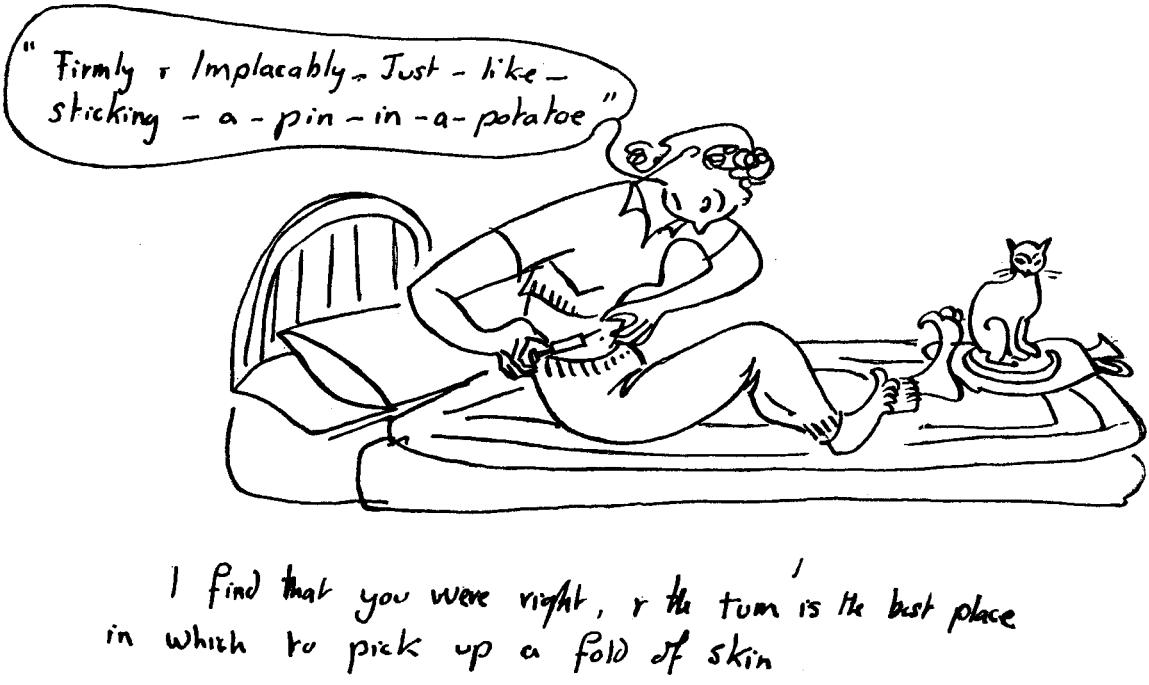


FIG. 33
Unsolicited testimonial from a self-inoculator in favour of the abdomen as a site for inoculation.

For small children, by far the best area is, I think, the upper part of the buttock. Exactly what is meant by this can be demonstrated to the mother by placing the child prone on a table or bench and demonstrating the exact spot with the forefinger. The mother should be told that the sausage roll should be picked up in precisely the same way as shown in Fig. 32.

With elderly people it is perhaps better to inoculate them behind the posterior fold of the axilla on either side.

Of course, the upper arm can be used for old or young who are being inoculated by friends or relatives ; but here it is particularly necessary to verify that you are not in a vein, as described below, and I'd say it was not such a good area as its present popularity suggests.

Nearly every self-inoculator will at least begin by choosing the area of skin in front of the thigh, so instruction on how to inoculate can usefully be confined to that area.

Sterilisation of Skin? The answer is that none is necessary or desirable. That is fortunate, for none is practicable either.

The ordinary non-selective antiseptics employed for supposed sterilisation of the skin, if pushed hard enough to do anything, will half kill the skin and thereby allow microbes to grow more easily on it than they otherwise would do. If ordinarily healthy, the skin will take care of itself by virtue of Sir Alexander Fleming's lysozymes.

Perhaps the prettiest demonstration of the folly of using ordinary antiseptics on our living tissues was given by Alexander Fleming when he showed by ingenious experiments that, though microbes (e.g. typhoid bacilli) might not grow in ordinary human blood, they would be permitted to grow if, say, carbolic is added to the blood—thereby hindering the blood's normal antiseptic action.

Experiments can show the like evil effect of antiseptics on the skin—though not so dramatically perhaps.

For us in our present work the chief disadvantage of making this useless and potentially harmful application of an antiseptic to the skin is that it doubles or even trebles the time taken up in the act of inoculating, and its ritualistic character makes it very much harder for the self-inoculator to make the puncture in a sufficiently carefree manner.

I have known a self-inoculator, who had been told to sterilise his skin ' efficiently ', to lie awake half the night sweating with apprehension lest this unnecessary ceremonial dab of antiseptic had been forgotten by him. That is the way to bring on ' reactions ' to a dose.

I am putting this strongly because I repeatedly hear ignorant parents, or ignorant matrons at a school, insisting on this stupid ceremony on the plea that you can't be too careful, in spite of the protests from the self-inoculator. During the last forty years I have myself given, or have been responsible for, well over a million inoculations of patients without any use of antiseptics on the skin ; I have never seen a pustule produced thereby, yet. But due care must be taken to see that the syringe, needle and inoculum are sterile, and that the puncture is made through ordinary healthy skin.

When one comes to think of it, how many of the self-inoculators of insulin—e.g. at a restaurant—' sterilise ' the skin? And how many make septic wounds by neglecting this?

Cold Abscesses are a remote possibility ; but they can have nothing to do with ' sterilisation ' of the skin, for there is no infection at all.

I have never seen such abscesses happen from doses of grass pollen or with any other inoculum in a P.T.D. treatment. I have never indeed seen them happen after any injection in our Clinics for Allergic Disorders.

Once, however, I saw it happen, and three times running too, in a patient receiving a bacterial vaccine for colds. In each case the abscess was of course sterile : that particular patient took no damage from these recurrent ' abscesses ' and in fact did very well under treatment.

In short, both because it is useless and because it is a hindrance, skin sterilisation is to be avoided.

The Puncture is to be subcutaneous and not intradermal or intramuscular. If a puncture is made freely and at right-angles to the surface of the skin it is probable

that the needle will go through the subcutaneous tissues and into the muscles beneath. To avoid this, as has been said, and shown in Fig. 32, a sausage roll of skin is picked up in the left hand of the self-inoculator.

I usually emphasise the necessity of pulling up a fold of skin by saying that if I want, for example, to inoculate between my trouser and my skin, I could only do this with certainty if I pulled up the cloth away from the skin and then punctured the cloth which had been so pulled up. Then I add : ' You do precisely the same when wishing to inject between the skin and the muscle ; you pull up the skin and insert the needle into the sausage roll of skin so pulled up. This you do whether you are making the subcutaneous injection on the thigh, belly or chest.'

The Movement of the Hand when inoculating should then be explained to the patient—perhaps using a capped fountain pen or a paper-knife for a preliminary demonstration on your own leg.

The movement of the right (or syringe) hand should be precisely that with which any woman would put a darning needle into a pin-cushion : it should not be hurried, but it should not hesitate. There must be no movement to and fro of the syringe like the preliminary waggle when addressing the ball at golf. There should be no pause with the needle just above the skin while the inoculator summons up courage—the slightest hesitation will cause the puncture to hurt.

No Pain. The patient can be told that if only the puncture can be performed precisely as a darning needle is thrust into a pin-cushion, and also with the same thoughts in the head as would naturally occur in putting a darning needle into a pin-cushion, the self-inoculator will be rewarded by feeling no pain whatever.

Aside to Colleagues ; I believe this to be universally true, only there must be no hesitation whatever ; in fact the act of ' summoning up the courage ' for a puncture seems to be little more than a psychological preparation for the nerves to feel the sting of the prick.

On the Teacher. After this exordium, I pick up the syringe charged with saline, holding it as previously by the barrel just as if it was a dart. It is wrong to hold the syringe with the index finger over the head of the piston-rod, which is what patients generally want to do—and is in fact shown doing in the sketch on p. 110.

Then I quickly, but without any hurry or hesitation, thrust the needle right through the skin and into the centre of the sausage roll held up by my left hand, as in Fig. 32. I then wriggle the needle thus buried beneath the skin backwards and forwards a little to make it quite clear to the onlooker that it is actually in the subcutaneous tissue, and neither in the skin nor in the muscle.

Why the dose had better be administered subcutaneously rather than intramuscularly is because, as I explained on p. 71, there is less danger of the needle breaking off at the haft ; and if it does so break, it is far easier to recover from the skin than from the muscle fibres below it.

Not into a Vein. If I make this self-puncture it is chiefly because it is useful to demonstrate the manoeuvre for verifying that the needle opening is not in the lumen of a vein.

Letting go of the sausage roll of skin with the left thumb and fingers, the thumb and first finger is transferred to the nipple-end of the syringe to hold this firmly (with the needle of course still under the skin) while with the right hand the piston-rod of the syringe is pulled *out*, as if to try to suck some fluid from the leg into the syringe (Fig. 34). This pull-out should be repeated three or four times quite gently to make quite sure that no blood can be drawn back into the syringe. If any should come, the inoculum will of course be carried back with the blood and eventually, if the vein is a big one and the needle is right in the lumen, the whole syringe will become full of blood and dose mixed.

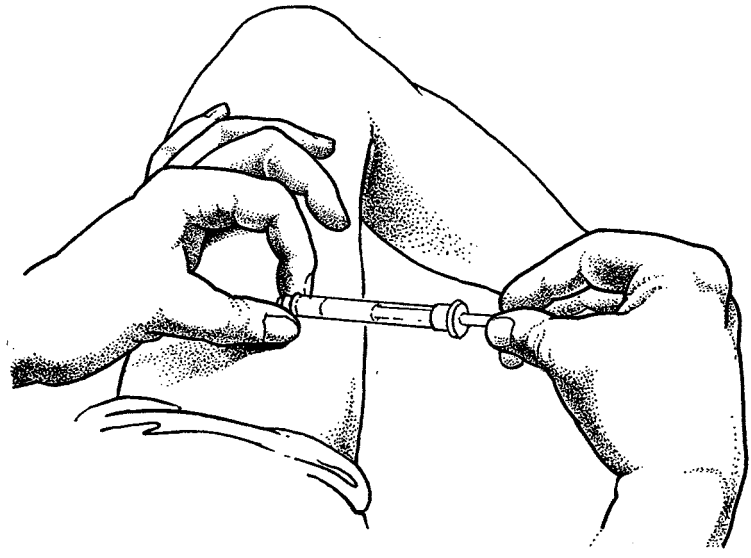


FIG. 34

Pulling out the piston-rod to see that the needle is not in a vein by accident.

If blood should come (and this happens only once perhaps in a thousand punctures) then the needle must at once be withdrawn from the skin, the inoculum should be ejected, and the syringe refilled with the current dose. Then a second puncture should be made elsewhere, and of course the drawing back of the plunger must be repeated. The actual bother to the patient will merely be the pulling back of the plunger to avoid this accident of intravenous injection. Blood will probably never come into the syringe ; but the test must not be omitted or the patient might get a severe shock.

The Injection. Having verified that the needle is not in a vein, I then inject the contents of the syringe under the skin by pressing the plunger and piston-rod home. Then the needle may be quickly withdrawn ; I always massage the puncture hole and track of the needle with the forefinger of the left hand to make quite sure that none of the inoculum will escape back along the track of the needle and out onto the skin. If any of the inoculum should escape the patient will not be fully prepared for the next dose.

Inoculation easy. The patient and his friends are accustomed to think that this act of subcutaneous inoculation will be the most difficult of all to learn, but in reality it is the easiest, the difficulties being almost entirely psychological.

One such psychological difficulty would be the slightest fear at the actual moment of inoculating that there might have been a mistake in the measuring of the dose. This is the chief reason for that rather pedantic check-up of the dose measurement described a page or two back.

When I first wrote about self-inoculation many years ago, I said that with a good technique the business soon became so ordinary that it took its place quite naturally between cleaning your teeth and saying your prayers on going to bed. Sir Squire Sprigge, the then editor of *The Lancet*, said he would not print my ribaldry in his beautiful paper which circulated in 'all the best houses'; so I amended the phrase to 'took its place quite naturally between cleaning your teeth and winding up your watch': but I objected that most of my children patients hadn't got watches. To this Sprigge replied: 'I'm afraid many of your grown-up patients don't say their prayers; so I prefer the watch version.'

Keeping the Record. As soon as the syringe has been withdrawn after emptying the dose beneath the skin, this dose, measured in units, together with the date on which it has been given, should be recorded in the space provided for these entries on the second page of the dose-card.

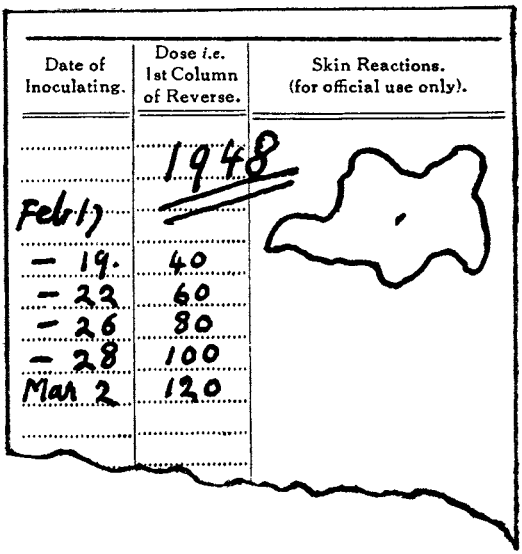


FIG. 35

The first five doses have now been entered on his card by the patient, together with the date of each injection.

Lazy patients will content themselves by putting a pencil tick against the dose on the front page, or they may even think they will manage to remember how many doses they have given; I think it is necessary to insist that they enter the number of units of that dose correctly and at once, before putting the apparatus aside till the next time.

I find that many people want to record, not the dose, but the amount of fluid they have had to withdraw from the bottle in order to get the dose.

I confess I can't understand the wish of the patients to put down on the card the fluctuating dollops of water which they have taken into their syringe; yet people do misunderstand this point time after time.

This seems very peculiar to me. It would be as if I recorded, not that I had gone to Paddington from my house, but had tried to convey that information by saying that I had taken the first two turns to the left from my doorstep and had then proceeded for exactly one mile—without mentioning Paddington at all!

If the patient puts down the actual dose and *not* the volume of water, then we shall have on our record a progressive series of doses which can be readily understood by the supervising doctor when the patient comes back for retesting. Also,

if skin-testings are recorded on the dose-card, as shown in Fig. 12, we shall see exactly how the steadily mounting dose is or is not affecting the patient's sensitiveness.

When to inoculate. This depends to a considerable extent on how much time the patient has in hand before the hay-fever season will begin. The best time to take the dose is perhaps on going to bed.

The patients can be told that, when they come back for a retesting of the skin reaction (e.g. at about the 1,060-unit dose), further instructions will be given them as to the rapidity of the remaining inoculations. This is important, otherwise the patient who begins in February may finish all the 54 doses much too soon or much too late.

When *not* to inoculate. The patient must then be warned not to inoculate when there is any considerable danger of a reaction being produced thereby ; and reactions are produced (as was explained very fully in the last chapter) by intercurrent illnesses of either the mind or the body.

Therefore, if any considerable infection develops during the course of treatment, and particularly if there is any rise in temperature, then the inoculation should be postponed till this trouble has passed away.

Also if the patient is sensitive to some foreign protein, such as horse emanations, and has inadvertently or deliberately made contact with these (e.g. has been spending a week-end on a farm, or has gone to a circus), then again the self-inoculation must be postponed till any considerable contact with horses and their derivatives, such as scurf and manure, has ceased.

If the patient is in a very worked-up state through some recent emotional shock, again the doses should be postponed ; and perhaps there should be added—when the patient is extremely fatigued or angry.

Alarm avoided. Patients, however, should be warned of these dangers with some discretion and an eye to the character of the patient ; otherwise the supervising doctor may find that when they pay a return visit in perhaps a month's time, only two or three doses have been accomplished by timid patients. A completely healthy person need not of course postpone the doses at all, and the majority of patients get no reactions.

In any event, all this business of reaction is discussed quite sufficiently in the pamphlet which the patient takes away at the end of the teaching ; the ultra-apprehensive ones will read more than enough there to warn them sufficiently.

Local Reactions. It is wise to warn patients a little (but not too much) about the possibility of reactions, both local and general. Patients are apt to get a swelling and stiffness, even a bruised feeling, at the site of the inoculation on the leg.

I don't as a rule say very much about this at the teaching session because it is undoubtedly true that the more the patient looks for such a swelling, the more one is likely to come.

These local swellings are never very serious, and the average patient makes quite light of them.

On enquiry as to whether there is any local reaction, I usually get some such answer as this : ' For the first three or four doses my leg did swell up and get tender at the place where I put the needle in, but since then I have forgotten to look at it, and I really don't think there is any swelling at all.'

General Reactions. As has been said, these will only occur if there is some definite tendency towards them caused by some change in the causal mechanism, as detailed on p. 83. Here again, it is better not to say too much about the chances of these reactions, save to sound the warning given above under the heading : ' When *not* to inoculate.'

The general reaction may of course take the form of any of the toxic idiopathies, as explained at some length in Chapter II, nettle rash being perhaps the commonest.

What to do. The best thing to do, if practicable, is to do nothing ; but if the beginning of a reaction is causing considerable fear, it is better to take a dose of adrenalin. The patient may be told that most people never have any reactions, and most of those that do have them don't bother to take the adrenalin.

Adrenalin. If this is to be given at all, it should be given promptly in small doses and repeatedly.

The teacher will notice that the adrenalin in the set is very weak—only 1 : 5,000 strength. I advise doses of 0·3, 0·4 and 0·5 of a c.c. progressively ; but usually there is no need to increase beyond the 0·5.

The adrenalin, if given at all, is to be given in precisely the same manner as the doses of vaccine above described, save that it is not necessary to re-sterilise the needle (because any such reaction would be coming on within from 5 to 10 minutes of the inoculation dose which has provoked it). As much air should be blown into the adrenalin bottle as you propose to take out of the fluid subsequently. The adrenalin should be injected subcutaneously, as above described for the pollen dose, the usual care being taken to see that it is *not* going straight into a vein. These doses may be repeated every 15 minutes till the trouble abates.

Seldom used. I find that the great majority of my patients never bother to use this adrenalin at all. If they try it from curiosity, they will get no sensation out of it, and so are not likely to acquire a craving for it ; at any rate I have never known this to happen.

I asked a man the other day on his coming to enquire about a fourth season of desensitisation treatment if he had ever used adrenalin.

' No, never,' he said. ' Oh yes I did, though ; I used it on my little dog last year who had been bitten by an adder. The poor little wretch was dying, so I filled my syringe with your adrenalin and gave him a shot, and that pulled him round in no time.'

Final Exam. After being taught along the lines given above, the patient is questioned thoroughly, and this time certainly by a doctor. It is this doctor's responsibility that the patient not only knows how to measure each dose exactly and inoculate with it correctly, but must be quite self-confident about it too. Though it doesn't very often happen, he may have to refer the case back for further tuition, or even direct that they come back for more teaching on another day. If

the patient has got into an ' Oh-this-is-dreadful ' state of fluster, it is better to make a good break—and change the teacher.

With smallish boys and girls the doctor must satisfy himself that they won't have too many difficulties with their grown-ups at home ; this can usually be seen fairly clearly from the demeanour of the home escort towards the child during the session of instruction (Chapter X). In some cases it may be found better, after all, to teach the parent, who must then of course undergo as rigorous an examination as the child has been having. This will be necessary for most children under ten years of age perhaps ; but it is fairly easy to teach children of seven or under, *if* they have very intelligent and self-restrained mothers.

After the would-be self-inoculator is passed as proficient and confident, then the box of apparatus, as shown in Fig. 16, complete with marked dose-card (Figs. 14 and 15), is handed over.

The Pamphlet, a copy of which is printed as an appendix at the end of the book, is designed to relieve the patient of taking notes of what is said ; it will therefore echo much of what has been said above, beside containing some extra matter such as ' pin-cushion drill '. Small boys and girls can't take satisfactory notes of course.

Further Visits must be insisted on before patients and friends leave the consulting room. If all goes quite well, three visits will be enough, and these should be roughly at a third of the way through the P.T.D. course, at two thirds, and when the course is nearly completed. What is much the same as that—you can say—when the dose reaches four figures, reaches five figures, and is almost up to six figures in Noon-units.

Patients must be told to bring back their dose-cards properly marked up with dates and doses so that the doctor can see just what they have done. The apparatus had best be left at home, unless there has been any difficulty with it which seems to need correction.

Not only is the skin to be retested and the resulting weal recorded on the card —and that can be given as the reason for these visits—but the doctor must see from the record of doses on the card whether these should now be accelerated or slowed down in order to be just in time for the pollen cloud when it arrives at the end of May. Advice on this will depend on past and future illness, etc., occurring during the P.T.D. course ; it is best to go ahead fairly briskly at the start and to go more slowly at the end of the course.

The patients should be told to come back at any time when they run into unexpected difficulties, and especially if they feel inclined to give up treatment because of reactions.

They may also have broken the syringe or have blunted the needle. The ' blunt ' needle is usually psychological, but if there is real damage it must be made good without question. It is useful to tell children that they must not try to break that syringe *more* than three times, and it is wonderful how very seldom any damage is done.

Jolts at New Bottles. On p. 72 I said that the more concentrated the dose, the quicker it would get into the circulating blood ; and the quicker it gets into the

blood stream, the more likely would be the occurrence of a reaction. Thus, half a syringe-full from the 2,000 unit per c.c. bottle might produce a greater shock to the patient than a whole syringe-full of the 1,000 unit per c.c. bottle, though they both contain precisely the same dose—i.e. 1,000 units.

Therefore there is a slightly greater expectation of reaction every time the patient proceeds to a stronger bottle of vaccine.

It is not worth while warning the patient of this little difficulty beforehand because few of them notice it ; but, if serious complaint is made when the patient comes back to be retested, then the remedy is simple, though at the expense of a few more doses. The last dose (i.e. number of units) given from the *old* bottle can be repeated in the first dose from the *new* before proceeding to the next dose on the card.

Top Dose. Patients desensitise fairly regularly, but some people respond more slowly than others, as shown by the skin testings ; in some cases it might even be necessary to arrange for several repetitions of the 100,000-unit dose at the end of the course if the size of the silhouettes is not diminishing satisfactorily. (Figs. 13 and 14, pp. 75 and 76).

More often it is a question as to whether we may not allow the patient to stop treatment a good while before this top dose has been reached because the desensitisation has been so complete as judged by the skin reaction. As a rule, however, I urge the patients to go on to the final top dose—especially in their first year.

Final Visit. At the final visit at about mid-May, the patient is usually told to finish the course of treatment by giving the few remaining doses, and then to put the equipment by till next year.

The strongest extract of 100,000 units per c.c. is very sticky, and next year the syringe will be clogged by this extract if any of it is left in the syringe. The plunger may be taken right out of the syringe, and left alongside it in the box, as explained in the pamphlet.

The patients should be told to come back for fresh inspection next February to see whether they need any further treatment.

By saying ' next February ', I find this successfully ' staggers ' their visits for all that month, and perhaps the first half of March. The very keen patients come at the beginning of February, the average patients come in the middle of February, and the indolent come at the end of February or during early March.

Next February. What about the people who have been through a year's treatment already, and want to know if they ought to have another ?

First, they can be assured that even if they have no fresh treatment at all, they will almost certainly have less trouble than they used to have before treatment was commenced ; but they must be told that they will probably have enough symptoms to make them wish they had gone through a second P.T.D. course.

We can be guided concerning the need for this by the size of the February skin reactions, which will be smaller than before any P.T.D. was started, but will be considerably bigger than the final record on the previous year's card. Fig. 36 shows

two series of skin responses, both taken during the P.T.D. courses of the year before, and again at the February visit in the new year. The first, (*a*), was judged not to need another course of treatment; the second, (*b*), was thought to need another year's treatment.

This regression to some degree of sensitivity is also shown in Figs. 10 and 13.

As much as possible, I try to leave it to the patients to decide whether they will be treated for another year or not.

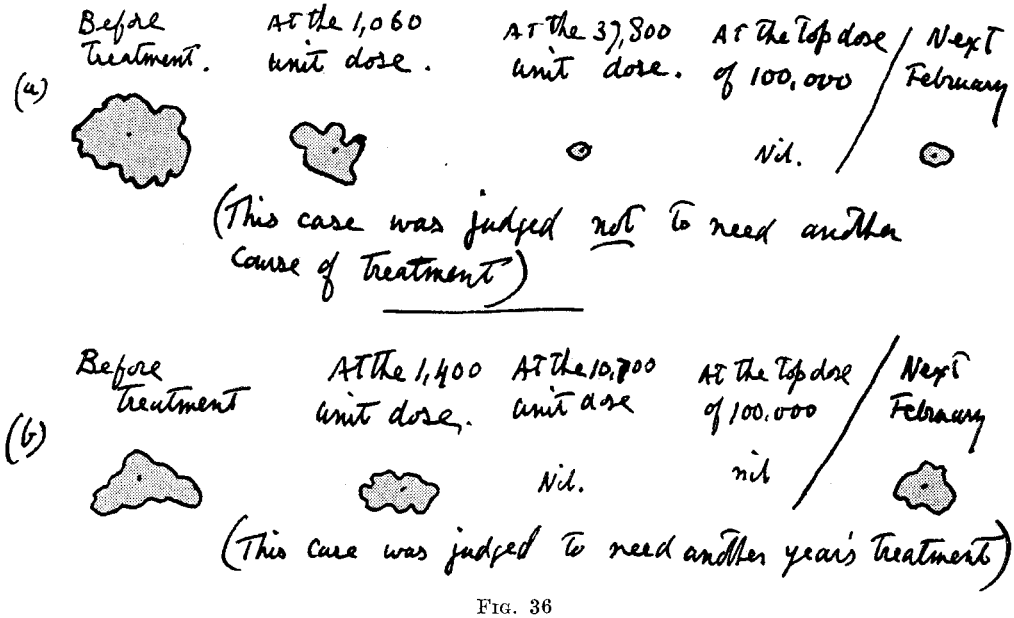


FIG. 36

Deciding, by next year's February skin test, whether or no a fresh course of P.T.D. treatment will be necessary for the coming hay-fever season.

I think it can be said in general that very few need only one year's treatment; most people need three or four. And some may need very much more than that.

As I have said elsewhere, there is seldom any difficulty in getting the patients to go on with their treatment in succeeding years; on the contrary, they are usually rather loath to stop.

It is of course not necessary to teach patients the business of self-inoculation all over again, except in the case where they have thrown in their hand and given up the attempt in the preceding year. Then I find a careful re-teaching is really necessary, so that the patient may regain confidence in himself. I usually remind patients of important little details in the technique, and particularly how they are to verify that they are not going to inject into a vein by pulling back the plunger before squirting the dose into the subcutaneous space.

CHAPTER VII

'CAUSE D'. SEROUS LEAK

The essential physical fact in any idiotoxic response is an oedema or serous leak from the blood-vessels at the parts of the body showing the symptoms of a toxic idiopathy. Obviously anything affecting oedema generally must concern us very much in the treatment of our allergic disorders.

The physiological principles involved in the production of oedema are complicated, and we must tread warily. Yet, because of the importance of this white haemorrhage in any orderly treatment of the toxic idiopathies, it is necessary to put down here the systemisation I have been working on. What *does* promote or retard this leakage?

I suppose we are on firm ground in ranging these retarders and promoters of serous haemorrhage under three headings :

(1) the porosity or permeability of the blood-vessel walls, and especially, of course, of the capillaries ;
(2) the viscosity or diffusibility of the plasma in the blood-vessels ;
(3) the pressure and supply of the blood to the part of the body in question.

These three divisions aren't quite watertight ; as we shall find in the next few paragraphs : there is some overlapping of causes for serous haemorrhage in these allergic disorders.

(1) **Porosity.** The idiotoxin-idioceptor poison mixture is said to act on the walls of the capillaries, making them more porous and thus making them more likely to leak ; and a ' histamine-like ' substance, which is unidentifiable, is said to account for this behaviour of the blood-vessel walls. Something was said of the poison mixture, i.e. idiotoxin already mixed with its idioceptor, in Chapter V, and more must be said of it in Chapter XIII ; it is the essential feature of the allergic reaction and there is no need to go over it again here, but this it must be which affects the porosity of the capillary walls.

It is not only the idiotoxic poison mixture which may affect the permeability of the capillary blood-vessels. Derangement of the blood-vessel walls by physical trauma (e.g. by external violence, or by excessive heat or cold) or by chemical traumata, presumably make these walls more porous and so lead to the escape of the contained blood fluids. That may be the mode of action by the trauma factor to be described in the next chapter ; indeed it is clear that idiotoxic and traumatic influences go hand in hand in producing an increased permeability of the capillary walls, thus leading to the oedema of a toxic idiopathy.

Violence sufficient to cause complete rupture of the capillary walls will naturally permit the escape of whole blood into the surrounding tissues, and so produce a

bruise or a haematoma. This is of course a break in continuity of the blood-vessel wall, and that is obviously not an allergic reaction ; but whole blood may occasionally be extravasated through the blood-vessel walls by allergic action, plus perhaps the very minimum of trauma.

The ordinary typical angioneurotic oedema is a pure serous leak, i.e. the holes or chinks in the capillaries will let through fluid but not the red blood corpuscles ; but when these oedemas are occurring very freely, with the minimum of trauma to help them on their way, they occasionally show as ' black swellings ' obviously produced by the leaking out of whole blood. That they are angioneurotic in nature is shown by being a sequel to, or by being interspersed among, ordinary colourless swellings of classical angioneurotic oedema. These black swellings are not so very rare in a clinic for the Allergic Disorders, yet I have known dermatologists to be surprised at them when they have been demonstrated. It is a question of the *degree* of porosity of the vessel walls attained by the combined allergic action and trauma action which produces the ordinary oedema.

(2) **Viscosity.** Clearly the more viscid the blood is, the less it will be able to take advantage of any weak point in the vessel wall ; the thinner and more fluid the blood is, the more easily will it diffuse from the lumen of the capillary.

The viscidity of the blood seems to run *pari passu* with its rate of coagulability : it is easy to demonstrate that any vegetable acid will lower the coagulation rate, and we shall see that the same acids will increase the tendency to oedema.

I remember in the old days that one of us was taking recordings every twenty minutes of the coagulability of the blood of a rather unwilling guinea-pig student ; throughout the morning they were finding the times to be changing only slightly—perhaps partly by errors of technique.

In the early afternoon the student pleaded for time off to get some food ; he returned 40 minutes later announcing that he had had a very good lunch. When the testing was resumed, the coagulation time was found to have more than doubled, and to be producing an indefinite clot even then.

' I say, what *did* you have for this famous lunch of yours? ' ' I had three large helpings of rhubarb tart ; there's nothing like it! '

It was some hours before the blood of that human guinea pig regained its clotting equilibrium.

Similarly the viscidity of the blood depends to a large extent upon the amount of physiologically available calcium contained in it. This may be thrown out of action by vegetable acid, so these vegetable acids will all tend towards a serous haemorrhage. Conversely, food containing calcium in a physiologically available form, e.g. milk, will tend to slow down any leak of serous fluid from the capillaries by making the blood more viscid. It is in this way that calcium, and preferably calcium in milk, will damp down the oedemas which constitute a ' reaction ' from an injudicious injection in a P.T.D. course of treatment—see p. 123, and also in the Instruction Pamphlet at the end of the book, on p. 311.

In my hay-fever prophylactic work, I have several times heard of schoolboys and schoolgirls who, thanks to P.T.D. treatment, have escaped all signs of hay-fever

—save on one or two occasions when they have eaten a large quantity of strawberries or some equally acid fruit. Without this decalcification, they were on the right side of the safety line to avoid all symptoms of hay-fever; but when their blood was sufficiently decalcified by the strawberry acids, then these patients at once became in danger of an attack. It would be hospitalising the patient to say that they must never eat any fruit—never eat a strawberry; it is far better to suggest instead that they should balance in their minds all the advantages and disadvantages of eating them—and then, if they wish to do so, take the risk. Parents often look down their noses at this levity in dealing with anything so precious as the health of their children, but I am convinced that it is a safer line to take than fussy caution, which defeats its own end.

Rhubarb, as we have seen, is a strong decalcifying agent by reason of the oxalic and other acids contained in it. So much is this the case that any marked recurrence of symptoms, say of urticaria or asthma, in the early spring prompts me to make enquiries as to whether the family is particularly addicted to eating rhubarb.

I remember a young mother complaining that one of her children got nettle-rash regularly every April; so I asked her about the rhubarb, and I got the answer: 'Why yes, doctor; you see, our large garden is just full of it, and it seems such a pity not to let the children have it. In the springtime they have lots of it every day.'

It seems the fashion nowadays to cram children with as much of the juice of the citrous fruits as the parents can get hold of, and I am told they do this on the earnest advice of health clinics. The process begins sometimes for babies of under two months, and I have frequently noted that this decalcifying process coincides very closely with either the first onset of eczema, or with its increase. I generally suggest to parents that they should go easy with the orange juice until the eczema is more under control.

A baby boy was brought to the Clinic by his parents; he had an idiotoxic heredity, for the baby's mother and her sister had persistent migraine attacks, while the father had had eczema, on-and-off, all his life. The baby himself had been born with an ichthyotic skin.

He was brought to me suffering from repeated attacks of urticaria which were often so intense as to cause serous bullae to appear on his skin; the condition had in fact been called pemphigus by dermatologists.

The first attack of this urticaria had been very intense and had occurred at the age of 11 months; it had followed within the hour after a large and illicit meal of unripe blackcurrants. Believed to be too young to walk, the boy had been left alone in a cottage; he had got on to his feet, and had staggered out into the garden where he had seized hold of a large blackcurrant bush and had liberally helped himself to the fruit.

One cannot doubt that that very first attack was powerfully helped by the decalcifying effect of the fruit acids. Though many subsequent attacks of urticaria had of course had no such help.

Knowing what I do of the emotional factor (Chapter IX), I should also strongly suspect that the agitation in the household concerning the anxious parents' inadequate supervision of the child, plus the emotion from his recent great adventure with the currant bush, would prove an efficient emotional starter of the baby's attack.

By the way, the little boy gave no response on the skin to blackcurrant proteins (nor to any of the other pathophanes with which I tested him); the conclusion we came to

was that the peccant idiotoxin was of a bacterial nature, and chiefly derived from the gut—as so often seems to be the case in urticarial attacks, but that in addition the decalcification and emotions must each be playing a part.

Cooked fruit is habitually eaten with the acids heavily masked by sugar, which of course does nothing whatever to diminish the acidity ; on the other hand the cooking will drive off all, or much, of the volatile acids such as acetic, though it will do nothing to diminish the non-volatile such as malic acid.

It is worth remembering that it is not only fresh fruit which can make the blood more fluid by throwing out of gear the blood calcium ; fruit derivatives, such as jams and wines, can do this also. Jams especially are more acid than the palate tells, for the fruit is preserved by an equal weight of sugar. Also, jams are 'reduced' to half their bulk by evaporation, which will concentrate the non-volatile acids though it drives off the volatile.

The very acid jams may approximate to 2% of malic acid (or its equivalent) ; champagne may approximate to ½%, but then much more bulk of champagne is frequently ingested by grown-up addicts than the bulk ingested of jam that the most fortunate child can get hold of in the ordinary way.

Of course sodium bicarbonate will lessen all acidity, but that deprives fruit and its derivatives of the flavour which makes such foods attractive. If the mother insists on large quantities of rhubarb, a little sodium bicarbonate will take the sting out of it.

Too much fuss can easily be made over the avoidance of vegetable acids, and fuss, as we shall see in Chapter IX, is on the debit side of the account. Decalcification is, however, a point to be borne in mind, especially with the attacks of eczema, asthma, urticaria or what-not which are observed to coincide with large ingestions of acid fruit.

Treatment is obvious : limit the acid and replace the calcium if necessary. Many people prefer injections of calcium into the blood stream. I don't do this, but prefer to give calcium by the mouth, preferably in the form of cow's milk if it can be got ; the cow manages to make more easily assimilable calcium than the chemist can.

Another obvious cause for loss of viscidity in the circulating plasma is the loss of protein 'body' in the blood fluid ; doubtless the oedema of starvation is of this nature. Here again we know what the treatment should be—more and better food.

(3) **Blood Supply.** There remains the mechanical factor of the amount of the blood supply and the pressure behind it. That must clearly affect the rate of leak of the fluid contents of the capillaries. Anything which flushes the part with blood will increase the possibilities of serous exudation. Anything which diminishes the volume or pressure of the blood will diminish the exudation. Violent exercise, a hot bath. X-rays, the heat of the sun, all these by flushing the skin with blood will increase the chance of serous leaks into the skin. Patients suffering from chronic urticaria, or indeed any of the allergic dermatites, will have noticed that effect of a hot bath. Conversely, a control of the arterioles by such things as adrenalin or ephedrine, or by cooling the skin, will counteract this tendency by curtailing the

blood supply. Therefore, one would say, keep the skin cool and clear of hot sun, keep up the tone of the capillaries all over the body, if the normal escape of serous fluid needs to be cut down.

It is of course because there is a comparatively greater head of blood at the lower extremities when the patient is standing up that there is an increased pressure of blood on the walls of the capillaries in the feet and ankles ; and it is consequently there that oedema chiefly occurs when the patient is up and about. This increased pressure and consequent oedema is, as we know, often due to loss of efficiency in the heart ; but, however it may be caused, mechanically it would increase a serous leak, causing e.g. eczema or oedema in the feet. The obvious remedy is to keep the feet up in the prone position, i.e. go to bed, a remedy that puts the patient out of action and so has its heavy debit side by making the patient into an invalid.

Asthma occurs with peculiar frequency at night ; and, when it does so, it may occur in an attack night after night regularly for weeks on end. We shall have to return to these nightly attacks and the panic they cause when dealing with the emotional factor in Chapter X (p. 167).

It has been customary to suppose that asthma is chiefly due to bronchial spasm, but I'm a heretic about that spasm, and would say that oedema—especially at the glottis—is much more important. It is hard to believe that contraction of the involuntary muscles of the bronchioles could sufficiently occlude the air passages to produce asphyxiation ; but I know that oedema can do so—for I have seen it at post-mortems.

It is generally assumed, and I believe it to be correct, that attacks of dyspnoea and of asthma at night are due to the prone position ; this necessarily increases the blood pressure in the head, and so tends to the engorgement of blood-vessels in the walls of the upper air passages. The oedema, and therefore the asthma which may result therefrom, is notoriously relieved by propping up the head and shoulders by means of pillows, etc. ; that is a simple matter of hydrodynamics.

Bed is bed, and the place to spend the night in for all respectable people ; but some experienced chronic asthmatics tell me that, to the scandal of their relatives, they prefer to spend the night sitting up in an armchair to avoid this prone position —and the consequent asthma.

'What time do *I* go to bed, doctor? I don't. I haven't been to bed for the last three months ; I always sleep propped up in a comfortable armchair. Why, I think I'd choke if I went to bed respectable-like as my old woman wants me to do.'

Parents sometimes complain to me that their asthmatic children *will* get out of bed and crouch on the floor of the room ; sometimes they are said to prop themselves in a corner of the bedroom to keep themselves more or less head upwards ; and they can go to sleep in that position.

Patients who try the effect of resting in bed in the early afternoon tell me that, after an hour or two of this, asthma frequently begins. We may surmise that this again must be (partly at least) the result of the prone position.

It has been stated that the slower rhythm of breathing which we all adopt in sleep conduces to dyspnoea and asthma. I see no reason for this supposition.

That slower rhythm doesn't afflict with asthma the people who sleep in armchairs, but they *are* so afflicted if they go to sleep lying down flat.

A much more important rival to the prone position for causal honours in producing night asthma are frightening or emotional dreams—as we shall see in Chapter XI ; I admit that similar dreams can come to people sleeping in armchairs, but when they do so come I suspect they are asthmogenic.

However all that may be, I'd certainly say that the chief reason for the tendency of the lower extremities to oedema and eczema when they *are* lower, and the tendency of the upper extremities to asthma when they *aren't* upper, is a question of hydrodynamics.

Bacterial Inflammation. One of the commonest causes of an increased local blood supply is the inflammation brought about by an infection ; that is, of course, the body's answer to the bacterial poisons : it is the local mobilisation against them. Minute blood-vessels which were previously invisible are seen (e.g. on the eyeball) to be swollen with blood. When they are looked at in the mass (e.g. at the back of an infected throat) the whole area, because of the swelling of the capillaries and small blood-vessels, looks lumpy, swollen and red ; it resembles a raw beef-steak. Other things being equal, there would be an increased serous exudation through the blood-vessel walls from '(3) the pressure and supply of blood' (p. 217).

There are, of course, other bacterial factors which serve to augment this exudation : the bacterial poisons, which have provoked the inflammation, also act on the vessel walls to make them more permeable, thus promoting the escape of serous fluid to produce local oedema and swelling. This swelling is often clearly seen in such an expandable organ as the uvula, which frequently becomes perhaps four times its usual size. It is this bloated condition of the fauces which the E.N. and T. people frequently refer to as 'The Allergic Throat'—without, I surmise, any very clear differentiation between Allergy I and Allergy II in their minds.

I have said above that the bacterial poisons 'make' the blood-vessel walls more permeable. Whether they do this by direct chemical action on the cells lining the capillary walls, or act on these lining cells by a nerve reflex, doesn't matter ; since this occurs with everyone who is infected, it must be part of Allergy I—if we are to use the word allergy at all. But with our allergy (sense II) patients (i.e. those with a disease similar to hay-fever) the bacterial poisons may also be making the vessel walls more permeable by acting as the specific idiotoxin ; it is possible ; but the point must be further considered in Chapter XII, when we deal with the general effect of bacterial infections on the toxic idiopathies.

CHAPTER VIII

TRAUMA : TISSUE DAMAGE THE LOCALISING FACTOR

In working chapter by chapter through our list of seven causes for the toxic idiopathies we now come to physical trauma which figured as Cause E in Chapter III. Some might prefer to call this the Histamine Cause, but I have for so long thought of it as tissue damage (which it certainly is) that I prefer to call it so in place of invoking a hypothetical ' histamine-like substance '.

In Chapter III I said that local tissue damage was frequently the factor which decided what particular toxic idiopathy would emerge, because it so often decided the site of its emergence. As it also frequently decides whether or no any notable ailment shall emerge at all, it may on occasion stop or start an attack ; if so, it would be obviously a cause and obviously also of value in treatment by manipulating that cause if we can.

That is the justification for much of our clinical work, and particularly on asthma ; so we must prove it up to the hilt if we can ; and ' up to the hilt ' means to demonstrate that trauma is part of the machinery of production of *all* the toxic idiopathies. I propose therefore to take these allergic disorders one by one and consider them from the trauma aspect ; for, as was said in Chapter III, if there is a common machinery, then any light on that machinery for any one of these diseases will reinforce our knowledge of the others.

Hay-fever is not the most obvious example of the action of the trauma factor ; the idiotoxin-idioceptor part of hay-fever—the pollen story in short—looms so large in its mechanism that it overshadows the rest ; also the locality for most of the hay-fever symptoms is determined by the places (eyes and nose) where the pollen grains most frequently make contact with the living tissues of the patient—where the marriage of idiotoxin and idioceptor can take place.

Let us keep to our custom, however, of considering hay-fever first ; if we find trauma playing its part here, *a fortiori* we shall be all the more likely to find it in the other toxic idiopathies.

We may include with hay-fever the para-hayfevers too : cases exactly like hay-fever but which derive their idiotoxins from some finely particulate animal or vegetable matter other than grass pollen—e.g. scurf from horses or wheat flour from a grain elevator—particles which may easily be carried on the wind to the eyes and noses of the patients.

Nose Operations. Hay-fever patients often say that damage to the nose either started a rhinorrhoea, which subsequently was proved to be hay-fever, or perhaps even more frequently they claim that previous symptoms of hay-fever, horse-asthma, etc., were made much worse by surgical interference. Sometimes this

increase is reported to have arrived with dramatic promptness after the damage, sometimes it is only experienced in the next or subsequent hay-fever season. Thus we have a young man who receives a blow on the nose and true horse-asthma (as proved by skin tests) promptly starts (p. 49). One must not give too much credence to grumbles about previous treatments made by patients who are changing over from one specialist to another, but these reports of the after-effects of nose operations, etc., on hay-fever patients are so persistent as to suggest that trauma does play a part. Ionisings, cauterisings, septum-straightenings, they all may leave a trail of heightened allergic response in the nose.

A woman came to me with severe hay-fever. The diagnosis was quite clear and exclusive. She told me that years ago she had only slight discomfort from the eyes and nose from the hay-fever, but a friend prevailed on her to go to an osteopath for treatment. This man told her that her hay-fever was due to adhesions at the back of the nose and that he could easily cure her by manipulation. This procedure proved both lengthy and painful, but in the end he said he had broken down these adhesions with his little finger tip. After that the previously very mild hay-fever became really severe, and each recurring mid-summer brought the symptoms back in this reinforced form.

Fortunately she was still an uncomplicated case of grass pollen sensitisation and so was clear of all rhinorrhoea and conjunctivitis from the end of July of one year till the end of May in the succeeding year. She responded perfectly well to a P.T.D. course of pollen treatment and that ended her trouble.

She was undoubtedly a case of true and classical hay-fever, but trauma had served to increase symptoms.

The otherwise very desirable enucleation of a septic tonsil may bring undesirable consequences to the hay-fever subject.

A typical hay-fever case had been completely freed of all symptoms for several years by the regulation P.T.D. treatment; by February 1945 the skin testing gave such a slight response to grass pollen that I questioned whether any further desensitisation treatment would be needed that year, and suggested a holiday from the injections.

The E.N. and T. specialist who had been watching the case decided, and the patient and I agreed, that he should seize the opportunity of this inoculation holiday to carry out a much-needed tonsillar enucleation early in May. This operation was completely successful; but in the succeeding hay-fever season, though the eyes and nose were unaffected by the grass pollen, the damaged tonsillar fossae which had been fairly comfortable after the operation became, with the arrival of grass pollen, so itching and oedematous that the patient found it was hardly possible to abstain from scratching them with the finger nails to allay the irritation. These attacks of itching in the damaged area came on and off throughout the hay-fever season in the way familiar to every hay-fever patient; then the tonsillar fossae became suddenly comfortable as soon as the grass pollen had left the atmosphere.

In consequence of all this it was decided in the spring of 1946 to resume the prophylactic treatment and this was again completely successful; this time not even the tonsillar fossae showed symptoms.

I have much less evidence of an increase of hay-fever symptoms through trauma to the conjunctival sac, perhaps because surgical interference with the eyeball is less often undertaken. Patients often complained to me that their doctors advised antiseptics such as argyrol by way of controlling the hay-fever inflammation of the eyeball, presumably in the mistaken belief that hay-fever conjunctivitis was

bacterially produced. As we might expect, this antiseptic damage heightens the symptoms of irritation and lachrymation from the eyes.

It is notorious, too, that the glare of sunlight increases the eye symptoms of hay-fever and that this can be mitigated by dark glasses which cut out the actinic light. Blazing sunlight will demonstrably induce an attack of hay-fever, but I don't want to insist too strongly that this is only a matter of trauma, for there may be also an emotional factor.

Bacterial infections undoubtedly influence the toxic idiopathies in various ways. A good instance of a bacterial infection influencing the allergic response to grass pollen is given on p. 212.

Catarrhal infections of the upper air passages prolong the normal hay-fever season by permitting less pollen than usual to cause trouble.

Bacterial infections exercise a boosting effect on hay-fever symptoms and part of that boost is from the trauma they cause locally. But the action of the bacteria is complex from the toxic idiopathy point of view and for that reason it is better to deal with it separately in Chapter XII.

The Allergic Dermatites give much better evidence of the trauma factor than hay-fever can do because trauma is more frequent and more measurable on the skin ; also the effect of it can there be better observed, for it is easy to compare the allergic responses on damaged and undamaged skin.

Urticaria can be shown to come up by preference at damaged areas : thus on an old scar, on the site of an extinct but recent boil, where a very hot fomentation has been applied, where the surface has been rubbed or scratched, or where a blow has been struck. I have seen oedema coming up with surface blisters at the site of an X-ray application. Ionisation (e.g. on the small of the back) readily determines urticarial weal to the area of skin so damaged if there is an allergic tendency to serous haemorrhage. This tallies, of course, with the effect of ionisation of the internal nares in hay-fever or para-hayfever which we have mentioned above.

I have got perhaps my clearest evidence of trauma in hay-fever from the artificial application of pollen to the patient either in the skin-testing for diagnosis or in the injections given for desensitisation treatment.

A very pretty demonstration of this is frequently produced by accident when making routine skin tests for diagnosis of some specific sensitivenesses.

It is usual, when several of these skin-testings are being made simultaneously on the skin, to identify the place and nature of each test with a pen stroke and hieroglyphic, as in Fig. 37. A sharp-pointed pen will inevitably scratch the skin slightly, especially if the ink chances not to flow readily in a fountain pen. If this scratched, and therefore damaged, skin happens to be too close to the prick hole of a positive test, the serous leak resulting from the test, in taking the line of least resistance in the skin, is often seen to escape to the scratched areas ; thus we get the pen-scratching outlined with oedema as shown in Fig. 37.

This obviously approximates to dermographia, but it can hardly be so-called because there is no oedema occurring round the pen-scratchings near a prick where *no* positive response has occurred from that particular pathophane. The border-

line between dermographia and urticaria (as between any two of the allergic dermatites) is hazy and indefinite. The point for us here is that if serous leak is imminent in any neighbourhood, then trauma will start or augment the leak.

Here perhaps we may also make the point that the prick itself is very definite trauma. It is usually assumed that the pricking through the drop of the specific pathophane does no more than introduce this to the living cells of the cutis vera, but I suspect that the trauma of the prick plays its part, too, in provoking the positive response.

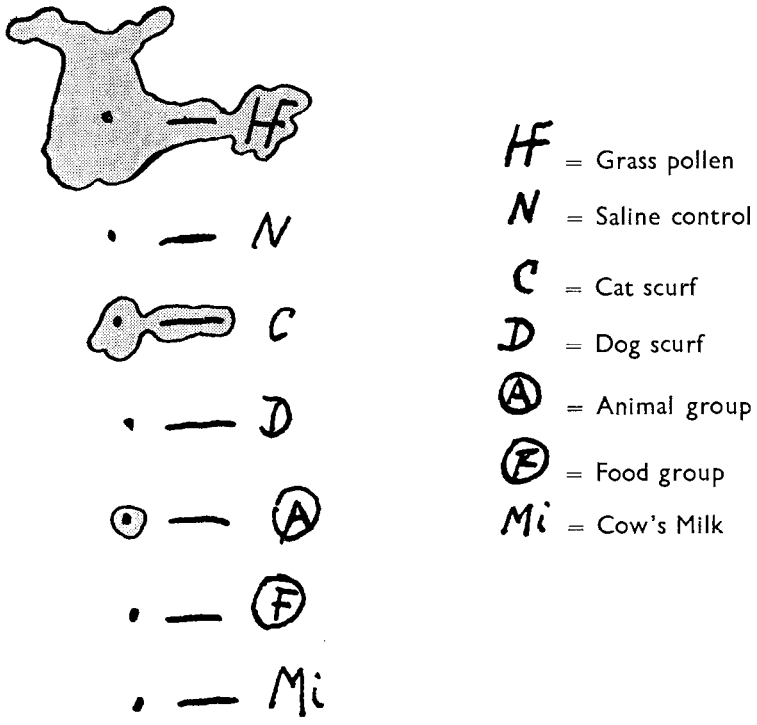


FIG. 37

Shows how any scratch on the skin may augment the oedema weal from any neighbouring prick test.

The figure is drawn natural size, but the prick-marks (as with others in this book) have been emphasised for clarity. In reality they would be invisible, or nearly so.

When a prophylactic dose, e.g. for hay-fever, is given in excessive amount or at the wrong time and a reaction has followed, this usually takes the form of urticaria or of angioneurotic oedema, and it is very noticeable that recently damaged tissues often determine the site of the oedema on the skin.

I remember the case of a young woman who, while inoculating herself prophylactically for hay-fever, had a slight reaction in the form of a few scattered patches of urticaria after one of the doses. This was negligible, however, and she had gone for a long bicycle ride with her husband ; when she got home again she found the shape of the saddle was exactly marked out on her skin at the place where she had sat on it. She told me the extent of the oedema was so considerable as to be readily palpable, ' it felt to the touch

almost like a blown-up bicycle tyre ' and in fact approximated to angioneurotic oedema. Here one suspects that in addition to the trauma from the rubbing of the skin, the warmth and consequent dilatation of the arterioles in the rubbed area would considerably augment the serous haemorrhage.

Of course there may be little or no damage available on the skin to draw the oedema to one particular area, but experimentally the effusion of plasma can be led to a scratched or otherwise maltreated area. In fact something like dermographia occurs on these occasions of general oedematous tendency after a slightly inappropriate prophylactic dose : a scratch produces a weal.

Dermographia. A scratch on the skin of the dermographic person with a fingernail or the butt end of a lead pencil is the last event before the characteristic weal comes up. This occurs, no doubt, with the assistance of histamine set free by the ' damage ', and by the action of the intact neural arc, etc., etc., as Sir Thomas Lewis has demonstrated ; but, like hay-fever, dermographia is not a disease *sui generis*. It fits into a toxic idiopathy pattern in that it is distributed by heredity, often accompanying such diseases as hay-fever or asthma ; it waxes and wanes according to the emotional background (if I may anticipate Chapter IX) ; and it is often clearly influenced by a bacterial idiotoxin (if I may anticipate Chapter XII). Dermographia is in fact no more than an urticaria, with the trauma factor peculiarly prominent, and the other causal factors absent from the mind of the observer.

Angioneurotic Oedema is a characteristically localised and intense urticaria ; because of this it often shows particularly clearly the localising effect of tissue damage. Thus a jagged tooth scratching the buccal surface of the cheek of a person prone to angioneurotic oedema may cause the whole side of the face to swell ; a bite on the tip of the tongue may cause this to swell during the next quarter of an hour until it can hardly be contained in the mouth and loses its capacity for movement. Injury to the lips or the surface of the face may produce equal or even greater swelling.

A patient who had for many years suffered off and on from these swellings (starter story No. 20, p. 191) came to me one morning with her whole face and neck so swollen that she could hardly see out of her eyes, and her two lips were with difficulty separable because of the great oedema. She was in fact hardly recognisable, and very frightened.

She told me she had got up that morning feeling apparently normal ; and, on going to her looking-glass to make the customary study of her appearance, she noticed with disgust a prominent and disfiguring blackhead on the tip of her chin. This she skilfully ejected by squeezing the offending black spot between her two forefinger nails, and she then commenced her dressing.

When she looked in the glass a few minutes later, she was surprised to see what a mess she seemed to have made of her skin round the ejected blackhead. The skin had gone all soggy for perhaps an inch in diameter. Soon after that the whole chin became rapidly and progressively swollen, then the lips, the cheeks and neck ; by the time she had completely dressed the whole face had swollen and the oedema was spreading up on to the scalp.

With this particular patient, scalp oedema had frequently coincided with an attack of migraine ; on this occasion, as soon as the scalp became involved in the swellings, the migraine commenced.

The patient had not the slightest doubt (and no more had I) that all this trouble had derived from that initial blackhead ejection.

The lips are the areas perhaps most often affected by angioneurotic oedema and especially if the lips suffer any particular trauma by being bitten accidentally, or if they get bruised or crushed.

A healthy young woman, who had previously had slight eczematic and urticarial attacks, but no angioneurotic oedema, got engaged to a young man stopping in her parents' house ; after announcing this to her people she escaped to her bedroom to receive the friendly chaff of her sisters, who then noticed her lips beginning to swell and swell, till she was not only unsightly but almost unrecognisable.

We may surmise that the vigorous osculatory encounters with which such contracts are appropriately concluded may have contributed the localising trauma. The emotional disturbance inevitable to such an occasion no doubt also played a prominent part ; but that is another story, to be told in Chapter XI.

I think that these strange swellings occur chiefly in the face because the face is one of the areas of the body about which we feel most emotion, but other areas are not exempt if these happen to get damaged. The old-fashioned stiff corsets used often to produce these swellings where the skin got rubbed at the upper and lower edges, also straps or braces over the shoulders may produce an oedema which is often intense enough to be dignified with the epithet angioneurotic. Though by no means the only factor, trauma clearly plays a part.

Paroxysmal Hydrarthrosis has close affinities with angioneurotic oedema. In fact the two conditions often occur together or merge into one another ; where an angioneurotic swelling occurs near a joint, e.g. in the fingers, there is usually an effusion into the joint also ; conversely, where effusion begins into a joint, this frequently involves the tissues round it in an angioneurotic swelling.

The similarity between these two conditions that concerns us at the moment is that hydrarthrosis also is usually started by an obvious trauma, such as a bumped knee or a twisted ankle. If there is any evidence of violence to a joint then any subsequent water effused into it is quite correctly put down to the injury ; but the fact that the patient is in the allergic category, and that the swellings and effusions may be regarded as a toxic idiopathy, is very often overlooked.

In January 1937, a young man came to see me with a rather confused story of allergic manifestations. There were cases of asthma and food idiosyncrasy amongst his close relatives. He himself had had migraine attacks from time to time which he attributed to constipation ; he had also had bad eczema, which had apparently yielded to persistent purging. ' I practically lived on Eno's at that time.'

He was sent to me for angioneurotic oedema and paroxysmal hydrarthrosis. The first big localised effusion had been into the pad of his left forefinger ; the next was into his left big toe which had ' come up like a balloon after stubbing it on a stone '. Quite recently his hand had swelled so much that he could not hold a pen because his finger and thumb joints would not bend. That had been diagnosed as rheumatoid arthritis, and the history of previous attacks of swellings had been politely ignored.

He proved to be an intensely ' streppy ' person both in the upper air passages and in the guts and though an alert and brisk man, his blood sedimentation rate was far above normal and fluctuated wildly. He responded well to minute but frequent doses of autogenous streptococcus vaccine (p. 235) and the swellings disappeared.

Because he had improved so much I soon lost sight of him. After eight years he reappeared as a recently-demobilised Air Force Officer. He had remained well till the

fourth year of the war when, to use his own words, he had ' taken a packet ', and this had started up his troubles all over again.

The ' packet ' was this : an explosion had blown him off a dock wall into a boat 20 ft. below. After that ' everything swelled on and off ' and he was reduced to such a nervous state that he jumped nearly out of his skin if anyone suddenly touched or spoke to him. His chief trouble was effusion into the joints. If he bumped his elbows somewhat carelessly on to the table, they swelled. If he strained a finger joint, that swelled. After ricking his ankle very slightly, his foot swelled so inordinately that it would not go down his trouser leg. If anything by chance fell on him, instead of ' just swearing and then forgetting it ', the place of contact swelled and swelled so that he couldn't get on with his job and he had to go sick.

His final medical trouble in the R.A.F., a trouble which had resulted in his ejection from that Service, had been caused by slithering over the gunwale of a larger boat into a smaller one. He had gone over feet first and face forwards ; by this means he had grazed his back and especially the lumbar vertebrae, but, as he said, ' nothing to cause me ordinarily a minute's concern.' As a result of this, all his back and especially the vertebral joints had swelled up, and his back had become so locked in one position, and so painful, that he had to go into hospital.

The condition was there diagnosed as lumbago, and treated with ultra-violet light. He tried to explain that the whole thing was due to scraping his back, and that he was quite used to such swellings, but he was demobilised as unfit for further service with the diagnosis of lumbago intact.

I have no doubt that the emotional experiences of the war, and particularly the shock of that ' packet ' which restarted his troubles, were powerful contributors to the serous haemorrhages ; but quite clearly trauma could and did start the swellings and serous effusions, one by one, as they occurred.

Eczema. We have wandered a little way from the allergic dermatites in considering these joint effusions. Let us return to eczema which is perhaps the commonest of all the toxic idiopathies of the skin.

During birth the skin, and particularly the skin of the head, comes in for some rather rough handling. Where the skin has been badly rubbed during labour, e.g. at the top or bottom of the ears, injured areas are formed which frequently develop into eczema in the allergic child. Where pressure has been marked on the scalp during delivery, there eczema frequently supervenes. Mothers have also to be very careful to prevent chapped areas from developing into eczema at the folds round the neck or the armpit if the child has an allergic diathesis.

During childhood and schooldays, eczema shows itself typically in the folds of the body, particularly at the elbows and behind the knees. This is presumably the damage caused by the frequent bending at these points just as a leather portmanteau ' goes ' at the hinge.

Eczema is often no more than persistently scratched urticaria—or scratched ordinary skin for that matter. In fact anyone prone to eczema can usually produce it by persistently scratching one area of skin. Eczema comes up at accidentally damaged points as was the case with urticaria and angioneurotic oedema ; but, because eczema is slower to come and go, it tends to lag some days behind the damage causing it. In this way a more permanent record of the area damaged may result.

I remember seeing a case sent to me for asthma, who had also occasionally attacks of urticaria, of angioneurotic oedema, and of eczema.

When I spoke of eczema she showed me her thigh where there was an exact print of her stocking suspender mapped out on the white skin in rough and red eczema (Fig. 38). This had only developed on her right leg; I don't know why.

This particular suspender partly consisted of a long metal loop, the skin beneath being protected by a cotton pad; the upper portion was made of indiarubber strands covered with cotton. The eczema could conceivably have been caused by the chemical action of the rubber (see patch-tests on p. 263) in the upper portion, but the metal loop and pad could, I think, have only been acting by mechanical irritation, i.e. by trauma. Even the chemical action of the indiarubber may have been more traumatic than idiotoxic—as we shall see later on.

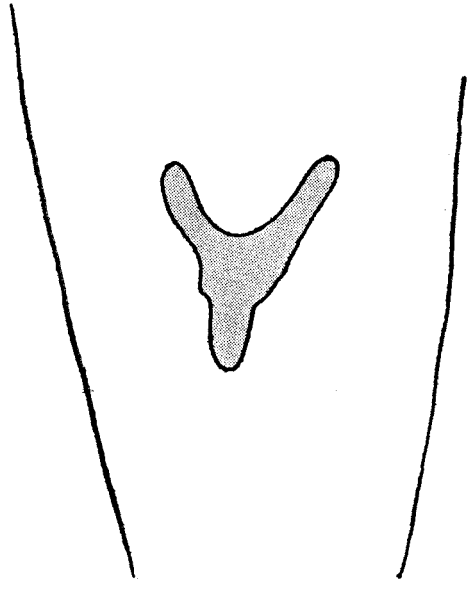


FIG. 38

Shows how the exact shape of a woman's stocking suspender was marked out on the skin of her thigh in rough red eczema.

I have been told several times by men who are prone to eczema, asthma, etc., that whenever they carry in their trouser pockets for a considerable time a heavy weight—a big bunch of keys or a handful of coppers—that this, by rubbing constantly against the front of their thighs, will produce typical eczema on the skin. When the weight has been removed the eczema has gone.

The frequent eczemas got from handling plants are probably more often in the nature of trauma than an instance of the idiotoxin making contact with the idioceptor. The handling and general contact with plants frequently produces an eczematic rash, and I once asked the head gardener of the Horticultural Society's Gardens at Wisley what were the plants which produced such rashes; he told me that there hardly seemed to be any plant that wasn't accused of affecting somebody. No doubt that is an intentional exaggeration; but not only do the notorious primulas affect people but I have seen terrible eczemas produced by chrysanthemums for example. Some of these rashes may partially be the result of idiotoxin-idioceptor action; I think that is so with the well-known 'lily-rash' which affects 'sensitive' people picking daffodils because they respond, though sluggishly, to a prick skin-test with the stalk juice, and P.T.D. treatment sometimes enables such people to pick with impunity, p. 265. I am convinced, however, that much of the damage from plants is from trauma.

I published an instance of this trauma effect in the July number of *The Practitioner* of 1929.

A girl in a smart Bond Street flowershop got an intolerable and unsightly eczema on her hands and wrists whenever these came in contact with the flowers, foliage, bast, etc., of her trade. Holidays quickly cured her, but a return to the shop promptly brought on a relapse. This, I am told, is a not infrequent occurrence on 'delicate' skins, and all

concerned in the case assumed that her work brought her hands into contact with some vegetable protein or proteins to which she was ' asthmatically ' sensitive. Extracts from a large collection of suspected flowers, leaves, dressings, etc., gave no dermal reaction, yet the girl had an authentic asthmatic history ; so search was made elsewhere for some possible protein irritant. A bacterial overhaul showed her to be a very streptococcal person, and in particular she had a very heavy implant in the gut of a delicately growing streptococcus ; and to an extract of this organism she gave a marked dermal reaction. Cautious doses of a suitable vaccine were followed by rapid improvement of the hands, and she can now work in the shop without the slightest difficulty. In spite of our first superficial analysis, if she were sensitive at all to the flowers of the shop this was only to a minor degree ; they acted chiefly in providing local and non-specific damage, which gave the eczematic tendency wandering in the blood the chance it was looking for : from the point of view of protein sensitiveness, gut-sepsis was at fault and not the flowershop.

This is the place, I think, to refer again to those pseudo-idiotoxins of dust, chaff, fluff, etc., mentioned in Chapter IV as not for the most part functioning as peccant idiotoxins but as traumata in the production of eczema, asthma, hay-fever, etc. They will necessarily be referred to again when discussing the making of allergic vaccines and pathophanes in Chapter XIV.

I think it clear that, for example, the dust from a chaff-cutting machine acts as trauma by tickling up the already oedematous and sensitive skin, or the mucous membrane of the nose and throat, and not by its chemical action as an idiotoxin. In my experience the sufferers are never sensitive to an extract of this dust ; yet the troop of farmers, land girls, and farm labourers who come for treatment know quite well that the chaff-cutting does give them asthma, eczema, etc.

Eczema is clearly a trauma-determined lesion when it occurs only where the actinic light of the sun strikes on the bare skin. Some townswomen dare not venture in the sunlight at midsummer save with faces and necks smeared with a yellow ointment, or wearing a thick veil and with their hands in gloves for fear of most unsightly blotches of urticaria-cum-eczema.

I remember one such, a Londoner, who on the important social occasion of her small son's athletic sports had risked sitting in the sun under cover of a large shady hat and a parasol.

In the fashion of that day the yoke of her dress was filled in with a heavy black lace, and when I saw her two days later her chest bore a beautiful photographic negative of that lace. This time it was a pattern in clear white skin on a background of reddish and scaly eczema.

Migraine is often denied the status of a toxic idiopathy because it can be shown to be caused by eyestrain and cured when this strain is corrected by the appropriate glasses ; that is of course the ' fallacy of the Single Cause '. I have long thought that this eyestrain which so many people consider to be the single cause of migraine is in reality functioning as the trauma factor helping to produce the toxic idiopathy. But, as was hinted (p. 30), ' migraine ' is probably not a homogeneous clinical entity.

Epilepsy, the mental relative of migraine, is also often discarded from the toxic idiopathies. It is, in my opinion, one of them, for reasons given on p. 26, and it conforms to that pattern quite clearly in the matter of trauma. In fact patients or their relatives or the doctor in charge usually brush aside the suggestion that epilepsy

could be allergic by pointing to the trauma factor. ' Uncle Tom, though he did have eczema and hay-fever all right, didn't as a matter of fact develop his epileptic fits till he got hit on the head through tumbling downstairs one night.' Or, ' She fell out of her pram on to her head when a baby and the doctors all said that was why she had fits, and not because her sisters had asthma.' Through fear of the stigma of having produced an epileptic child, I have constantly heard the trauma factor being stressed in epilepsy to the exclusion of all else.

I had a patient who was definitely in the allergic category in that he had been previously treated by me for his hay-fever and also because he had several close relatives with other allergic manifestations. He was being relieved from the front-line trenches in the 1914–18 War when a bullet hit a glancing blow on his steel helmet and drove the metal down on to his cranium, knocking him out temporarily. During the next month or two he had several slight but definite epileptic attacks, and these for the first time in his life. In view of what I have written in this chapter I would certainly say that the blow on the head had localised his general allergic tendencies, judging by his involuntary movements and from the locality of the injury, on the Rolandic area of the brain.

Asthma. This has been my main objective all through this chapter because the removal of the traumatic area in the upper air passages plays a very considerable part of our rather successful routine treatment of asthma.

It is notorious how very frequently an eczema of infancy is reported to have ' turned ' into asthma under the influence of whooping-cough, pneumonia, measles, chickenpox and the like. Very frequently parents say, ' Well, Doctor, my little boy had eczema when he was five years old, but an attack of whooping-cough changed this eczema into asthma.' Again it is not uncommon to hear of urticaria or migraine having been ' changed ' into asthma by way of some air-passage infection which damaged the throat and larynx. Hay-fever frequently becomes asthma in this way, but naturally is confined to the grass pollen season ; it would be, according to our nomenclature, ' cryptic hay-fever '.

Soldiers who had been gassed in the 1914–18 War told me that they had had their first attack of asthma in consequence of it, and some of them reported that other toxic idiopathies such as urticaria or eczema had stopped when the asthma began under the influence of the gassing. I have no doubt that the horror of an unexpected gas attack, and particularly of that first gas attack, must have gone a long way to precipitating the asthma by reason of the emotional shock ; that could hardly account for the shift of the lesion from the skin to the upper air passages, for by itself the emotional shock would presumably merely accentuate the skin lesion.

Very interesting to me, and fitting in well with our present hypothesis, is the story which I have twice heard from parents that whooping-cough had ' changed ' epilepsy into asthma. In both cases this was volunteered by the relatives before I had had time to ask any questions at all, and they obviously regarded this curious shift of the disease as a fact demanding an explanation. The explanation is throat trauma.

This possibility of changing from one toxic idiopathy to another clearly suggests a method of treatment. The danger is of course that the unhappy patient might

acquire two toxic idiopathies in place of one, or the substitute toxic idiopathy provoked by trauma might prove more distressing than the original; so we must tread warily. Working on this plan, blistering or hot baths might deflect, say, epilepsy to a dermatitis: success, presumably, would depend on the seriousness of the original cerebral trauma which had precipitated the allergic response into the cerebral area. My reader may consider these as idle or dangerous thoughts; but for the pathologist they should at least be borne in mind as a possible utilisation of trauma in treatment—quite apart from treatment by its removal.

At all ages pneumonia is frequently given credit for ushering in the first attack of asthma which thereafter becomes a lifelong habit. I suppose that in about half the cases of asthma that one sees, bronchitis or chest colds are insisted on by the patients or the relatives as the constant precursor of an asthma attack. They will relate that at first everybody, including the doctors, took the illness to be 'just a cough', or 'just bronchitis', but that this throat condition had gradually become unmistakable asthma with very little bronchitis about it.

So regular is this trauma effect at the back of the throat that I have long since had the suspicion that there must be some such trouble in the upper air passages of all asthma cases or the patient wouldn't be having asthma; and I don't exclude from this the instances where definite foreign idiotoxins can be demonstrated by skin-testing as in horse-asthma or hay-fever attended with asthma. In fact where typical hay-fever is attended with asthma symptoms it should be the general rule at once to swab the throat or at least to examine it. I am not suggesting that such trauma at the back of the throat can be causing asthma all by itself without any of the other causal factors to help it; that would be the single-cause fallacy. Bacterial infection can act as a trauma as we have seen; we shall be considering in Chapter XII whether the dissolved bodies of the bacteria might not be acting as the idiotoxin too.

I doubt if we can prove that, but if they can't so act, there must be some idiotoxin in place of them; if they can so act there might be any number of other idiotoxins helping them.

Our point in this chapter is that there is always a trauma factor to be found, even in such an overwhelmingly 'allergic' business as hay-fever; this factor is particularly noteworthy in asthma where it usually takes the form of an infection by bacteria at the back of the throat or nose. This infection acts as trauma by reason of its bacterial toxins shed locally, and by the resulting inflammation, exudation of the plasma, and oedema.

If we can stop or diminish that infection (even if we leave untouched the idiotoxin which our general hypothesis of a common machinery demands) we shall go a long way, if not all the way, towards stopping the asthma. It is a logical treatment.

CHAPTER IX

EMOTIONS, MOODS AND TENSIONS

Why not ' Psychology '? When, long ago, I began to write this chapter, I headed it ' The Psychological Factor ' ; but I was a little shy of the word. Since then, ' Psychological ' has become so overlaid with queer meanings that it seems too esoteric for the very ordinary—not to say homely—facts which I wish to bring forward in this chapter.

' Psychology ', according to the *O.E.D.*, is ' the science of the nature, functions, and phenomena of the human soul or mind '. On the same authority, ' soul ' has twenty-five main meanings in English literature, and ' mind ' has twenty-one ; but *pace* any quibbling about these two notorious playthings of the metaphysicians, I do not here want to invoke this science concerning them. I wish only to give the evidence that has convinced me personally that all the toxic idiopathies are, for better or for worse, influenced by the state of mind (or soul, if you prefer it) of the patient ; and the chief states of mind which seem to do this are the ordinary emotions, moods and nervous tensions. Hence my chapter heading.

Anyone who has read thus far in this book has observed, first with hay-fever in Chapter I, and then with the other toxic idiopathies for which hay-fever is our forerunner, that some mental factor has been repeatedly suspected of influencing these diseases. It seems to me particularly significant that this should occur even in hay-fever where the idiotoxin-idioceptor element is so pronounced as usually to distract all attention from the other causal factors.

In Chapter III (p. 39) this emotional element figures as Cause F in our short selected list of causes which we proposed to deal with by way of treatment for the toxic idiopathies.

Suggestion. I have found that, when in the past I have tried to discuss the ' psychological ' element in hay-fever (or of any of the other toxic idiopathies) with chance acquaintances, they often assume that I mean the effect of suggestion. Though suggestion does of course come into the story, for it comes into all medicine, it is not exactly what I mean ; but perhaps I had better deal with it first. Suggestion is certainly one of the ' phenomena of the human soul or mind '.

Bedside Manner. Every doctor must have some manner in which he receives his patients and conducts the interview with them, even if it is only a bad manner. This is a question of human relationships, and concerns every one of us in our approach to our neighbours : the doctor and patient relationship is only a very special case.

The ' bedside ' or consulting room manner, though it may be considered a joke, is really important, for it will give confidence to the patient or the reverse ; what is

said will alarm him or dissipate his fears. All this will be taking effect via the patient's mind, either consciously or unconsciously ; even this non-specific treatment by a confident bedside manner may be labelled ' suggestion '.

But, whether we like it or not, suggestion in a much more downright form is a commonplace throughout medicine. That a bread pill will produce diarrhoea or constipation, whichever has been suggested, is most surprising ; all the same it happens, and must be taken into account. As might be expected, we can see the effect of suggestion with the toxic idiopathies too. Its effect on hay-fever attacks will serve particularly well as evidence—it has an *a fortiori* value.

As a student I read in Sir William Osler's famous text-book that a hay-fever patient, when given an artificial rose to smell, had had an attack of hay-fever ; this, he says, ' strikingly illustrates the neurotic element in the disease.' That the patient (and apparently Osler too) was wrong in believing that real roses produced hay-fever is beside the point ; strong belief works equally well whether right or wrong—*vide* the bread pill. Indeed, judging by the description of the disease which Osler gives in his book, it is at least doubtful whether the rhinorrhoea which the patient exhibited was itself even true hay-fever.

This suggestion effect could be paralleled over and over again ; note the tomato obsession story (p. 53). I will relate an incident that I took a hand in myself.

An imaginative and rather hypochondriacal business man firmly believed that the yellow mustard flower was the cause of his hay-fever symptoms. This is an easy mistake to make because mustard, like the roses, flowers chiefly in June. I deliberately gave him a word-painting of the dusty English lanes in midsummer : the high hedges, the gate on which he could lean gazing at the nearby fields, and the hillside beyond them in the afternoon sunlight ablaze with his yellow mustard. At this point the patient interjected, ' Stop, stop, you're giving it to me,' and I found his eyes were red and streaming.

This scene took place in my consulting room during the early days of June, when there probably *was* enough pollen in the air to cause trouble—if he was in the way of it. My emotional interference was the last straw.

A similar story was related to me by a woman hay-fever patient.

She was with a friend at the Royal Academy Spring Exhibition of paintings, and they stopped to look at a large canvas realistically representing an English hay-field at the height of summer. ' To look at that ought to give you hay-fever,' said the friend. Forthwith my patient experienced a rhinorrhoea and began to sneeze.

It is significant that, certainly in the second and third instances of suggestion-produced hay-fever, the symptoms occurred while there was a grass pollen in the atmosphere, and therefore possibly on the nasal mucous membrane of the unsuspecting victim. Osler, however, gives no indication of the date of the rose incident.

Also suggestion must help to promote recurrence of attacks at the anniversaries of any really memorable attack. The well-known anniversary nature of hay-fever must with former hay-fever patients increase the thoughts of it each English May-time, and so increase the probability and severity of the attacks at midsummer. If asthma occurs at, or about, Christmas time for several years in succession, then the expectation of trouble at that time helps to make it come again—and come it

does, very often. Again, the weekly recurrence of ' week-end asthma ' (p. 158) must very much help its return every Saturday or Sunday when it is expected.

The minds of children in particular are so accommodating that it is quite easy for unwise grown-ups to suggest disease to them. Parents who are always on the hunt for asthma symptoms, or habitually describe these before their children in exaggerated terms, or declare that their children ' never eat a thing ', ' never sleep a wink ', and so forth, are really doing their best to inculcate these disadvantages by suggestion. As they say—they are putting ideas into their children's heads, and thus panicky parents make sick children.

Obviously suggestion may be used covertly for treating our cases ; but Coué, the famous French quack before the 1914 War, openly used suggestion as his chief, if not only, means of treatment.

Coué had deflected from me a very neurotic elderly male hay-fever patient whom I was just beginning to treat with the rather inadequate course of prophylactic desensitisation which I used in those early days.

On meeting my ex-patient by chance, he told me confidently that now he had been ' couéd ', he could not possibly have any hay-fever at midsummer ; so I rather wickedly said that it would be interesting to see if the diagnostic skin reaction which he had shown to grass pollen had disappeared also. He eagerly agreed to have this test ; but he very sensibly forgot to come to me to get it done.

Looking back now at the incident, I would suppose that, if Coué could successfully suggest away a man's hay-fever, he could also suggest the disappearance of the skin reaction ; for this is of course only a hay-fever attack in miniature, and governed by the same conditions as those which govern the disease of hay-fever itself.

My ex-patient claimed considerable relief from ' Doctor ' Coué's treatment ; but he was shy of talking to me about it, and the effect certainly didn't last.

Much more than Suggestion. The title of this chapter signifies a mental effect which is, to me at least, stranger than suggestion as described above. I propose now to show :

(i) that acute emotional shocks can influence the allergic attacks, and with surprising frequency.
(ii) that long continued moods and nervous tensions may influence the probability, or otherwise, of these attacks, and
(iii) that these shocks, emotions, moods and tensions are found to affect the patient so regularly that they may be presumed to play some part in all attacks of any toxic idiopathy.

Once again, let me insist that these mental influences are never the whole of the story of causation, though I shall claim that they are never entirely absent from it ; to think otherwise is the Single-cause Fallacy once again. We may, therefore, discard the idea of the ' psychological ' or the ' non-psychological ' type of attack.

First Intimation. Naturally we can only see a close correlation between a causal emotion and the resulting attack of a toxic idiopathy when both are striking and notable ; fortunately this happens often enough to be quite convincing as to a cause-and-effect relationship—as examples will show.

To me the first intrusion of the emotional element, beyond such things as Osler's ' suggestion ' above cited, occurred in 1910, and was quite sudden and unexpected by me. I made one of those remarks which surprise you as you say them ; it is as if someone else was speaking for you : I asked a patient, fortuitously as I supposed, what had caused her first attack of hay-fever. She startled me by answering without any hesitation that it had been caused by hearing of the death of her favourite brother. I suppose I had expected her to say that she had gone for a summer holiday into the country from London, or that she had walked through a hay-field in June.

When I saw her in 1910 it was twelve years after this first attack ; she was just an ordinary typical case of hay-fever ; she had the classical symptoms recurring every midsummer ; these began and ended at the correct dates ; and she gave on the skin a suitable wealing around the grass-pollen prick, and gave a skin response to nothing else. She had had similar hay-fever every year since that first sudden onset ; this she ascribed, as I have said, to the death of her brother.

(1) She and her brother had caught pneumonia simultaneously ; she had recovered from it, but he had not. When my patient was at last convalescent, and on a mild June day was sitting well wrapped up in a garden surrounded by hay-fields, the nurses looking after her thought she was perhaps now well enough to be told of her brother's death.

At the news she began to cry, and went on and on until her nose and eyes got sore and red ; then she found to her horror that she could not stop crying. She told me frankly that she soon found herself more concerned about her state of uncontrollable weeping than she had been at her brother's death.

She had never had hay-fever previously, and indeed knew very little about it. It was only after repeated attacks that midsummer of twelve years ago that the condition was identified as hay-fever by her doctor.

When I heard all this I thought it was the usual ' patient's nonsense ' : *I* knew of course the true cause of hay-fever, which was grass pollen! I did, however, remain sufficiently on the *qui vive* to ask succeeding patients precisely the same question, and it soon became clear that there was a good deal in this emotional shock idea. I calculated at the time that roughly half my hay-fever cases could be found to have some kind of emotional start if enquiries were made carefully enough ; but in retrospect I think I may have overestimated. Here are a few more causal shocks told to me by patients at that time :

(2) A bitter quarrel of a girl with her step-mother started an attack of hay-fever.
(3) Just missing a train connection at a country junction = hay-fever.
(4) Being unjustly punished as a child and not allowed to protest = hay-fever.
(5) Seeing a thriller play as a child = hay-fever.
(6) Bad work in the office, and being very justly sacked for it, as the patient freely admitted = hay-fever.
(7) Going off on an exciting holiday = hay-fever.
(8) Going to a party when not used to them = hay-fever.
(9) Learning by accident that her husband had been unfaithful—result, hay-fever.

These were all first attacks, but the phenomenon is not confined to the first by any means ; there does, however, seem more resistance to be overcome in launching the first attack, i.e. it generally needs more causal pressure to start it. Perhaps this is only the result of the familiar ' The worst cause of asthma is having had it ', i.e.

after the first attack, hay-fever is expected, but before the first it is not expected. Similar shocks or mental states are found to augment attacks which have already begun, or to cause a recrudescence of the disease after it had been ' grown out of '—according to the patient's belief.

Any Toxic Idiopathy can be started in this way, as I found later on when I came to deal with them.

I am not sure what was the first story to show me that toxic idiopathies, other than hay-fever, could be started by an emotional shock, but the following was certainly an early one :

(10) A boy of 15 years was sent to me by his doctor for his recent asthma. I was able to report after skin-testing that the patient was sensitive to several foreign proteins (e.g. horse scurf and cat scurf) and that this put him squarely into the allergy group, adding, however, that I did not think that this was the sole cause, for I thought that a throat infection (trauma) was also implicated.

There was also an emotional causation. According to the boy's story he had committed a minor delinquency at school which was more of an academic than of a moral offence. This, again according to the boy's story, had unreasonably enraged his House-master who had staged a full-dress monitorial beating which took place before the whole House. The boy said it was the ridicule and disgrace of the ceremony rather than the actual beating which he had minded most.

Previous to this, the boy had been quite clear of asthma for over six months ; but it came on suddenly and severely on the evening of this over-emphasised ' disgrace ' scene, and had persisted on and off ever since.

There was no doubt in the mind of the boy or of his parents (who had promptly removed him from the school) that this ceremony was responsible for the renewal of the asthma attacks. There was even some talk of claiming damages, and I should think they could have proved the causal character of this punishment scene in a Court of Law.

Since those days I have found that any sudden unexplained onset of an attack suggests the possibility of some emotional cause ; and I have often made a ' hit ' with the patient, or with his G.P., by asking just what event had been happening prior to this sudden and unexpected attack. Sometimes patients even accuse their doctors of collusion with me : ' That's too bad of him! I especially asked Dr. So-and-So not to tell you about that story '—when Dr. S. had told me nothing at all about it.

Here is an asthma case where I guessed from the sudden and unexpected onset that there was likely to have been some violent emotional shock preceding it.

(11) I remember asking a man what had been happening just before his first sudden onset of asthma. He looked in his diary at my request and found to his surprise that on the day before the asthma came he had run down and killed a pedestrian in Teheran ; he had then been seized by an angry mob of Iranians, and had been put into a filthy jail by the police to save his life.

It had never occurred to him to connect the asthma with his near escape from lynching : on the contrary he had pessimistically reflected that ' troubles never come singly '.

To save the holding up of my argument I give here only a sample half-a-dozen of such dramatic ' starters ' ; many more will be found on pp. 190 to 195 in Chapter XI. Including the hay-fever starters already given, each story has a serial number attached to it for convenience of reference in argument.

I have selected them a little, perhaps, for their dramatic value, because it is the emotion that they arouse that is interesting us ; but I have tried to avoid giving preference to any particular emotions. Most of these instances have been volunteered by the patients, some have been brought to light during the questioning, and all were regarded as ' the cause ' by the patient or by the patient's friends. For the most part I have avoided leading questions.

(12) Nearly run down by a bus. That produced asthma within the hour, and unexpectedly. An incident like this is a frequent ' starter ' of a toxic idiopathy attack.

(13) A girl of six years had her first attack of asthma when her elder brother pretended to cut her throat with a knife. He actually made a scratch on the skin.

(14) A child of 2½ years found a pot of freshly-made mustard, and put a large spoonful of this into its mouth ; this produced prompt asthma, and for the first time in its life.

(15) A boy of 10 was caught stealing apples, and he had his first attack of asthma in consequence. This is a fairly frequent story, by the way.

(16) The first glance at an unexpectedly big income tax demand caused asthma promptly, and for the first time.

(17) A woman told me she had developed the first of a long series of angioneurotic oedema swellings when caught smoking a cigarette by her parents when she was a child of 12 years of age.

The commonest events to be credited with starting attacks for the first time, or with re-starting attacks after a long period of freedom, recur so often as to lose all dramatic interest in their recital.

A death or injury of a beloved relative is very often reported by a patient to be causing a severe attack. In my notes, the commonest of these bereavement causes is the death of a brother, as in story (1), (p. 140) ; then perhaps of a father ; but very frequently, too, it is the death of a child or a marriage partner. Often I am told of a series of recurrences, each developing after some fresh fatality or accident.

Girls get allergic symptoms very often for the first time when, without any warning from their mothers, they experience their first menstrual period. This may, of course, be an internal secretion business ; but I suspect emotional shock which may be very great on such an occasion.

Children get attacks on their birthdays, or at Christmas time, or at parties. Sometimes there is a string of Christmas or birthday attacks recurring year after year ; but on the later occasions this may be largely suggestion, as I have said above.

At the end of the war the sudden return of an unknown but much talked-about daddy, produced crops of nettlerash or asthma in allergic children (see story on p. 163). The angioneurotics frequently tell me how going to a party, or even receiving an invitation to go to a party, will cause the familiar swellings to arise. The arrival of a much desired new dress, or worse, its non-arrival, may produce nettlerash, migraine, or angioneurotic oedema if the girl is prone to this.

Historical Examples. The war, which is made to account for so many things, must take the blame for many an attack of a toxic idiopathy. The first notable war incident was the Munich affair ; I'd say that the sharp incidence of allergic symptoms in the autumn of 1938 gave evidence to the doctor of the increase of nervous tension in the general public caused by the goings-on of home and foreign

politicians. People did not show very much perturbation otherwise—they ' consumed their own smoke ', but the allergy clinics disclosed the hidden trouble.

Then came the official beginning of the war in September 1939, which was less of a shock than Munich as judged by the allergy clinics ; then in succession the Norwegian crisis, the German break-through on the Meuse, the beginning of the London blitz. Later there was the arrival of the ' doodle-bug ', of the rockets, and so forth. All were claimed by very many patients to have caused allergic trouble of some kind, and often for the first time.

The bomb raids on London produced such a crop of asthma attacks occurring for the first time, or after years of quietude, that I took to marking these asthma attacks on the case sheet as ' Autumn Bombing 1940 ', ' Doodle-bug 1944 ', and so forth.

Sometimes the patients recognised the connection between attacks and shock, but often they did not. If it was suggested to them, they would often declare that they were not really frightened, but were fussed and bothered by things in general.

' Atmosphere ' of Shelters. Very frequently the mothers declared that there must be something injurious in the atmosphere of their Anderson shelters, because on the very first time they put their little children into them, these children began to have asthma ; parents used to ask me if the air could be analysed to find out what the ' asthma poison ' could be. Here the reader may be reminded that all small children reflect the emotions of their parents, particularly of their mothers ; if the mother remains calm in the raids, then the child is quite unaffected by the ' atmosphere ' of the shelter, or for that matter by the banging of the bombs. Young women often think that they owe it to their femininity to get flustered on these occasions ; they had only to be told that they owed it to their motherhood to have guts for two, and this was generally enough to stop the baby's attacks on going into a shelter.

Laughter. By way of contrast, and lest it should be too easily assumed that it is only a matter of fear or anxiety which is at the root of all the trouble, let me now take the case of excessive hilarity. Much laughter, especially if it verges on the hysterical or forced laughter, will produce asthma or nettlerash very easily. Over and over again I hear something like the following : ' I daren't let his daddy returning from work be too funny with my little boy, or the poor little chap will have asthma all night.' Though I don't suppose grown-ups let their laughter get out of control as often as children do, this effect of hilarity is true of the grown-ups too ; witness, perhaps, the story (No. 49, p. 192) where the woman got an attack as a result of seeing Mr. Leslie Henson making faces on the stage. Patients sometimes volunteer the statement that laughter causes attacks, but usually they seem not only surprised but rather ashamed of it. I found that if I put the leading question to them, a really heavy percentage of patients say they have found to their surprise that laughter does cause an attack.

(18) I remember a rather stolid-looking middle-aged lady stumping aggressively into my consulting room, saying, ' Laughter causes my asthma. You'll say I am a liar. All doctors do.'

Moods. All the examples so far given have been incidents which can be correlated with attacks; even the hearty laughter just mentioned is noted by the patients or their friends as apparently causing the toxic idiopathy. In my experience, however, it is not the more dramatic events, but the niggling annoyances that can hardly be named or noted, that cause most trouble to the patients; it is rather some long-continued mood than the sudden event. As one might expect, this is seldom recognised, or reported by the patient, as causing an attack because there can be no obvious correlation. A long continued mood of depression or annoyance can hardly be sharply synchronised with a little chronic asthma which occurs nearly every night for months on end, thus spoiling sleep and gradually weakening the patient. If I am to bring conviction on this effect of mood in the patient I must detail some rather lengthy stories, and naturally I choose the cases where circumstances approximate to the conditions of a crucial experiment.

(19) I was asked by a practitioner to see a small boy of $6\frac{1}{2}$ who had asthma. The boy had a large nursery, and there were present the child, his two parents, the head- and under-nurses, the doctor and myself.

The father was behaving in a very hysterical manner: he demanded angrily why doctors had not found 'a cure'; he then asserted that it was an insult to any child to have a doctor in his nursery. He implored his small asthmatic son to 'look at the pretty puff-puff, darling, that your poor daddy has brought you'. He asked the head-nurse when the child had last eaten a proper meal and, without waiting for an answer to this, he began to tell me that the little patient had not had a good night's rest for months. While he was behaving in this way he gesticulated violently, and talked in a hysterical way with tears in his voice.

He was so insistent in his outpourings that I couldn't get on with my work until I had turned that father out of the room, explaining that I would come and talk to him about it all afterwards.

The child had little or no asthma when I saw him, but looked sullen and resentful; he was perhaps what the psychiatrists call 'aggressive' in his demeanour. The history was that he had had asthma from babyhood. It had not been preceded by eczema. The asthma had followed short colds, and had usually been worse in the winter than in the summer. The boy's health, however, had progressively deteriorated, until he hardly ever had a night's clear rest from an asthma attack. He had been scared off eating food by what I found to be the unnecessarily restrictive advice about it, and so was rapidly losing weight. I made some skin tests, including of course all the prohibited foods, but got no response to any of them. There was good evidence that there was a bacterial factor, and I took routine swabbings from his throat. But I told the practitioner privately that I was sure the father, who had nearly given *me* asthma too, was responsible for nine-tenths of the little boy's trouble. 'Nonsense,' said my colleague, 'I have called you in to see what I know to be a genuine case of asthma, so I don't wish you to say any of that psychological stuff to the parents.'

I had, of course, to say to them what I believed to be the truth and for the good of my patient. I did so—with the natural result that I was soon relieved of the care of the case.

About two months later the mother came to me to say that the child had been taken on a round of 'all the asthma specialists', but was steadily going downhill. It made her heart ache to see him; he was nothing but skin and bone. She really thought the child would die.

I agreed that this was probable, and repeated that the father was killing the boy by his excessive care. She agreed that the father perhaps was silly with the child, but protested that I was much too hard on him and even rude to him. After calming down a little she asked if it would not be a good thing to send the little boy to a 'purer atmosphere', adding that they knew of a very good little kindergarten abroad where the master

was sensible with children and the air was pure. I told her that the air didn't matter a scrap, but that it was the foul *personal* atmosphere of her nursery that was at fault; however, they certainly should send the little child to the kindergarten with a nurse, who should promptly return to England. The parents in particular should *not* go with him.

What happened when the boy got abroad is very revealing. Within a week he had lost nearly all traces of his asthma; he then began to eat and sleep well, and did his lessons with a will. He became the little dare-devil of the party, and was everything one wants a small boy to be. He put on weight, grew fast, and obviously improved in health.

Eight months after leaving England, when he had not had any asthma for a long time, he was told by the head of the kindergarten that his 'dear mummy and daddy' were arriving on the morrow to see him: the child got his asthma back again that night.

Those devoted parents had been conscience-stricken at having 'neglected' their darling for so long, so they had taken rooms for two months to be with him for a nice long holiday; but when they found they were so obviously half killing him by being there they departed after two days.

After they had gone away the child quickly regained his former good health, and remained thriving until the same thing happened again four months later, that is to say a full year after he had gone abroad.

The master said, 'Your father and mother are bringing their motor car to-morrow, so that you will be able to go for picnics. You'll like that, won't you?' 'Yes,' said the boy—and got asthma that night.

'What *are* we to do?' asked the distracted mother when she came to see me about it. 'I must admit that he has become a son to be proud of during this last year, but we can't leave him abroad all his life.' 'There is no magic in the locality, as I told you before. Change your husband, or send your boy to an English boarding school.' 'Oh, you are hard on my poor husband! Well, a boarding school it must be.' The patient was clear of all asthma at that school; but, very significantly, they found that he was trying to arrange to stop with another boy's parents during the holidays.

The above story shows, as a prominent element of causation, some pathogenic emotion or mood in the small boy. This psychological disturbance was almost certainly derived from his home environment, and particularly from his parents. Removal of the boy from his home, either abroad or to a boarding school, was sufficient to turn the balance of causal factors against the repeated attacks of asthma.

On two occasions the mere imminence of a visit from his parents to his foreign kindergarten was enough to cause a recurrence of those attacks. There could be no question of the father and mother bringing a peccant idiotoxin from the boy's English nursery: their bodily presence even was not necessary. The thought of them, and all they stood for to the child, was enough.

A captious critic who had read this story asked if this was an average sample of the effect of moods on a small boy. Of course it isn't; it is an extreme case, told in illustration of what may happen to a child with allergic diathesis if the home environment is for him sufficiently pathogenic.

If we are concerned to prove that there are jewel thieves about, we don't look for an *average* incident in the suspected person's life; we tell, if we can, how the dubious stranger was seen surreptitiously shovelling the Countess's pearls into his trouser pocket.

Grown-ups too. My next illustration of this queer effect of mood shows that it is not only children who derive trouble from a cloying environment; this story is about an over-nursed man of 50 years.

(20) I was asked to visit this patient by his practitioner, and I found all the little household eagerly awaiting my arrival. Two young men, the sons, had stopped away from their business to be present. The wife of my patient rushed at me excitedly, saying that she *knew* I was going to make a cure! It was perfectly wonderful that I should be able to do it, because her husband was so very, very ill; would I promise to work a miracle? Then the daughter came in and said, rather more restrainedly, but with considerable excitement, that they had been waiting for my visit most anxiously, and they expected great things of it; she added that the position was desperately serious for her father. Then the old mother of the patient came in, and she started violently kissing my hands; she was so incoherent in her excitement that I could hardly tell what she was saying, but she was horror-struck at the illness of her son, wanted a miracle, and knew I could work it for her if I only would.

The patient was lying quietly in bed, and didn't seem to be paying much attention to the hurly-burly all round him. He was not having asthma when I saw him; I made a routine examination, making tests and taking cultures.

I told the practitioner that I would be reporting to him within two or three days as to what I thought should be done to eradicate causes; but I added that in my opinion those three women were nursing the man into asthma with their over-attention and over-excitement.

Here again the practitioner was annoyed with me for supposing that he had sent me to a 'sham' case. On reassuring him that I knew the asthma was of course quite genuine, I made the suggestion that, if the treatment I proposed did not bear fruit, I would take the man into hospital, if only to get him away from the relatives.

Three weeks later, I was rung up in my laboratory by the practitioner who said I must take the man into hospital as I had promised to do: 'Not that you can do anything that I have not tried here; but I simply must have a decent night's rest. They have been getting me out of bed two or three times a night!' I arranged for the patient to come into a laboratory ward at once.

The man arrived at the hospital in such a collapsed condition that he had to be carried across the ward by a porter, and was quite unable to undress himself. I gave the Sister of the ward my simple instructions: she was to treat him chiefly with the routine 'calculated neglect', and I told her she would probably work another of her miracles by a little play-acting of this kind. The Sister doubted that: 'The man is really very ill, Sir, and ought perhaps to go on the danger list straight away.' I told her to put that off as long as possible, or we should have those three women down on us; she was in fact to keep the relatives away as much as possible, for I believed them to be the main cause of the patient's illness.

In the press of other work I forgot this man for two days, but hoped for the best as my very experienced Sister had not sent for me. When I returned to the ward the patient was quietly reading a newspaper; he shook me warmly by the hand, telling me I was the cleverest doctor in London because I alone understood his case. If I understood something of his case, it was not in the way he meant.

That man had no asthma or signs of illness during the six weeks that he stopped in hospital. I then told him that we ought not to keep him in much longer. He said his stay in hospital had been like heaven to him: nobody had bothered him, and nobody had asked him how he was. While he had been lying quietly in bed he had been wondering; and now he found that he dreaded returning home! I asked why; he said he had been thinking: what he dreaded most was that all his family would rush at him, ask him how he was, and want to put him to bed. Most unwillingly he had given up his business to his two sons, and allowed his women folk to 'nurse' him. I asked him if he could take back his business if he wanted to; yes, he could and would. I advised him to do just as much and just as little work as ever he liked, and to make his two sons dance to his tune. He said he would do so, but asked about the women. I suggested that he should pack a bag of clothes for the night, show this to them, and say that if there was any suggestion of his

being put to bed or wanting nursing, he would go off and stop in an hotel. He promised he would do this, and I believe he has never had any trouble with his asthma since.

A man of 50 this time ; but he seems to have derived his emotional trouble from much the same behaviour in *his* human environment as that which we found to be so upsetting to the small boy in the earlier story (No. 19, p. 144). The man, according to what he told me in hospital, had not previously been aware of any particular domestic tension in his home ; he became aware of it on analysing his sense of relief while in hospital, and of disgruntlement when contemplating a return to his loving family from the hospital.

Neither the nursing staff nor I had suggested to him the idea of an unhealthy home atmosphere ; he had come to that of his own accord, and I see no reason to doubt his conclusion.

It is difficult to tell what small boys of six or seven have in their heads ; but after his year abroad, and while he was at that boarding school, the child in story 19 had clearly become aware that his home was uncomfortable—*vide* his attempt to stop with another boy's people during the first holiday.

I think those two cases (Nos. 19 and 20) prove my point, but I'll give a few more to show clearly the kind of thing that may perturb the patient and so provoke the asthmogenic mood. (More stories still will be found in Chapter XI.)

(21) I remember an upstanding intelligent boy of 16 years who was an old-treated asthma case of mine. He came to see me, apparently of his own accord, on his way home from school for the summer holidays, to report how very much better his asthma had been for the last year. He had to admit, however, that he had had a slight return of his trouble during the last fortnight of term.

Mindful of my own schooldays I hazarded that he was perhaps worried by the end-of-term examinations. He said with conviction that he did not believe it was that ; and then his manner became much more hesitant. After a pause he said reluctantly, ' It seems utterly ridiculous, but I think it was the thought of going home for the holidays.' Then, apparently overcome by the disloyalty of this remark, he added, ' I'm very fond of my home, and I'm deeply attached to my wonderful mother ; I have everything a boy could want—fishing, shooting, riding and everything—yet I am a shade daunted at the thought of going home to be questioned as to my health and symptoms.'

That mother *was* a dominating personality ; she had rather frightened me at a previous visit. I did not tell the boy this, though I saw his point of view.

I had not seen the home of this boy, but we may take it from his description that it was to him an extremely pleasant place. The only fly in the ointment seems to have been the human environment in the shape of a beloved but too dominating mother. I suspect she was, from the highest motives of course, an unmitigated planner of other people's lives : we all feel it so pleasant to be that kind of person, but find it so unpleasant to be planned by them—they usually produce a mood of unrest.

My next story is of an adult, the grown-up daughter of just such an unmitigated planner ; but this time the mother had far less ability than the mother in the previous story. If *this* patient had ever rebelled against parental dictation she must have long stopped doing so when I saw her.

(22) A well-nourished unmarried woman of twenty-five was brought to me by her widowed mother who tried to answer all the questions addressed by me to her daughter ;

indeed the mother didn't wait for them, but embarked on a long and sad story of how ill her poor daughter was. The ' child ' could eat nothing! ' As soon as she finds that she likes a food she quickly becomes allergic to it ', and so was poisoned by it. So scanty had the list of permissible foods become that the girl was rapidly losing weight, the mother said.

I couldn't help querying this last statement, and was told with asperity that her daugher *had* lost weight—' till recently '.

' She is our breadwinner! Yet every food in the end upsets her ', and the mother moaned about the desperate situation, and gave me details about the weird dietary they had resorted to in consequence of these 'sensitisations '. On testing her I found the young woman was not allergically sensitive on the skin to any food I tried on her, and I tried a large number.

All this while the daughter sat fidgeting endlessly with her hands held in her lap, her eyes downcast, and with a pained look on her face in conformity with her mother's tale of woe. When she did get a word or two in edgeways she talked baby-talk, smiling the while uneasily ; and she was clearly childish-minded for a grown-up woman.

By skilled manoeuvring, and by refusing to allow the mother to retire to the water-closet to help her daughter to produce a bowel specimen for me, I contrived to get the older woman back into the waiting room ; so I was able to see the daughter alone when she returned with the specimen.

The girl at once began to speak briskly and sensibly, and became quite normal and adult ; she told me that her mother fussed and irritated her endlessly. She said her mother was quite ridiculously over-anxious about her health, and this fretted my patient ' terribly '.

When I said I would speak to the mother about all this, she implored me not to do so ; she said it was quite unnecessary, and that she would hate to hurt her mother's feelings. She was sure her mother was ' doing everything for the best ', and the patient began to cry.

The abject state to which that ' good ' mother had reduced her beloved child made me suddenly feel quite sick ; yet I'm sure the mother had no doubts whatever about her own conduct. That amount of supervision would have been pathogenic if applied to a child of eight or under.

Quiet, friendly laughter at her mother would have been the best tactics, but it should have been begun before the daughter reached her teens ; unfortunately she wept instead, and wept in secret. Perhaps the fault lay on both sides : the two women had slipped gradually into this impossible relationship.

My next story concerns the asthmogenic effect of two temperamentally contrasted aunts when looking after an asthmatic orphan nephew.

(23) An eleven-year-old boy, well built and of good intelligence, came alone into my children's clinic, but bringing a doctor's letter of recommendation with him.

The boy had some bronchial asthma from babyhood. When he was eight years old his mother had died, and he went to live with two aunts in succession.

The aunt first selected had been, to quote the boy's words, 'exactly like a mother. Oh, she *couldn't* have been kinder, doctor! ' Unfortunately the ' air ' where this kind aunt had lived seemed not to suit his asthmatic tendencies ; so he had gone to live with the second aunt.

' *She* wasn't a bit like a mother : very strict she was—sometimes.' However, the air where this less agreeable aunt lived evidently suited him well, for he had hardly any asthma there at all.

Recently the first and more agreeable aunt had shifted her home, and the boy had been trying-out the new atmosphere. However, his luck was still bad, for again the new ' air ' didn't suit him—so he told me.

I told him that the air of the second aunt would probably suit him best till he could strike out a new line on his own account; and I reported to his doctor (amongst other things) what I had said about atmosphere, and why.

Everyone, even the doctor who sent the case to me, had blamed the air surrounding the house of the boy's loving aunt, and had exonerated the air round the house of the aunt who was *not* so loving.

Of course, air *may* contain an idiotoxin to which a patient is sensitive—grass pollen, cat scurf, mould spores, etc.—but usually in cases of localised asthma it is the human atmosphere which is to blame, as we found with the Anderson shelter mentioned on p. 143.

(24) I call to mind an occasion when a very distinguished but asthmatic member of the British Asthma Research Council seriously suggested to its Medical Advisory Committee that a small party of perhaps three people might be sent out to Switzerland to collect samples of air at various heights for analysis. He said he wished to see what it was in the air which prevented anyone from getting asthma at over 5,000 feet above sea level.

I am afraid I ruthlessly torpedoed the project of that very pleasant jaunt—the other members of the Advisory Committee concurring.

Later on we learnt with amusement that this Very Distinguished Member had himself gone to investigate and had experienced severe and continuous asthma at well over the prescribed height.

Perhaps the recollection of his setback in Committee had soured him, or shaken his simple faith in height. Previous to this fatal voyage, I had always suspected that, by chance or design, he had left behind him some domestic irritant whenever he went for a Swiss holiday; he had often spoken to me of his miraculous immunity on these occasions.

Moods do Affect. I must refrain from digressing any more over the effect of 'atmosphere' in producing the toxic idiopathies; those five stories (Nos. 19 to 23) were told to show that a long-continued mood, as opposed to a sharp shock, may certainly on occasion promote chronic attacks of asthma on the patient; and especially is this shown by the first one (No. 19) which, as I have said, is almost a crucial experiment.

These five stories have all pointed to home influence, and particularly to the people in these homes, as being partly responsible for the allergic troubles of the patients in question. That, of course, does not show that every psychological trouble must be derived from the home, but, as all these five stories have been selected simply with an eye to showing the effect of a long-continued mood on ameliorating or on exacerbating the patient's symptoms, it can't altogether be an accident that homes come under suspicion so often. We shall do well to bear that in mind, and Chapter X is concerned with this influence of the home.

Moods *always* Affect. If we may take it now as proved that some moods increase the probability of attacks while other moods can remove that probability, what evidence have we that there is *always* this influence in the background of a toxic idiopathy case?

To get evidence of the general influence of moods as affecting all allergic people, we must not consider specially selected instances, but mass experiments—so far as this is possible. We must look for the occasions when a considerable number of the

population change suddenly from one pattern of life to a contrasted pattern. Since our stories have suggested that where the emotional shoe pinches most is in the home, we had best examine what happens when the patient's home life is suddenly changed for something quite different.

Three such changes suggest themselves. They are :

(*a*) The change from home life in the holidays to life at a boarding school for boys and girls from, say, eight to eighteen. Also the reverse change from school to holidays.
(*b*) The change on leaving home surroundings to go into hospital, and the change on going back home again.
(*c*) The change on leaving civil life to go into the armed forces ; and of course the change back again on demobilisation.

The changes from home to a *day*-school or from home to the day's business, do not last long enough to show results, especially as most of the attacks occur at night, and therefore under home influences only.

There are other changes in life which induce moods which may effect the attacks of the toxic idiopathies.

(*d*) The change from the unmarried to the married state ; this change is too diverse, too much of a lottery, to show any really steady effect. It will be shortly discussed after mass experiments (*a*), (*b*) and (*c*) above have been described.
(*e*) The emotional change in our life from the work-a-day week to the domestic week-end on a Saturday or Sunday : that also will be discussed later (p. 158) under the heading of ' Week-end Asthma '.

(*a*) **Boarding School.** Perhaps the most significant evidence is furnished by contrasting the effects of life at a boarding school in term-time, and life at home in the holidays, on a boy or girl with some toxic idiopathy. It is possible that no change of symptoms may be noticed, but when there *is* a distinct difference in the effect on the patient of these two kinds of life, it is far more likely that the child will be better of his ailments at a boarding school and worse again in the holidays, than the reverse of this ; if I were pressed for a ratio, I would say perhaps ten to twenty times more likely.

This is not at all what the parents expect :

(25) One mother naively said to me, ' You would expect they would be better at home, where every possible care is taken of them ; but no, my wretched children persist in being better at school, where I don't believe they are really properly looked after at all. They invariably spoil the holidays by being ill all the time.'

Parents often say, ' We are very anxious indeed to get him better of his asthma so that we may send him to a boarding school.' In my experience, the argument should usually run : send him to a boarding school so that he may have the best chance of losing his asthma.

Many is the time I have been supposed to have worked a miracle by the simple advice ' take a chance on it ; send him off to a boarding school '. That advice is, of course, given when it seems to me from their behaviour in the consulting room that the parents are grossly over-anxious and are fussing a perturbed child. I am

tempted here to make a technicality by resurrecting the old English verb 'to 'conturb' and speak of a '*con*turbed' child—the state of mind being that of conturbation'.

It is true that I am often told by the parents that no difference can be detected between the frequency of allergic attacks at home and at school; but it happens with suspicious frequency that, while the mother *says* there is no difference, the child may claim that he does get comparative immunity at school compared with what happens to him in the holidays. Mothers are reluctant to believe this; in fact the evidence of many parents is tainted by their sense of what is seemly. It is always well, therefore, on this as on other points too, to let the child give his or her own version of the story, and if possible, unhampered by parental suggestions. It is comic sometimes to watch the parents on tenterhooks, waiting to see how far their child will go in contradicting their story of his ailments—if he is allowed to speak freely. Often a brisk altercation takes place between the slightly indignant parents and their little pet who has been allowed to have his say at last.

Even after the logic of events has compelled a woman to admit freely that her only child *is* far healthier at school, or at any rate away from his parents, her instinct for 'mothering at all costs' may be too much for her sense of logic; it may compel her to forget all she has learnt. I have known such a mother to jump at the excuse of a little illness at the school to bring her boy back home again to be educated there by a private tutor 'because he is so delicate'. Naturally she complained later on that her boy's asthma had relapsed—'in spite of *all* the care I have taken of him.'

The truth is that the minute families of today are really quite abnormal, and natural instincts may work the wrong way round. The hutch rabbit, impelled by *her* maternal instincts in an abnormal environment, may eat her young through a mistaken sense of her duty and an absence of logic. Perhaps mothers would rather be compared to a motor car than to a rabbit. I tell them that nature provides enough maternal instinct to run a family of twenty children; if they devote it all to an only child it is like putting an eighty horse-power engine into a Baby Austin car. If that engine is let full out, the car is racked to pieces. So it is with the only child.

Sometimes, but I find rarely, a child is really better of his asthma, eczema, urticaria, or whatever it is, at home as compared with school; the less emotional mothers may guess why this should be so:

(26) I remember a mother who said to me once, 'I try not to pamper Joan in the holidays, and I find she is better that way; but they coddle her at school. The slightest sniffle of a cold and off they pack her to the San. I'm afraid she spends most of the term away from her lessons. I think that may be why her asthma is usually worse at school; it seems to be always worse if I keep her in bed at home.'

What can there be in life at a boarding school that can make this difference? There are schools and schools, of course, likewise homes and homes; but, on the whole, life at school is more Spartan. There is less food as a rule, or at least less rich food; there is usually also less time in which to eat it. Probably no one will fuss very much if there is some slight loss of appetite, and this is also not so likely

to happen. Mothers frequently present a plump and smiling boy or girl to the doctor with the bitter complaint that they are eating ' nothing ' ; a complaint like that doesn't happen at school. I think I'd say that the chief difference is in far less supervision at school by adults, or at least far less apparent supervision ; boys herd with boys and make little contact with the grown-ups, save on official occasions. There is not much yearning or brooding care to worry the children ; there is far less danger that they will become the little pets of the adults, or of their school-mates.

There will be no pressure on children, such as there often is at home, to pretend to be more childish than they really are in order to win the good-will of the adults. On the contrary, boys and girls at school have got to stand on their own feet, and are constantly supplied with a task which is sufficiently difficult for their years.

Children from small families, where perhaps this parental yearning has developed almost into a cult, will find plenty of other children to rub up against in place of the excessively adult society of their own homes.

Children are marvellously adaptable, and the pet of the small home soon finds its own level at school ; the other children see to that.

Parental Visits to Schools. By chance I got some evidence of the effect on allergic children when parents come to visit them at a boarding school ; this took me quite by surprise, and in fact was discovered by my secretary and not by me.

I must premise that every year I have a considerable number of boy and girl hay-fever patients who undergo the P.T.D. treatment with grass-pollen extract ; usually they go right through the midsummer term comfortably and without symptoms, in contrast with the two months of misery which they used to have every year before they were treated for it. But these treated cases, and especially during the first year of treatment, are perhaps only just on the right side of the safety line, and any strong asthmogenic factor might easily put the child on the wrong side of the line for an hour or two ; the patient might then have, perhaps, an afternoon of hay-fever in consequence.

In Chapter VII I said that a guzzle of strawberries might have this effect on a school-boy by the decalcification of his blood ; i.e. he might in consequence have a bout of classical hay-fever which he would have otherwise escaped.

There is another and a stranger cause for this trouble for the child at a boarding school. Time after time, I remember it was nine times one year, the only attack suffered by boys and girls after correct P.T.D. treatment for hay-fever was on the day on which the mother (or both parents) went down to the school to visit the child.

The parents of course ridicule the idea that a visit from them could possibly conturb the minds of their sons and daughters ; they forget their own feelings when they were children.

(27) I remember the horrified protests from the very dignified mother of a public school-boy aged 17 ; she told me indignantly that the idea that *she* had caused her son's afternoon of hay-fever was preposterous nonsense.

' How *could* my going down to the school upset Basil? ' she asked me indignantly.

The boy was in my consulting room, so, drawing my bow at a venture, I said, ' Let's ask him ; *do* you get worried at all when your mother comes down to visit you? "

I hadn't the least idea what the boy would say, and there was a rather agitating

pause ; then he said slowly, ' Well, Mother, I must say '—(pause)—' much as I had been looking forward to your visit of course, and glad as I am always to see you, and—er—all that, I must say you *were* rather a problem. I did heave a sigh of relief when I got you safely back into the London train again.'

A candid and intelligent boy or girl will tell you that they find parental visits very worrying ; they never know what Mummy will say or do next! Little snobs? Not a bit of it! They know perfectly well that behaviour, and emotions too, which seem perfectly natural at home, are ridiculous and out of place at their school, and they feel conturbed about it. It would seem that it is precisely these emotions which may, on occasions, become asthmogenic if carried to excess.

I subsequently found that other toxic idiopathies could be started in the same way ; parents have volunteered the statement to me that they have found their visits ' unsettling ' to the child :

' I don't go down to see my boy at school now. The Matron says he always seems to get asthma when I visit the school, so I think it better not to.

(*b*) **Hospitals.** Except during visiting hours, a hospital ward, though kindly enough, is a much more matter-of-fact place than the average home where considerable emotionalism or even panic is often displayed over sick persons. Healthy homes are, or should be, places suggesting complete security for the young children, but the moment the grown-ups concerned get seriously agitated about the illness of a much cared-for child, that feeling of security is largely gone.

There is another marked difference between home and hospital ; at home the sick person usually holds the centre of the stage and is full in the limelight ; in loving homes fathers and mothers, if not alert as to its dangers, will like to have it so. In hospital the patient finds there are others who are equally, or even more sick ; there is never the same monopoly of attention.

In many respects the hospital is a less austere place for children than is the average boarding school. It is a commonplace with the hospital staff that, before the distraught mother has had time to leave the building, her child, if well enough, may be laughing and joking with its neighbours in the ward, and ' having the time of its life '. In fact, when the mother sees her child again, she is often a little hurt at discovering that, instead of being shy or unhappy without her, it has been remarkably contented while in hospital.

We can, I think, sum up the chief emotional differences as an atmosphere of confidence, and an absence of panic and fuss, in hospital as compared with what we so constantly find in the home from which the patient has just come. (See last paragraph of story No. 20, p. 146.) Illnesses such as asthma, urticaria or eczema respond to this difference readily.

Case Histories of patients interviewed in the consulting room show, time after time, that the patient has suddenly lost all his allergic troubles while in hospital, but has always relapsed on going home. With elderly chronic cases this may have happened many times over : the stay in hospital has been a little oasis of health in a desert of asthma, urticaria or what-not. The patients and friends usually ascribe this beneficent effect to the Wonders of Science, and of course a timely dose of

adrenalin (or, better, the knowledge that it can be had when needed) does help powerfully ; but one suspects that it has been the freedom from fussing, and the feeling of security, which was often the most efficient part of the hospital treatment.

My own experience of taking toxic idiopathy cases, whether grown-ups or children, into hospital tallies exactly with these case histories. My instructions to the Ward Sister and her staff (as was mentioned in story No. 20, p. 146) are chiefly to treat the case with ' ostentatious neglect '. Nurses, while really helping as much as possible, are to take apparently no interest in the patient's symptoms, and are not to let the other patients in the ward take any interest either ; the patient is to be treated as very ordinary when regarded as a ' case '. If a child tried to discuss his own symptoms the nurses are to have, unfortunately, some pressing job on hand which must be attended to first, and then they forget to come back to hear the story.

In reality they are to neglect nothing. If any help is needed, or if any special treatment is ordered for the patient, the nurses should do the work in as perfunctory a way as possible, and then hurry off to some other, and apparently more interesting, job. If any injection is given, e.g. of adrenalin, the preparation of the syringe should be made out of sight of the patient ; with my cases his skin is *not* to be ' sterilised ' for any injection, because that is not only unnecessary but takes time and allows the patient's feelings to be worked up about it. (See instructions for self-inoculation in P.T.D. treatment, pp. 110 to 111, also p. 310.

They tell me that when the doctor in charge makes a physical examination, this usually quickens the breathing of all the newcomer asthmatics in the ward, and often starts some wheezing ; I therefore deny myself the daily privilege of making them say ' ninety-nine ', etc., and usually confine myself to tweaking their toes through the bedclothes, or making some such friendly gesture, as I say good-morning. I never discuss the cases in front of the patient, but safely at a distance and without too much apparent concern.

The frequent result of these ' masterly neglect " manoeuvres is to stop all symtoms within 24 hours, or 48 at most ; these are the cases which, when at home, have kept people up night after night, for weeks maybe, in a desperate attempt to afford relief to symptoms.

These patients, who have had this surprising relief on going into hospital and submitting to the unemotional hospital discipline, usually experience a sharp relapse when confronted by their relatives on visiting day. So frequently does this happen that the nurses get out of the way of reporting its occurrence after the friends and relatives have left the ward. The patient usually regains normal breathing quite quickly after they have departed.

Also, as I have said above, these cases relapse promptly on reaching home again, unless meanwhile something radical has been done along other lines of treatment.

(28) Colloquy in the passage outside the male Laboratory Ward :

J.F.—By the way, Nurse, how is the boy in No. 15 getting on?

Nurse.—Tommy, Sir? Oh, at first he used to have a whiff of it when he saw you coming into the ward. Now he only starts symptoms of asthma when he sees any of us tidying up the flowers on the central table ; he knows that means visitors.

J.F.—How *do* his visitors behave?

Nurse.—Pretty foolishly—especially the father; but he only comes on Saturday, thank goodness. Tommy gets over his ' visitors ' attack almost directly they've gone.

J.F.—Perhaps you might have a quiet word with the mother.

Nurse.—Yes, I'll try.

Thus the effect of visits from relatives to wards is very similar to the effect of parental visits to a boarding school: the emotional quarantine is broken. Often of course the visit makes no perceptible difference; but if the visited person is trembling on the brink of an attack of some toxic idiopathy it may be easy to see what temporary harm a visit does.

This emotional quarantining in a hospital is only effective if the nurses know their business. Sometimes, I think, they do not; in some convalescent homes, for instance, I have heard of enthusiastic nurses who have brooded over chronic asthmatics excessively, with deplorable results. If the nurses do fuss over their patients, or allow the other patients to talk to them too much about their symptoms, the results in hospital may be just as bad as at home, or very nearly so.

(29) Once when our experienced Sister of the Laboratory Wards was away on a short holiday, a nurse was put in charge who had been insufficiently briefed by the Sister, or me, concerning her manner of treating the allergy patients.

This novice, determining to be ' the born nurse ', went round patting my asthmatics on the shoulders, listening sympathetically to their sad stories, and telling them to be brave and '*fight* their attacks '. They obediently concentrated on this supposed ' fight ', and all forthwith succumbed to the old symptoms.

When I discovered this, I had to ask Matron to move that nurse for activities elsewhere; and when she had gone, they all recovered again.

That to my mind is a very useful little control experiment, and is not the less convincing for being quite unpremeditated.

Private Wards. As might be expected from the above, the taking of patients into a nursing home or a single-bed ward is not nearly so salutory, though ' the resources of science ' are there just as powerful. In the single-bed ward the patient feels that he has paid for even more sympathy and care than he had been having at home. A nurse will appear whenever he rings the bell; there is usually no emotional quarantine whatsoever, for visitors can come at all hours of the day. I never, if I can help it, take an asthma or other toxic idiopathy case into a single-bed ward, except for a quick overhaul, or for Rush Treatment (see pp. 86 and 87).

(30) On one occasion a G.P., after I had told him that we really had no room in our Pay-Bed Block for his patient, over-reached me by telephoning in my name for a bed. He was most unfortunately told that there *was* a bed available before I could stop them from saying so, and in the patient went.

The patient was a fat and very frightened man of 53 years, whom I had already seen in my consulting room. I had then reported that we couldn't do much for him until we could control his obvious emotional factor—though probably a bacterial vaccine would help too.

Since we had him in hospital willy-nilly, I decided it was time for a little strenuous ' psychology '. I found the patient gasping away, and asking to be put out of his misery. I felt his pulse and peered into his eyes, and said to the nurse brusquely, ' Not time yet, he'll keep! ' At my second visit I had a look at him, and noted with relief that he was breathing more easily; I said to him, ' I'll be back in about an hour; you'll be ready for my dose by then.'

At my third visit I gave him exactly one c.c. of physiological saline intravenously. He began asking anxiously about food, and I said, ' No food tonight : a big breakfast tomorrow.' As I was making a quick get-away he demanded to be given something to make him sleep ; I said over my shoulder as I left, ' You won't need it tonight! ' (And he didn't.) Before I left the Block I briefed the nurses strictly as to their calm behaviour with him.

The next day the patient was quite clear of trouble, and ate that breakfast. I had a hard tussle, however, with a beautiful lady who said she had been nursing him, and demanded to see him at once. I asked what the relationship was, and she said I might call her the patient's ' permanent fiancée '. I explained the situation, and she agreed not to see him.

He had no more asthma all the time he was in that Pay-Bed Block ; and he had no treatment, save that one c.c. of physiological saline injected into his median basilic vein.

I reported all this to the G.P., but I was told that the patient relapsed on going home.

(*c*) **The Armed Forces.** It is natural for people who have been treated as semi-invalids since childhood to wonder if they will be able to stand up to the rigours of army life ; and usually their relatives and friends wonder about this even more. They nearly always think that the previous occasional troubles will be aggravated into something really serious in the Army, Navy or Air Force ; so they often, and conscientiously, ask for a medical certificate to say the patient could not possibly serve if called up.

When patients *are* in the Service, they generally assume that the very slight degree of asthma they perhaps are experiencing will clear up as a matter of course after demobilisation when they can be ' properly looked after ' at home.

Of course, it sometimes does happen as they expected, but precisely the reverse of this is usually what they get. After the 1914 war, I heard with surprise that men who had had inconsiderable attacks of asthma when with the Forces, had developed the trouble in real earnest on their return home. Sometimes they would add that the kindly Medical Officer had hustled them out of the Service to prevent the slight trouble ' from getting too firm a hold '. In spite of this they had got worse at home.

I have seen the same thing happen in this last war, only this time I did not hear their stories with any surprise. A sudden increase of symptoms after demobilisation is so common that I may hear it two or three times in a single session of a clinic.

(31) A man aged 25, who after six years of service had been recently demobilised from the R.A.F., made the usual complaint that his asthma had surprisingly become very much worse on his return to his parent's home. Yes, he replied to my question, he had had it rather badly as a school-boy and youth, but he found it had got much better in the Service.

I asked him casually what he found life to be like now that he was having his first experience of it as a grown-up civilian. This question drew from him quite an emotional outburst.

' Coming home to live with father and mother does make a difference! I used to know heaps of chaps in the Raf ; I hardly know anyone in our district now. Mother keeps worrying all the time about father and me ; hardly a joyful outlook, I must say! I find home life so dull ; the Raf was much more lively-like. Perhaps if I could get a job I'd be O.K.'

The average case, whether man or woman, with any tendency to an allergic disturbance—eczema, angioneurotic oedema, migraine, and of course asthma—is, as

a rule, better than he or she had expected to be when called up, and is unexpectedly worse when home again. It has the same effect as going to a boarding school, and then coming home for the holidays again.

To parallel the deleterious effect sometimes noticed when a mother intrudes on the boarding-school life of her child (p. 152), or when a patient's relatives crowd into a ward on visiting day (No. 28, p. 154), we have the occasional effect of home leave on men and women in the Forces. Of course, ' Embarkation Leave ' or ' Compassionate Leave ' might very well be highly emotional events, but as a rule the short home leave in the early days of training should not have been so very exciting ; yet time and time again I got the story from both men and women that they had got unexpected attacks on going home.

(32) A young woman just demobilised from the Naval Service came to see me about the asthma which had begun to trouble her again recently.

She told me, as a remarkable thing, that on three leaves in succession, and at varying times of the year, she had had asthma attacks all the time at her mother's home. This effect had been so striking that for later leaves she had always arranged to stay at the home of another Wren also on ' home leave ' ; on these later occasions she had completely escaped the attacks, she was glad to say.

(*d*) **Marriage.** As I've said, the changes from the unmarried to the married state are too diverse to give very constant effects on the moods of the married couples : as the marriage service says, the change is for better or for worse. As might be expected, marriage does often produce some change in the intensity of the attacks, and generally for the better—perhaps especially so for the woman.

Where the patient is emotionally weaned from the parents, and has already got one foot planted firmly outside the home, marriage seems to make little or no difference : where a man, or more often a woman, has been rather tightly bound by the mother's apron strings, and for too long, then marriage often affords a relief from the asthma, angioneurotic oedema, or whatever it may be. But I have found that no relief but just the opposite of it is usually obtained if, as so often happened in war-time, the young couple have no home of their own, but live on in the house of the parents of one of them. It must be conceded that, when through stress of house-shortage a young married couple have to make their home with mother or mother-in-law, the emotional tension is likely to be unhealthy for some of them, if not for all. This tension is often not detected apparently, or not detected as a contributing cause to an increase of symptoms.

(33) A man came to consult me concerning his recent recrudescence of asthma symptoms. He told me the following tale :

(i) As a baby he had had eczema, and this had changed to asthma when he was a small child ;

(ii) He had lost the asthma almost entirely when at boarding school ;

(iii) He had regained it when living as a bachelor in his parents' home, and working at his father's business ;

(iv) He had lost it again on marriage and going off to live at a distance. Finally,

(v) He had recently regained the asthma when he and his young wife came to live next door to his father and mother ' so that the old people can look after me properly '.

This last change was what had brought the patient to see me ; he, of course, ascribed

the recrudescence of his trouble to the ' atmosphere ', adding that he always had been delicate and highly strung. *I* should have described him rather as ' a softy '.

(*e*) **' Week-end Asthma.'** Very frequently the patient volunteers the statement that he only gets his special toxic idiopathy—migraine, angioneurotic oedema, and, of course, asthma—at the week-ends. Sometimes it is *only* then, more often it is *chiefly* on the Saturdays and Sundays. Before the days of rationing, patients and their relatives often ascribed this peculiarity to the Sunday roast beef, or at any rate to eating more food at the week-end than during the working week. But patients complained, too, that they regularly got their attacks as soon as they neared their homes at mid-day on Saturday. Sometimes, if they didn't go to work at all on Saturday, they got their trouble on getting home on Friday evening ; others again had their chief trouble on Sunday morning.

If you ask them what they do at the week-ends when their asthma is coming on, they generally say something like, ' Oh, nothing much. I just sit about, and have a good rest.'

Asthma attacks (or, for the matter of that, migraine, urticaria, angioneurotic oedema or even eczema) may occur at the week-ends with the greatest regularity, as the patient and his friends are the first to tell you. Perhaps the following story will serve as illustration.

(34) I remember the case of a strong healthy-looking youth of 18 who was brought to me by his mother because he got asthma every Sunday—so they both declared. They complained that he was so bad with it on Sunday that he was hardly ever able to proceed to work on Monday morning ; he could and did work hard all the rest of the week, and quite free from his asthma too.

Pressed for details of the domestic routine of the week-end, the patient told me that his mother always *insisted* on his spending all Sunday morning in bed, and she made a great to-do of giving him his breakfast in bed.

The mother told me she was sure that it must be something in the food, though after experimenting she could not make out what this injurious food could be.

I found he was not sensitive to any of the food substances I tried, including, of course, all that his mother suspected. Besides, similar food at other times of the week had no ill-effect on him. Yet, as he lay in bed on Sunday, his asthma came on, and he and his mother clearly felt aggrieved about it ; they seemed to think that Providence was not being very reasonable with them.

I suggested that the boy should get up at the usual breakfast time on Sunday, and go for a good walk afterwards with a congenial companion, or even go to church. That, I said, might stop the weekly attack.

This was not at all well received by either mother or son, the mother declaring that her boy *must* have a good rest on Sunday, and she was going to see that he got it—asthma or no asthma!

Several times I have heard sad stories from men suffering from this week-end asthma who had been so sure it had been the place which caused the trouble that they had shifted their home to what was previously a salubrious spot, only to find that the ' air ' of this new place had deteriorated and was now no better than that of the previous home.

Also, occasional week-ends spent away from home may follow the established pattern of attack. No doubt this periodic recurrence can be due to suggestion : the

patient gets what he is waiting for ; but I am convinced that the human environment may operate powerfully also. As a cynical psychiatrist colleague once whispered to me at a meeting of the Asthma Research Club, ' The effect of the seaside air seems to depend on the girl he takes with him, and the girl he leaves behind.'

Occasionally one gets a mid-week sequence of attacks ; if so, there is usually a human element to be detected.

(35) During the war I remember a case where a father brought his 11-year-old daughter to see me. He showed the usual symptoms of excessive parental anxiety and care by holding the girl's hand, smoothing her hair, smiling constantly at her, and she back at him. When I asked the daughter a question, the father invariably tried to answer it for her, until I protested that it was the patient's opinion that I wanted, and not his.

So marked was this solicitude and wish to interfere that I turned the child out of the room, according to my usual custom, in order to have a few words with the father about his detrimental behaviour.

After my short talk on the dangers of cosseting he said it was impossible that *he* could be taking too much care of his daughter because, owing to his war-work, he was only at home on Wednesday and Thursday nights.

In view of what I had just seen in the consulting room, I hazarded that those were precisely the times when Mary's asthma was at its worst.

He said with surprise, ' Why, *yes*, doctor ; but what an extraordinary thing! That is just what my wife tells me, now I come to think of it. She says that Mary only gets her asthma on Wednesday and Thursday nights when I am home, but naturally I didn't believe her. What ought I to do about it? '

Conclusions from the Foregoing Evidence

Suggestion, there is good evidence to think (pp. 137 to 139), can start ordinary attacks of some toxic idiopathy, e.g. hay-fever.

Emotional Shock can also start them, augment them, or cause them to recur after long periods of remission. This is found to have happened so frequently in the histories of our patients, and these shocks are of such varying kinds as to amount to the general rule that sudden emotion can promote attacks. The reader can, if necessary, read a much fuller list of ' shock starters ' on pp. 190 to 195.

Moods *sometimes* can be clearly demonstrated as affecting special cases for good or ill. With some cases indeed (e.g. with the boy in story No. 19, pp. 144 and 145) no other explanation seems possible but that a long continued state of disgruntlement and conturbation has adversely affected the patient and provoked chronic symptoms. But we can go much further than ' sometimes '.

Moods *always* affect the toxic idiopathies ; this is probable, and indeed almost deducible, because it can be shown that a changed outlook on life is so regularly followed by a change in the frequency and intensity of attacks.

In the three mass experiments of (*a*) Home *v.* Boarding School, (*b*) Home *v.* Hospital and (*c*) Home *v.* the Armed Forces, the trend of advantage for our patients is usually in being away from home influence. The effects of (*d*) Matrimony and (*e*) of the week-end ' rest ' were also given ; and, though the effect of matrimony is not so constant, in these two experiments also the results are much the same.

Once more, I am *not* saying ' Allergy is all nerves ' ; I am saying that emotions, moods or tensions are always part of the story. It is my thesis that not only do these emotional states always play a part, but that all the other causal elements enumerated in Chapter III (and others as well, no doubt) are in operation too. An allergic attack is the resultant of all causal factors and is not produced by only one —however important that one may be.

All the same, I ask the reader to accept the proposition that the mind, conscious or unconscious, is always more or less playing a part in making the attacks more likely or less likely to occur. Moods don't just do this ' sometimes '.

It follows that we can make no logical division of our cases into psychological or non-psychological types. There are cases where this emotional element is apparent to the observer, and also cases where it is not so apparent ; and that perhaps depends as much on the discernment of the observer as on the patient.

Potentially, therefore, this effect of emotion or of mood is always ' a handle manipulable for treatment ' (see p. 37) ; if we can only alter the mood, tension or emotion of our patient to his advantage we shall have a psychological treatment ready to our hands.

In casting back over the stories which I have used to illustrate this chapter I'd say that the 'shock starters ' are very varied in character ; most of them are accidents, and so bear little relation to the way of life of the patient.

The stories illustrating the effect of mood, on the other hand, have much to do with the patient's way of life, and of his upbringing (stories 19 to 35). Here the influence of the home crops up with monotonous regularity ; this was only to be expected, seeing that it is in the home that our characters chiefly develop, and especially for the first five years of life.

This is a burning question ; yet it is clearly of the utmost importance in any psychological treatment of our cases. It will be well to leave the nature of this home influence to be discussed in the next chapter.

CHAPTER X

HOME INFLUENCE

The last chapter showed clearly that, when a patient is suffering from any toxic idiopathy, this disorder is considerably influenced by his state of mind. Without my intending it, much of that chapter also amounted to an indictment of some influence derived from the patient's home. Naturally enough, that idea is disliked by the patients and their friends, and also frequently by their doctors. It is, therefore, as I have said, a burning question ; it had better be further investigated.

' Highly Strung.' When once it is admitted that the emotions, moods and tensions of the patient can be of great importance in his disease, it is especially acceptable to all concerned to put the blame on heredity. Mothers often say something like the following : ' Yes, my poor child has always been very difficult and highly strung ; I am afraid she has inherited her father's unfortunate temperament.'

No doubt a more mercurial temperament will have bigger ups and downs of mood ; but this ' highly strung ' character, when it really exists, is, I fancy, largely caused by the parents' injudicious handling of the child—and is not really congenital.

Often this high stringing is a mere maternal myth :

I well remember when I took my eldest boy to his Preparatory School, I was laughingly told by the Headmaster that there must be something queer about the child. Did I realise, he asked me, that this boy was the first entry to the school for over eight years who hadn't been labelled ' highly strung ' by his parents?

That school was none of your ' jolly little nurseries ' ; it had over a hundred boys in it, and was rather of the ' tough ' variety. Yet, if he spoke truthfully, every one of those boys had been labelled ' highly strung ' by one at least of his fond parents.

' Heredity.' We mustn't be too cavalier with the idea of a congenital idiotoxic temperament merely because many of our patients cling to it unduly.

We know that there is a marked hereditary distribution of all the toxic idiopathies. It is our ' Cause A ' for them (p. 38), and it is tempting to think that an asthmogenic temperament may be the inherited quality which determines the attacks. Facts, however, don't seem to support the idea.

Here, as usual, our prototype of hay-fever is convenient for the argument. Is hay-fever, which is certainly a hereditary disorder, accompanied by abnormal hereditary temperament? I'd say—certainly not ; the specialist psychiatrists—driven by their theories, sometimes say—yes (p. 208).

Because my treatment for hay-fever is mainly prophylactic I see my hay-fever cases for treatment usually in the off season when there is no pollen in the atmosphere ; thus several thousands of such cases are under observation from February until mid-May of every year. At that time they are as normal mentally as you or

I, though in the hay-fever season, i.e. from mid-May to mid-July, they often say they feel neurotic, ' highly strung ' or hysterical.

The Vicious Spiral. Emotion increases the tendency to hay-fever—as we saw in Chapter IX ; as opposed to this, a mental disturbance (occurring only when there is grass pollen in the atmosphere) was given in Chapter I as a notable symptom of hay-fever. The Tale of the Neurotic Doctor (p.10) is a good illustration.

The spiral clearly works this way : other causal factors being appropriately present, a little emotional disturbance may start an attack of a toxic idiopathy ; the attack itself causes emotion or even panic, and this will reinforce that attack ; the increase of symptoms, thus produced, causes still more emotion—and so onwards, till the body is responding as much as the causal machinery will permit at that time.

The potency of a vicious spiral can be shown by a simple illustration from mechanics. If the ear-piece of a telephone can be put against the mouth-piece, then the slightest noise occurring between the two (e.g. the click of contact) will travel out from that mouth-piece and come back to the ear-piece electrically reinforced ; this magnified click will start on its travels again via the mouth-piece, and will return again still more reinforced to the ear-piece—and so *ad infinitum*, round and round, gathering increased volume at each circuit, till the whole machinery is vibrating to its physical maximum. This may wreck the apparatus, or break the eardrums of an operator wearing ear-phones in the exchange ; that is why, of course, the detachable ear-piece is now out-moded.

I should suppose that a chronic illness must be due to a vicious circle, while an ' attack ' is due to a vicious spiral. We should try to substitute ' propitious ' circles in place of them.

To return to heredity, an inherited temperament no doubt plays a part in the life of a patient ; but, I should say, rather a minor part in most cases of the toxic idiopathies.

If all the stories given by way of illustration in Chapter IX are reconsidered, there is, I think, little or nothing to suggest a constant abnormal temperament in the patients ; most of the stories (if we exclude the shock stories) suggest trouble from the home environment.

' An Undetected Idiotoxin '. If it is the home that is found to be at fault, it may be urged that there is ' some allergy in the home '—as I have heard doctors put it ; they mean thereby that there may be some undetected idiotoxin in the house which is responsible for a considerable amount of the patient's trouble.

That is a possibility which must not be overlooked, of course ; but if such an idiotoxin is present, one generally gets wind of it while the patient and his escort are giving the case history. It should be tracked down while the patient is being skin-tested.

The ' undetected idiotoxin ' idea is, I find, chiefly paraded by people who have not sufficiently thought out and investigated the case in question, or are over-resistant to the ideas of psychological and bacterial causation.

' The Human Environment '. If it is *not* an undetected idiotoxin, what can it be that makes the home so obviously pernicious to so many of our patients? As

stated above, the case histories suggest over and over again that the trouble comes from the *human* environment.

' Allergic trouble ' from the human inmates of a home is not, however, the first idea that occurs to the parents ; if they can be convinced that the home surroundings *may* in any way be injurious to their children, they prefer to blame the nature of the surrounding soil, or the proximity of water—even the sea ; they distrust the altitude of the home, blaming either the ' bleak ' hilltop or the ' damp ' valley ; they are worried at the presence of trees near the home, or the absence of them ; they complain of the loneliness, or crowded nature, of the surroundings : I have heard them credit the trouble from their home to almost anything rather than the human environment.

A mother, in introducing an asthmatic little girl of five years, explained the cause of the trouble as follows :

' We moved from a small, but airy, house on the hilltop, to one right down in the valley below ; the lower house was more convenient certainly, but it seemed to me airless and there were mists in the valley sometimes. I am sure that was why Eva began to have her asthma in the new home. Oughtn't we to try and get back to the old house up on the hill again, doctor? '

Enquiries showed that Eva's asthma also coincided with the arrival of an unknown, but much talked about, Daddy returning from the wars. This excitement was much more likely, in my opinion, to be the home-derived cause than any amount of mist or fog.

Little girls soon adapt themselves, even to brand-new fathers, and that is apparently what Eva has done. After the first week in the new house she had had no further dyspnoea, in spite of the continued damp surroundings.

' Home Inmates '. It is an unpleasant shock to most people that a loving home can be emotionally harmful to our patients ; it was so to me, I remember. If such an idea is presented to the parents or friends of an asthmatic child, it is only natural that it should be hotly resented by them. Indeed, some doctors who send cases to the allergy clinics dislike the imputation and dispute its truth. In the introductory letter which they are asked to send with the patient they may try to steer me clear of it by writing something like this : ' The parents are most happily married, and I can assure you that it is a charming family ; there can be no question, therefore, of any of this psychology.'

It seems to these people that to call a loving home harmful is like saying that air is bad for birds or water for fishes : but nothing sickens fishes so readily as polluted water. If we insist, as we must, on the outstanding importance of the home atmosphere in moulding the character of the child, we must debit it with some of the failures in character as well as credit it with some of the successes. What upsets people so much is that the evidence from the case histories points so often towards loving care, maternal affection and careful supervision, as being responsible for the trouble in the child. If we knew how this comes about, knew where the domestic shoe so often pinches, we might be able to ease it.

Chapter IX cast suspicion only on the homes whence had come our toxic idiopathy cases. Do these homes differ in emotional tension from the normal homes? Not absolutely, of course, but on the whole I think they do. But let us first consider to what extent the average normal home *is* biologically normal nowadays.

' The Modern Home '. The thing most noticeable about the modern family is its small size ; and this, from the point of view of biological development, is of very recent date indeed. Metchnikoff, in speculative mood, used to tell us at the Pasteur Institute that the healthy human female living a quite natural life need never menstruate, but should produce her babies at the rate of one a year from the time of puberty till late middle age.

Without going all the way with Metchnikoff in that matter (and it would certainly not seem to fit in very easily with what we are pleased to call ' our way of life ') it must be conceded that quite recently the size of our families has shrunk out of all recognition. History tells us, for example, that the usual families of Queens in this and other lands were, by modern standards, enormous ; yet these ladies got about a bit, and many of them put through a surprising amount of work.

Many of their children died young, no doubt ; but the stronger survived, and there was always a far higher percentage of young people in the home (and indeed in the population) than can be found nowadays ; in those days the normal companions of a child in the home were the other children—*not* the parents.

Of course the recent war dislocation of normal family life is responsible for many of our present-day small families, but this cannot account entirely for the extraordinary prevalence of the Only Child in the Allergy Clinics. I'm not saying we should, or could, go back to the old conditions ; I'm merely indicating an outstanding difference in today's home environment from what was normal until quite recently. For good or ill, families are now unnaturally small.

The behaviour of the hen with only one chick is proverbial ; yet, when the special dangers which beset the only child are pointed out to the mother by the doctor, she usually says, I find, that her attendance on her only child takes up all her time, adding that she could not possibly manage to look after any more of them. I think that all this well-meant zeal for dancing attendance on their tiny families must be due in part to the incessant propaganda to which young mothers nowadays are being subjected from books, lectures, welfare centres, clinics, the wireless, and so forth ; according to some at least of these admonitory voices, the mothers cannot take too much trouble in brooding over their young : according to the evidence of the asthma clinics, they most certainly can overdo it.

There is another cause of this customary overwatching of the small family, and particularly of the only child. If there are many children in the family, the danger of losing one of them does not become such a paralysing obsession as the loss of an only child would be—and particularly if this singleton has been treated as an only child, and made into the universal household pet by elderly parents.

These admonitory voices of today would do better if they told the mothers to relax, to be less fussy with their children, and to interfere less ; in short, they should teach the mothers to wean their children emotionally, as well as from the breast.

I sometimes wonder if the best advice, from health centres and doctors alike, shouldn't be ' For Heaven's sake keep clear of us unless you really need us '.

I remember a distraught young mother of a quite healthy only child exclaiming, ' Thank goodness you've come, doctor! I've just found out that baby is a pound and a quarter heavier than this book says he should be for his age.'

After inspecting the book, the W.P.B. was recommended ; and the mother was advised to relax somewhat ; the child would grow naturally if only she could leave it alone.

The effect of the modern minute home also tends to change children. They are encouraged to think of the adults in their home as their proper companions : rather exacting and condescending at times, but still the only playfellows they are likely to get.

Children are plastic in their mental attitudes, and they soon learn that it pleases their grown-ups if they appear to be a shade more babyish and thoughtless than they really are : they become childish ' to taste ', while all the time these young opportunists resent this infantility which has been thrust upon them by their elders and are conturbed by it. Similarly, of course, children are ready to pick up any other undesirable characteristic that their grown-ups may be rash enough to suggest to them—sickliness, stupidity, cowardice, untruthfulness, and so forth.

So far I have only been discussing the modern home with healthy children, and I submit that even here there are often stresses and strains which are bad for both old and young.

I'd like to quote a passage from a non-medical writer which bears on the point. Mrs. Agatha Christie, who was speaking, of course, of normal healthy homes, makes one of her characters say :

' Naturally in the course of my work I have seen a good many aspects of the child-parent problem. Many children—most children I should say—suffer from over-attention on the part of their parents. There is too much love—too much watching over the child. It is uneasily conscious of this brooding and seeks to free itself, to get away and be unobserved. With an only child that is particularly the case, and of course mothers are the worst offenders. The best thing for a child, I am convinced, is to have what I should term healthy neglect on the part of both its parents. This happens naturally enough in the case of a large family and very little money. They are overlooked because the mother has literally no time to occupy herself with them. They realise quite well that she is fond of them, but they are not worried by too many manifestations of the fact.'

Mrs. Christie boldly stigmatises an excess of ' love ' as a prominent harmful factor in the modern home ; and I must say that love does seem to be at once that quality which my patients and their relatives are most pleased about, and also the quality which often seems to the outside observer to have most run to seed in them.

Doubtless we can have too much as well as too little of a good thing ; but, apart from that, ' love ' is one of the most ambiguous words in the language, with the added disadvantage that most people think they know exactly what everyone must mean by the word.

The *O.E.D.* gives sixteen different meanings for ' love ', with many more sub-meanings, in a full nine of its long columns ; a couple of illustrations, however, will serve to show the widely different ways in which the word may be used unwittingly. I remember hearing a young woman say ' I love whitebait, and simply *adore* oysters ! ' She would probably think she was using the word in the same sense when she said she loved to hear birds singing on the bough, or to see children playing happily in the park. Parents, conscious of their excellent intentions, think of their love for their children as being exclusively in *O.E.D.*'s first meaning, which is

' solicitude for a thing or person '. The more thoughtful of the children may wonder sometimes if they are not cast for the role of whitebait or oyster. There is love and love.

Most of us can call to mind an example of an otherwise normal house where the misguided inmates seem to think that they can't have too much of such a good thing as mutual love and affection. If things go wrong, and apparently go wrong because the home *is* ultra-loving, we may speculate whether the love in that home really was the best type of those sixteen varieties given in the *O.E.D.*

I remember one such home : two parents, and two ' sweet ' little girls. It was a perfect love-nest, as people frequently remarked who saw it.

When the two children had grown older they still remained very sweet, but the emotional atmosphere was found by many outsiders to be a little cloying ; each of the four habitually addressed the others as ' dearest ' and ' darling one ' ; perpetually, at least when on view, they were asking for, and getting, expressions of emotion. There were frequent little caressing actions and shoulder pattings. It struck me at the time as being a sort of emotional masturbation—a continual goading of one another to more and more demonstrations of love.

Both these charming young women made a ' good ' marriage, and each with a pleasant, capable young man ; both young husbands could not stand the pace, and left their wives within the year. That little family of four was filled with surprise and consternation at what had happened.

The father subsequently committed suicide.

This was for us a ' normal ' family, i.e. exempt from the toxic idiopathies ; they were all healthy, intelligent people, so far as one could judge. There was, therefore, no asthma, etc., to be derived from this terribly-strained atmosphere of affection, for, of course, an unhealthy home influence cannot by itself produce any toxic idiopathy unless the other causal factors are present : ' nerves ' cannot do it alone.

If there had been any idiotoxic tendency in that home, such as eczema or genuine migraine ; if the skin of one of the four had yielded a positive wealing to such things as grass pollen or cat scurf, then we should have had, I fully expect, some toxic idiopathy brought out by that home atmosphere.

The Asthmogenic Family. Turning now from these reputedly healthy homes, i.e. these homes showing no toxic idiopathies in them, to the homes whence come the cases which flood our allergy clinics, I think that these asthmogenic homes do, on the whole, show a greater degree of emotionalism and nervous tension. This is obviously not a constant rule (witness the story of the ultra-affectionate, yet reputedly ' healthy ' home just related), but in the main it usually happens that way.

I think we can say, as indeed Chapter IX has implied, that if in a home we get constant attacks of some toxic idiopathy which goes when the sufferer gets away from home, if the trouble is regularly accentuated at the week-ends, or on returning home after absence, then we must suspect that the emotional tensions are wrong in that home. Thus an asthmogenic home should indicate to us the probability of unhealthy tensions therein, and we should do our best to get those tensions relaxed —or allow our patients to escape from them.

Asthma or any of the toxic idiopathies thus become, so to speak, our piece of litmus paper from which we may be led to suspect an abnormally strained atmosphere in the home. Even asthma may be useful to sociologists.

We find evidence of this excessive nervous tension by means of :

(i) Going to patients' houses and seeing the actual emotional conditions there on the spot.
(ii) The patients and their friends readily tell us what goes on in their homes because they are genuinely proud of their excessive loving care, and will even brag to us about it.
(iii) We can observe for ourselves in our consulting and waiting rooms how the patient and his escort behave to each other. This is the most valuable, for it applies to every case we see.
(iv) Independent observers, e.g. school teachers, tell us of the parent-and-child relationship.

(i) **The Visited Home.** Obviously very bad human atmospheres are described in many of the stories in Chapter IX. Other ' allergic ' homes I have visited nearly always show the same kind of thing, though perhaps less obtrusively. In practically all of them there is great over-emphasis of the illness of the patient ; there is a feeling of panic in the air. Very seldom indeed is there any suggestion of that calm commonsense such as is found in the properly run wards of a hospital. Frequently, also, there is an excessive number of relatives and neighbours hanging around, all wishing to give their views on past treatment and the possibility of a cure in the future.

It may be urged that, from the nature of his work, a medical man must frequently be walking into a home in a state of crisis and turmoil ; but far too often the panic and emotionalism round the asthmatic patient is wildly excessive.

(ii) **The Reported Home.** The hysterical behaviour I have seen in the houses of idiotoxic patients is more than corroborated by what the patients and their friends often tell me in the consulting room : as suggested above, they seem anxious to relate how foolish they can be at home.

The Nightly Panic. They often describe, with a mixture of complacency and desperation, how the whole family (what there is of it) has been up night after night with the sufferer, ' fighting ' the asthma. The harassed G.P. may tell you privately that they have sometimes called him up several times in the night ; he may add wistfully that, if perhaps the patient could only be taken into a hospital, he himself might get a decent night's rest. And these are those very patients who, as described in Chapter IX, usually lose their symptoms on being treated with ' calculated neglect ' in hospital by nursing staff and doctors.

Night is the time for panic : unfortunately also, owing to the prone position in bed, it is the likely time for asthma to start (p. 123 and 124). From 2 a.m. till 4 a.m. is the usual time for these nightly attacks ; also a very usual time for children is at or just before midnight, because children go to bed earlier. Attacks often come on, too, with all ages when rousing in the early morning. On the whole, however, the ' middle of the night ' is the most constant time for the regular attack ;

that is the time when the patient and his friends are, or should be, all in bed and asleep.

The lengths to which people go in this nightly cosseting of asthmatics are surprising. From what they tell me, they wander about fetching glasses of water, brewing unnecessary cups of tea, holding hands, patting or moving pillows, putting kaolin or other poultices on the patient's chest, arguing whether they shall ring up the doctor yet, making the wretched patient say at intervals how he is feeling. If the patients themselves are asked if they like all this, they frequently say (and this is especially true of the children) that all *they* want is to be left alone so that they can get some sleep. If, as often happens, two or three people insist on being up all night holding hysterical little colloquies within the hearing of the patient, it is small wonder that patients generally feel less worried in hospital.

The same is true, but I think to a less extent, with grown-up or elderly patients : wives agonise over the supposed need for looking after their husbands all night, and vice versa ; grown-up daughters are ' up all night ' looking after their aged parents ; grandmothers strive to relieve the sufferings of their grown-up sons and daughters. They all say, 'We cannot let them suffer alone ; it wouldn't be human nature.'

Though the friends of the patients are thus, as they claim, battling every night to help the sufferers ' fight ' their trouble, one has the suspicion that they are often doing little else but relieving their own feelings and consciences. It seems a hard thing to say, but the nightly panic may easily become a queer form of self-indulgence on the part of the parents, though they would, of course, hotly deny any such thing.

I find that if you ask a young patient point blank : ' Who is the more concerned about the illness—you or your friends? ' you are likely to be told that it is most certainly the friends. A boy may say : ' Oh, *I'm* not worried at night at all, it is only that mother minds.' Often patients old and young declare with some heat that they don't want people messing about in their rooms at night. The parents are usually indignant at their children's ' ingratitude ' after all this extra care has been lavished on them.

It may seem strange that behaviour, which is to an outsider little more than tiresome fussing and interference, should be so very disconcerting to the person who is called on to suffer it ; most of us know, however, that persistent but unwanted loving attention can be excruciating by producing a mood of helplessness and futility.

On hearing that a mother was up nursing her son night after night, I asked the ten-year-old asthmatic boy if he liked having his mother in his room all night. He grinned broadly, but uneasily, as if at the queer idea of asking *him* any questions while his voluble mother was present ; then, after a long pause, and after looking at her apprehensively, he said explosively : ' No, I keep telling her to go back to her room, but she won't! ' The mother then indignantly interrupted with the statement that she *had* gone out of the room at his request ; whereat the boy rejoined : ' Yes, you *did* ; but you coughed and scuffled outside the door to let me know that you were still there! '

This was admitted by the mother, who said she couldn't bear to let her child suffer alone. ' Not even if he wants you to? ' ' Oh, I couldn't just go quietly back to bed again, could I doctor? It wouldn't be natural! '

This annoyance at such excessive parental attention at night is perhaps the normal response of healthy-minded children ; but they can soon start taking ad-

vantage of it, and so get spoilt. Sometimes they are really timid and want to have some grown-up, or a much older child, in the room with them ; but not nearly so often as the parents think. Weak-minded patients in time become spoilt or hypochondriacal, and take pleasure in their power of demanding sympathy ; but this seems chiefly to be a grown-up vice.

I have known women in hospital to exaggerate quite unmistakably some slight dyspnoea and demand to be put on the danger-list so that they may make their husbands come and see them. ' It will do the lazy man good ; he doesn't take half enough care of me ' I have heard a possessive woman say. The same thing happens at home of course, but such demands are more often resisted by the relatives when coming from the grown-up patients.

' It is no good your telling me, doctor, not to make my husband's asthma a regular breakfast-table topic. To tell the truth, I am by now heartily sick of it all ; but if I *didn't* keep on asking how he was whenever I see him, he'd say I was cruel and unkind. He says that even now! '

' He'd like me to stop up all night with him, if I would ; but I've got our living to earn, and the children to look after. So I don't stay up, especially as there is nothing to be done.'

Sleeping Arrangements. Parents tell me that at night they often tiptoe into their children's rooms to see if asthma is or is not developing ; they will even admit that they may creep into their children's bedrooms off and on through the night to see if the child is still breathing, or has quietly died in the night. On the other hand, if the child is breathing heavily, they may wake it up because they don't quite like the sound of the respirations : these over-anxious mothers and fathers think their children will not survive if they are not always interfering with them ; they oscillate between wanting to hear that the child *is* breathing and thinking that they hear too much of it.

Parents with these excessive yearning proclivities often make the asthmatic child sleep in their rooms ; or perhaps one of the parents will sleep in the child's room. In this way, the continual brooding care can be kept up more conveniently with less physical fatigue. The unconventional lengths to which the parents or relatives may go to keep up this constant surveillance give some evidence of the state of fluster and anxiety that they may get into. Mothers will sometimes take their quite big sons into their own beds so that they can brood over them more closely.

I remember the case of a well-grown boy of fifteen for whose sake the father was regularly turned out of the mother's bed whenever a little asthma showed in the son. The mother was naturally a little bashful about giving me details, but readily did so on being told that it was important for the boy that we should know. Presumably the boy got quite a ' kick ' out of having the old man turned out of bed for him ; but, from the behaviour of the mother and son in my consulting room, I am convinced that was all.

Unfortunately, as the mother complained, this treatment did *not* stop the boy's asthma.

It was almost comic to see the sharp emotional reaction on the part of the G.P. when I told him the details of the family's nocturnal habits, which were, however, no more than an outstanding example of pathogenically excessive cosseting.

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in both parents and children on these occasions. There seems (to the non-psychologist at least) to be no evidence of anything more abnormal than extreme sloppy-mindedness on the part of the parents, though of course this has led to gross and harmful mismanagement of the child.

Naturally these foolish mothers know that their behaviour is unusual, but their main idea seems to be to cherish the child ; they have unwisely, and perhaps unwittingly, allowed this abnormal bedroom practice to go on till long past the babyhood of the baby.

The war, and expecially the blitz, accounted perhaps for many of these unorthodox sleeping arrangements, which were, however, kept up by the not unwilling parents till long after the war made them necessary if they had the excuse of watching over an asthmatic child.

A family of two parents and two sons (aged 8 and 12) were bombed out of their old house and forced to sleep in an underground tube station.

When the raids stopped, they left the tube shelter, but had to make shift in one room only, and all four slept in one bed at night. When a smallish second room could be added to their menage, the older boy of twelve years was put into it, but the younger boy still slept in the parents' bed in the bigger room.

The mother at the first visit to the clinic explained (and got away with it) that the exiling of the twelve-year-old boy to a cold new room all by himself had caused a sharp increase of the boy's previously slight asthma. ' That was why he has been brought to the clinic, doctor.'

At a second visit, careful questioning disclosed that this elder boy had cried on the first night of his exclusion from the family bed ; so ' to make up to him for this, and to be able to watch him more carefully ' the mother had joined the boy on the second night in the new small room, leaving the small brother with his father in the big bed as previously. The mother reluctantly admitted to me that it was only *after* she had joined him in the little room that the asthma had returned to the elder boy ; one has frequently to be a shade sceptical of the home-made theories of causation advanced by parents.

I suggested that perhaps a more orthodox sleeping arrangement would be for the two parents to share the bigger room and the two boys to share the smaller. This little change was apparently enough to stop the twelve-year-old's asthma.

Here again it is merely a matter of a fond mother who had allowed her maternal instincts to run away with her to the detriment of her young. It was quite clear from the mother's demeanour, when I questioned her, that in her view there could not be too much display of maternal solicitude to help her child ' fight ' his asthma ; also I am sure that she had not fully realised that her family was growing up.

Emotional Infantilism. I think these queer sleeping-arrangement stories, and one gets plenty of them, may very possibly give us a clue to part of the abnormal mental attitude of the anxious relative towards his or her idiotoxic dependant. The instinct of most parents, and especially of the mothers, is to go on thinking that their children are far more babyish than they really are. I once heard an excellent mother declare that the greatest sorrow of her life was when her babies ceased to be babies, and therefore no longer entirely dependent upon her.

Foolish parents often collect and chuckle over the childish errors of their sons and daughters as avidly as the weaker schoolmasters collect ' howlers ' from their pupils ; and both parents and schoolmasters tend to invent these ' howlers ', or at

least to improve on them in the re-telling. I think this is very largely due to the wish of the adult, but perhaps second-rate, mind to go on feeling itself superior to the immature, but possibly finer, intelligence of children. Wives like to tell their cronies that their middle-aged husbands are just like great big boys ; and, of course, husbands counter with ' silly little women '.

Parents sometimes (*usually*, I suspect) do not like the idea that their children are growing up, and especially that their brains have grown up. I have often been amused in the consulting room at the naive delight of the father at finding that his child can't do, or doesn't know, something which he, the father, happens to know and understand. The children in their turn often take the line of least resistance, that is to say they are generally ready to oblige by being extra childish at home to satisfy the parents' wishes ; thus we get an affectionate infantilism to suit the taste of the parents. A good example illustrating this amiable adaptability on the part of children is the story of the boy trying to measure out doses of vaccine accurately in the teeth of parental interference (pp. 90 and 91).

This foible of thinking that their children are younger than they really are goes on all through life. Parents aged sixty or seventy, with middle-aged ' children ' still living in the old home, will *themselves* try to make the appointment with the doctor ; they will want to follow these elderly children into the consulting room, and often try to answer all the questions put to the patient. These brow-beaten adults pathetically put up with this treatment in a dazed kind of way. It is distressing to see a hulking man of twenty-five, sitting looking down at his hands in his lap while his relatives sit around talking about him, or to see him glancing sideways at his mother before he dare open his mouth to answer a question. If it is possible to interview these grown-up children alone they frequently complain that their parents still seem to think that they are babies, but one finds that they lack determination and guts to assert themselves effectively.

What the parents often fail to realise is that the child's thinking power grows up far quicker than his body does, and far quicker than his knowledge and experience, which will accumulate all through life. The more foolish parents, by confusing thinking-power with experience, badly underestimate the rate of growth of the minds of their children, especially, I should say, at the ages of six, seven or eight.

It was an eye-opener to me to see how quickly and surely children can master the rationale of self-inoculation in the P.T.D. course (p. 90) as compared with their parents, when both generations start from a base-line of complete ignorance of the business.

(iii) **Consulting Room Behaviour** of the patient and friends may be the only indication the doctor can get of emotional stresses and strains in the home. If the G.P. is to be helped on this so very important side of his profession, it must be largely by observing what goes on in the consulting room.

Perhaps I should here emphasise once more that, between their characteristic attacks, the patients suffering from toxic idiopathy are normal people. This is not true of course with chronic diseases like eczema, but in the main it is true. For

example, perhaps 95% of asthmatics coming for consultation are more or less normal at the time of their visit : probably nothing will be detected on auscultation, or not enough to be troubling the patient in any way ; there will be no dyspnoea. We might, therefore, expect the patient to show his usual and normal behaviour.

The Parents' View-point. The impression gained in the consulting room of an overweening emotional dominance imposed by the anxious yet grasping parents on their pliant dependants is hard to put into words ; so also is the childishness adopted by the dependant to meet this demand. Neither party is aware of any insincerity : both are being ' good '.

I chanced to find in a U.S.A. newspaper advertisement an excellent example of how pathologically silly parents can be supposed to be towards their young. Admittedly it was fiction ; but it was in a straightforward advertisement of a fountain-pen, and presumably the script writer would set down nothing which in his opinion was likely to annoy or disgust possible purchasers of that pen.

The picture (above the text which I am about to quote) first caught my eye as being of a very familiar scene ; a round-faced characterless youth of perhaps 16 years held the marvellous pen in his hand, but he was looking upwards with a far-away soulful look in his face ; beside him was an amused and possibly cynical father registering pride ; to the boy's left rear was the yearning young mother clearly making the well-known emotional appeal. That parental demand for emotion and the youth's response to it was no doubt what seemed so familiar to me.

Underneath was printed what was apparently an extract from a letter written by the mother when describing the incident to a friend ; the text ran thus :

' Mixed feelings of pride—love—and fear gripped my heart. Must Tommy's graduation be so wonderfully complete, and yet so cruel? Had our lovable little boy suddenly outgrown his boyishness? Would his mother's tender kisses be set aside for the false pride of manhood?

Disturbing thoughts I might well have spared myself. For when Dad and I gave him his graduation gift—a marvellous new ..... fountain-pen—I saw that Tommy was still our " boy ". There were in his eyes words of love and appreciation that made our hearts sing with joy ', etc.

According to the script writer, they had very efficiently solidified their emotional dominance, and the boy's infantilism, by a bribe of this wonderful fountain-pen. ' The false pride of manhood '—what do American he-men say to that? I have, as a fact, never met any mother quite so silly as that—or at least, not so open in admitting it.

The Patient, if he is well brought up, would naturally sit sedately in the chair indicated by the doctor, quietly interested in the proceedings, and ready to answer questions as accurately as possible. Often, of course, the boys and girls are very co-operative, and help in every way they can ; and indeed it is often easier to get useful facts from them than from their parents. Sometimes, however, they don't help at all ; at all ages they may sit huddled up in their chair, looking like lumps of misery, and glancing repeatedly at their parents or other escort. As the mother and father compete in telling the tale of woe, the patient seems to shrink, and try

to live up to the Dismal Desmond character that he has been accustomed to hear ascribed to him at home by his very worried parents.

This role of extreme dejection is clearly very superficial in the young patient, and, I think, is chiefly assumed to placate the parents. If the children are suddenly asked, but in a friendly way, whether they like drinking milk or eating eggs, or whether they like playing with the other children at school, they usually drop at once their harried care-worn expressions and answer cheerfully and interestedly. When the parents' tale of misery has at length run dry, and the small patient looks as if he could never smile again, if you then turn to the child and say briskly but cheerfully, ' What a darned little nuisance you must have been to everybody! ' at once a delighted grin breaks over the doleful face, and the child relaxes and looks quite happy and human. I think I can say I have never known that experiment to fail—either in hospital or in private practice ; children don't mind being considered to be nuisances—it is their prerogative. If they are described as Little Miseries, then they are annoyed, but dutifully proceed to look and feel miserable to order.

Alternatively the patient may not seem overwhelmed by his misfortunes as detailed by his parents, but may over-act a rebellious indifference towards his escort by sitting starfished in a big armchair—legs straight out and wide apart, head lolling back, and arms stretched out and hanging over the arms of his chair. Such children often interrupt by talking irrelevances in a loud tone of voice to show they are not interested. The escorts seldom seem to resent this, but continue to watch their Infant Phenomenon go through his tricks with a mixture of pride and apprehension, while relating to the doctor all the terrible difficulties and symptoms that occur at home.

This showing-off, this play-acting of off-handedness, is not confined to the young ; I have known grown-up patients to sit in a dégagé attitude with arms outflung, and both knees crooked over one arm of the chair, like a ham film actor registering gentlemanly unconcern.

This unconcern of young or old may also be very superficial. Small boys who have been showing-off badly may crumple up at a suggestion of a swabbing or venepuncture, especially if the escort unwisely tells them that it won't hurt, or begs them to be ' brave '. The ensuing tears and struggles of the child are then usually met by cooing noises and unnecessary protestations of love from their grown-ups ; but if these ' terrified ' children are sent back to the waiting hall (while I have a private word with their parents) I find they quickly regain their previous nonchalance, and the spoilt child becomes the life and soul of the waiting room party—or so my receptionist tells me.

Sometimes the children are really terrified of the doctor and are quite incapable of co-operation ; they have usually been frightened beforehand, and the bribes or threats of the escort naturally do more harm than good. Such children are greatly helped if they can be seen together with other children with steadier nerves, as can usually be arranged in the hospital clinics. It is a good plan, sometimes, to call up a smiling child or two to explain to the frightened one that nothing very terrible is going to happen to him.

The Escort of friends and relatives is usually excessive in numbers: both parents may accompany a small child, the father often having to take a whole day off work to do this. I have known as many as six grown-ups wishing to crowd into the consulting room with one little girl or boy patient. When, as I frequently do at the end of a consultation, I ask my receptionist to remove, and look after, a small child while I have a talk with the parents, I may be told that there are also in the waiting room perhaps a grandmother, a nurse, an aunt or two and maybe a neighbour—all waiting to entertain and protect the urchin when he re-appears. It is indeed a family affair, which I take to signify that the patient and his symptoms are *the* burning topic of interest in *that* home.

Escorts are too often fidgety and interfering, and seem to find it difficult to leave the patient alone; they are determined to act the part of a nurse to an incapacitated sufferer (who is perfectly well at the moment, however). They insist on taking caps, mufflers, coats, gloves, etc., from patients quite able to look after themselves, or help them to put them on again, and often in spite of my protests. They help the boys and girls of over seven years to take their seats, lifting hefty children off the floor for this purpose. I have often seen mothers holding the hands of boys of eleven or so, apparently lest their knees should give way on crossing the consulting room, when clearly these children were much more nimble on their feet than their parents could ever hope to be again.

Sometimes one of the escorts will pull her (or his) chair up till it almost touches the chair of the patient, or may even want to sit on the arm of that chair to yearn over the child in what seems to be almost a burlesque of affection. When they try to pat the knee or shoulder of the patient, or smooth the hair down in a proprietorial way, I am sometimes reminded of the stage ventriloquist and his doll—except that the patient's escort usually want to do *all* the talking.

In several of the illustrative stories I have told of the constant effort made by parents to answer the questions I put to their children; they seem to think that a child should not be asked anything at all when there is a grown-up present who can answer for it. Parents even keep up this practice until their ' child ' is quite grown-up as in the story on p. 147: I usually explain that it is the *patient's* point of view that I want and not (for the moment) the view of their parents or friends.

If the mothers and fathers can refrain from doing all the answering, they generally think it helps if they echo persistently all my questions to the children—as if the boys and girls could not be expected to understand me unless my remarks were lovingly translated to them.

This often leads to a dialogue conducted more or less as follows:

J.F. (reading from case card, to small boy)—' William Thomas Smith: what do you like to be called at home? '

Mother (interrupting)—' Tell the doctor, darling, what we all call you at home.'

Boy (very promptly)—' Tommy.'

J.F.—' How old are you, Tommy? '

Mother—' Now tell the doctor, dear, how old you are. You *do* know how old you are, don't you, darling? '

Boy (as soon as he can possibly get a word in)—' Seven last birthday.'

J.F.—' When, by the way, *is* your birthday? '

Mother—' Tell the doctor, dear, what day your birthday is. It is always when you have that grand party, you know! '

Boy—' Seventeenth of June.'

J.F.—' Do you like school? '

Mother—' The doctor is asking you, darling, if you like your school. You know you do, don't you, dear? '

Boy—' Yes, I like my present school fairly well ; but I'm glad I'm going to a bigger school to be with older boys next term.'

Mother (gushingly)—' He's getting *such* a big boy now ; aren't you, darling! '

(and so forth endlessly)

I wrote that particular dialogue down as soon as the mother and son had gone ; it is a sample of many such. One can describe that woman as ' maddening ', and leave it at that ; but imagine what that maddening behaviour means to an intelligent and defenceless child. It may seem to us to need very little self-restraint from the boy to tolerate it with a good grace ; but if he is an only child, he has got to endure it day-in, day-out, week-in, week-out—apparently (to a child) to all eternity —except when he escapes to school. If there were several children in the family they could discuss their mother's foibles together, and even chaff her gently about them ; the child in a minute family has not this backing of youthful public opinion, so he either loses his temper and becomes rude and angry, or, perhaps more often and especially with the only child, takes refuge in infantilism and becomes the little family pet. The choice is between appeasement or fighting, but the effect on the home atmosphere and the child's health is bad either way : he becomes conturbed.

Baby Talk. When the escorts speak to the patients they frequently talk down to them to an infuriating degree as in the above dialogue. It is noticeable that parents tend to use baby words like ' pussie ' or ' tiny doggie ' to their children already at school ; and quite intelligent children often will reply in the same terms. Sometimes parents go as far as to use the nursery third person in sentences like this : ' Mummy wants Dickey to help the kind doctor. Dickey must try hard to be good, or he'll make Mummy cry.' ' Won't Tommy sit quiet as a little mouse to please Mummy? ' That kind of thing, when addressed to a boy already expected to have some knowledge of Latin grammar, might seem to be particularly galling, but for the fact that the wretched boy is evidently quite used to it.

This ' affectionate and intimate ' language is often, but I am sure mistakenly, supposed to have a peculiarly soothing effect on the child or grown-up patient. It is, on the contrary, the natural lingo of inane emotion to be used rightly when inanity is in place. I quote from memory the conversation of a newly married couple as reported in *Punch* : ' Who's plump ickle partridge is 'ou? ' ' 'Ors!!—Hubby's! '

This emotional infantilism may account for the use in the U.S.A. of the word ' Baby ' to signify a nubile young woman who has aroused sexual interest.

Clearly if baby talk is an asthmogenic factor it should be corrected at source. Psychiatrists tell me that it denotes the mother's ' difficulties ' (e.g. an unhappy marriage) ; it seems simpler to suppose that through vanity she has forgotten that her child (and herself) are growing older.

Unwanted Help. When socially ill at ease a child is naturally pleased to be asked to do some simple act well within its capacity ; grown-ups find this helps them too, e.g. the lighting of a cigarette when introduced to a stranger. If for diplomatic reasons I ask the patient to do something, if I ask a small boy (on the verge of tears from a throat swabbing) to pour himself out a glass of water to sip, the parents will almost collide with each other in their efforts to help the child unnecessarily. When a patient is asked to put a bag or coat on a table, there is often the same scramble to relieve him of this exertion.

The doors of my hospital consulting room have lever handles in place of door knobs, and children of two or three years find to their delight that they can open the doors quite easily. If I ask an infant to go and open the door, the chances are that its mother will sweep the child aside and open it herself—thereby spoiling the child's day.

When parents thus usurp a child's natural right to do things for itself in the consulting room, this clearly suggests that they are likely to do so also at home. On questioning, you usually find that it is so ; children are not allowed to bath themselves ; children are not allowed to dress themselves ; I have even heard a mother of a well-grown boy of almost eight boasting that he cannot do up his boot-laces—she has to do them up for him.

In my children's clinic the other day I suggested to an over-anxious mother that she might let her only son of eight bath himself with the very minimum of maternal supervision.

A visiting doctor who was sitting with me told me privately that he had just found a mother who was insisting on bathing her only son of seventeen years because he wouldn't do it properly for himself. She had done it for him all his life.

I think we have a strong suggestion here of that love of the grown-ups for demonstrating their still retained superiority over their children, i.e. there is a certain amount of jealousy and vanity behind this business of not allowing children to exercise their faculties to the full.

All this is very conturbing to an active child, or alternatively convinces a slow-witted child of its own incompetence. When reporting to the doctor-in-charge that there is apparently an unhealthy emotional tension in the home as judged by the behaviour of the patient and escort in the consulting room, you frequently get confirmatory letters like this :

' Your suspicions were confirmed as to " tension " at home. The mother has been so much afraid of upsetting the boy's nerves that she has humoured him in every way—even to the extent of dressing him, as if he were a baby, and he is *thirteen years old*! '

Dignity. For all their solicitude, parents are often neglectful of their children's dignity and self-possession. Healthy children, and boys particularly, object to all this overhandling, and try to edge away from their parents who are trying to make them sit almost in their pockets. They will say : ' Oh, don't, mother! ' or ' Do leave me alone ' when their hair is being patted or their tie is being straightened in public. No doubt a ' good ' child should conquer its resentment, and be tolerant of parental foibles ; my point is that it perturbs the children, and puts an added strain on their tempers—thus introducing an asthmogenic frame of mind.

Inappropriate Dress. Boys certainly resent being dressed in a feminine or inappropriate way. I have not noticed that girls resent being dressed like boys, but they don't like unbecoming or too childish dresses.

To please the mothers, the boy's hair is often kept too long ; a girl's hair-slide is then used to keep the hair out of the boy's eyes. If he is asked if he likes this effeminate device, the mother is frequently startled at the eruption of hitherto only smouldering wrath. On the other hand, these sartorial assaults on masculine dignity are sometimes received in silence and with sullen resentment by the child.

I will quote an extreme case. A five-year-old asthma patient had on its case-card the epicene Christian name of ' Evelyn '. The child sat stolid in its mother's lap without uttering a word—no matter what questions I put to it ; it was apparently both sullen and stupid. Evelyn might have been a deaf-mute for all I could tell.

The child was elaborately turned out. There was no hat, but the long well-brushed hair was parted in the mid-line, and the locks on either side were kept in place with tortoiseshell hair-combs. The chief visible garment was a long-skirted, much trimmed and braided overcoat, which reached the bare knees. The shoulders were covered with a filmy lace collar, and the child wore strapped baby-shoes with white cotton socks.

I naturally spoke of Evelyn as ' she ' ; the mother did not correct me, but I noticed that she was careful to avoid all personal pronouns which denoted sex. So I called a woman assistant, and asked her if my patient was a boy or a girl ; I was told with much scorn for male ignorance : ' A girl, of course.' When I asked the mother, she admitted that Evelyn *was* a boy ; when challenged about it, she pulled up the overcoat to disclose a pair of vestigial trousers which were about two and a half inches long in the leg. She told me that when she was pregnant she had wanted a daughter, and so she tried to make a girl of him.

She was told to abandon all this pseudo-femininity and to have the boy's hair cut short ; she was to bring him back to me in a week, wearing uncompromisingly masculine clothes.

That mother did the job handsomely. Next week the door was banged open, and a sailor-suited boy dashed shouting into my consulting room ; he had his arms up in the air, and I thought at first he was going to turn a catherine-wheel for joy. He needed no further treatment from me.

Little boys are sensitive about anything they consider effeminate, as the last story shows ; but this often takes an unexpected form :

At the height of the doodle-bug bombardment, a mother brought her six-year-old asthmatic son to see me ; amongst other things, he wouldn't drink his milk. If she forced him to take it there was a scene, and the little boy got asthma forthwith.

On testing his skin I found that he was not allergically sensitive to milk proteins ; so I asked him why he wouldn't drink it. He was shy of answering, but at length he whispered to me that it was ' sissy stuff '!

On being told that his hero Monty drank it regularly, he consented to try again. He then found that he could take his milk ration without asthma or any other discomfort.

Children often have strong preferences about their names ; and if there is any doubt about it, it is helpful to ask them what name they really like to be called by : e.g. ' Sarah ' or ' Sally ', ' Richard ' or ' Dick ', and so forth ; their very definite and heart-felt preferences sometimes startle their parents.

A sturdy red-headed youngster of five and a half years had begun his asthma when he was deserted by his mother who had bolted with the lodger—or so his much too gushing aunt informed me at considerable length.

His names were down on the case-card as ' John William ' ; so, according to custom, I asked which of his two names he preferred. Before the child could speak his aunt answered for him that he was always called ' Jackie-boy '.

As the boy remained silent with tightly shut mouth, I motioned his aunt to be quiet for a minute and asked again what name he really *did* like ; he told me uncompromisingly that he really wanted to be called ' John '. ' Oh, darling,' said the much distressed aunt, ' but you know you are always called " Jackie ".' ' I like John ', said the boy, and shut his mouth like a rat-trap and stuck out his chin.

In view of the emotional onset of his asthma, I thought he was perhaps entitled to the more adult ' John ' ; besides, at his age, I had been annoyed by that detestable ' Jackie ' myself. So I advised the change.

Small beer perhaps for grown-ups ; but these little annoyances and twists in emotional tension loom larger for small children.

I have often thought that a flaw in the emotional atmosphere of a home is shown when the child vehemently prefers its other christian name—the name its parents never use. This difference of opinion as between parents and child usually takes the mother and father by surprise and they feel distressed. The parents usually prefer the more fancy or sentimental name, and the child the more ordinary.

Of course a sportive child may be only ' having a game ' with the doctor ; but, I am sure, not always. I think that for both parents and child the fancy name may become symbolical of ' the little pet ', whereas the plainer name suggests self-respect and independence to the child. The concluding paragraphs of a report sent to a doctor about a little asthmatic girl of five will show better what I mean.

' Finally, of course, there is the emotional aspect of this case, and you are without doubt quite right about the nervous tension in that home. The mother seemed to be rather pleased about a " nervous breakdown " she had had, and she declared that the father was far worse with his nerves than she was. I should think that until we can get a more matter-of-fact home environment for that child you will have great difficulty in getting her clear of the asthma.

' You may regard it as insignificant, but I was interested to find that the mother always calls the little girl by the somewhat sentimental name of " Shirley " whereas the child much prefers the more ordinary name of " Jane ". I find that where you have a maladjusted atmosphere in the home there is frequently a divergence of opinions about the child's names. I have sometimes even suggested that they should mark the change in their behaviour towards the child by adopting the other christian name. I don't know if that would be a good thing with this case. I leave it to you.'

Parents begin by being unnecessarily fussy and interfering in a loving way ; they may soon become tyrannical if the child is too tolerant—though doubtless with the child's best interests at heart.

Here is a story of a mother who was not only over-anxious, but also very rude to her big son :

I remember a father and mother bringing to me a pleasant, well-grown public-school boy of fourteen. He looked, as most of the children do look, in the picture of health, for he was not at that moment having an attack of course.

The mother was of a commanding personality ; the father seemed not so significant, and I soon forgot that he was in the room. The mother kept on interfering with her son, answering for him most of the questions I put to him ; when the boy managed to speak at all, she seemed to query all his statements as a matter of course. This apparently had become an ingrained habit with her.

She was so bossy and interfering that I determined to speak to her about it; so I asked the boy to go back quickly to the waiting hall as I wanted to have a word with his mother.

The boy had his coat off, and the links of his shirt-cuffs were undone because I had been testing him on the forearm with a number of pathophanes. He gathered his shirt-sleeves together, and put on his coat forthwith, evidently intending to do up his cuff-links in my waiting room.

At this the mother exploded with wrath at what she called 'this slovenliness'. 'I won't have that sort of thing! Take off your coat at once and do up your cuffs properly. I will *not* put up with such disgraceful slackness!'

The boy coloured up at being spoken to like this, but did as he was told in a tense silence—his mother standing over him the while.

When at length he had gone, I said I had originally wanted to speak to her about the way she tried to live her son's life for him by constantly telling him what to do, and by answering herself all the questions I had put to the boy; but later I had been really shocked by her rudeness to her son in public. She *must* give up her habit of insulting him publicly; and if she felt these criticisms of his behaviour were so very necessary, she should make them in private. I added that boys quite rightly objected to such inconsiderate behaviour as I had just witnessed: 'They don't like it!' I said.

At this the father, whom I think we had both forgotten, took courage to pipe up, 'And I think I may add that *husbands* don't much like being treated in that way either!'

No doubt this mother was a very duty-loving and conscientious person, but lacked imagination when dealing with other people—old or young.

The Spoilt Child. Occasionally, of course, we do see in the consulting room some lack of good temper and kindliness on the part of the escort towards the patient; far more frequently, as we have seen, their attitude is one of gushing sentimentality, and they tend rather to smother their children with an excess of yearning care. Especially is this the case with the only child.

But though we find that from the demeanour of the child, and the habits and behaviour of his escort, the patient has clearly been made into the self-satisfied pet of the home, yet the escorts are always strongly resistant to the idea that the child can have been spoilt, and spoilt by *them*.

This accusation of 'spoiling' has now become little more than a conventional term of abuse for supposedly unwise parents; so let us make it quite clear what is meant by the term in this book.

It should not mean that parents have been habitually too kind and understanding towards the dependent patient, but rather that by their handling of the patient they have given that person quite a wrong idea of his or her position in the universe, and thereby 'spoilt' them for contact with it.

The child who, by the unwise behaviour of its human environment, has been led to believe that it is the one important person in the home, round whom all the domestic economy must revolve, is naturally spoilt for reasonable contact with its neighbours by being put into that very false position. The spoilt person is wrongly orientated.

(iv) **School Teachers** relate that the anxious mothers of asthmatic or otherwise allergic children are for ever begging them to take extra care of these children. Perhaps it is only with difficulty that the parents have been persuaded to let their

child go to the school at all, and they pour a flood of anxious advice on the teacher : this delicate child must be watched constantly, he must never be overtaxed with work, he mustn't play with the rough boys—or perhaps shouldn't play at all, he must never go out of doors if it looks like rain, he must never sit in a draught, and so forth—endlessly. In fact, the teacher is implored to carry out the fussy over-anxious behaviour which, as we have seen, may do so much harm in the thoughtlessly emotionalised home.

A very experienced woman teacher of a day school told me she had herself observed the following incident :

A little boy of seven, who had occasional attacks of asthma, and whose attendance at school was therefore very irregular, was always (according to his mother) to be handled and watched by the over-worked teachers with especial care.

While keeping herself unobserved, the school-mistress had quietly watched this ' delicate and highly-strung ' urchin while he was in the play-yard. He was obviously having a quarrel with his good-tempered elder brother, for he was angrily shouting that this two-years-older brother must give way to him. ' You know what Daddy said you were to do : you must let me do as I like. If you don't I'll have an attack of asthma now, and *then* you'll catch it from Daddy! '—and the little boy proceeded to work himself up into a screaming fit.

The school-mistress told me that at the next exhortation to extra solicitude she received from the mother, she had described this incident, but the mother had remained quite unconvinced that the father's instructions had done any harm.

There could be no doubt these parents had between them spoilt the little boy.

These unwise preferential treatments are not uncommon. I have been told repeatedly that the other children in the family are penalised in favour of the sick child ; and of course the parents and other adults in the home will themselves give up cherished amenities in the supposed interest of an asthmatic child. I have been told, for instance, that a much older boy is made to go to bed every night at the same time as his asthmatic little brother, because the father thought it would seem so unfair to the delicate little one if he had to go to bed first. Quite often the healthy children have it drilled into them that they must give way in all respects to the sickly one. Life is made all too easy for the spoilt child—unless he has tough brothers and sisters who will correct this.

Soft Schools. Parents often demand that their ' delicate ' asthmatic child shall be sent to an open-air school, and indeed the school doctors often prescribe this too ; I believe it is sometimes likely to do more harm than good by grading these children as delicate—which, as a rule, they are not. If the boys and girls are asked point blank if they want to go to an open-air school, I find that the majority do not want to go for the very good reason that they will be leaving their old school friends to be herded with a number of devitalised children ' who are no fun at all '. I think we should be very clear in our minds as to the advantage to be gained from these schools before interfering to that extent with our small patients' private lives.

Altogether the children themselves seem to take a more healthy view of schooling than their parents often do. When a mother has been explaining to me how her child now rarely goes to school because of the sleep lost at night through the asthma panic, and deplores the lack of schooling involved in this enforced abstention, it is

usual, I find, for the child to declare that he or she often wants to go to school but the parent forbids it. I think it is a fairly safe rule that if a child has got no fever and *wants* to go to school the mother should let it go.

The same objection applies sometimes to sending children off to convalescent homes when they would much rather not go away from their friends. It may make healthy children believe that they are sickly and need special care : it may hospitalise them.

What of the Neglected Child? People may nowadays realise that the character of a child—of all of us, in fact—is chiefly determined by the treatment received at home up to the time of going to school, and thereafter by home and school combined ; but the home environment will probably always be the chief factor, unless perhaps the child goes to a boarding school.

It is popular to think that excessive parental *neglect* of a child does much more harm than excessive loving care. ' What ', the reader may ask, ' of the neglected or maltreated child? Why have we not been told of the asthma, etc., produced by this treatment too? '

My answer is simple : I haven't seen them in the clinics. It would be too much to suppose that not one of the cases I have seen has ever been harshly treated by their human environment ; some of them have, and when this has produced a severe emotional shock it may have started the first attack of a toxic idiopathy or the recrudescence of such attacks (see stories Nos. 4, 6 and 10 on pp. 140 and 141). This is just in the same way that a motor accident, or the arrival of doodle-bugs, or any other mental jolt, may start attacks ; but steady neglect or hardship doesn't seem to evoke the asthmogenic mood—rather the contrary.

The patients I have been considering in this and the previous chapter are nearly all of them very well fed and well dressed ; in fact better dressed very often than their parents. The anxious parents complain that the plump and smiling child they have brought to me ' won't eat a thing! ' They are really not neglected in any way, but pampered if anything, and of course overhandled.

Police Court v. Clinic. The evil effect on my patients of excessive love and care has been one of the main themes of this and the previous chapter. Here I would like to make a digression concerning what may happen to the *neglected* child and contrast his fate with that of our pampered children.

I was talking of this difference to an experienced woman magistrate who has for many years interested herself in the careers and characters of the ' delinquent ' children who are brought into her court. She told me promptly that, in *her* experience, too little love in the home might easily drive children, and some of the most worthwhile and vital children too, into the police courts. To make her point she told me the story of Edward.

Edward was an undersized, anxious-looking boy of eight years, who had been brought to her Children's Court by his father—a widower—who represented the boy as being absolutely uncontrollable and incorrigible.

Edward had, for some considerable time past, been making hay of the Education Authorities of the Metropolis. He was dirty, untidy and swore horribly, till the schools

wouldn't have him. He was not sent to a school for difficult boys (as he ought to have been) but was left to cool his heels at home alone, while his father went out to work. He cooled his heels by making the neighbourhood too hot to hold him : he broke windows and committed a long series of minor depredations and thoroughly exploited his nuisance value (just like the militant suffragettes) till he arrived in court with a dossier a foot high setting forth in painstaking detail all the trouble the authorities had had with him.

Edward was sent for further observation to a carefully selected approved school, and was received with fear and trembling because of that dossier ; but he proved to be extremely co-operative and obedient.

When the magistrate ran down to the school a fortnight later to see if the boy had attempted murder, or had perhaps blown up the school with dynamite, she was informed that ' Edward is, I think, the nicest boy we have in the school! '

He has been there for two years now and keeps his good character : his only trouble seems to be that he may be removed and sent home to his very unwilling father sooner or later.

Boys like Edward don't come to my allergy clinics, and I think we can safely say he was suffering not from too much, but from too little attention. If he is *now* made a pet of and spoilt, he may yet come my way.

Psychiatrists are fond of stressing ' insecurity ' as an asthmogenic factor. Who could have been more insecure than Edward, so neglected by his father ; yet the might and majesty of London on his tail couldn't knock a dyspnoeic wheeze out of this small boy.

Advice. But now I must really call a halt in giving details of the pampered patient and his pamperers. I have said quite enough to show that, habitually in the home, grossly excessive care and attention may be lavished on patients old and young, and, in the case of allergic patients at least, to their detriment. What can we do about it? What advice can we give?

First, what advice as to mental attitudes can we give to the patients themselves? We must be careful, for we mustn't make our patients too self-conscious.

This advice direct to the patient (as opposed to the escort) is particularly necessary if the patient is adult. Indeed, where this is so, it is doubtful whether the escort should be allowed to be very much in the picture—though they will, of course, expect it, and may even demand it. In dealing with an understanding escort it is often advisable that they should be asked to go back to the waiting room, and they can be privately told why : i.e. to re-establish the patient's feeling of responsibility and dignity. If a second visit is called for, it may be suggested to the escort that the patient should come alone, and this is helpful, even with boys and girls in their teens, for it encourages a sense of responsibility and independence. Patients should not be told this, however, but merely that mother is too busy at home to be able to afford the time, so they can, of course, quite well come alone.

I find that parents, particularly hospital parents, often fear to let their children —well-grown boys of fifteen and sixteen sometimes—move about London alone ; that is symptomatic of the children's home trouble of course. We must make due allowance for parental caution, but we must point out the dangers of this excessive caution for the child. The stifling care, like so much cotton wool padding, must be peeled off the asthmatic whenever practicable.

Even with quite young children it is advisable to address *some* of your remarks

to the patient, and not to the escort. Especially is this so when it is clearly the child's tastes and opinions which are wanted, e.g. ' Do you like drinking your milk at school? ' ' What do you like to be called at home? ' ' Do you like your lessons —or the teacher—or playing with the others? ' As said elsewhere, the real preferences of the child are often widely at variance with what the parents think they should be, or believe them to be.

In short, we doctors must take care to treat our patients of any age, not as nonentities brought along for inspection by their anxious owners, but as co-operating human beings with minds of their own—even if that mind *is* the mind of a child of six.

I am told that psychiatrists make it a rule to interview their patients, of any age, away from their escort. That, no doubt, is necessary for their particular technique, and because it seems to be an article of faith with them that there is always something in the *patient's* mind which must be winkled out. I don't believe that is true for the majority of my patients : I think it is the escort who often need some treatment ; so I see *them* by preference, and in private.

Normal Between Attacks. The main fact we should aim at establishing in the patient's mind is that when not having an attack of asthma, migraine, angio-neurotic oedema, and so forth, he or she is an ordinary normal person. If the patient is a child, that normality must be driven home to parents and guardians ; the child can get the knowledge by inference. They are treated and spoken of in an ordinary way, therefore they are ordinary. They must never be called delicate or highly strung if it can be helped.

If you put an ordinary healthy person to bed for a week and can arrange for doleful looks and sympathy from relatives, the odds are that you will develop quite a bit of hysteria and probably a symptom or two within the week : *experto crede.* It takes great determination and strong common sense if any ' patient ', old or young, is to stand up against this course of suggestion ; don't subject people with asthma, etc., to this terrible strain if you can help it.

I find, by the way, that there is a widespread belief among my more clinical colleagues that there must be some characteristic lesion in the lungs of asthmatic patients between the attacks. They sometimes chaff me at not finding it at a post-mortem examination.

' That poor old Mrs. So-and-So, whom you have been treating for asthma on and off for years, was knocked down yesterday in the street and killed. They did a P.M. on her this morning and you will be *surprised* to hear (this with a friendly but wicked grin at me) that they found no trace of asthma in the lungs at all! '

Well, there shouldn't be any trace when once the attack is over—unless of course the asthma has been accompanied by, or ' caused ' by, bronchitis or some such lung disease, when traces of *that* will be found.

In the same way, one supposes, their psychiatrist colleagues must postulate some small abnormalities in the brain structure of hay-fever patients to account for the causal complexes, etc. ; though, to do them justice, they wouldn't expect to find such lesions.

Behaviour of the Patient. ' Manners Makyth Man ' does not apply only to young Wykehamists ; a good deal of a person's attitude to life, and the capacity for facing the hazards of it, depend on quite small points of behaviour.

Children out of babyhood should not sprawl ostentatiously in an armchair on their first visit to a doctor ; and, if their escort doesn't correct them, they should be told to sit up straight by him. When with other children, they would naturally relax and be off-hand ; but with grown-ups, and on a formal occasion, they should ' behave '. I find it a good plan to tell the children, but in a friendly way of course, to sit straight in their chair (i.e. not sideways and facing their mother) and to sit well back in it. If appropriate—i.e. if they are without symptoms—they should be made to sit or walk with straight back, to put their shoulders square, to keep their chins up and their mouths shut, except when speaking—and that not too often. Attention to these details of behaviour unconsciously increases a patient's self-confidence and self-respect. Parents can usefully be asked to see to this at home.

I believe this question of stance, with shoulders square and chin up, is perhaps the most important part of the so much vaunted ' breathing exercises for the asthmatic '.

Discipline. Freedom from all discipline does *not* make patients easier in mind ; it does not relax their tensions in the relationship between children and grown-ups in the home. A light, but firm, discipline, which has grown into an easy habit of mutual tolerance as between old and young, is the ideal to be aimed at, and doesn't diminish mutual affection. The absence of any such discipline is very frequently to be noted in the consulting room and should, I think, be corrected there in the interests of our patients if it is to be corrected at all nowadays.

I strongly suspect that part at least of the freedom from care to be derived from (i) life at a boarding school, (ii) a stay in hospital, or (iii) life in the armed forces (all these were discussed in Chapter IX) depends on the relatively stiff discipline to be found in these places as compared with those lax conditions at home which my patients often suffer from.

A direct approach to children is the easier because they usually use their brains more, and their emotions less, than their parents believe possible. In fact, sentimentality in children is largely an attitude of mind adopted to suit the grown-up taste for it.

At a clinic, the question of the disappearance of a pet rabbit came up, and secretly the mother whispered to me that at a food crisis that rabbit had been cooked and eaten under the guise of ' chicken '.

Subsequently, the small child-owner said to me quite cheerfully, ' Of course I knew all the time that it was *my* bunny that I was eating. I was fond of him, but I liked eating him too ! '

We must certainly never talk down to children or under-rate their intelligence, or we may get caught as this mother was. An unforced man-to-man attitude when talking to children is the best, and the emotional appeal such as one hears so often from parents in the consulting room usually cuts no ice with the intelligent child.

Lastly, if we *are* going to discuss with the parents a child's emotions, or the way to handle him or her for the best, that child should be sent out of the consulting room first.

Treat the Parents. Whether we judge that the child gets too much or too little attention, it is clearly the parents or other guardians who must be chiefly tackled to put things right if possible. We have to deal almost always with parents who are *too* conscientious ; so we at least have their goodwill, if we can only convince them of the importance of a change in their attitude towards the patient.

Usually by telling parents stories like those in this and the previous chapters, by telling them of the magic effect of school or hospital and the equally tragic effect of returning home again, it is possible to convince them. If so, they may say : ' Very well, doctor, you have made your point ; but tell us what to *do*! ' Advice is easier to ask for than to give, I find.

We must remember that they have nearly always been trying to do their best already ; if they have erred, it has been probably through too much zeal. It is not much good to tell them to follow their instincts—these have been falsified by the unbiological state of the present minute families. It is no good to tell them to use their common sense, because that is just what has broken down. It is no good, usually, to tell them to tighten up discipline, unless they are told in what way this should be done.

I have found that such advice may land the wretched patient in a worse mess than I found him in.

I remember the case of a badly behaved and spoilt little girl. The parents, and especially the father, seemed to me to be much too ' lovey-dovey ' and yet were interfering with her at every turn. When the child became quite unco-operative, the parents responded by still more saccharine expressions of love and approval, and tried to bribe her with chocolates into better behaviour. The child had, of course, responded to similar previous treatment by being spoilt and showing complete lack of discipline.

I sent the girl out of the consulting room with the mother, and then tried to explain to the father that the highly emotional attitude they adopted towards their only child was really helping to keep up the child's asthma. I rashly advised the father to tighten up the discipline of the home—and left it at that.

I had to see the girl a fortnight later and found her and her parents as unhelpful as they had been at the previous visit ; if anything, they were worse. So again the girl went out of the room with her mother, and I expostulated once more with the father.

He was most indignant. He told me that he could not possibly be spoiling the girl *now* because, on my advice, he had several times punished her *quite* unnecessarily ; and once, to do the job thoroughly, he had been deliberately unjust.

This example very well shows the emotional harm which may result from over-indulgence alternating with nagging and punishment : we must tread more warily than that ;

> ' 'Tis an awkward thing to play with souls
> And matter enough to save one's own.'

Perhaps it would be safer to start our advice to parents, guardians and escorts of patients generally, with a few ' don'ts '. If some of these are felt to be unnatural, seem to take most of the fun out of parenthood, remember that they are really therapeutic measures designed to safeguard the patient's health. Well then :

(1) Don't get worked up and excited about the patient. Remember the instructions given to the nursing staff concerning the asthma and the toxic idiopathy cases in our allergy wards (p. 154). Above all, don't panic, even if the patient's condition is distressing to look at ; in fact *don't* look at it more than can be helped. Don't stand by goggling ; but, if nothing is to be done, go to bed, or read a book, or go on with your own chores.

Enough has been said on these dangers from over-attention, or even panic treatment, of a loved one to justify the often-heard comment that it might be better for the patient if we confine our treatment to the parents or friends instead.

Epistaxis is a peculiarly unnerving disease, not only for the patient, but especially for the onlooker ; a very little blood, if well distributed over pillows, bedclothes, pyjamas, etc., will make a good deal of mess. It is currently reported that, at a very famous London hospital, the favourite and most successful treatment is to give the nursing-sister in charge of the case a double dose of bromide.

I have often seen parents of a toxic idiopathy case who would be the better for such a double dose.

Mothers get peculiarly distressed at any blemish on the skins of their otherwise ' perfect ' babies : please, mother, don't fuss ; the skin trouble almost certainly affects you far more than it worries them.

(2) Don't fuss the child (and yourself) by giving it unwanted help. Don't dress or undress or wash it when it is quite able to do this for itself. Don't hold a patient's hand while walking across a room unless this is really needed or wanted. Even hand-holding of children in the street as a safeguard should be no more than is really necessary, or the child will grow up to be incompetent to be out by himself. Hand-holding as a demonstration of love should be indulged in sparingly with children, and should not be demanded by grown-ups.

In short, don't do anything for a child (or other dependant) that it can do, and wants to do, for itself—even if it gives you pleasure. Unwanted help increases nervous tension with most of us.

On reflection I believe that more irritation is experienced, and certainly more is shown, by boys than by girls at over-attention ; perhaps this is because girls of all ages may consider such attentions to be a tribute to their charms ; or (as they themselves would claim) because they are naturally more docile. It seems to be a sex difference ; a craving for help from a male is notoriously a favourite gambit of The Sex.

I remember rounding on a perky little cockney mother who had, in my opinion, quite unnecessarily lifted her solid, stolid daughter of eight years into a chair. ' How would *you* like it ', I demanded of her, ' if I lifted *you* into *your* chair? '

The little wretch looked at me obliquely, with her head on one side, and exclaimed with a theatrical gush : ' Doctor! I should simply *love* it! '

Touché : I acknowledged the hit. All the same, I doubt if too many lollipops of over-attention are good—even for grown-up girls.

(3) Don't under-rate the intelligence of a child : intelligence is not the same thing as knowledge or experience. At the age of six or seven a child's brain may, as an instrument, be as good as yours : be humble-minded enough to think that it

may even be better. At any rate it is common knowledge that a small child can, and often does, develop logically quite an intricate line of argument from its premises which would be no discredit to a professional philosopher. However, they usually try these argumentative flights on playmates of their own age; they are seldom allowed to argue at this length with grown-ups—nor do they want to.

I think children are assisted in this occasional intellectual swoop by the paucity of their facts; the few facts they have they see in a clear light 'as with the eyes of a child', and their minds are not overlain and distracted by side issues or by accepted (but possibly erroneous) conventional thought.

It was said of Sir Frederick Banting, the discoverer of insulin, that he was able to work straight to his objective with a pregnant single-mindedness because he had *not* bemused his mind by reading 'all the literature on the subject'. He had not had to wade through all the speculations, evasions, explanations, contributions and warnings of other workers in the same field.

He had been saved from all this by the 1914 War when, in the Canadian Medical Services, he had plenty of time to think, and but few books to read.

Respect their brains; and don't, to satisfy your vanity, force an unnecessary infantility on your dependants. On the contrary, make it easy for them to 'be their age'. Don't, through vanity, take a delight in childish errors.

(4) Don't make emotional demands on any dependant: it is really a discreditable piece of domestic blackmail. By emotional demands I mean the kind of behaviour described in the story on p. 166; the sentimental behaviour of that most amiable family was the ruin of it.

'Come and kiss Mummy, darling! You do still love your Mummy more than you love your old black golly, don't you dear?'

That kind of thing is said by many a mother, and half in jest; yet there is plenty of hurt to be felt by the mother if the child, straining its infant conscience, fails to play up to the demand. There is also plenty of hurt to the child if it is 'good', and always responds handsomely with a kiss or two. As I have said, mothers should wean their children emotionally, as well as from the breast. I'm not contending that parents and children should never kiss each other, but that parents should be chary of demanding this.

(5) Don't focus all attention on the patient. Don't be 'a born nurse'. Don't treat the only child *like* an only child; and the same is true particularly for the adopted child, who is usually an 'only' child perforce. This warning is by now a platitude: yet parents, child-welfare centres, and even doctors, often blandly disregard its truth. The minute modern family is clearly the nigger in the emotional wood-pile again. Do not let anyone's symptoms, asthmatic or other, be a constant subject of interest in the home. Without neglecting to do anything that would really help the patient, affect a mild degree of boredom over the symptoms, or even forget them where practicable. Emulate the behaviour of the nurses in an asthma ward of a hospital. Enforce, by example and precept, that other people in the home follow the same line.

(6) Don't be always ready to play with or instruct your child: ration yourself. Don't think that grown-ups are suitable constant companions for children, however

amicable and self-abnegatious both parties may be. Anyone's normal companions should be of the same age. In a small family and in a small house old and young must unfortunately be in close contact, but they must behave with mutual restraint, and not consider the situation normal or ideal.

The solution I recommend is to send children to school as soon as they are able to go—the only child and the adopted child particularly. This does not, however, absolve parents from making every effort to provide playmates for their children when they are not at school, and in their holidays.

Application to both Child and Adult Patients. I find on retrospect that I have chiefly considered the possible evil influences of the home on *children*. That is because children are the most easily and most often offended against. Older people can, and should, fend for themselves; and if (like the young woman on p. 147) they don't want to help themselves, that is only because their characters have been warped in childhood.

All the same, what has been said about children and parents applies, *mutatis mutandis*, to adult patients and their keepers—including fussing wives and fuming husbands.

Are All Homes Bad? May I now fling up my elbow in self-defence and say that I am animadverting against *some* homes and in *some* respects. As I have said in the text, I believe there is a higher percentage of over-emotionalism in the homes of my toxic idiopathy cases. Of course it is just in these homes that people must be particularly on their guard, for it is vitally important for the health of the inmates.

Also, perhaps, the somewhat less well educated homes from which my hospital patients are largely derived seem on the whole to be more harmfully emotional than the wealthier and perhaps tougher homes. I think I would ascribe the advantage of the more sophisticated home to the greater use of boarding schools by both children *and* parents.

CHAPTER XI

FURTHER CONSIDERATIONS OF EMOTION: AN APPENDIX TO CHAPTERS IX AND X

It may be remembered that in Chapter IX I strove to prove, and did prove to *my* satisfaction, that in the toxic idiopathies there is always an emotion or mood as one of the causal factors of the illness.

In Chapter X we considered why so many of these asthmogenic moods or emotions were derived from the modern home, and what might be done to ameliorate that possibly evil influence.

To keep these two chapters within bounds, and to avoid distracting readers more than could be helped, much evidence of a psychological nature was left out which still seems to be worth putting on record as ground for future discussion, or as springboard for further research. Such things I propose to put into this Appendix Chapter.

At the end of this somewhat heterogeneous collection I discuss the unfortunate mental gulf which seems to separate the Ordinary Doctor from the Psychiatrists, on the subject of the toxic idiopathies at least. It is important to find out what this difference is due to, and what may perhaps be done about it: the question is really urgent for the advancement of medicine.

A Fuller List of Emotional Shocks as Starters of Attacks

In Chapter IX, I was only concerned to show that emotional shocks could, and frequently did, contribute towards the production of an attack, so I only put in the few examples sufficient for this purpose. I now give a much fuller list; and, to make it the more complete, I will make brief reference to the stories I have already used in Chapters IX and X. I will then continue with some fresh examples of the way that shocks can produce an attack. As I said in Chapter IX, to carry any conviction that emotion was really a promoter of an attack of toxic idiopathy, the shock must be of a dramatic nature; so also must be the idiotoxic attack which is started by it, if they are to be convincingly correlated.

These sudden and dramatic emotional starters will, of course, not present anything like the total psychological influence on the toxic idiopathies; as a matter of fact, *chronic* emotional tensions are, as we saw in Chapters IX and X, found to be much more potent and frequent in asthma production than are these shocks to be now related. Nevertheless, it is just because of the dramatic values of the shock starters that it may be the easier for us to note and understand the emotions uppermost in the minds of the patients when the attacks of some toxic idiopathy are thus set going by a sudden shock. Conturbation plays a bigger part than fear, it seems.

When a man gets asthma immediately after being nearly run down by a bus, at least he knows why he is feeling so queer and shaken. But when a man is constantly moody and depressed in his apparently comfortable home, the chances are that he will not know why he feels as he does ; he may even believe his loving wife when she tells him quite inaccurately that his liver is out of order, or that he is working too hard, or that the climate doesn't suit him.

Many obvious starters of attacks are of a standard type and recur in patients' case notes far too frequently to be worth recording one by one. For instance, I won't put down, save as a group, all the ' bereavement ' starters of attacks ; they are all so very much alike and occur so often, involving in most instances very similar emotions. It is unnecessary also to put down *all* the occasions when an air-raid warning has produced an attack, or all the occasions when people, particularly the children, got attacks on going into an air-raid shelter. Many of these emotional starters may produce unexpected or unorthodox emotions ; for example, it is conventional to suppose that a ' selfless sorrow ' is the only emotional experience on the occasion of a bereavement, but in truth we all know that not to be so : fear of the future or relief from the past are strong, and to some extent shameful, sensations : to me, I think the chief feeling in a sudden bereavement is a very disquieting sense of drifting, as if an anchor to life had given way.

In my notes I haven't collected these bereavement starters with enough system to make reliable statistics ; also I do not know what percentage of the people who *think* that their attacks were started in this way have told me so. However, the prominence of some kinds of relationship in these stories seems worth putting on record. (i) The death of a brother easily heads *my* list. Then in order have come (ii) the death or disappearance of a father, (iii) of a husband, lover or male friend. After these, perhaps, (iv) the death of a child, (v) of a wife, of a sister. As this admittedly rough list perhaps suggests, most of these stories have been given me by women ; I think perhaps men are more reticent, especially when talking to a male doctor. Children naturally don't tell ; but their mothers seldom, in my experience, attribute a toxic idiopathy to a childish bereavement. Perhaps the story of a pet chick (No. 54) is as good as any other story of the asthmogenic bereavement of a child.

Groups of Emotional Starters

A. Bereavement cases : loss of a brother, of a father, of a husband—lover—friend, of a mother—wife—or sister, etc.
B. Sirens warning an air raid ; bombs dropping in the distance (but usually not actual hits or near misses) ; going into an air-raid shelter. (This very common, especially with small children.)
C. The historical shocks, such as Munich in 1938, or the beginning of the war in 1939, over-running of Norway, etc. (but *not*, so far as I can remember, the Dunkirk evacuation or the imminence of our own invasion).
D. An unknown Daddy coming home from the wars is a fairly usual asthmogenic factor.

The next four groups are perhaps more a matter of mood than shock :

E. Laughter.
F. Academic examinations—both before and after them.

G. Mother visiting child at school.
H. Relatives visiting patient in hospital.
I. Cases where girls in their teens had been frightened out of their wits by the first of the menses without proper maternal preparation for them.
J. Some people declared themselves allergic to the dye of their khaki uniforms in the Home Guard. Also people in other home uniformed services—e.g. fire brigade —thought they were allergic to the dye of the cloth. Presumably they were excited at the cause for putting on the uniform. They none of them, of course, gave skin-test positives to anything which could be extracted from the cloth.

Individual Stories. (The first eighteen of these are only mentioned shortly because they have already been given as shown by the references) :

1. (p. 140). Death of a favourite brother started the first attack of typical hay-fever. (I heard this story in 1910, and that was the first time I realised that emotion *could* start a toxic idiopathy.)
2. (p. 140). Quarrel of a girl with her step-mother—hay-fever.
3. (p. 140). Missing a train—hay-fever.
4. (p. 140). Unjustly punished—hay-fever.
5. (p. 140). A thriller play—hay-fever.
6. (p. 140). Getting the sack—hay-fever.
7. (p. 140). An exciting holiday—hay-fever.
8. (p. 140). A children's party—hay-fever.
9. (p. 140). A husband's unfaithfulness—hay-fever.
10. (p. 141). A rather theatrical monitorial beating at school—asthma.
11. (p. 141). Killing a pedestrian in Teheran—asthma.
12. (p. 142). Nearly run down by a bus—asthma.
13. (p. 142). A pretence murder with a knife—asthma.
14. (p. 142). A baby swallowed a spoonful of mustard—asthma.
15. (p. 142). Caught stealing apples—asthma.
16. (p. 142). Income Tax demand—asthma.
17. (p. 142). Girl of twelve caught smoking by parents—angioneurotic oedema.
18. (p. 143). Laughter—asthma.

The following stories are new :

19. A Home Guard leader saw the bodies of two of his men blown through a doorway by a bomb—asthma.

20. A motor smash where one person was killed—' I seemed to freeze for a moment, then started to run away ; there was a tingling all over, and angioneurotic oedema lumps developed round my neck and hips, with giant urticaria down my back.'

21. A child of four years of age turned on the hot water in the bath, and then forgot to turn it off again ; the house was flooded. When this delinquency was discovered by the child and her parents, asthma followed in the child.

22. A man who was expecting to be a father for the first time suddenly heard of his wife's miscarriage. He attributed the first attack of asthma to that disappointment and excitement.

23. Similarly, a man heard that his wife was pregnant for the first time ; he felt very excited and upset about it, and got asthma.

24. A man was doing some important research work in chemistry ; his experiment had reached a critical stage when he was interrupted by the telephone ringing. He had to attend to this, and the experiment went wrong. Asthma followed.

25. A girl of thirteen was bitten in the leg by a dog, and asthma followed promptly.

26. The excitement of a burglary in the house caused asthma.

27. Hearing that her father had lost his leg during the first world war, a devoted daughter started asthma, and has had it ever since.

28. The excitement of the first night of amateur theatricals developed a sharp attack of asthma in one of the players.

29. The turmoil of the General Strike in 1926 was claimed by a patient to have started his asthma.

30. A man underwent an A.R.P. gas test. This started a recrudescence of his asthma which had been in abeyance for a long time. (There may have been a trauma effect here : Ch. VIII.)

31. A severe burn on the arm caused asthma. (Burns are common starters.)

32. A girl sliding down the banisters fell off and landed on the base of her spine. This produced a crop of nettle-rash and slight asthma.

33. A woman claimed that getting up in the morning, and unexpectedly seeing the ground white with snow, produced an attack of asthma.

34. A woman said that her husband had ' brain-storms '. Whenever *he* did this, *she* got a crop of nettle-rash.

35. A young woman, who had a Church of England parson as a brother, became a Catholic. She said she was so distressed at the ' loss ' of her brother that she began to have asthma.

36. A patient said he was ' officiously ' disinfected by an Air Raid Warden on coming out of a shelter. This disinfecting produced asthma, so he said. I rather suspect that it was perhaps the quarrel with the Air Raid Warden resulting from this ' disinfection ' that started the trouble.

37. A patient went to Southend and had a most exciting time on a switchback railway there. This excitement produced asthma.

38. A child of three years old swallowed sixpence, and asthma began in the hubbub which followed.

39. A child fell on a tea-cup and cut her leg with it. This produced asthma.

40. A man had his first attack of asthma on seeing the film of ' Dracula, the Vampire Man ', and has had it ever since, off and on.

41. A man always gets urticaria when he sees an exciting film. He also gets it on driving a car for four or five hours.

42. A big unintelligent mother told me that her four-year-old son had developed his first attack of asthma at the age of two from a shock ; the child had had asthma ever since.

When asked about that shock, she told me : ' He burnt his poor little behind, doctor.' On further questioning—' Well, it was like this : I knew I'd give him a chill if I sat him on a cold chamber pot ; so I warmed the little article well, and held him down onto it. Blistered something cruel he was behind ; and he got asthma from that. How can you account for it, doctor? '

43. A sharp attack of asthma on seeing Charing Cross Hospital on fire in a blitz.

44. A small boy of four years saw a hen on the roof of an outhouse. He ran into the house to tell his mother of this strange event ; in doing so he got more and more excited and incoherent, ending up with a sharp attack of asthma for the first time. Chronic asthma followed.

45. A woman was shocked and frightened when she heard her first-born child give its first cry. She said it seemed so ' inhuman '. Hay-fever promptly followed. The child was born, by the way, at midsummer.

46. A young woman got asthma on being promoted in the W.R.N.S. She said that she felt that she had not had enough experience to cope with so important a job.

47. A woman got a short, sharp bout of asthma when she lost her baby, then recovered. She got another sharp attack of asthma when she lost her husband subsequently.

48. A young Wren was with a party climbing the mountain Cairngorm. A blizzard caught them when they were nearly at the top, and they got into difficulties. This produced a crop of nettle-rash and asthma, but only when the patient got safely back to the hotel.

49. A patient watched (with *horror*, she told me) Mr. Leslie Henson making faces on

the stage. This produced a crop of nettle-rash. I rather suspect that this may have been caused by the hilarity produced thereby, rather than by the shock of seeing the faces.

50. A patient was stung on the mouth by a wasp, and this started off a course of asthma.

51. A boy aged four was riding his tricycle alongside the little river Pym at Hillingdon ; inadvertently he rode it right into the middle of the stream. It was only a few inches deep, but he was so perturbed that, when he was pulled out by an adult—tricycle and all —he was having asthma.

52. A middle-aged woman, who had recently had a peculiarly sharp attack of asthma (which had in fact brought her to see me), was asked what the incident was which brought on this attack.

She had been told that she had been left an unexpected fortune by a distant relative, and was elated. Then, soon after, she was told it was all a mistake—and asthma began.

53. Boy of ten had got the core of an apple sticking in his throat which had needed much back-slapping to dislodge it ; result, asthma.

54. Little boy of two and a half got asthma for the first time on the death of a pet chick to which he was much attached.

55. A little boy at the age of three was hit in the stomach by a pear which was aimed at another boy. Result, asthma.

56. A girl of ten started asthma when her mother went into hospital for an operation.

57. A child started asthma for the first time from being winded by falling full length in the street. Asthma off and on ever since.

58. First attack of asthma occurred when patient was vainly trying to crank up his car on a cold day, while his passengers laughed at him.

He gets his asthma especially at week-ends. He doesn't like waking in the dark, has to get up and sit in a lighted room.

59. Man was blown twelve yards by blast through the doorway. Result, asthma.

60. The fall of a yellow ochre ' bomb ' near patient in a pre-war bombing practice : asthma. ' The doctors all told me that the yellow powder had burnt up my stomach as I breathed it in, and that caused the asthma to come on.'

61. First attack of asthma brought on by girl running a needle into her knee, and being terrified thereby.

62. Rocket bomb on school : boys were not there when the bomb fell, for the school was closed down. Two got asthma when they heard of it.

63. First attack of asthma when waiting for a train in a fog. He said he got into a panic because of the fog, but thought the dampness had caused asthma. I suspect the panic.

64. Asthma came on suddenly just before marriage ; the man was of a nervous type, stuttered as a boy.

65. An only child, a well-grown boy of seven years, was brought by his mother to my clinic for his repeated asthma attacks ; she volunteered this story of their origin.

The boy had been quite free of asthma till he was four years of age. One day she left him in his ' push-cart ' on the pavement outside a shop while she went in for the domestic supplies. The pavement was on a slope, and the push-cart, with the boy in it, ran away down the road for fifty yards with ever-increasing momentum till it ran into a telegraph pole and upset.

The boy had his first attack of asthma in consequence, and had had it off and on ever since.

66. A boy of eight swallowed a small iron nut off a bicycle. There was panic in the family concerning this, and the boy developed asthma in consequence. The nut passed through naturally and without doing any harm.

67. A boy of five and a half, one of three small brothers, was deserted by his mother who went off with the lodger, leaving the children alone. My patient was found (by a neighbour) having asthma in consequence. The other boys were unaffected, though, of course, terrified.

68. A boy of five tried to retrieve his toy boat from the Round Pond in Kensington Gardens. He bent farther and farther over the concrete rim of the Pond till he slithered head foremost into it. His mother had to wade in and pull him out, together with the boat. His first attack of asthma followed that night.

69. A young woman dated her first attack of hay-fever by getting engaged to be married ; she said (on enquiry) that she had regarded this event as a much-desired success, and had had no misgivings.

70. ' I always get these swellings of the lips ' (typical angioneurotic oedema) ' whenever my husband kisses me, and after ten years of marriage too! Isn't it silly of me! ' (Possibly trauma action.)

71. ' I always tend to get asthma on listening to highly emotional music.'

72. ' I got my first attack of hay-fever this year on witnessing a particularly blood-thirsty dog-fight in the street.'

73. ' I got my first attack of asthma, and a particularly bad one, on hearing that I had failed to pass an important examination.'

74. First attack of angioneurotic oedema, which an elderly woman called ' appalling ', was caused by someone being very rude to her on the telephone.

75. On hearing by cable that her fiancé in the Merchant Navy, whom she has twice nearly married, was coming home, young woman got angioneurotic oedema ' because of the excitement of this unexpected third chance ', i.e. of securing him.

76. First attack of asthma attributed by an hysterical young woman to the squeezing of a ' *real* lemon ' over a pancake during the war.

77. Asthma came on suddenly to a woman in her first pregnancy when her baby ' quickened '.

78. Angioneurotic oedema in a man on hearing that his wife was pregnant (for the first time).

79. A very inexperienced driver had to take his car through a thick fog along the coastal road up the extreme west coast of Wales—a dangerous road in the circumstances. Asthma developed (for the first time) during that drive.

80. A man got asthma after a motor-cycle sidecar had gone over his chest.

81. A boy of three and a half, with long curls, got first attack of asthma on being taken to a hairdresser to have them cut off. (Probably emotion via the mother.)

82. Woman got asthma on seeing man cut his throat.

83. A child went out for a walk, got caught in a heavy thunderstorm, and this brought on an attack of asthma.

84. When a woman discovered that her first-born was minus an ear the shock produced an attack of asthma.

85. An elderly opera-singer, who was beginning to find that her voice was failing slightly, had an attack of asthma when trying to get her top note in an opera.

86. A woman was very upset on learning that her daughter was ' going in for ' Christian Science ; this produced an attack of asthma.

87. When walking through an apple orchard, an apple fell on a young woman's head. ' This gave me such a start, Doctor, that I began to have asthma straight away.'

88. A small boy got frightened of a friendly dog, and ran away from it. The dog naturally ran after the boy, who got asthma in consequence.

89. A small girl of six was told by her mother that she was to go to a harvest festival at the village church. The child had often gone willingly to children's services before, but the idea of going to a real grown-up church service was too much for her. The mother remonstrated with the girl but she remained frightened, and when going to church developed asthma for the first time.

90. An adolescent Jewish boy got extremely worked up with religious excitement on going to Bar Mitzuah. The mother reported that, as a result of this excitement, he began his first attack of asthma.

91. A mischievous small boy stretched a cord tightly across a garden path. A girl

running down the garden path was tripped up and fell headlong : result, much perturbation and asthma in the girl.

92. A man fifty-seven years old, who has had chronic asthma all his life, told me this story of its onset. When he was a child of three and a half years his nurse was taking him for his customary morning walk, when they chanced to meet a friendly soldier. The nurse soon became more interested in her soldier than in the child, who fell into a water-splash when her eye was off him. He was pulled out and hustled home ; but the accident brought on asthma forthwith, and he has had it off and on ever since.

93. Small boy of six years was sitting astride the wall of a static water-tank built on a bomb site. He fell in and was nearly drowned ; but he was pulled out by a man who happened to be passing : asthma forthwith, and for the first time, followed the accident.

94. The family were working on their allotment garden ; and the father was lent a very precious spade by a neighbour and promised to take great care of it. My patient, a lively little girl of four years, kept playing with that spade, though repeatedly told not to do so. In the end she accidentally dropped that spade through a grating into a land-drain, and it was never recovered. The child got asthma that night for the first time in her life.

95. A boy six years old, on holiday in the Isle of Wight, coasted downhill rather faster than he liked on his new bicycle. In his agitation he pulled the handlebars completely out of the front pillar of the machine, which, therefore, proceeded downhill out of control to the inevitable crash. Surprisingly the boy was not very much hurt, but developed asthma forthwith and for the first time.

96. A mother was carrying her eighteen months old baby boy from his Granny's house at midsummer when a thick fog came on and she lost her way. She arrived home very late, and weeping ; and her baby developed asthma which he has now suffered off and on for many years.

The mother attributed his attack of asthma to the dampness of the fog, but I think it more probable that the baby was frightened by the mother's perturbation—as in the Anderson shelter stories (see p. 143).

97. A middle-aged man told me that he'd had a ' delicate skin ' all his life. In childhood, for example, the east wind or the direct rays of the sun would bring him up in a rash which sometimes was more like eczema than urticaria.

In 1930 when home on leave from India he got completely lost in a Dartmoor fog for nine hours and regained his hotel very exhausted. The next day he developed eczema (from which he had been completely free for many years) ; this eczema gradually spread all over his body and he was in bed with it for five months and, according to his story, lost most of his skin. During the next two years the eczema gradually went away, though there were occasional outbreaks of it till he saw me in 1947.

98. An overseer in a factory should have stopped a manufacturing operation because it had become dangerous. He forgot to do so ; and, although as it happened there was no accident or trouble to anyone, when he remembered his omission that night he developed asthma after being quite free of it for several years.

99. A small boy was much addicted to fishing for tiddlers in the Regent Canal, and while doing this he frequently lost all sense of time. This disrupted the family meal-times ; after repeated warnings the boy was put on the mat by his disciplinarian father : result, asthma that night and subsequently.

100. A boy of ten years had a fight at his school. He won the fight ; but for some reason he became very unpopular with the other boys. I couldn't get a very exact story of this from my patient or his mother, and I suspect that the reason for the unpopularity was ' more true than tellable '. The result was a sharp attack of angioneurotic oedema.

Dreams and Visions starting Attacks

When sorting amongst my notes to collect the preceding 'emotional starters', I found some stories of the effects of dreams, which seem to come into a rather different category.

I will here put them on record, not because I have any particular theory to prove, but that my experience, for what it is worth, shall not be wasted. Also, I wish to lead up to the question of visions, which also emanate from the unconscious mind.

It is known to psychiatrists that nightmares sometimes cause asthma. I have not often had the word nightmare used to me by patients, but they do use it sometimes—thus :

(i) A mother told me that her little girl of 9 years was always dreaming of being chased by wolves. As a result of these dreams the child would wake up screaming—and always with an attack of asthma.

The next two stories of dreams causing an asthma attack can hardly be described as nightmares.

(ii) A woman patient had had a certain amount of asthma all her life. As a schoolgirl and young woman she frequently got attacks after hard exercise, such as playing hockey—as is common with asthmatics. Obviously the sudden demand for increased oxygenation of the muscles, and the need for eliminating waste products from them, would naturally cause ' breathlessness ' ; and that breathlessness may start up or increase asthma.

The patient stated that, when in middle life she was living a comparatively sedentary life, she had only to *dream* she was running to wake with an attack of asthma.

My next story is much the same :

(iii) A boy of 13 had had asthma for several years, and reported that hard exercise, amongst other things, produced an attack.

A week before being seen at the Clinic he dreamt that his dog destroyed his collection of birds' eggs. In his dream he was furiously angry, and he ran after the dog to thrash it, but as he ran his asthma started. He awoke with an acute attack.

In neither of these stories as told was there any trace of fear. It seems simply that asthma had got associated with exercise in the minds of the patients ; and so asthma had come in consequence of dreaming about it. It suggests to me the case of the lady (see p. 138) who saw a picture of a hayfield and got hay-fever in consequence ; it suggests Suggestion, in fact.

My next story seems more closely to resemble a nightmare :

(iv) A young man in the R.N.V.R. was the look-out in his ship which was going to South Africa ; he suddenly saw a mine straight ahead in the track of the moonlight on the water. He gave the alarm with a promptitude for which he was later highly commended, but he was too late, and the ship was blown up. He was thrown into the water with three of his ribs broken, a leg broken, and very badly concussed. When he was taken out of the sea three hours later, he was still unconscious.

Eventually he arrived at Cape Town, where they patched him up ; and as soon as possible he was sent back to England as a stretcher case.

On his way home, although he was still in splints, he had to help man an anti-aircraft gun during the air attacks, because of the shortage of crew ; but, in spite of these extra adventures, he remained quite well—save for the surgical damage.

In England he got pneumonia ; when in hospital for this, he began to have recurrent dreams of seeing that mine bobbing about in the moonlit waters just ahead of his ship. For the first time in his life he began to have asthma—and always after that dream.

In the last dream-starter given above, there could clearly be no element of suggestion, for—mine or no mine—he had not had any previous experience of asthma before these dreams began.

My psychologist friends (or some of them) might wish to point out that this sailor had been concussed, and thereafter had been in the water unconscious, or semi-unconscious, for three hours before he was rescued ; during that time he was presumably half-drowned. They might suggest that asthma was the nearest thing to drowning that the body could manage on dry land when convalescing from pneumonia. Perhaps, too, pneumonic dyspnoea is not unlike half-drowning, and also not unlike asthma.

Such a symbolic explanation has been given me by psychiatrists for other ' starters ' of asthma. For example, in story 11 (p. 141), a psychiatrist said that the man's asthma resulted from the thought of being lynched by an angry mob. I have never, myself, seen a Persian gaol, but I'd suppose *that* might produce a feeling of dyspnoea too. This symbolic ' explanation ' of asthma I find unconvincing. It seems to me not an explanation, but merely a rather wild hypothesis.

Whatever the explanation, dreams may certainly start asthma. In Chapter VII, p. 124, we said that the prone position in sleep naturally brought about some engorgement of the blood-vessels of the upper air passages ; that, no doubt, is one of the reasons why rest in bed so often meant waking up with an attack of asthma.

We now can see another possible cause for the regular nocturnal onset of asthma, or indeed of any of the toxic idiopathies. I must have missed recording many examples of distressing dreams helping to produce some toxic idiopathy, but on reflection it is fairly certain that the patients have often missed them too. We are told we do not remember most of our dreams ; and we all know that those we do remember we very quickly forget unless we record them at once on waking. It is as if the recording mechanism of memory is only set in action very feebly by dreams ; unless they are, as we say, very vivid, they are forgotten almost at once on waking. May not even these lost dreams help to account for the recurrence of trouble night after night in sleep? It seems possible.

I am reminded of a rather cruel practical joke which precipitated an attack of urticaria just as, or just after, the victim was falling asleep.

A young man of 20 went to a dance in London about 40 years ago ; while he was away, his elder brother removed all the light bulbs in the bedroom of the young man, and put an electrically-driven vacuum cleaner behind the door, connecting the flex with one of the light points as a practical joke.

When, at 3 a.m., the young man returned from the dance, he naturally tried to switch on the lights of his bedroom, but he got thereby only a siren shriek in the darkness. In those days vacuum cleaners were a rarity, and were much noisier than they are now. After a natural declamation against the crass idiocy of elder brothers, the room was soon

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returned to its normal state ; and the young man undressed and went happily to bed, feeling not at all disturbed by that startling shriek.

Just as he was dropping off to sleep, or just after dropping off—he couldn't tell which —he was woken by a sudden intense itching and tingling all over his body ; and on looking at his skin he found there was a generalised urticaria.

When questioned about it next morning, he said he hadn't any memory of being frightened by the sudden shriek, but supposed he must have been, ' else *why* the nettle-rash? ' And he himself commented on the curious fact that the nettle-rash only came on at some considerable time afterwards, and after lying placidly in bed for quite a while.

It is even possible that the attack came on only when he had been asleep for some time ; but asleep or not, the affair of the shriek was apparently well out of consciousness when the urticaria began.

The comment might be here that the urticaria only began when the young man was in bed and relaxed completely. The shock starters numbered 48 and 69 (to be found on pp. 192 and 194) show a similar effect of *relief* from anxiety.

Visions

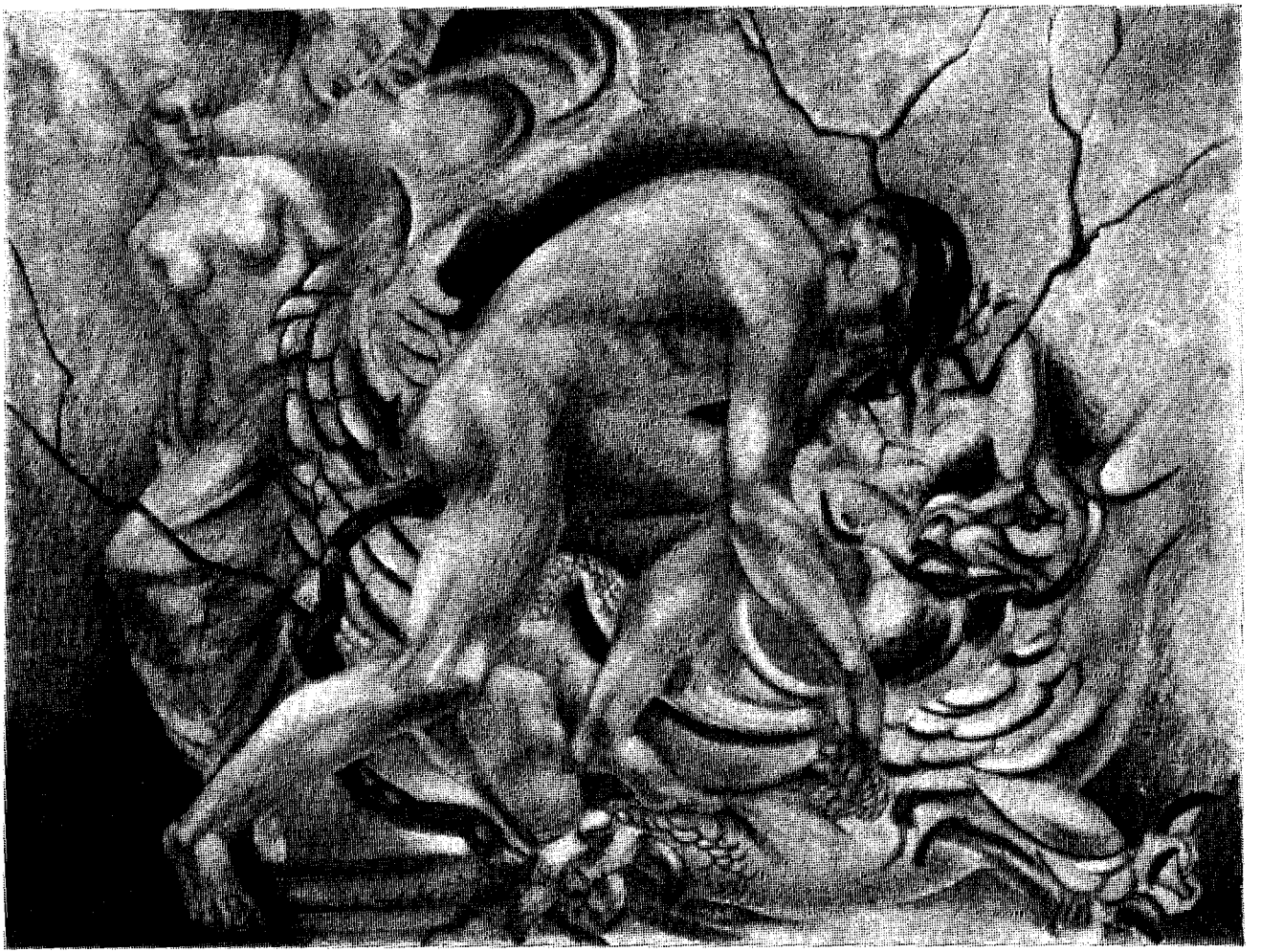
The meanings given for ' a vision ' in Fowler's *Concise Oxford Dictionary* are : ' thing seen in a dream or trance ; supernatural or prophetic apparition ; thing seen in the imagination.' Leaving out the supernatural part, we might say that ' a vision ' is a picture generated in the unconscious mind, and it emerges *thence* into consciousness, and not *via* the optic nerve ; but some degree of consciousness does seem to be necessary, and the ' unnatural ' passage from the unconscious to the conscious mind may well be what sometimes gives to visions a surprisingly horrifying aspect.

Hypnogogues. People often scoff at visions and doubt their existence. Some visions are, however, very common and not at all frightening ; in fact we are so used to them that we may never notice them. As we are dropping off to sleep most, and perhaps all, of us see a succession of visions which have been called ' hypnogogues '—i.e. ' leaders towards sleep '. Like dreams, they are not remembered unless they can be written down at once. They can only be seen apparently when the mind is balanced somewhat precariously between the consciousness of waking and the unconsciousness of sleep.

My own hypnogogues come at the rate of about six a minute, thus :

A man and a dog in a wood.
Some dark and mouldy cellar stairs.
A bottle on a long deal table.
A cat washing itself in the sunlight.
Three men sitting at a table having a dispute.
A vast nose and part of a right eye.
An indeterminate cloud, finally dissolving to show a butler doing something to the back of a heavily carved oak chair.

This kind of thing goes on till consciousness of anything at all disappears into sleep. I should explain, for the benefit of those who don't experience them or can't remember them, that these little ' visions ' are like the old-fashioned ' dissolving views ' of the magic lantern ; one picture fades into another without any volition



THIS PLATE PROBABLY REPRESENTS AN ATTACK OF ASTHMA AS SYMBOLISED IN THE UNCONSCIOUS MIND OF AN ASTHMATIC PAINTER (PP. 199 TO 202)

on the part of the observer. One picture does not call up another, but seems, to me at least, quite unconnected. So far as I am concerned, the visions produce no particular emotion, but the sleepy person lies quite placid.

Here is another sequence of hypnogogues which I have found recorded in my notes:

A camel standing on a hill-top outlined against the sky.
Fountain-pens being filled up.
A big screen composed of turkey feathers.
A rowing eight on the river.
An ice-cream in a cornet.
An old wooden pump.
A lot of children picking raspberries.
A fortune-teller with snaky hair and pursed-up lips.
Aneroid barometers hanging on a wall.
Snow on the ground in a wintry landscape.

I suppose that any one of these unsought pictures can be made symbolical of something—probably deleterious. In pursuance of the policy of not playing for safety, I make a present of them to any psycho-analyst who may chance to read these lines. To me, hypnogogues are the inconsequent fragments of the unconscious mind which chance to appear on the surface while we still retain just sufficient consciousness with which to note them, but not enough to keep them in place. With different people, I am told, they vary in intensity and in duration.

Some people see far more elaborate pictures; I have been told by Miss Mona Wilson that this is supposed by some to have been the inspiration of the curious drawings of the poet Blake, who always drew his startling pictures at night time. Whatever may have been the nature of this poet's visions, he had the necessary draughtsmanship to be able to put what he saw down on paper, and very curious and 'symbolic' these drawings were, as we all know.

Sometimes, I am told, the people in the vision speak to the observer, joining in a regular conversation. Sometimes there is nothing to be seen at all, but there is something to be heard; though whether Mr. Fowler would have let me call that a 'vision', I am not sure.

At this point I would like to refer the reader to the reproduction opposite of a painting. This is a sample of a considerable number of such paintings and drawings, some are as complete as the one reproduced here, others much less so, and some are no more than a few chalk lines on a scrap of paper which show a dragon, a man in difficulties, etc. They all represent a few of the vivid and persistent mental images which have appeared to a young asthmatic woman off and on since childhood; as there are grounds for thinking that these visions may represent asthmogenic emotions, this one is reproduced here. The observer of these visions has obvious ability in draftsmanship and painting; her father was a sculptor of note, and she herself has very successfully illustrated several anatomy books for one of the Professors of Surgery of the University of London, besides exhibiting pictures in exhibitions. She was able, therefore, to record what she saw in her mind's eye.

When this patient first came to me, she had had distressing asthma since childhood; her brother was a very intractable case of eczema, and there were other cases of toxic idiopathies in the family.

I made the customary clinical and laboratory overhauls, and decided to treat, as I often do, a chronic bacterial infection of the upper air passages ; and for perhaps a year I saw her once a week for this purpose. As time went on, this treatment seemed to have very considerable success, the asthma steadily diminished in intensity and frequency.

One day the patient came to my consulting room carrying in her hands a bundle of six rolled-up drawings of an average size of perhaps 18 inches square when laid flat. She was in a great state of excitement, with staring eyes and trembling hands, and she thrust these half-a-dozen drawings and paintings on to me, begging me to destroy them ; she *never* wanted to see them again, she said. ' Take them, oh take them away please! They are horrid—please take them away and burn them! I don't mind *what* you do, but take them away! They make me feel horrible! ' I tried to calm her, and meanwhile I unrolled some of the rectangles of brown or white paper, or of canvas, to find very strange drawings on them ; in colour or in monochrome. They were dashed off with ability but most of them very roughly.

Over the space of six months she brought me more of these pictures from time to time, and she handed them over with gradually lessening emotion ; in the end I had 42 of them. They differed very much in elaboration, and they also differed in the things depicted. There were men and women in most of them—usually a man and a woman, sometimes only a man ; and though they wore no clothes, or were only half draped, the pictures were not at all ' sexy ', and none of them was in the least pornographic—as I think my example clearly shows to the ordinary person ; but that excludes some psychologists.

Out of the first batch of six, the man was seen in one to be trying to break his way through bars ; in another he was escaping through a small hole while thousands of people watched ; but that 'escape ' idea was not repeated, I think. Very frequently there was a tree without any leaves on it : a dead tree apparently ; in one of the early drawings the man was cutting this tree down with an axe ; in another, he was digging it up with a spade, or perhaps he was planting it—it was difficult to say which. That dead tree comes into many of the pictures, and can be seen quite clearly behind the dragon in the coloured reproduction. She had at one time a phase of dragons—sometimes many of them, and sometimes only one, as in this coloured picture.

I asked for the history of these drawings, and was told that they were all visions which stayed with her for days, or even weeks, till she had got rid of them by ' sicking them up on to paper '—her own words ; in fact, she constantly referred to them as her ' vomits '. She declared that the visions were never consciously composed, but flashed fully formed into her sight ; they did this in the most queer and unexpected places—in a bus, in church, in a bath, and so forth. These prefabricated visions were always to her utterly disgusting and unwanted, and she got a wonderful feeling of relief from a ' vomit ' of them ; in fact, from her description, she got the relief we all get on vomiting after a long period of nausea. Naturally I did not hear this at receiving the first batch of six ; but, as I've said, she became more easy-minded on subsequent occasions. On repeated questioning, she was quite positive that the visions were never consciously composed, they stayed

unaltered for considerable periods till she ' vomited ' them. They were always loathsome to her—I should say far more so than the content of the pictures would seem to justify ; but then I have never, I believe, had a dragon loose in my brain. I must add that this patient is a particularly good witness, very matter-of-fact and exact in her statements.

As some at least of her ' horrors ' had obvious artistic merit, I asked her one day if she had used them in her profession. ' Oh yes,' she told me, ' I got one of them, one of the least horrible, into the Spring Exhibition of the Royal Academy when I was only twelve years old.' ' Did you touch it up for exhibition? ' I asked. ' I tried to, but I only made it worse ; it was best just as it came to me in the vision.'

All that has an obvious bearing on the nature of artistic inspiration, which, however, is not our business here. I believe the same to be true of poetry (cf. Kubla Khan, which was almost literally a ' pipe dream '), but that is another story.

As time went on, the patient's asthma tended to get better, and the visions to get worse ; but there were fluctuations in both processes, and I found that an improvement in asthma as a rule coincided with an exacerbation of the visions, and vice versa.

I said nothing of this to the patient ; but for perhaps two months more I watched this interchange (as I considered it) of asthma and visions. In the end she told me she had noticed it herself : ' I find that now my asthma is getting better my horrors are getting worse, and I find them particularly upsetting. I should very much prefer to keep my now quite tolerable asthma in exchange for worsened horrors.' So we stopped treatment.

I once invited three psychiatrists to see all these drawings of visions. I got no very good explanations from them, though they agreed that they were psychologically interesting. One of the three ventured that the dragon in the painting (opp. p. 199) was the emotional self of the patient, that the man drooping over the dragon was the patient failing to cope with her emotional self, that the figures behind were again the patient at different removes watching herself, watching this watching of failure to cope—and so forth. Perhaps ; but the other psychiatrists made no comment. I think there was general agreement that the pictures were full of symbols which kept on recurring in drawing after drawing, but these symbols were new to them and quite unorthodox and non-Austrian.

I have for the last two years had the original of the painting here reproduced propped up in a conspicuous place in my consulting room to see what unsolicited comments I might get from patients and their friends. Mostly, of course, those who made any comments at all were chiefly anxious ' to say the right thing " but quite often I got interesting remarks. People commented very frequently on the Blake-like character of the drawings, as if they were visions or dreams ; they have been so consistent in that comment that I began to wonder if by chance the poet William Blake had had asthma or any other of the toxic idiopathies.

Blake was, on the whole, a remarkably healthy man ; but then so also are the toxic idiopathy people. He had an idea that the atmosphere of any place north of the Thames was unsuitable for him, causing ' gastric attacks ' ; that apparent effect of geographic ' atmosphere ' is, as we have seen in Chapter IX, a frequent

feature in many asthmatic stories, but one could not make a positive diagnosis of a toxic idiopathy on that alone, I fear. If diagnoses of, say, hay-fever are often erratic now, diagnoses at the end of the eighteenth century were even more peculiar—so we don't know what these ' gastric attacks ' of Blake's could have been.

I have said above that most *ordinary* people would call this picture by my asthmatic patient very definitely *not* erotic. I put in that proviso of ' *ordinary* ' because it seems that some psychologists are able to call almost anything erotic. I showed that picture to a lady who had no medical qualifications but who claimed to have sat at the feet of the great Freud himself. Her immediate comment was : ' Of course the picture simply *reeks* with sex.' When I dissented from this, she went on to claim that the so-called ' modesty ' in not underlining sex characteristics went to show that sex was an obsession in the patient's mind. I am afraid I regard that remark as typical of some psychologists' double-dealing methods.

Though this lady had had no medical training—beyond, that is, ' sitting at the feet of Freud '—she was so excited by what she called ' the sex obsessions ' shown in the picture that she offered to psycho-analyse the painter free of charge. I declined the offer on my patient's behalf ; could anything be more like a mischievous but ambitious small boy wanting to mend a school-mate's watch with a broken pen nib. Such a boy may learn something about watches—or he may not ; he will not necessarily improve that watch.

Safety Valves. It is a favourite tenet of some specialist psychologists, I find, that the toxic idiopathies act beneficently as safety valves, outlets for suppressed emotion. That seems to be nonsense for most of the toxic idiopathies. It would seem to mean that our patients must be in better mental health when they have, say, asthma or hay-fever, than when they have not got these diseases. That does not at all square with the usual clinical observation that the asthmatic patient is an absolutely normal person, when *not* having an attack.

Consider the hay-fever subject : during the ten months of the year when there is no pollen about, he is sound in mind and body—so far as the ordinary doctor may observe ; but patients are apt to become ' neurotic wrecks ' when they are having their hay-fever at midsummer. There are, however, some cases which do corroborate this safety valve idea of the psychiatrists ; they are few in my experience, but this case of the artist lady with the visions does seem to be one of them.

Here is more evidence which might be adduced in support of the safety valve theory of the psychologists :

Twenty-five years ago, an elderly woman had a very little asthma, but her chief trouble was some very bad eczema round the neck. She came to me from the skin department, and I was asked to see what I could make of the case.

She was admitted to the Laboratory Wards, and a search was made for causal factors, but without very much success (I'd say, nowadays, that an insufficient investigation was made concerning gut sepsis).

In my notes, I see I described the skin of the neck as ' looking like pork crackling ', and said it was dripping with serous exudations. In short, the neck was extremely unsightly, and the patient *professed* to be very disturbed lest anyone should notice her affliction. My very experienced Ward Sister, however, found that the patient resented her

neck being covered up from public view, and seemed anxious to parade it in the ward. She seemed to enjoy her unsightliness.

So, too, with her asthma. On several occasions I was called to the ward to find her in a hysterical state, and stertorously gasping for breath, though there was good air entry, the heart was apparently normal, and her temperature was normal too.

The patient was clearly in an unstable mental state, and subsequently went off her head, needing a special nurse in the ward. While she was in this condition, the eczema, to quote my notes, ' cleared up like winking.'

After a time, the patient regained sanity, and as she became mentally more normal, so the eczema returned. This suggests that the eczema, and perhaps the asthma too, gave relief from some mental strain by acting as a safety valve.

De minimis non curat lex, but any sudden discrepancy in a general rule may lead to something, and so seems worth following up. It occurs to me that those safety valve cases of the psychiatrists and the ordinary run of the toxic idiopathies may not be quite in the same category.

I have recorded (see p. 135) two cases of epilepsy which were changed into asthma by whooping cough trauma. Would it be stretching the language to say that here asthma acted as a safety valve for the mental disturbance of epilepsy? I think perhaps it would—we must not press these metaphors of safety valves too hard ; but it is an interesting idea that the idiotoxic response, if given a better opportunity of manifestation, may change its locality and organ, and so its type.

A Bridge?

Is a bridge—an intellectual bridge—between the Ordinary Doctor and the Psychiatrist needed? And, if so, is it now possible to build one, at least with regard to the diseases discussed in this book?

The ordinary doctor does not, as a rule, dispute the importance of *psychology*—meaning thereby ' the science of the nature, functions, and phenomena of the human soul or mind ' ; but will he, to put the cat well among the pigeons, equally grant the importance of psychologists and their views?

As we know, not all psychologists are medically qualified ; those who are, and see patients for diagnosis and for psychological treatment, prefer to mark the distinction by calling themselves psychiatrists. We must remember to do so too. They would agree that every psychiatrist must be a psychologist, for presumably psychology is the science on which they base their medical work. I seem to detect, however, in some psychiatrists, a slight resentment at being called psychologists as if these were of a lower intellectual grade, as if that science was a shade phoney.

It is no good blinking the fact that the average ordinary doctor pays very little attention even to the psychiatrists. When they make statements which, as he would say, conflict with his common sense, he is inclined to laugh, shrug his shoulders, and pass by on the other side.

When this happens, it is customary for those psychiatrists who take any notice of the ordinary doctor to smile at him a little among themselves. ' If they only knew, as we know, what their " common sense " was worth ; if they could only understand the workings of their own minds! ' I have heard the less charitable

feature in many asthmatic stories, but one could not make a positive diagnosis of a toxic idiopathy on that alone, I fear. If diagnoses of, say, hay-fever are often erratic now, diagnoses at the end of the eighteenth century were even more peculiar—so we don't know what these ' gastric attacks ' of Blake's could have been.

I have said above that most *ordinary* people would call this picture by my asthmatic patient very definitely *not* erotic. I put in that proviso of ' *ordinary* ' because it seems that some psychologists are able to call almost anything erotic. I showed that picture to a lady who had no medical qualifications but who claimed to have sat at the feet of the great Freud himself. Her immediate comment was : ' Of course the picture simply *reeks* with sex.' When I dissented from this, she went on to claim that the so-called ' modesty ' in not underlining sex characteristics went to show that sex was an obsession in the patient's mind. I am afraid I regard that remark as typical of some psychologists' double-dealing methods.

Though this lady had had no medical training—beyond, that is, ' sitting at the feet of Freud '—she was so excited by what she called ' the sex obsessions ' shown in the picture that she offered to psycho-analyse the painter free of charge. I declined the offer on my patient's behalf ; could anything be more like a mischievous but ambitious small boy wanting to mend a school-mate's watch with a broken pen nib. Such a boy may learn something about watches—or he may not ; he will not necessarily improve that watch.

Safety Valves. It is a favourite tenet of some specialist psychologists, I find, that the toxic idiopathies act beneficently as safety valves, outlets for suppressed emotion. That seems to be nonsense for most of the toxic idiopathies. It would seem to mean that our patients must be in better mental health when they have, say, asthma or hay-fever, than when they have not got these diseases. That does not at all square with the usual clinical observation that the asthmatic patient is an absolutely normal person, when *not* having an attack.

Consider the hay-fever subject : during the ten months of the year when there is no pollen about, he is sound in mind and body—so far as the ordinary doctor may observe ; but patients are apt to become ' neurotic wrecks ' when they are having their hay-fever at midsummer. There are, however, some cases which do corroborate this safety valve idea of the psychiatrists ; they are few in my experience, but this case of the artist lady with the visions does seem to be one of them.

Here is more evidence which might be adduced in support of the safety valve theory of the psychologists :

Twenty-five years ago, an elderly woman had a very little asthma, but her chief trouble was some very bad eczema round the neck. She came to me from the skin department, and I was asked to see what I could make of the case.

She was admitted to the Laboratory Wards, and a search was made for causal factors, but without very much success (I'd say, nowadays, that an insufficient investigation was made concerning gut sepsis).

In my notes, I see I described the skin of the neck as ' looking like pork crackling ', and said it was dripping with serous exudations. In short, the neck was extremely unsightly, and the patient *professed* to be very disturbed lest anyone should notice her affliction. My very experienced Ward Sister, however, found that the patient resented her

neck being covered up from public view, and seemed anxious to parade it in the ward. She seemed to enjoy her unsightliness.

So, too, with her asthma. On several occasions I was called to the ward to find her in a hysterical state, and stertorously gasping for breath, though there was good air entry, the heart was apparently normal, and her temperature was normal too.

The patient was clearly in an unstable mental state, and subsequently went off her head, needing a special nurse in the ward. While she was in this condition, the eczema, to quote my notes, ' cleared up like winking.'

After a time, the patient regained sanity, and as she became mentally more normal, so the eczema returned. This suggests that the eczema, and perhaps the asthma too, gave relief from some mental strain by acting as a safety valve.

De minimis non curat lex, but any sudden discrepancy in a general rule may lead to something, and so seems worth following up. It occurs to me that those safety valve cases of the psychiatrists and the ordinary run of the toxic idiopathies may not be quite in the same category.

I have recorded (see p. 135) two cases of epilepsy which were changed into asthma by whooping cough trauma. Would it be stretching the language to say that here asthma acted as a safety valve for the mental disturbance of epilepsy? I think perhaps it would—we must not press these metaphors of safety valves too hard ; but it is an interesting idea that the idiotoxic response, if given a better opportunity of manifestation, may change its locality and organ, and so its type.

A Bridge?

Is a bridge—an intellectual bridge—between the Ordinary Doctor and the Psychiatrist needed? And, if so, is it now possible to build one, at least with regard to the diseases discussed in this book?

The ordinary doctor does not, as a rule, dispute the importance of *psychology*—meaning thereby ' the science of the nature, functions, and phenomena of the human soul or mind ' ; but will he, to put the cat well among the pigeons, equally grant the importance of psychologists and their views?

As we know, not all psychologists are medically qualified ; those who are, and see patients for diagnosis and for psychological treatment, prefer to mark the distinction by calling themselves psychiatrists. We must remember to do so too. They would agree that every psychiatrist must be a psychologist, for presumably psychology is the science on which they base their medical work. I seem to detect, however, in some psychiatrists, a slight resentment at being called psychologists as if these were of a lower intellectual grade, as if that science was a shade phoney.

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psychiatrists say : ' If they could only know how discreditable their real motives are for thinking as they do ! '

The ordinary doctor, if he hears of this condescension, may retort angrily that he hasn't observed that psychiatrists' brains are necessarily any better balanced than his own ; he does not believe that analysts produce acuter brains by what he calls the mumbo-jumbo of analysis, but rather the contrary. At which point there is nothing for the psychiatrist to do but stick out his tongue—in a dignified way, of course.

Have I exaggerated the gulf between these two? I don't think so. It is customary for practitioners, when introducing a new case to a specialist, to have, say, five minutes of conversation with him first before producing the patient. From time to time it is my business to ask what their experience of psychiatrists' ' interference ' has been in similar cases (I use the word ' interference ' of course in the sense in which one speaks of ' surgical interference '). It is arguable that practitioners who send me a case must think that the psychiatrist's powers are strictly limited in dealing with the toxic idiopathies, or naturally that patient would be going to them and not to me ; thus the practitioners I meet may to some extent be selected with an extra anti-psychiatrist bias. What these practitioners tell me cannot all be accounted for in this way : though on occasion I have heard from them of successful treatments, these have been exceptional ; doctors usually say something like this : ' Yes, I have sent several of my patients to a psychiatrist, but no good has come of it,' or, ' They seem to confine themselves to telling small children the Facts of Life as a cure for everything,' or, ' I have only had one case that analysis seemed to help, and I am doubtful of that one.'

I remember one (to me) very intelligent doctor who explained such a success as follows :

' If a man, who likes talking about himself, is ordered to do so steadily for an hour, day after day, at a guinea a session, and if he can be persuaded to go on long enough with this, he must in the end get bored, even with *that* subject. That is all to the good ; he may possibly then turn his mind to something more important. It is like the satiation said to be produced in people who work in sweet factories by allowing them to eat as much as they like of the sweetmeats.'

Sometimes those practitioners will keep patients waiting in the waiting room an extra minute or two to tell me comic tales at the expense of the psychiatrists. Thus, fat little Tommy Brown is having nightmares and wetting his bed because he wants to murder his father, or unknown to himself, he has an incestuous desire to sleep with his Great Aunt.

Here is one such tale as told me by a practitioner I have known for forty years :

I once took a rather lazy but quite healthy public schoolboy to a noted psychiatrist at his parent's urgent request. They wanted to know why he was not getting on with his school work as well as his Form Master thought he should.

After various leading questions which the boy evidently didn't understand, the great man fixed his piercing eyes on him, and said abruptly, ' Do you masturbate? '

The boy answered cheerfully, ' Why, of course I do! Everybody does.' Then he added conscientiously, ' But perhaps not as much as you do, Sir.'

The psychiatrist flinched rather, but asked faintly, ' How much *do* you masturbate? ' ' Well, Sir, not as much as Mr. Gladstone.' Then, seeing the man's bewilderment, he added, ' Not as many as forty-nine bites with each mouthful, I'm afraid.'

We should know by now that different psychologists speak with different voices ; but as the psycho-analysts are much more colourful in their statements about patients, they naturally attract most attention—and most distrust ; so in general I think it is true to say that the ordinary doctor, thinking perhaps chiefly of the psycho-analysts, usually treats the psychiatrist as something of a joke ; he doesn't really believe in all these strange things which are said to have been dredged up from the unconscious by analysis. He may declare (I have often heard him doing so) that for him this so-called science of psychology is little better than a string of hypotheses—some plausible, but most of them improbable or ridiculous.

This attitude of the ordinary doctor is unfair—at least to many psychiatrists, and, I am told, particularly to the more modern and moderate of them. As the misunderstanding is retarding the advancement of medicine, it seems worth while to see, if we can, how it all comes about. We don't talk in that way of chemists, or physicists, or physiologists : why, medically speaking, are psychologists so often regarded unfavourably?

(i) **The Archipelago.** The first difficulty we have to face is that psychology, the science on which, as I have said, psychiatrists must found their work, is admittedly very far from being an exact science. It seems to be partly religious, partly a branch of physiology, and also for many millions of people a rather salacious (and so entertaining but reprehensible) topic of conversation. But the splitting goes further than that ; if you question the different exponents of psychology, you are shocked to find as many kinds of belief among them as there are sects of Christianity in Scotland—or, for the matter of that, in many villages and townlets in England. In short, a good deal of psychology seems to be more a string of religious beliefs than any science. All this makes it very difficult to come to terms with ' the psychologists ' : one is not attempting to bridge to a mainland but to an archipelago.

One of the younger and, to me, more sane of the psychiatrists, after looking over my shoulder at the above, delivers himself as follows :

> ' The Bridge is of fundamental importance and urgently required. I agree with the concept of an archipelago, but feel that it is surely rather unkind to emphasise the gulf between psychiatry and the mainland of general medicine by focusing on the most distant and smallest island—The Analytic Isle! It is after all inhabited by a small, often mystic, sect ; the main islands of psychiatry are much closer to the mainland. . . . If you could send out a span of your bridge to the main island in the archipelago, perhaps the furthest isle may one day submerge under the effect of one of its own volcanic eruptions.'

This at least makes clear the archipelagian nature of psychology in the minds of some psychiatrists. With regard to the ' Analytic Isle ', this must not be neglected by us when attempting to come to terms with the psychiatrists, because analysis is perhaps the chief psychological bugbear for the ordinary doctor, as I have said above.

Sometimes I have gone to a lecture on the subject of psychology and have got a restless feeling at the apparently unsupported assertions I heard ; it has the disturbing effect most of us have experienced on listening to an earnest but not very expert sermon in church. You feel you want to ask what is the evidence for *that* statement, or to say that such and such an explanation is not at all convincing.

I was invited by two psychologists, husband and wife, to go and hear a lecture given by the pre-eminent psycho-analyst, Dr. Ernest Jones. I was surprised on arrival, and so, I think, were my friends, to find that the lecture was to be about hay-fever—an interesting choice for me.

Dr. Jones lectured most eloquently for, I think, three-quarters of an hour, and he mentioned very many things, including the reputed non-existence of the Queen of Spain's legs ; but he never mentioned that grass pollen had anything to do with hay-fever.

When I got up to mention this item of grass pollen, he expressed polite surprise at my information, as if he thought it perhaps irrelevant ; but he readily accepted it, saying to his faithful flock of listeners : ' That only shows the importance of what I have always told you : you *must* consider the physical aspects of these neuroses as well.' He then explained them to his followers in a way that did not, in my opinion, square with the facts, and I had to tell him of skin tests. This again was explained away on a purely psychological hypothesis, and I had to tell him of the Prausnitz-Kurtner reactions (see p. 241), which cut right across his explanation.

With increasing reluctance I got up to speak seven times in all. When I looked round at all those shocked but earnest faces which surrounded me, I felt as if I were being rude enough to heckle a bishop in his own cathedral on why there had never been a rainbow in the sky before the Flood (Genesis IX, v. 11-17). In short, I felt guilty of bad manners, not of bad science.

Clearly, Dr. Jones and I had not got the same objectives ; he wanted faith and an attitude of mind : I wanted physiological fact.

(ii) **Salacity.** When Viennese psychology arrived in Britain, it found a comparatively sex-starved nation—that is, as judged by Central European standards. There was, for example, a good deal of monastic seclusion at the Universities. I should say that undoubtedly many psychologists cashed in on this situation for its advertisement value ; as a Lancet article once remarked, sex perversion and incest seemed to have become a favourite topic of conversation at Bloomsbury tea-tables. This social acceptance by the intellectuals was to the psychologists' immediate advantage certainly, but I think to their subsequent detriment.

When a psychiatrist of Central European extraction came to my allergy clinics to see what he could make of them, and to instruct me in his science, I asked him, because of this salacious reputation of psychology, not to talk too much or too specifically about his special investigations, or he'd get the wrong kind of interest taken in him by the patients. Soon I noticed there was a stir of excitement among the more flighty of the young women patients, and they began whispering among themselves and giggling at the chance of being ' psycho-analysed '. I didn't like this very much, but said nothing about it ; soon my various assistants and students told me of it too. Indeed, some of the more adventurous ladies began to demand to be ' done ' by the psychiatrist. I believe this young psychiatrist was very discreet in what he asked of the patients, concentrating at first on the patient's life as a small child, etc. ; he was merely suffering from the popular reputation that psychology had by then acquired.

In the end, this worker convinced himself of the general abnormality of my patients' mental make-up, but failed to convince anyone else among us ; indeed, he seemed to me to produce with enthusiasm a series of mare's nests.

For instance, he told me excitedly—' That poor girl I have just been examining is living under a terrific mental strain : quite enough to cause a collapse. She is not at all sure that her fiancé loves her as much as he used to do! '

I'm afraid I told him that probably seven million women in England were in that particular plight at the moment ; and if he added all the wives, and all the men married and unmarried, he might count on half the population at least being in that state.

(iii) **Fluidity of Classification.** Even if we eliminate, so far as is practicable, the religious element of psychology and its occasionally queer obsession in sex also, many psychiatrists seem to us to be particularly prone to the Fallacy of the Single Cause. It seems as if to *them* the toxic idiopathies are merely symptoms consequent on the anxiety neuroses.

I have related above how Dr. Ernest Jones once took hay-fever for his subject when lecturing on these anxiety neuroses. Good enough : we have been finding hay-fever to be a useful try-out all through this book ; let us keep to it here if we are to attempt any bridge-building between ordinary medicine and the psychological archipelago, let us see how far hay-fever will serve as a handrail to our bridge. So far as I am concerned, if we can see clearly about that, we shall have clarified the whole business.

Do not let us be too captious, however. I hold that it is foolish for psychiatrists to believe that some mental lesion, guessed at by them but unknown to us, is the sole cause of hay-fever, and many of the psychiatrists would think so too. But, if they do believe such an alleged mental lesion to be a cause at all, it is their right—their duty, in fact—to work along those lines for the amelioration of the patient's troubles, if psychiatry is to have any say in the treatment at all.

Dr. Ernest Jones has told me that by psycho-analysis he himself could always tell in January whether the patient will, or will not, get hay-fever next June ' if only that analysis is thorough enough '. Of course, if a patient under analysis happened to mention that he sneezed and had sore eyes from mid-May to mid-July each year, even I could do that piece of analysis. I think it is fair to say that Dr. Ernest Jones thought of hay-fever as a rhinorrhoea consequent on a mind lesion ; and, though he is by no means representative of all psychiatrists, I find he does represent most of them in that idea. He obviously can't make his conception of hay-fever tally with mine because apparently he didn't know, or would not accept, the role grass pollen plays in this disease.

On my side, I confess I was very surprised to find in 1911 that the ordinary emotions *could* play any part in the production of classical hay-fever ; I was still more surprised when, about twenty years later, I gradually came to the conclusion that ' emotions, moods and tensions ' probably *always* played a part in its production. How can we make these two views, i.e. Dr. Jones's and mine, true up to one another?

Let me put the difference as clearly as I can. Psychiatrists postulate a mental lesion which produces hay-fever. On the other hand, I find that hay-fever is influ-

enced by ordinary emotions or moods, and see no evidence for this pre-existing mental lesion, at least so far as the vast majority of my cases are concerned.

A possible explanation of this discrepancy is that we tend not to see quite the same run of cases. I see and treat every spring some thousands of hay-fever cases : these all give a marked reaction to grass pollen on the skin—or they wouldn't get treated. Psychiatrists see an equal number of psychotic cases—i.e. cases judged to have some mental screw loose because of their behaviour ; of these, some few are said to have hay-fever (which, however, is often not very exactly diagnosed). Psychiatrists say that when this happens the hay-fever is ' caused ' by a mental lesion. If they said ' probably or possibly influenced by the mental lesion, if one exists ' I would agree ; when they tell me that every hay-fever patient has a screw loose, I most emphatically disagree with them ; if their theories lead them to say so, those theories must go.

It seems to me that psychiatrists are trapped by their belief that, say, the disease of hay-fever is merely the reflection of some mental trauma, therefore there must be a trauma to be reflected ; *Q.E.D.* My experience is that they briskly react if you tell them there is no trauma at all—as the following story exemplifies :

I remember going to a meeting of psychiatrists at No. 11 Chandos Street and listening to a string of papers showing how psychical trauma causes the anxiety neuroses.

After listening for some time, I summoned up courage to ask if hay-fever was a good example of anxiety neurosis. On being told that it was, I said that for very many years I had seen hay-fever cases in March, April and the first half of May for prophylactic treatment, i.e. before the grass pollen cloud of midsummer could possibly be affecting them. During this time, and indeed at all times outside the hay-fever season, their minds were perfectly normal, though in the hay-fever season they frequently became quite neurotic. So apparently their neurosis was due to the hay-fever and not their hay-fever to the neurosis. I ended by saying : ' In fact, outside the hay-fever season my hay-fever subjects are just as sane as I am! '

At this, my friend Dr. Crichton-Miller said with a laugh : ' Well, no one could call *you* quite sane, Freeman! ' So I changed my assertion to ' just as sane as all you gentlemen are '.

Whereupon half a dozen colleagues rose to their feet to tell me that ' no one considered most psychologists to be quite sane '.

In more serious vein I was then told that no mind was quite sane or normal, and my hay-fever patients couldn't expect to be more sane than the rest of humanity.

Thus my protest was laughed out of court, and the meeting continued to consider the more agreeable subject of the neuroses.

I think, if we winnow the corn of the psychiatrists' argument from their chaff, it would not be unfair to state their argument, given above, thus :

> All patients are, or may be, neurotic in some way or other.
> Some patients have hay-fever.
> Therefore hay-fever may possibly be due to a neurosis.

True : but I would commend in exchange the parallel argument :

> All patients have, or should have, two feet.
> Some patients have hay-fever.
> Therefore hay-fever may be due to their two feet.

That is equally true ; but I wouldn't propose to work on that hypothesis.

Psychiatrists naturally doubt the ordinary man's capacity to detect a mentally abnormal case ; but their argument goes overboard when they are driven to claim that hay-fever cases *must* be mentally abnormal because everyone is abnormal. It all boils down to what one can consider ' normal '. True, no two human beings are alike, either in mind or body ; therefore any one person's mind is different (slightly) from everybody else's mind. But that is true, too, of every heart or every right hand : the ordinary doctor would say I was talking utter nonsense if I said there could not be a normal heart or normal hand because all are slightly different.

The truth is, of course, that the psychiatrists know very much less about the physiology of the human mind than we all know of the body—and who shall blame them for that. In the days of my youth imaginative novelists could safely place beautiful white queens in the centre of unexplored Africa and no one could contradict them. The human mind is an equally unexplored area and many a queer theory can be found nesting there. If the psychiatrists like to believe that hay-fever depends upon some slight accidental difference between one mind and another, we needn't be concerned to deny it—or to prove it. That is a long way from postulating a neurosis to account for every case of hay-fever.

How do Moods work. We have seen almost *ad nauseam* in Chapters IX and X that mothers, if their children are of an idiotoxic diathesis, can make them much more liable to attacks by expending all their maternal instincts on their minute families and giving their children a cloying amount of loving care—by unmitigated fussing, in fact. Why should the psychiatrists look for something wrong in the mind of the child? It seems to me to be almost as natural for children to be irritated or perturbed by constant fussing as for them to jump up again if they chance to sit down on a tintack. Are we to believe that the tintack produces a central trauma?

I think we are all of us afraid to face the fact that we can have too much of some kinds of ' love '. Clearly, if something goes wrong there must be a fault somewhere ; so in the sacred name of motherhood we blame the child and look for a mental lesion which can be winkled out. Or is the ' lesion ' in the child's mind a diplomatic or Pickwickian lesion designed to soothe the mother while the psychiatrist turns his attention to curing *her* behaviour? If so, the psychiatrist may perhaps deceive the mother, but he mustn't deceive himself—and us.

How do Emotional ' Starters ' work ? I asked a psychiatrist who believed in the pre-existing neurosis theory how all the emotional starters, mentioned on pp. 191 to 195 of this appendix could produce this neurosis in time to launch an attack. Take the young woman onto whose head an apple fell from a tall tree (No. 87, p. 194), or take the case of the two-and-a-half year old baby who got asthma almost immediately, and for the first time, after putting a spoonful of freshly made mustard into its mouth (No. 14, pp. 142 and 191). His explanation was that these accidents did not start an attack by themselves, but woke up a pre-existing mind lesion, and it was always this previous trauma which started the attack and not the emotions produced by the accident. I have heard likelier theories than that.

It would be quite ridiculous for me to say that no mental lesion could possibly accentuate an attack of a toxic idiopathy, or make it more likely of occurrence ;

that is precisely what one might expect it to do by influencing the patient's moods or emotions. It would be equally ridiculous in my opinion to postulate a mental lesion because a toxic idiopathy has occurred—that would indeed be twisting facts to fit theories.

This leaves it still open to us to say that the psychiatrists and the ' allergists ' are not seeing quite the same run of cases, and that may help to explain the differences of the two points of view when diagnosing and treating hay-fever. Psychiatrists naturally see psychotic cases—some few of which are hay-fever also : I see hay-fever cases—some few of which are psychotic also. Looking down the list of shock-starter stories given earlier in this chapter, I'd say that the men in stories 58 and 64, p. 193 had some considerable mental trouble possibly, but such cases seem to be the exception.

Evidence by ' Cures '. How comes it, we may be asked, that ordinary cases of hay-fever are reported to have been cured by psycho-analysts when they have removed the alleged psychical trauma. In my experience these cases are very few, and might well be mentally abnormal : if there really was a neurosis in any way increasing the attacks, its amelioration would presumably diminish those attacks.

Suggestion. Outside this, a chance mental trauma complicating hay-fever, it seems to me that none of the psychiatrists' technique can escape the strong probability of suggestion. That is a perfectly reputable psychiatric method of treatment, but it does not prove the presence of mental traumata. Or does it, to psychiatrists' thinking? It wouldn't to mine.

The psycho-analysts claim that if they put their patients into a dim room, with grey painted walls, and no pictures or ornaments, and no furniture save a couch for the person operated on and a chair for the analyst, they have eliminated all suggestion. That is not so : such a setting has most of the mumbo-jumbo of a spiritualistic séance. Monsieur Coué apparently modified a hay-fever case by suggestion (see p. 139), and one might assume that the analysts might do so too—on occasion. I have been accustomed to say that if only we could chalk the noses of our patients blue with sufficient ' scientific ' enthusiasm, and with sufficient belief in our chalk, we should help many of them. I have helped them with a meticulously measured $\frac{1}{2}$ c.c. of normal saline injected into the median basilic vein (see p. 155)—and that certainly was deliberate suggestion.

It is only fair to say here that any sufficiently impressive treatment must often depend in part at least on suggestion. The strange ' reactions ' that occur after injecting normal saline subcutaneously are well-known ; and I feel sure that part of the success of my P.T.D. treatment is due to faith. This operates particularly strongly, of course, in a hospital clinic waiting room, with the old cases telling the new ones, ' My dear, he's perfectly *wonderful*! I had a *marvellous* time last summer, with never a sneeze.' In fact, nothing succeeds like success.

Summary. We have seen that the attacks of the toxic idiopathies are influenced by psychological considerations, and enough has been said to show that some sort of intellectual bridge is needed, as between the psychological specialists and the

rest of us non-psychologists. If, because of their diversity, it is impossible to reach all the islands of the psychological archipelago, we can possibly reach the nearer and probably more important islands, inhabited for the most part by the younger and more modern of the psychiatrists.

We ought not to equate ' the allergic disorders ' with ' the neuroses '. The attempt to do so springs in part from the unfounded belief that all medicine, whether of mind or body, has been neatly mapped out into separate disorders which are valid for everyone. They are not.

When a psychiatrist says ' hay-fever ' he probably means quite a different thing to what I would mean : he means, I think, a neurosis which happens to have the end-effect of eye and nose trouble ; I mean an abnormality of cyto-serous reaction which makes grass pollen into an oedema-producing poison. We are surveying medical territory from very different standpoints ; we don't see the same lie of the land that the psychiatrist sees.

May I add an aside to my fellow ordinary doctors : let us all remember that psychology, that *enfant terrible* of the sciences, is still very young and innocent really—in spite of its occasionally dreadful language.

CHAPTER XII

CAUSE G: THE BACTERIAL FACTOR

Bacterial Infections. Apart from anything we may have been taught in the books about asthma, etc., it has needed very little practical experience of our clinics for Allergic Disorders to demonstrate conclusively that bacterial infections can often influence the onset, intensity and termination of an attack of some toxic idiopathy.

Bacteria, or their products, not only influence these spontaneously occurring toxic idiopathies, but they influence also reactions from P.T.D. doses: those artificial toxic idiopathies which can be provoked by an inaccurate or inopportune desensitising injection of any idiotoxic vaccine (such as an extract of grass pollen for hay-fever).

Bacterial poisonings also alter the diagnostic skin tests: i.e. they alter the extent of that small patch of artificial dermatitis which may result from any of the customary prick tests on the skin.

Because of this importance of infections in the causal machinery of the toxic idiopathies, bacterial investigation and subsequent anti-bacterial treatment have taken up a good third of our time in the allergy clinics; therefore, in giving any account of my stewardship therein, I must ventilate so far as I can this matter of bacterial causation.

Then I will end the chapter with some description of the bacteriological and immunological technique we have employed in diagnosis and treatment.

An Extreme Case. Let us take first a case where an unsuspected but severe bacterial infection undoubtedly played an unexpected and dramatic part in idiotoxic phenomena.

Britannia Girl. I remember seeing a square-shouldered, well-set-up young woman with a fresh complexion sitting in the waiting hall. 'Looks like Britannia on a penny', I commented to myself approvingly as I hurried past her to my consulting room.

When she told her story, it suggested a straightforward hay-fever. The pollen prick testing gave a huge wealing response on the forearm which was altogether too big to be natural; it extended far beyond the narrow confines of the prepared microscope slide on which I habitually trace the silhouette of the diagnostic weal. The swelling, in fact, proceeded almost to oedema of the arm.

In view of this excessive wealing, I had a discussion with a colleague as to how much he should give for the initial dose of the P.T.D. course we had decided on. In place of the customary 40 units we gave 20 only.

That evening I was rung up from the house in which the girl was working; she was in bed with a high fever, her face was swollen, and she looked very ill. When I examined her throat, which of course I ought to have done five hours previously, her tonsils looked like mouldy cauliflowers which might be removed with a spoon. Her inflamed face looked remarkably like an erysipelas.

The story, which she ought to have told me in the first place, was that she had been in bed for a week with a raised temperature and a bad tonsillitis. Her G.P. who had been looking after her for this, when questioned by her about her coming hay-fever, had advised her to come to me ' as soon as she was able to do so '. She had obeyed him literally : as soon as she could manage to walk, she came. I found that her country complexion had come out of a box ; her set jaw and square shoulders were merely an expression of her grim fortitude in face of extreme exhaustion.

It was difficult to persuade this very determined young woman to come into hospital at all. When I did, my E.N. & T. colleagues got her throat straight. We taught her the art of self-inoculation against hay-fever, and during the few days that she consented to remain with us she gave herself four doses of the P.T.D. course per day. When she got home again, she went straight through the course up to the 100,000 unit dose without any trouble at all, and had a summer clear of hay-fever.

We can't doubt that with this girl the bacterial poisons augmented the skin reaction out of all recognition, and made even that very cautious first dose too big. Were the bacterial substances functioning as an idiotoxin? I think so, though no doubt (as I shall explain) they may very well have been acting in other ways to produce the effect they did.

Average Cases. The above is admittedly an extreme case. Let me instance now a very common starting point and ' cause ' for asthma. Patients are constantly telling me that their asthma comes, and only comes, after a sharp cold in the head ; and they may add that if only they can keep clear of these colds, they have no trouble at all. People make statements like : ' I never have any asthma in the summer because I don't get colds then,' and ' I did not have any asthma at all when I wintered cold-free in the South of France '.

Here is a sample doctor's letter sent with a boy coming to my hospital allergy clinic :

> This boy has had attacks of asthma for the past 3½ years, and they are becoming more frequent and more severe : typically, the attacks start as an ordinary cold. The temperature rises to 104° F. and over. Breathing becomes acutely distressed, often he is cyanosed with a loose frequent cough.
>
> After two or three days the temperature falls and he rapidly recovers, although the tight audible breathing persists for a week or more.

We all know, of course, that the common cold is not a definite clinical entity in that many different bacteria or viruses, or rather combinations of them, may be part of the causal machinery ; but can we doubt that ' the tight audible breathing ' mentioned in this letter was started by some bacterial or virus infection, or more probably by the two combined? Just how it was started we are to discuss later.

Are the bacterially induced asthmas true toxic idiopathies? Doctors, in their letters to introduce patients, often draw a distinction between a true ' allergic ' asthma and a dubious or bronchitic asthma ; they sometimes ask me to decide between these two conditions for them.

This is on a par with the unreal clinical distinction between ' true ' asthma and the ' phoney ' psychological type—a distinction which we came across once or twice in Chapter IX. I think we must consider that the bacterial and the non-bacterial toxic idiopathies are all of a class ; both are idiotoxic phenomena, for very much

the same reasons as those which made us, in Chapter II, classify as toxic idiopathies hay-fever together with asthma, eczema, migraine, etc. I will list some of these reasons.

Reason (a). As we have seen, some attacks of asthma are said, both by the patients themselves and by their medical sponsors, to be derived solely from bronchitis, colds, etc. If the case is one of clinical asthma at all (i.e. if there are marked attacks of dyspnoea occurring beside the bronchitis symptoms) we find that it is linked by heredity with such undoubted toxic idiopathies as hay-fever, angio-neurotic oedema, migraine, etc., quite as surely as these are linked to each other in the family tree.

Reason (b). We usually find that the so-called ' purely bronchial asthmatic ' suffers also from one of these undoubted allergic disorders, either simultaneously or at some period of life.

Thus, on taking the case histories of candidates for treatment against hay-fever every February, I frequently find that bronchial asthma in the winter alternates with their hay-fever at midsummer.

Reason (c). However much a case may be guaranteed as being a pure bronchial asthmatic by the practitioner, I always make routine tests on the skin with ten or so pathophanes, and I get unexpected positive responses to some of these in perhaps 20% to 30% of the cases so tested—and I don't count ' dust '. I have no doubt that if I tested more extensively or intelligently, the percentage of positives would mount.

Note : I said unexpected positives. These skin responses, though quite definite in character, are nearly always too small to be a considerable contributor to the patient's symptoms ; otherwise the case would probably not have been branded as ' pure bronchial asthma '. These slight pointers merely serve to confirm the general idiotoxic diathesis, and are usually not a cause of symptoms.

Reason (d). Bronchial asthma will summate with asthma from, say, grass pollen or cat scurf ; that is to say, the two influences together will produce a greater show of symptoms than either of them can produce alone.

We saw above (Reason (b)) that winter bronchial asthma often alternates with midsummer hay-fever. Sometimes the season for the one may encroach on the season for the other ; then we get a double effect : where they overlap they add up, usually producing extra asthma or extra hay-fever rather than a mixture of the two.

To put that in a slightly different way : if a little bronchial asthma worries the patient all the year round it will almost certainly be augmented at midsummer if there is also some slight and unsuspected grass pollen sensitiveness present—a phenomenon which I have elsewhere called ' cryptic hay-fever '.

Reason (e). The tendency towards a toxic idiopathy derived from bacterial infection can be subtracted from the total allergic response.

The Cat Lady. Long ago, before the first world war, an old lady was sent to me by her practitioner as a case of bronchial asthma.

She claimed also to be a cat-sensitive person. I knew little or nothing of cat sensitiveness at that time, save that it sometimes seemed to be a variant of hay-fever—

what I should nowadays call a ' para-hayfever '. I said I would attend as best I could to the bronchitic part, and we would study the cat sensitiveness.

I was remarkably successful in diminishing the chest and upper air passage infections ; and the old lady declared one day that, to her surprise (and mine) she could now, and for the first time for many years, nurse a cat on her lap with impunity. One could only deduce that cat scurf alone was not enough to produce symptoms ; it needed the bacterial poisons in addition.

From what I know now I think it probable that if I had taken a series of skin tests with cat pathophane I should have found these to be diminishing as the bacterial treatment proceeded.

Diminished Wealing. In corroboration of the last paragraph in the above story I may say that if any chronic bacterial poisoning is removed satisfactorily it usually has the effect of diminishing the size of the weals of any positive prick tests. Here is an extract from a report I sent to a doctor :

I saw that boy, X—— Y——, again last Thursday at the Clinic, and I was glad to hear that he has done so well with his chronic asthma during the winter. I agree with you that we must give some of the credit for this improvement to the minute doses of his vaccine which you have been giving him week by week ever since last September.

These doses have had another effect. You'll remember that when I saw him last July, and again in September, he gave strongish positives to grass pollen, and also weak responses to both horse and cat scurf. When I tested him the other day, the horse and cat wealings had practically disappeared, and the grass pollen response had so much diminished that I am not sure now that we need give the boy prophylactic treatment for his hay-fever. Of course, if we don't treat him for hay-fever, we shall be gambling on keeping him fairly clear of infection ; but, now that the rough winter weather is presumably over, I think we might risk it. This would have the great advantage of letting the boy feel free of us doctors.

It is clear from the above story that the bacterial toxaemia had been adding its effect to the skin responses from grass pollen, horse scurf and cat scurf. With the bacterial toxaemia effect subtracted from the skin response, the remaining weals had shrunk to insignificance.

Anomalies. The responsibility of bacterial substances in providing true idio-toxic phenomena seems clear enough, yet there are anomalies in their action.

For example, skin testing—which, as I have said, is really an artificial toxic idiopathy in miniature—does not work at all well with bacterial extracts : there seems to be little or no specific sensitivity to this or that microbe, such as there is to cat scurf or to grass pollen.

If there is such a specific sensitivity to bacteria, it can't (in my experience so far) be broken down or neutralised by steadily mounting doses of bacterial vaccine, as in P.T.D. treatment. It is difficult to account for this discrepancy entirely, but let us see what can be done.

' Cause G '. In Chapter III, when discussing the philosophy of Causation, we decided that out of the thousands of causes, *conditiones sine quibus non*, the only practicable thing to do was to consider only those causes which we proposed to alter by way of medical treatment—Collingwood's ' manipulable handles ' wherewith we might hope to switch diseases on or off.

I recorded that, in the gradually developing allergy clinics, I had in effect limited myself to seven such causes—to seven such means of treatment. The seventh, or Cause G, was bacterial action.

I then dropped the hint that we should find that this Cause G was, in reality, a conglomeration of Causes B to F.

The cogent practical reason for lumping all bacterial action together is that in whatever way bacteria work, the investigation for detecting and treating them for the benefit of the patient is by bacteriological technique. There is but one switch wherewith the toxic idiopathies caused by bacteria may be turned on or off : we must treat the infection.

Bacterial Products. We shall get a little closer down to reality if we keep steadily in mind that bacteria hardly ever act directly as living things. They produce their multitudinous disease effects (and the toxic idiopathies are no exception) by means of their secretions and excrements, and by their own bodies in solution. They don't act mechanically : bacteria don't bite. Perhaps we should add that, for our toxic idiopathies, the bacteria may not only act by their own dissolved products but also might act by means of the human body fluids which they have denatured by their chemical action ; see Patch Tests, pp. 263 to 264.

Even in the case of septicaemia, it is not the living bacteria in the blood which do the harm, but their dissolved products.

Action via the Blood. These products of bacterial infection must be in relatively strong concentration at the site of any infection, but only very weak at distant parts to which they are carried by the blood-stream. Thus we might expect them always to show their chief production of any toxic idiopathy locally at the infected point—we shall see that they can do this in the production of asthma by throat infections.

But it does not necessarily follow that they produce their biggest idiotoxic effect locally ; the toxaemia may find at a distance some tissues of the body which, for various reasons, are more capable of showing the typical idiotoxic response than were the tissues near the site of the infection.

Multiple Lines of Action of Bacteria. It is probable that when considering how an infected throat could cause asthma, or how an infected gut could cause migraine or eczema, most people only think of the bacterial products as acting in the role of an idiotoxin—they think exclusively of ' allergic ' people being ' allergically sensitive ' to the bacteria.

Bacteria may act in this way, in fact I feel sure they sometimes do so ; but to think of them exclusively in this role is to fall once more into the Single Cause Fallacy : we most certainly must not assume that bacteria cause toxic idiopathies only as idiotoxins.

In reality, it is just this idiotoxin role, i.e. one parallel with that of grass pollen in hay-fever, which is the most difficult to prove with bacteria ; notice, too, that of the anomalies mentioned above, all occur with regard to the action of bacteria as an idiotoxin. I will, therefore, first consider other ways in which bacterial infections may promote toxic idiopathies before considering this perhaps more dubious idiotoxin action.

These other ways are at least three :

(i) some bacteria markedly augment the normal serous leak from the capillaries ;

(ii) localised infection often forms the trauma factor dealt with in Chapter VIII ; and

(iii) they affect the emotions by their intoxicating effect on the central nervous system.

(i) **Bacterial Cause of Serous Leak.** In Chapter VII, we discussed this question of serous leak, which forms our Cause D in Chapter III. Since oedema produced by serous leakage from the capillaries is the very foundation of all idiotoxic responses, it follows that anything which can influence that leak will alter any idiotoxic disorder.

At the end of Chapter VII, after discussing other things which might affect the exudation of plasma from the capillaries, bacterial action was mentioned shortly ; but it was to be considered more fully in this chapter.

It should be noted that this Cause D, i.e. serous leak, exerts a non-specific action on idiotoxic or non-idiotoxic subjects alike.

Inflammation occurs with any local infection of the tissues ; the first riposte of the body is usually by providing an extra weight of blood to the part. The next move is the exudation of plasma and the emigration of leucocytes from the capillaries thus engorged.

With some invading bacteria, with the staphylococcus for example, the chief response is by the escape of leucocytes from the capillaries. This does not, I think, concern us very much in the toxic idiopathies.

Other microbes however, and in particular the streptococci, cause by their bacterial poisons not so much an emigration of leucocytes as an escape of plasma. This plasma, if enough of it escapes, will waterlog the tissues and thus cause oedema. Streptococcal lesions tend to be boggy or runny.

Itching is characteristic of any such waterlogging of the tissues ; it is produced by reason of the stretching, and perhaps tearing, of the tissues as the oedema fluid infiltrates them. This is clearly shown in an acute urticaria ; the itching is intense so long as the fluid exuding from the capillaries is still stretching the tissues locally. The same thing happens when streptococci invade the tissues : there is intense itching, and the part becomes soggy with oedema.

Sir Almroth Wright in his bacteriological lectures used to say : ' Whenever you have folliculitis, or a furuncle on the skin, you need not look to see whether it is caused by staphylococci or streptococci, for the itching will tell you. If the little pimple burns, it is caused by streptococci and not staphylococci ; and the burning is caused by the serous exudate into the tissues. Streptococcal lesions always burn unless there is a very free vent for the exuding fluid.' That is why we scratch.

And thus it comes about, I think, that in bacterial allergy we are more concerned with streptococci than with other organisms. They promote more serous effusion.

Distant Action. Bacterial poisons are of course more intense at the site of any infection than elsewhere in the body, but as these poisons get into the general circulation through the lymphatic channels, they must produce a general oedema tendency throughout the body, though this action would be only to a diluted extent. All the same, I can conceive that if other factors are pushing in the same direction, this slight extra tendency to oedema derived from bacterial toxaemia might be the last straw in precipitating an attack of asthma, migraine, urticaria, or whatever it might be.

(ii) **Bacterial Trauma Factor.** In Chapter VIII we were concerned to prove that damage to the tissues could provoke an allergic response in that particular neighbourhood. For example, the squeezing out of a blackhead on the chin may easily produce angioneurotic oedema in that region if the person happens to be subject to that disorder. (Anecdote on p. 130.)

I also said in Chapter VIII that mechanical or chemical damage to the upper air passages could precipitate an idiotoxic response in that region and thus produce asthma. I gave, as one instance, the German gas attacks in the 1914 war. Then I went on to say that a far commoner cause of local damage to the throat was a bacterial infection, and the mechanical damage such as an infection might cause—and particularly by whooping-cough.

Always an Infection ? So frequently does acute coughing, or an acute laryngeal infection, provoke asthma in a hay-fever subject when the infection happens to occur in the grass pollen season, that I am led to wonder whether an infection at the back of the throat, or at least some damage there, may not be essential to an attack of asthma supervening on classical hay-fever. The infection seems to do its work, chiefly at any rate, by pinning down the oedema to the bacterially damaged area.

If this is true, or even partly true, it does mean that for every case of asthma that comes to us we have an obvious line of investigation—to look for infection of the upper air passages. And we have also an obvious and almost universal line of treatment for asthma, to remove or reduce such an infection if found.

This is the chief justification for the great amount of bacteriological investigation and treatment that we have found to be necessary when dealing with our asthma cases.

Caveat. This is not to say, of course, that asthma is ' all a matter of upper air passage infection '. Once more let me protest that to talk like that is to fall headlong into the Single Cause Fallacy. All I maintain is that if bacterial infection of the throat is a routine part of the mechanism of asthma production, this would justify anti-bacterial measures as a routine method of treatment.

(iii) **Bacterial Effect on the Brain.** That moods, emotions and mental tensions do play a part (and sometimes a dominating part) in launching attacks of a toxic idiopathy was discussed at length in Chapter IX. Here I want to emphasise what we all of us know quite well already : bacterial poisons profoundly affect the mental outlook on occasion, and presumably always have an effect on it to some slight degree.

Though we doctors know all this quite well, we do not invariably take account of it.

I remember an occasion when I was at what proved to be the deathbed of a very important person suffering from a septicaemia ; the company at that bedside consisted of two practitioners, a well-known physician, the attendant nurse, and myself.

The physician got into a long argument with the patient concerning some of his symptoms, etc., and it was quickly apparent to all of us that they were badly at cross-purposes.

The nurse in charge of the case gradually became more and more embarrassed by this ; in the end she plucked up courage enough to draw the physician away from the bed and whisper to him that the patient was completely delirious, and had not said anything really coherent for the last three days.

These mental aberrations are much more easily noticed by the nurse than by the doctor, who only sees the patient for a short while and occasionally. It is noteworthy, I think, that none of us rather experienced people round the bed (save that nurse) had noticed that the famous intellect that was dying didn't know what it was talking about.

Subjective Effect. Anybody who has been ill in bed with a fever knows how difficult it is to do any serious reading. The mind will not concentrate, or quickly tires of the effort ; only the lightest reading is possible, and the patient quickly has enough of that. The patient becomes tired and irritated by talkative visitors, for the mental effort of coping with them is too great. And the patient often becomes querulous and depressed. All these things the nurses know well.

The ' suicidal ' feeling that comes with a bad cold in the head is almost proverbial ; a small abscess, like a boil, has a queer effect on the temper or on the mood of the sufferer ; and the aberrations of Spes Phthisica are notorious.

Bacterial Vaccines, as they travel in the blood-stream, must of course produce a slight but transient toxaemia. Tuberculin injections or doses of other vaccines sometimes have a marked effect in making the patient irritable or more unable to put up with the cussedness of things. This again is chiefly by intoxication of the central nervous system.

If it is wished to find out about any possible mental perturbation in a patient after a dose of bacterial vaccine, we can of course ask the relatives how the patient behaved after the inoculation. But it is often as good or better to ask the patient what his human environment was like ; with children, one sometimes gets a more candid answer that way :

' What was your mother like to you, Susie, after I gave you that dose last week? ' I asked a girl, anxious to know if perhaps I hadn't given a little too much bacterial vaccine to the child. ' It's funny you should ask that, doctor ; she *was* rather rough and cross with me for two whole days.' ' After that? ' ' Oh, after that she was just ordinary.' ' And now? ' ' She has been quite good : rather nice to me, in fact.'

I had given the child a little too much the week before, and her mind had been for a day or two sufficiently poisoned by the bacterial substances to make the mother's customary behaviour seem rather brutal.

Mental Derangement. In the serious bacterial poisoning of high fever, the mental derangement may proceed right up to the madness of delirium ; but we

usually forget that there is every possible degree of mental derangement between this insanity and the slight irritability derived from a boil or a dose of vaccine.

This mental effect of bacterial poisoning on the central nervous system is so important to our argument, that I will tell a cautionary tale told me by my friend, the late Dr. John Matthews. This story is not concerned with any toxic idiopathy but with typhoid fever; yet it shows very clearly that bacterial poisonings can lead to mental upsets which in this one case range from the mild querulousness of slight neurasthenia right up to apparent insanity.

Holiday Girl. In preparation for a long winter holiday in a warm and rather insanitary country, a family were all prophylactically inoculated against typhoid fever. None of the party had more than the usual slight and transient indisposition after this, save a daughter who claimed to have been made very ill by it. Little notice, however, was taken of these complaints because for some time past she had seemed to be rather hypochondriacal and always complaining of her health. She had an uneasy abdomen.

Her stay abroad seemed to do this girl no good at all, and her uncertain health and temper spoilt the holiday for the rest of the party.

Coming back to England, she fell into a decline, with fits of terrible mental depression; and it was generally assumed that her mental state must be accounting for her preoccupation with her own physical symptoms.

After an attempt at suicide, she went voluntarily into a mental home, where she could be more easily watched and cared for.

The girl persistently declared to all who would listen that her trouble flowed from 'that horrible typhoid dose', an idea which her doctors professionally pooh-poohed.

She was so persistent about that dose that John Matthews was called in as inoculation expert and asked about the possible effects of typhoid vaccine. The prophylactic dose, he explained, would only cause serious trouble if the patient were a typhoid carrier at the time of the inoculation. If her present mental troubles flowed from that dose, then she was presumably a typhoid carrier still—'and', he added, 'I propose to look for B. typhosus.'

At the seventh attempt, Matthews isolated the typhoid bacillus from the stool. Under the influence of this new conception the infection was then cleared up; the girl regained good health in mind and body, and lost her hypochondria. The bacterial poisoning had caused the mental derangement, and 'that horrible typhoid dose' had merely stirred up this long-standing infection.

We can safely say that, but for the acumen of my old friend, this bacterial poisoning of the central nervous system would never have been suspected, and the case would still be considered as a mental derangement *sui generis*.

If what I have said in Chapter IX about the effect of emotions, moods and tensions is true, it seems inescapable that bacterial poisons must affect the onset of toxic idiopathies through their action on the central nervous system. They do this sometimes only slightly no doubt; but sometimes they do it profoundly.

Combined Action. For the foregoing reasons we may take it that bacterial products can affect the onset of a toxic idiopathy in several different ways; but, of course they are not confined to one single way in any given patient. I have said above that it is difficult to distinguish between, on the one hand, the action of some bacteria in making the plasma more easily diffusible from the inflamed capillaries, and, on the other, what I have called the trauma factor of bacterial infection: they reinforce each other.

Similarly, whenever an infection is prompting an attack of a toxic idiopathy either by trauma or by increased serous leak, then a toxaemia from that infection will also predispose to this attack by its action on the central nervous system.

What of Bacterial Idiotoxin ? If a bacterially induced toxic idiopathy, for example the ' purely bronchitic asthma ', has a causal machinery which runs parallel with that of hay-fever, then such an important part of that machinery as the idiotoxin-idioceptor couple cannot be omitted. Is it possible that the bacterial substances, beside their other modes of action which have been detailed above, act also as the idiotoxin? As we consider more of the examples, and especially where the manifestations of bacterial action occur at a distance from the infected area, it becomes more and more difficult to escape that probability.

Let us then consider these examples, and analyse the causal elements as we go along.

Up-and-down Disease. I apologise for this name, but that is what quite a number of my idiotoxic patients call it. It consists of a widespread oedema, and especially on the surface of the body ; it may end in a great increase of size and weight in the patient, and I suspect it is then often called ' water retention ', or perhaps ' toxaemia of pregnancy '.

However, it appears in toxic idiopathy families—so far as I have observed it—though they are of course the people I see most often. It begins in attacks of urticaria or angioneurotic oedema in the early stages, and in other respects falls into line with the allergic dermatites (p. 26). Judging by the number of cases I have seen, it must occur fairly frequently.

Apparently it is a disease chiefly experienced by women, or maybe they would notice it more than men ; they usually complain that, when they try to put on a new dress for which they have been accurately fitted by their dressmakers only a week or two previously, the dress is either far too loose or much too tight to get into. This sort of thing, they say, happens over and over again, and particularly at times of abdominal unrest ; also it may seem to synchronise with the comings and goings of dermographia.

Bathing Belle. I remember well a case of this type, a woman who came to see me soon after the first world war. She was massive and ungainly, and somehow unhealthy looking ; when she gave me her age I found she was really quite young.

She had only recently returned from India, her husband being in the Indian Civil Service. In India and until quite recently she had had a slim figure and had been in great demand for—and had thoroughly enjoyed—the bathing parties.

As I remained unimpressed by this story of recent elegance, she hunted in her handbag and produced the photograph of a very slim young woman about to dive into the water, and told me that was a snapshot of herself as she was only three months previously.

She related that she was a noted hostess in India and had been very energetic in organising a big and successful dance. She had achieved this in the teeth of considerable feminine opposition and she had been very afraid that the dance would be a social failure. According to her story, she had been under great emotional strain till she saw the dance was going well ; then she felt a blessed relief from anxiety and prepared to enjoy herself.

She was wearing a completely backless ball dress (in vogue at that time) and one of her women friends, who I fancy was also one of the opposition, said suddenly : ' Oh

darling! What *have* you done to your back? It's like a lobster!' Soon after the skin began to itch. She found the exposed back was mapped out in urticaria which gradually amounted to angioneurotic oedema. The oedema spread over the body, till she gained the appearance with which she came to me.

The patient laid great stress on the emotional strain she had undergone, and the complete relaxation from this conturbation, which had immediately preceded the urticaria. Conturbation was an asthmogenic agent with which I was becoming very familiar at that time, but when I saw her all mental strain had gone so far as I could make out ; yet she remained elephantine.

I could find nothing to which she was sensitive by skin testing, and she was taking no drugs. She had had bowel upsets at the time of the fatal dance but these she had put down to nervousness. However, the slight diarrhoea had continued (together with the swellings) on the liner coming home and till she saw me.

The only abnormal factor I could find was a vast overgrowth in the stool of green type streptococci. Mindful of what I had found with dermographia (of which more later) I dosed her with minute but steadily repeated doses of an autogenous vaccine made from this strain ; the swellings gradually went away and she regained, permanently as far as I know, her previous shape.

At the time I thought of the streptococci or their products as presumably representing the idiotoxin factor in spite of the anomalies : somewhat close to Allergy I in fact.

Length of the Hay-fever Season. It is impossible to tell exactly how badly a hay-fever patient is going to suffer from the action of the grass pollen at mid-summer ; so, though appearances may suggest it, we cannot say with confidence that a slight bacterial infection *has* added anything appreciable to a man's hay-fever.

It is quite possible, however, to tell how long his sufferings from grass pollen alone are likely to last ; this will be for as long as the grass pollen cloud in that neighbourhood is sufficient to produce symptoms—and that is, in *uncomplicated* hay-fever, roughly May 20 to July 15 in the South of England. Now the grass pollen cloud does not of course begin and end with a sudden jerk at these dates. (See Fig. 3 on p. 13). It comes on gradually and goes off gradually ; but these minute quantities of pollen in the air occurring outside the usual hay-fever season are not, if they are acting alone, sufficient to produce any symptoms that could be recognised as hay-fever in even the most sensitive patients.

It is often very noticeable that a slight but chronic tendency to bronchial asthma will lengthen the hay-fever season at each end ; we must conclude that the reason for this extension is the presence of bacterial substances from the upper air-passage infection which have added their effect to the otherwise insufficient pollen effect before and after the true season.

It seems clear that, if a bacterial infection adds its effect to the otherwise insufficient grass pollen before and after the true season, it will also be adding this effect to the *sufficient* pollen during the true season, i.e. the hay-fever is made worse by the bacterial toxaemia.

Have the bacterial substances functioned as an idiotoxin acting in addition to, and so augmenting the effect of, the pollen idiotoxin? It seems likely, though it is difficult to prove.

The alternative explanation that occurs to me is the extra effusibility of plasma due to bacterial poisoning (p. 217) which would allow the pollen idiotoxin to work more efficiently ; the psychological and trauma effects of bacterial action seem to me to be ruled out in the circumstances.

Gingivitis. Mothers often say that the eczema of their babies was started by teething.

They will sometimes describe how, when each tooth was being ' cut ', the eczema got worse ; then, when the inflammation of the gums died down, the eczema had subsided again. Mothers are particularly sensitive concerning their children's skin, and seem to be always watching for blemishes ; they are good observers therefore.

' My two children never got eczema at all, doctor, except when they were cutting their teeth. As each tooth came, the eczema came too ; but it always went away when the mouth got comfortable again.' . . . ' No : *my* children have *never* had a spot on them—except at those times.'

Here, I think, there must be bacterial action. We may guess that the skin of a small baby is chronically in a ' delicate ' state from contact with the outside world (clothing, washings and dryings, etc.), but trauma from the gums cannot be operating on the skin of the body. Again, the effusibility of the blood plasma can hardly have been much altered by the gum infection.

It seems that the responsibility for the eczema must rest between the emotional upset (' fractiousness ' and ' bad temper ') from the pain of cutting the teeth, and bacterial substances from the inflamed gums which act as an idiotoxin.

Tooth Extraction. By a series of blood cultures it is easy to prove that a bacteraemia results from extraction of very septic teeth ; and, if a bacteraemia, then certainly a bacterial toxaemia. We find that this, in the idiotoxic subject, may produce asthma, or some other toxic idiopathy.

A doctor reported to me in the letter which he sent with the patient that on four separate occasions an extraction of a septic tooth was promptly followed by an attack of asthma. Though the patient had had asthma previously, and had also had eczema as a child, yet for a considerable period before each of the extractions (which occurred over the space of several years) all toxic idiopathies had been absent ; the patient was therefore surprised and annoyed at the fresh attacks, which he considered gratuitous.

' The worst cause of asthma is having had it ' : one may guess perhaps that this patient was not so surprised at the third or fourth attack after the extraction ; but on the first occasion certainly it was unexpected, and he had undergone the extraction quite light-heartedly, expecting no trouble afterwards.

All toxic idiopathies produced at a distance from the tooth sockets must have been produced via the blood-stream ; this rules out the local trauma. It doesn't rule out the thinning and effusibility of the blood fluids due to streptococcal poisoning ; and, for most of us, tooth extraction is an emotional business. But once again, are these adjuvant causal factors adequate? Is it not likely that the streptococcal toxaemia had also an idiotoxin action in the anecdote related here above?

So far as I could find, there was nothing but bacterial substance to play the role of idiotoxin in that case.

Bowel Infection is derived from swallowing infected food or drink. It is quite possible that people so infected may have a local toxic idiopathy—for example, oedema of the gut wall, amounting to something like angioneurotic oedema on occasion (see the case reported on p. 31). Patients with an idiotoxic diathesis often describe a queer feeling of distress abdominally; they often say they feel 'blown up'. However this may be, there is no doubt that bacterial poisons in the gut can, in susceptible persons, produce a variety of toxic idiopathies at a distance, and therefore by a toxaemia. Let us consider some of these.

'Fish Poisoning'. Whenever an unexpected crop of nettle-rash occurs in a patient, if any fish has been eaten at all recently, doctors are apt to suggest the probability of 'fish poisoning'.

Of course, patients who are sensitive to fish myoplasm are liable to an idiotoxic response whenever they swallow any fish; but such cases, though fairly common in an allergy clinic, are comparatively rare in the total population, and doctors are seldom referring to them. It is nearly always some bacterial taint in the fish which causes the vomiting and diarrhoea, and causes it in practically everybody; some people are more resistant to the poison than others, but all of us are liable to suffer in an attempt to expel the bacterial poison.

But why do they suspect *fish*? Simply that dead fish goes bad rather more quickly than most protein food, and therefore doctors remember more trouble from bad fish than from bad beef or mutton. Tinned food goes bad very quickly also—after the tin has been opened; so any tinned food frequently gets named, justly or unjustly, as a cause of gut sepsis.

Whatever food it may have been derived from, it is a bacterial taint we nearly always have to consider; and this may have been swallowed by the patient either as bacterial products already preformed, or as living bacteria which may infect the patient's gut, and perhaps cause trouble at some later date when these microbes have caused a serious infection. A good example of this delayed toxaemia from the bowel is of course the story told of Dr. Matthews's patient on p. 220.

In either event, it is the bacterial products which will usually do the poisoning and not the normal protein of the food, either locally in the gut causing vomiting and diarrhoea, or at distance by toxaemia.

A Case in Point. Here I would ask the reader to turn back to pp. 84 to 85 and read again that long story of the six hay-fever patients undergoing Rush P.T.D. treatment. The story was given in that chapter as a warning of the serious reaction which might occur if any acute bacterial poisoning should take place during the process of P.T.D. treatment.

The story also shows quite conclusively that bacterial poisoning at such times, i.e. after what would be quite a proper dose of pollen vaccine in the ordinary way, may precipitate a number of different toxic idiopathies which would not have occurred but for the poisoning. In the accidental mass experiment on these six

hay-fever patients, a universal bowel poisoning in the nursing home produced among these hay-fever patients the following : asthma and urticaria twice, urticaria and migraine once, urticaria alone once, and asthma alone once : these various toxic idiopathies were distributed among five out of those six patients undergoing Rush Hay-fever Treatment. Inescapably it did this in each of these five cases by the help of a grass pollen dose given a few minutes previously, but it was obviously co-operation between the grass pollen idiotoxin and the bacterial poisoning which caused the trouble.

What makes this particular incident almost a crucial experiment is the fact that it was so well controlled. The bad fish was swallowed by a large number of non-idiotoxic persons, i.e. by the patients of other doctors and by the nursing staff. Almost all these people had the bowel poisoning, and most of them with its attendant vomiting, diarrhoea, etc. ; yet none of them derived any toxic idiopathy thereby save those five out of my six patients.

At the time of the incident, I thought the bacterial poisons must be acting exclusively as idiotoxins accessory to the pollen. I still think this must have been one of their modes of action, but I now realise that there were other possibilities, though no doubt the idea of trauma via the blood-stream must be ruled out.

As we saw on p. 217 in this chapter, the bacterial poisonings of the blood-stream must have made the effusion of plasma from the capillaries into the surrounding tissues proceed to some slight extent more swiftly than usual. It is hardly conceivable that the toxaemia was acting only by this increased effusion of plasma, because everybody's blood—all the patients and all the nurses—would have been affected by streptococcal poisons to make their plasma more easily effusible ; in the mass experiment under consideration, it was only my hay-fever patients (or rather five out of six of them) who suffered from toxic idiopathies.

No doubt it is possible that the toxaemia was producing mental disturbances such as I have described in p. 220. So far as I could make out, there were in my patients no particularly mental upsets, save natural annoyance with me for the unexpected reactions to my doses.

My seven causes listed in Chapter III don't pretend to cover the whole ground of causation. Excluding for the moment the idiotoxin-idioceptor function, there may well be other methods of action for bacterial substances other than the trauma, effusibility, and emotion effects which we have already been discussing ; if so, I don't know what they can be. I find colleagues are anxious to invoke the phrase ' histamine or some histamine-like substance ' ; but so far as that means anything to me, it seems to coincide with the effusibility factor already discussed.

With this particular mass experiment, I find it difficult to account for what happened to those five patients unless we assume that there was some bacterially derived idiotoxin acting in conjunction with the grass pollen of the doses which caused these reactions. I don't think the other modes of bacterial action can account for it.

There does remain some strange action of ' Allergy I ' to account for these idiotoxic phenomena, but that I don't understand—nor I think does anyone else really !

Dermographia. My next example of bacterial poisons causing a toxic idiopathy via the blood-stream is dermographia, which in Chapter II was claimed to be a true toxic idiopathy. I will restate why I think that bacterial substances come into its causation story.

When speaking of the occurrence of the trauma factor operating in the various toxic idiopathies in Chapter VIII, dermographia was described as a condition where only the damage to the surface of the skin was obvious. This trauma, i.e. the scratching of the skin, is the first, and possibly the last, thought in the mind of the onlooker ; the rest of the machinery, e.g. the presumably bacterial idiotoxin and the allergy-promoting emotional state are out of sight ; but they play their parts none the less, as I hope to show.

The tendency to dermographia may persist for very long periods in the life of a patient ; and dermographic people can, and often do, test the sensitiveness of their skin regularly by more or less standard scratching.

A Scotch woman of 50 years came to consult me concerning her tendency to oedema. She had an idiotoxic heredity, but I could find no idiotoxin which was demonstrable on her skin by prick tests.

She had had a dermographic skin off and on all her life ; when a schoolgirl it had been considered a social accomplishment that she could draw a rough sketch of Harry Lauder on her leg. Sometimes the lines were very faint ; sometimes they were so diffuse that the whole sketch ran into one big blur, and sometimes the accomplishment disappeared altogether—it had depended, she said, on her mood and on her state of health.

Dermographic Variations. It is a common experience that the intensity of the dermographic rash varies greatly from time to time. What can cause this? Hardly the amount of the trauma with its liberation of ' H-like substance ' ; the test scratching can be made uniformly, and is the one factor of causation which is well under the observation and control of the experimenter. Besides, when the weal response on the skin is in abeyance, the fiercest scratchings will not produce the result which is readily obtainable from the mildest stroke when the skin condition is favourable. It is not the trauma factor therefore, but some other part of the mechanism which is varying.

I have been observing several cases of dermographia for very many years, and two changes seem to me to be correlated with its waxing and waning ; these two changes are the state of mind of the patient which concerned us in Chapter IX, and the state of toxaemia (and particularly from gut sepsis) that concerns us now. In the patients under observation, gut sepsis has been conducive to an increased degree of dermographia, while a comfortable abdomen with regularly acting and comparatively uninfected bowels has tended to diminish dermographia or even abolish it.

Bacterial Overhauls confirm this rough generalisation ; it is often possible to prophesy from the extent of the dermographia how much gut sepsis, and particularly how much streptococcal activity, will be found on examining a fresh bowel specimen ; on the other hand, refusal of the skin to produce the characteristic oedema after the scratching usually denotes a more normal bowel flora. A bacterial intoxication is not the only possible variant, but it is the most constant one.

Probable Idiotoxin. It seems to me presumable that in dermographia these bacterial substances are acting, in part at least, as an idiotoxin in the causal mechanism. If they don't act in this way, we must make some dubious suppositions: we must credit all the idiotoxin action to some other and as yet unknown foreign protein, and must rely for the bacterial effect of gut sepsis on the adjuvant methods of plasma effusibility, trauma and emotional upset.

With dermographia persisting on and off for nearly a lifetime, this, as I say, seems doubtful. As with those rush hay-fever patients (p. 84) who had eaten bad fish, why should not bacterial poisons act as an idiotoxin in dermographia?

To be candid the why-not is fairly clear. The difficulty of ranking bacterial substances as an idiotoxin homologous with grass pollen in hay-fever was given under the heading of Anomalies on p. 215 of this chapter. Briefly, you can't make useful diagnostic prick tests with bacterial substances, nor can you give a regulation P.T.D. course with them with any success.

Bacterial Specificity. How can we account for these aberrations? There is no sound reason why we should expect any high degree of specificity as between microbe and microbe in our skin tests.

We shall see in Chapter XIV that not only is there very little difference between the pollen of the various meadow grasses when it comes to prick tests or P.T.D. treatment, but there is not much difference between these English grass pollens and the pollens of such exotic grasses as bamboo, sugar-cane or maize: it is a family characteristic, like the Hapsburg lip.

We should not be surprised, therefore, if there is similarity of allergic reaction between gram-positive cocci and gram-negative bacilli, between pathogens and the saprophytes; and that is what we seem to get.

It is more disturbing to find that there is little noticeable difference between the skin response of the idiotoxic and the non-idiotoxic subject to extracts of any of these bacteria. Perhaps it would be truer to say that the differences, such as they are, between microbe and microbe and also between person and person are capricious and don't seem to follow any known rule. That, no doubt, is just our (or my) ignorance of the rules.

The Unique Position of bacteria should not surprise us. Ehrlich used to say that, in the business of keeping the specifically human character of our body proteins free from contamination by, or dilution with, the proteins of other organisms, animal or vegetable, we originally used the same machinery whether these proteins were living bacteria or dead food. With the amoeba, the bodies of living bacteria are at once an invasion and ingested food, and no doubt it was and is the amoeba's digestion which provided the successful defence for it. That is, of course, the way we humans keep ourselves from becoming sheep-like from eating mutton, and the cow from becoming grass-like from grazing.

This analysis of our food proteins into the amino-acids, like a house being disintegrated into single bricks, does not completely prevent the leak of specific foreign proteins in the food through the gut wall and into our systems (p. 242); but digestion is perhaps 99% of our defence; what traces of alien protein do get through

make us feel uncomfortable and antipathetic to the food in question till it is removed—presumably by the mechanism of bacterial immunity. ' After drinking all that vast quantity of milk I feel I never want to look a cow in the face again.' This removal, both of the foreign proteins and of our antipathy to them, takes about two hours as measured by the Prausnitz test, and I imagine it was this machinery that Ehrlich was referring to as the jumping-off point for bacterial immunity.

Evolution. Ever since life began on this planet there must have been a fierce and continuous fight between invading organisms and the tissues of the invaded ' host ' ; in this fight, failure has meant extinction, and success has meant life and the capacity to hand on life. In propitious circumstances for it, the bacterial enemy may be capable of multiplying a million-fold in seven hours ; it seems no wonder that, as these rivals twisted and turned through the ages, fighting by immunity and counter-immunity for the right to reproduce their kind, the antigenic liabilities of bacteria have become subtle and queer.

That, it seems to me, may account for the intricacy and abnormality of idiotoxic response to bacterial substances.

Importance of Bacteria. Whether bacterial products achieve their results in the toxic idiopathies by functioning as idiotoxins as I think they must do, or manage it by tissue damage or increased fluidity of plasma as I hold that they do, or affect the mind and so the emotions as I have explained that they do, or whether perhaps their asthmogenic function is derived from some other new and ' Allergy I ' influence that is out of my knowledge, there can be no doubt whatever of their importance to us. Infections will be well worth diagnosing and treating in any allergy clinic. If successful, it would be radical treatment in whatever way the bacteria worked.

This is not a text-book on routine bacteriology ; and it may be said that those without a laboratory at their command are not interested in technical details, while those working in a laboratory don't need to be taught them. All the same, the closest possible co-operation is needed between the clinic and the laboratory : they are the right and left hands of any worker on the toxic idiopathies. So I will take leave to set down in this chapter what I think should be done—or at least what we have ourselves done for some considerable time. There has never been anything very special or remarkable in our bacteriological work, save perhaps that we may claim considerable thoroughness.

Natural Immunity. Though it is always desirable to take account of the bacterial situation (see the story of the Britannia Girl told on p. 212) it is not always necessary to carry out a bacteriological investigation.

There is a great deal of natural immunity protecting us, or we should all die the first time we were infected. This constant self-defence has been managed pretty well by our ancestors for millions of years, so do not let us handicap the patient with any well-meant but unwise interference if we can help it. We may do good or harm, or more probably a mixture of the two, by altering the patient's natural routine of life. We should watch carefully to make sure which way the balance of advantage goes when we suggest a change of routine.

Food. My feeling is that food matters surprisingly little, as one might expect when one thinks of those millions of ancestors.

The only food to be generally avoided by idiotoxic persons on bacterial grounds is an excess of fatty or oily matter, for this promotes the intake of bacterial substances, particularly of streptococci, from the gut. Beyond that, the less the patient is fussed about food the better—from the bacteriological angle.

Nowadays we ask : *is* Vitamin C worth worrying about? How far are we to go in urging orange juice (which is contra-indicated by its decalcifying action, pp. 121 to 123) on our patients? Must we join in the chorus from child-guidance clinics in insisting on cod- or halibut-liver oil in our efforts to keep down infection?

I must own to an uneasy scepticism which stops me from prescribing these things for my patients for antibacterial reasons ; yet this doubt is not strong enough to urge me to countermand the advice of my colleagues. Milk will contain all that is absolutely necessary for children : the perfect food.

Hygiene. Fresh air and sunlight are good for us all, of course, and certainly pleasant ; but I doubt if fresh air kills microbes, even the tubercle bacilli. I don't know if the vogue for sending asthmatic children to an open-air school is with a view to killing microbes of the upper air passages ; if so it is a mistake, due I think to some mental confusion between asthma and phthisis. The advantages and disadvantages of the open-air school are chiefly psychological and are dealt with on p. 180.

Breathing exercises are frequently advocated for asthmatic children. There is certainly a clear bacteriological advantage in nose- as opposed to mouth-breathing, but I should put the healing advantages of a well-built chest, prominent chin and squared shoulders more to the account of the psychologist than to the bacteriologist.

Though painful to the nearby grown-ups, probably the best and certainly the most natural chest exercises for children are the yells and screams they utter in the playground during the mid-morning break or at other times of recreation.

Spontaneous Auto-Inoculation. The normal bacterial defences both of children and of grown-ups are actuated by minute auto-inoculations of microbic material from small and casual foci of infection. This material gets into the blood-stream and so helps to immunise the patient. For example, it can be shown that a person with an infected throat will send small doses of the infecting material into his system via the blood-stream by singing, shouting or coughing ; he thus makes protection against the infection. We deprive people, young or old, of much of their immunity if we wrap them up in metaphorical cotton wool.

It was for this reason of course that the street arab of any big town was far more often Schick-negative than was his opposite number in the thinly populated countryside. This was because the street arab made very frequent infection contacts with undiagnosed diphtherias of very low virulence, and so was constantly being stimulated to make immunity to it.

The same is true of all infections, and particularly perhaps of the upper air passages.

At the end of the Boer War it was found impossible to get food to the outlying Boer farms without at the same time provisioning the Boer Commandos in the field. The

women and children of the more inaccessible farms were therefore brought into ' Concentration Camps ' in the nearest town in order that they might be fed.

The result of this intentionally humane action was deplorable : the children from these isolated farms had made hardly any immunity from casual contact with other children ; therefore the Concentration Camps, which were in no sense prisons, were swept with epidemics of all infectious diseases, and there were many deaths in consequence. The blunder was medical, not military or political.

It is notorious that explorers in the Arctic circle lose much of their immunity from upper air-passage infection if they stay for long in the bacteria-free Arctic ; when they return to civilisation, we are told that they at once fall victims to a series of coughs and colds.

When in the winter a coasting steamer from Glasgow pays one of its rare visits to a village cut off from the rest of Scotland by high mountains, the inhabitants succumb to what they call the ' steamer-cold '. In their isolation they have had insufficient auto-inoculation for adequate protection.

The ordinary contacts and activities of life promote these normal forms of auto-inoculation, which, if not excessive, are distinctly beneficial. Thus it comes about that we can do much harm by keeping anyone in bed unnecessarily when he would much prefer to be up and about, or to go off to business or to school. The harm is done, not only psychologically by hospitalising the victim of excessive care (see p. 168), but also bacteriologically by cutting off the auto-inoculations of ordinary life. This is one of several reasons why children with asthma, or indeed any toxic idiopathy, often do so much better at a boarding school than in their own homes where they are ' better cared for ', i.e. coddled.

A serious infection (roughly one involving a raised temperature) dictates rest in bed lest the auto-inoculations should be excessive. If a normally healthy and lively child *wants* to rest, the probability is that he has need of it to avoid over-poisoning from bacteria.

On the other hand, normal activities may not be enough. It is well known that a mild cold (i.e. upper air-passage infection) can often be terminated by ' walking it off ', thus providing increased auto-inoculation.

It is the business of the doctor, or much more often of the mother of course, to decide between stimulating the patient to increased activity, leaving him to his own devices, or keeping him at rest. Usually the middle course is best, and is the line adopted by intelligent parents. Their problem is one of bacteriology very often—the parents don't realise this, but would call it common sense.

Planned Auto-Inoculation. It is clear from the above that these auto-inoculations, which should be going on all the time and are in the long run as essential to life as breathing and the beating of the heart, can be increased or diminished by the doctor. It is a question of the escape of bacterial substances into the blood-stream, and he can alter this escape in many ways : active and passive movements of the affected part, increase of blood supply to that part (e.g. by a poultice), massage, or exercises (e.g. deep-breathing exercises or singing and even shouting) for the upper air passages.

The details must be arranged according to circumstances, and as carefully controlled in amount and frequency as would be a bacterial inoculation.

If the focus of infection is large and active it is very easy to overdo things, and the doctor must know exactly what he is about if he is to try active interference.

Forty years ago I demonstrated that we can clear up infected gonococcal joints by very gentle massage strictly limited to only a few minutes every five days or so ; the treatments were controlled by innumerable blood testings, and were really very successful though laborious.

On hearing of this, an up-and-coming surgeon (now long since dead) wasn't going to be beaten by ' one of these laboratory fellows ', so he ordered the job to be done thoroughly with an hour's thorough massage a day.

As might have been expected, his results were so unsatisfactory that the method was dropped forthwith. Anyway, it was too difficult for routine use by clinicians.

The best rough and ready guide is the inclination of the patient. If a child *wants* to keep quiet, there is usually danger of overdosing by planned auto-inoculation. If a child feels lively and wants to racket about, probably a little stirring up of the focus of infection will do not harm but good ; but even so it must be done with understanding.

Abdominal Massage will serve as a good example of a deliberately stirred-up focus ; it is important to us because mild gut sepsis is so frequently connected causally with idiotoxic dermatitis, paroxysmal hydrarthrosis, migraine, epilepsy, etc.

The abdominal infections causing these things are usually from low grade but chronic infections of streptococci or coliforms. Such cases (if our suspicion of abdominal cause is strong enough) may be treated by light or even deep abdominal massage. As with vaccine therapy, the worse the infection the lighter the dose should be, and vice versa.

Dose. The massage dose can be regulated by the length of the time of the manipulation, and also by its depth and thoroughness. I find that the enthusiasm of the masseuse must usually be tactfully controlled by the doctor in charge. At the beginning of the treatment, the patients generally say that they would like massage to go on ' for hours ' ; but in about seven minutes a grey and worried look usually comes into their faces. If you ask them then, they tell you with a surprised voice that they would really rather like you to stop the massage. This change of view is brought about, I feel sure, by bacterial substances circulating in the blood and thus affecting the central nervous system ; so when that happens we shall have given enough of a dose. It is desirable for the doctor to be present at any such massage unless he is sure the masseuse thoroughly understands what she is doing, i.e. inoculating the patient with bacterial substances.

Frequency. The repetitions of treatment are important ; they should not be too frequent. A heavy dose of auto-inoculation might last for a week ; a very minute dose might be repeated in a day or two. It must be remembered, too, that the size of the dose depends as much on the size, condition and place of the infection, as upon the extent of the manoeuvres designed to stir it up. The doctor thinking of a planned inoculation must be cautious up to the point of deciding not to interfere

with the patient at all if the case is unsuitable. On the contrary, he may find rest in bed imperative.

' Good Nursing '. Looked at with a bacteriologist's eye, it will be seen that a surprising amount of ordinary nursing manoeuvres are of value chiefly because of their useful and sensible control of auto-inoculation effects ; I even suspect thorough purgings of the bowel, or high irrigation of the colon, of working largely by the auto-inoculations produced thereby.

In general, these artificial stimulations of it will be unnecessary—in fact, will be undesirable because hospitalising ; it is best of all if the patient retains or regains a healthy zest for life and indulges in all the natural activities of an active useful person, e.g. games for schoolboy asthmatics when they feel like it.

The best auto-inoculations are unplanned, for they are entirely under the control of the patient ; that is the natural state of things. The best nurses know when *not* to nurse : excessive nursing is a vice.

Chemotherapy. If penicillin, the sulphonamides, and so forth, kill microbes, and microbes can be such a multiple cause of allergic trouble, surely that ends for us the bacteriological problem.

Not quite so easily ; if the peccant micro-organisms are ' soft ' to, say, penicillin, then the effect of such a drug would be, and is, magnificent at a bacteriological crisis ; but these beneficent effects are very short lasting, and the normal immunological rhythm of auto-inoculation (by which we maintain life) is not at all improved and may be much worsened. The drug will cut short the small and constantly recurring auto-inoculations which maintain a decent level of protection ; the result is that in, for example, chronic asthma we shall repeatedly hear stories of a dramatic improvement and then immediately afterwards of a serious relapse. It is not practicable or even desirable to continue the administration of such drugs for long, and they must never be used as thought-saving nostrums.

It comes to this, that while chemotherapy is a life-line with which we may pull a patient out of deep water, we should also teach him to swim. This can be done by inoculations of a vaccine—if he cannot manage to auto-inoculate spontaneously.

Before leaving this brief reference to chemotherapy, it must be remembered that the wholesale killing of bacteria probably results in their lysis, either by the drug itself or by the body juices. Therefore the drug should produce one big auto-inoculation, and then stop them entirely. As we shall see when dealing with vaccine treatment, these single big doses are to be deprecated when dealing with such chronic states as the bacterially produced toxic idiopathies, and particularly with asthma.

It is possible, by the way, that the people said to be ' allergic ' to the sulphonamides or to penicillin are really giving a response (urticarial, migraine or what you will) to that one massive dose of bacterial substance.

Awareness Only. What I have said so far on bacteriological treatment demands an awareness of the bacteriologist's point of view, but needs none of his technique. Nor need the use of stock vaccines (such as ' Mixed Vaccine for Bronchial Asthma '

which I shall mention later) call for any laboratory work ; but they need some general knowledge.

Technique

Routine Examination. In view of the importance of trauma in the throat in localising infections to the upper air passages (p. 136), it is well to make some routine examination of the state of the nose and throat, and especially in all cases of asthma.

It is not so useful, I think, to examine the sputum of patients—which is what the doctor who has sent the case usually suggests. Except in cases of lobar pneumonia, the sputum contains a very mixed collection of organisms, and most of them perhaps saprophytes.

Cultures. If the condition found in the throat calls for it, swabs should be taken of the post-nasal space, tonsillar crypts or fossae, ulcers or suppurating gums. As it is chiefly for streptococci that we are looking, these are planted promptly onto rich blood-agar plates.

Auto-Haemo-Culture. It is also my practice to plant the swab into, say, ¾ of a c.c. of blood freshly drawn from the patient ; this is incubated for 24 hours, and then a loop-full is replanted onto another blood plate. This method of auto-haemo-culture (' A-H-C ') was first described by Almroth Wright as a method for selecting potentially pathogenic organisms, and is strongly to be recommended.

With regard to the A-H-C isolation, a very distinguished laboratory colleague insists that the patient's blood happens to be a good medium for streptococci in general and is nothing more : it is not, he holds, in any way selective of the more pathogenic forms.

I cannot agree. A-H-C is not only selective *for* streptococci, but also on occasion against them. Thus a haemolytic type may be showing only in scanty numbers as compared with other streptococci on the direct plate, but be strongly predominating when sub-cultured after A-H-C ; conversely, a direct plate showing mixed streptococci, including many of, say, a haemolytic type, may be changed to a plate showing a pure growth of a viridans type after the A-H-C ordeal.

Sometimes, again, there may be plenty of different streptococci on the straight plate but none at all may survive A-H-C.

To my mind, failure to survive this test must be taken as a distinct point *against* the pathogenicity, or perhaps we should say asthmogenic potency, of a streptococcus, even if it was present in heavy growth on the straight plate.

Faecal Specimens. As has been mentioned several times in this chapter, many of the toxic idiopathies, and in particular the allergic dermatites, including ichthyosis and dermographia, seem to derive their idiotoxins chiefly from gut sepsis. In fact, excluding asthma, hay-fever, the para-hayfevers and idiotoxic rhinorrhoeas generally, I should say that the bowel specimens are perhaps of more use than swabbings, etc., from the upper air passages.

From the gut it is highly desirable to get a specimen quite fresh, and I myself make it a rule to send the patient from the consulting room to a water closet to pass the specimen.

I find that nearly everyone at first reacts to this suggestion by protesting that this is impossible ; but, given goodwill on the part of the patients and mental concentration

on the job in hand, then, in say nineteen cases out of twenty, they return smilingly successful to the consulting room bringing their specimen with them in the appointed receptacle.

Babies, of course, are looked after by their mothers when a specimen is needed.

It is the children of from 8 to 12 who are the most difficult. They often have such a hang-over of nursery inhibitions that they may burst into tears at my inordinate request, or at their own failure to comply with it.

If I want such a specimen, and the patient can't or won't produce it, I make them come back to me again and again until they can and do.

If practicable, e.g. in hospital, a fresh rectal washout is convenient. A carefully taken rectal swab is handy too, and especially from babies.

Specimens of faecal matter sent through the post, or even brought from home by the patient, are of very little use on most occasions.

Faecal Cultures. These bowel specimens are cultured for streptococci precisely as were the swabbings taken from the upper air passages; they are cultured onto the straight blood plate, and also they are sub-cultured onto such a plate after auto-haemo-culture for 24 hours. The only difference is that in A-H-C from the bowel, the amount and concentration of implant makes such a difference to the nature of the resulting growth that I make it in four different concentrations, the heaviest is a loop-full of direct faecal matter into $\frac{3}{4}$ c.c. of freshly drawn blood from the patient. The second is a hundred times more dilute, the third is a hundred times more dilute than the second, and the fourth is a hundred times more dilute still. Usually growth from bowel specimens through A-H-C show coliform colonies preponderating over the streptococci, or they may even swamp the field, from the first concentration; often in the second the streptococci emerge; and in the third and fourth they may preponderate or even swamp the field. Occasionally, and with a very 'streppy' gut, all four dilutions show only streptococcal colonies after the A-H-C- ordeal. As we have found with the swabbings from the upper air passages, the preponderating streptococci in the straight plates (enterococci perhaps) may not be represented at all after the A-H-C- test, or may only show up scantily in the heavier concentrations.

Coliforms. When looking for pathogenic coliform bacilli from the bowel specimen, precisely the same manoeuvres are carried out save that lactose-bile plates are added to the blood agar plates, both without A-H-C and after it, in order to select the non-lactose-fermenting, and therefore presumably pathogenic, colonies. It is very noticeable sometimes that pathogenic or semi-pathogenic types of coliform bacilli may emerge, or even be in pure growth, after A-H-C which could perhaps hardly be detected on the straight lactose-bile or blood plates.

Womb Sepsis. If there is evidence of womb sepsis, we should certainly get, if possible, a swabbing directly from the cervix uteri taken carefully through a speculum; but this is of course not always practicable, and fortunately is seldom called for. Here the culture should again be on a straight blood-agar plate. It is particularly useful, in addition, to use the A-H-C selective method to eliminate vaginal saprophytes, however skilfully the swabbing may have been taken from within the cervix.

The above is more or less routine procedure for isolation whether from the upper air passages, the bowels, or the uterus. Of course, if a particular micro-organism is being hunted for, e.g. B. influenzae from the throat, or typhosus from the gut, then special selective media should be employed appropriately.

Requisite Blood. With regard to the taking of blood from patients for the purpose of A-H-C, usually $2\frac{1}{2}$ c.c. are enough for a swab or two from the throat, but 5 or even 10 c.c. will be needed if a very extensive overhaul is contemplated.

These amounts of blood should leave over a spare 1 c.c. of blood for a sedimentation test. My routine is to put exactly this 1 c.c. into a ' baby ' test tube containing a $\frac{1}{4}$ c.c. of sterile $2\frac{1}{2}\%$ sodium citrate—the two fluids being intimately mixed at once.

I find that 6 millimetres of sedimentation in an hour is about the upper limit of the untoxic normal person. If the patient is auto-inoculating heavily and spasmodically, that sedimentation may easily fluctuate from 5 m.m. to 20 in one day.

There is usually plenty of this citrated blood left over for haemoglobin estimation or even for complete blood counts. The results have, of course, to be multiplied by 5/4 to allow for the citrate dilution.

Blood films are not so very reliable when made from citrated blood, but will serve at a pinch. It is preferable, of course, to make such films at once from untreated blood.

Autogenous Vaccine. If, on the evidence, a particular micro-organism is thought to play an important part in producing our patient's symptoms, and if an autogenous vaccine is to be made for him, then this strain is grown in pure culture and emulsified in normal saline. This crude vaccine is then estimated for strength—usually by counting microbes per c.c., and is sterilised by heat at 60° C. for an hour ; after verifying the sterility it is diluted down in ' Carbol-salt ' (i.e. normal saline containing Phenol up to 5%). That diluting down has to be very considerable to be convenient for administering the very small doses I shall advocate.

If the peccant organism is in doubt, then suitable stock vaccines can be employed, or a dubious autogenous vaccine can be stiffened with a little of such a stock vaccine. For the many occasions when a stock vaccine is needed to deal with bacterial trouble in the upper air passages, I find Messrs. Parke Davis & Co.'s ' Mixed Vaccine for Bronchial Asthma ' is very useful—if given in the doses I shall advocate.

Size of Dose. Toxic idiopathy cases treated with a vaccine—stock or autogenous—are by most doctors given doses which I would consider much too big and much too few.

Those two or three heavy doses of typhoid vaccine which had proved themselves so successful as a prophylactic against typhoid fever were no true guide for us when, in the early years of this century, we attempted to repeat this success against the catarrhal infections of the upper air passages. The typhoid treatment was pure prophylaxis because, in a well sanitated country, we could be reasonably certain that our patients had no typhoid infection when we dosed them ; on the rare occasions when a mistake was made about this, there was serious trouble (e.g. in the case of the Holiday Girl, recounted in p. 220, who proved in the end to have been a typhoid carrier).

In England, and especially in the autumn, winter and spring, we are all carriers

of catarrhal organisms, so prophylaxis is impossible. We are all in the position of that Holiday Girl.

Persistent and Small. Looking at our problem from another angle, that which has kept us humans going in big, chilly, damp cities like London in the winter, has been a great multiplicity of very minor infections occurring frequently : perhaps two or three times a week ; these have produced a series of minute auto-inoculations which have on the whole made us more resistant than we would have been without them ; thus the bane has been its own antidote. Natives of Angola, like the people exploring the Polar regions already referred to, fail to receive these beneficial auto-inoculations because, in respect of air passage infections, the locality they are living in is more or less microbe-free. They are not ' salted ', so they invariably go down with these air-passage infections when they arrive at more civilised, and therefore more infected, surroundings.

If anyone *should* receive big prophylactic doses against ' colds ' it would be just these polar explorers, or the natives of places like Angola, who should get them ; and they should get them, and respond to them, *before* they leave the microbe-free area. In fact, these Angola natives *do* have such doses before they are allowed to work on the Rand, and it has proved a marvellous life-saving expedient,

Per contra, in the 1914-18 war, the London Cockneys suffered far less from coughs and colds in the wretched conditions of the trenches than did the troops coming from the more salubrious Australia : the Cockneys were better salted than the Australians.

Copy Nature. Until we know more about it, that is to say until we can gauge correctly the exact immunological effect of any one vaccine dose, I feel we should be wise not to depart too far from that rhythm of auto-inoculations which has kept the human race going during these millions of years in face of gradually increasing danger from upper air-passage infection. To do that, we must imitate what naturally occurs—that is to say, we must keep the doses very small and very frequent.

Here I must again enter a caveat : I am not suggesting that everyone in Great Britain and Ireland ought to be dosed weekly in the winter with minute doses of catarrhal vaccines—autogenous or stock. That would be to hospitalise us all worse than we are now : there is a psychological debit side to any professional dealings with doctors, and we must make sure that it is outweighed by the gains from a visit. The people I have been considering above are the people who really do need professional help in the year-in year-out struggle against their bacterial environment, and in particular the asthmatics ; for, as I hinted on p. 136, I have come to the conclusion that they couldn't have asthma unless there were some damage (probably bacterial) to fix the symptoms in the air passages. It is one, but one only, of the causal factors.

Whenever the symptoms are placed at a distance from the causal infection (e.g. migraine, urticaria and so forth, derived from gut sepsis) we cannot expect these to be affected by the trauma factor which plays such a big part in asthma for example.

These symptoms experienced at a distance from the causal infection often do not respond satisfactorily to a routine dosage ; all the same, I think that the weekly

small dose I shall advocate is as near to optimum as our present ignorance will allow us to make it.

Size of Dose. With regard to streptococcal vaccines, from whatever source the streptococci have been obtained—throat, gut, cervix uteri, etc.—I find it a sound policy to give the same minute dose weekly for several months on end. As a rule I give one million cocci only, but repeat at these short intervals perhaps all through the autumn and winter.

When, as so often happens, we have one type of streptococcus dominant in one area and another type in another (green type streptococcus from the throat, a blood-indifferent type from the bowel, and a haemolytic type from the cervix uteri, let us say) it is then my practice to make a separate autogenous vaccine from each, to blend these equally, and make the total dose up to the one million.

We have been speaking of streptococci, and I believe that this group of organisms accounts for the majority of bacterially produced idiotoxins ; of course other organisms may be involved, though these are usually of considerably less importance. In the upper air passages I would say the B. influenzae affects people sometimes, as also Friedländer's bacillus ; with both of these I would make the dose $2\frac{1}{2}$ million, and not 1 million as with streptococci.

I do not think that microbes of the catarrhalis type are very often concerned with our present problem ; I disregard them unless they give very poor growth on plain agar, or unless they can survive auto-haemo-culture. When this does happen, I use a dose of $2\frac{1}{2}$ million.

With regard to coliform infection of the bowel, and this generally means the prevalence of a non-lactose-fermenting bacillus which survives auto-haemo-culture in heavy numbers, I think round about this same $2\frac{1}{2}$ million is as good a figure as one can fix.

I have put down these rather arbitrary round figures of 1 million or $2\frac{1}{2}$ million micro-organisms, not because a slightly higher or lower figure would be necessarily wrong, but because I wish to emphasise the need of very small doses ; but, so long as they are minute and persistent, I don't think the exact size matters very much—as is the case with the beneficent auto-inoculations. These are the doses I have been using for many years now. There seems to be no advantage to be gained by working the dose up and up : quite the contrary in fact.

In dealing with chronic air-passage infections, where the patient has much worse asthma in the winter and rough weather than in summer, it is usually a good plan to start this continuous ' drip ' of weekly vaccine in the late autumn and continue to the late spring ; then it is possible to give a holiday from treatment until the cold rough weather comes again. This arrangement, as I have hinted in several places, has an obvious advantage when we have the combination of winter asthma and summer hay-fever.

With the gut infections perhaps the reverse is true, since there are more food and drink infections in the summer, and therefore more gut sepsis at that time than in the winter months.

Virus Infections, we may be sure, must play an important role in chronic catarrhs of the upper air passages ; also, presumably, they influence chronic infections in other places, e.g. in the gut. I have made no routine use of virus vaccines with the toxic idiopathies, for I don't know enough about them ; I imagine there is a big field opening out for us there, but not yet, I think.

How Many Years ? Are we then to continue this vaccine treatment off and on for all the patient's life? Possibly something like this may have to be done, and especially with elderly people, with ' hopeless chronics ' in fact. But even they respond, sometimes surprisingly well, to this very mild but persistent vaccine treatment.

I recall a case of bronchial asthma I had long ago ; an elderly man of 75 years had been ' nursed ' through many winters with difficulty, but with considerable profit to his practitioner.

Bacterially speaking, it was a straightforward case of streptococcal infection ; I considered this was keeping up the chronic catarrh, and with it the asthma. I found it difficult to persuade his doctor to let me give his elderly patient these continuous small doses ; he told me blandly that his patient was in and out of his consulting room several times a week.

I contended that the patient would live all the longer as the result of my treatment, and eventually was allowed to proceed with it. I soon heard that the results were ' apparently rather good ', and then I lost sight of the patient and doctor.

Eighteen months later I met that doctor by accident, and asked how his old man was getting on. He shook his head, and told me with mock lugubriousness that the patient was financially almost a total loss : ' He used to be coming to me, or I to him, several times a week but now I hardly ever see him. Oh yes, he's quite well—never better.'

Both the doctor and his patient are now long since dead.

After they have had a winter or so of treatment, patients may tell you they seem to have ' grown out of it ' and stop treatment. We must suppose that those minute doses can't have much permanent effect ; but, to quote Sir Arthur Hurst's dictum : ' The best cure for asthma is not having it.' Partly, no doubt, they no longer expect to be ill : partly their friends have got out of the pernicious habit of considering them as chronic sufferers whose symptoms have got to be discussed at all costs ; the patients may even have taken an interest again in their work. Often, however, people come back and report that they were much better, or even clear of all trouble, for a number of years, two or three up to twenty perhaps, but say that the catarrhal infections have now returned, and with them the asthma, and demand to have the same treatment once more.

Adjunct to P.T.D. When discussing the causes of unpleasant reactions such as migraine or urticaria which sometimes follow promptly on a desensitising dose in a P.T.D. treatment, it was said that an intercurrent bacterial infection was often to blame ; patients inoculating themselves for a P.T.D. course were told to stop the doses if they got a feverish cold, diarrhoea and vomiting, and so forth. If such infections recur very frequently, or become chronic, the P.T.D. treatment may have to be given up till the infection can be dealt with successfully.

If one year's hay-fever treatment has been abandoned in this way, then inquisition must be made—and especially into infections of the upper air passages.

Usually the abandoned hay-fever treatment can be carried through successfully next early spring if the patient has been treated with minute weekly doses of a stock or autogenous bacterial vaccine through the winter. Often such a preliminary bacterial treatment is necessary before a P.T.D. treatment to any foreign protein.

As I have said before (p. 215), a very successful bacterial treatment may even render a hay-fever P.T.D. treatment unnecessary. It would remove most of the bacterial factor from a cryptic hay-fever; and the residual hay-fever symptoms will either be negligible or can be treated by pollen P.T.D. with much less fear of reactions therefrom.

To Sum Up. All things considered, bacterial treatment, or at least bacterial comprehension, is in my experience a very big part of the 'allergist's' stock-in-trade. Knowing when to interfere in the activities of his patients, and above all knowing when *not* to interfere, is perhaps the most beneficent part of the doctor's work—whether bacteriological or psychological.

CHAPTER XIII

EXERCISES IN PHYLACTOLOGY

This will be a backwater in my general argument; into it have drifted, from time to time, the things that I didn't want to leave out altogether, but which would have held up tiresomely what I wanted to say in Chapters IV and V. I don't pretend for a moment that it is in any way a complete exposition of the ' immunity ' element of our subject, but only a record of what I have myself experienced and speculated about in the past forty years.

' Phylactology '. I am taking French leave to call this chapter by that name in place of ' Immunity ', which is in many ways so unsatisfactory a word. Lord Justice Moulton, then the Chairman of our Inoculation Department, suggested the change long ago when he began to read up ' Immunity '. With his tidy lawyer's brain, he said: ' You all use *Pro*-phylaxis; then why don't you use *Phylaxis*—a more exact word than immunity? '

It does seem far better to think of ' protection against ' in place of ' exemption from ', because so often we don't get exemption. *Phulasso* = I guard, and *Phulax* = an armed guard; these words give us ' Phylaxy ' = protection—and so Phylactology, the science of biological protection.

Phylaxy would then take its proper place as head of a distinguished family: the well-known *Pro*-phylaxy = protection provided beforehand, and the almost equally famous *Ana*-phylaxy = artificially reduced protection; also Sir Almroth Wright's useful word *Epi*-phylaxy = increased protection at a given place, and *Ek*-phylaxy = drained of protection at a given place.

Then I might introduce, but diffidently, my (or rather Sir Richard Livingstone's) *Hama*-phylaxy—protection at the time of attack, to replace the unsatisfactory ' therapeusis ' as the opposite of *Pro*-phylaxy. I would have preferred *Syn*-phylaxy, to represent synchronous protection—running repairs so to speak—as more likely to be understandable by the average man, but the Hellenist said it wouldn't do.

Ana-phylaxy is a little like *Hama*-phylaxy, and is also the least satisfactory linguistically of the -phylaxy family. It is, however, only second to *Pro*-phylaxy in popularity, so we must not touch it, I suppose, though *Kata*-phylaxy would express the meaning better—as Sir Almroth Wright always maintained. *Ana*-phylaxy has an international literature ten feet high, and one hesitates to disturb that!

Idioceptor. As said at the beginning of Chapter V (p. 60) the difference between the hay-fever subject and his normal neighbours is accounted for by the presence in his blood of a quality which we call the idioceptor; this idioceptor is specific and is reciprocal to the idiotoxin. Idioceptor is a quality of the serum in the sense that

precipitins or agglutinins are also a quality of the serum. I remember Sir Almroth Wright being asked many years ago whether he could demonstrate opsonins on a plate or in a test-tube. With his customary fondness for biblical language, he said : 'No ; but by their fruits ye shall know them.' We have no knowledge of the idioceptor as a substance any more than we have knowledge of a precipitin or an opsonin as a substance—it is a function of the blood, and we know it by its fruits. It will, however, be convenient, and a concession to our finite minds (at least to mine), that we should speak of the idioceptor as though it were a material entity circulating in the blood of, let us say, a hay-fever subject.

Not a Minus Quantity. Hay-fever serum does not exert its influence through some deficiency, or from the lack of some protective substance which is found in the normal person ; this is demonstrated by showing that normal serum injected into a sufferer does not in any way hold up, or prevent, or reduce, any of the toxic idiopathy phenomena ; and in particular, it does not hold up the diagnostic skin reaction when injected into that piece of skin.

Blood Transfusion. I have transferred to a very anaemic but quite horse-normal man 800 c.c. of blood from a strongly horse-sensitive donor. The anaemic recipient became slightly horse-sensitive for about a fortnight as tested with a horse-scurf pathophane. Nowadays, one takes it for granted that if blood is taken for a blood transfusion from a hay-fever subject and given to an individual who is normally exempt from this disease, then the recipient of the blood will become temporarily a rather feeble hay-fever subject, i.e. his eyeballs will give the characteristic inflammation when pollen extract is dropped onto the conjunctival sac, and he will also very feebly give the diagnostic skin reaction. I have no doubt (though I have never carried out the experiment) that if such a non-hayfever recipient of hay-fever blood were turned loose in a hay-field in midsummer he would clearly show slight symptoms. From these experiments we can deduce that the idioceptor has a positive sensitising quality which can be transferred with the blood from a sensitive subject to a previously non-sensitive.

The Prausnitz-Kustner Reaction (P-K). It can be demonstrated that this quality of the blood resides, in part at least, in the serum, because if the serum is transmitted, the sensitivity is transmitted with it. The experiment was first performed with the serum of Kustner, a fish-sensitive person. Kustner's serum, both undiluted and diluted 1 in 10, was injected into the skin on the arm of Professor Prausnitz, who is in respect of fish a normal person. On neighbouring patches of skin, control injections of saline and normal serum were made. Twenty-four hours later a solution of fish muscle was injected into another place. In 15 minutes, all the areas into which Kustner's serum had been injected gave a weal surrounded by an erythematous flush, while the controls did not show this.

P-K = A General Phenomenon. Professor Prausnitz told me the above story, adding that he assumed this was a general reaction for all the sensitivities, but that he hadn't been able to bring it off in the obvious case of hay-fever. I think this difficulty was undoubtedly due to the fact that he hadn't at hand a sufficiently

strong grass pollen extract ; I tried it there and then, and found that it came off with the greatest regularity with my home-brewed supply of pollen extract. By the way, as we shall find when we are discussing the skin reactions, most of the trouble which people experience in getting sharp diagnosis is caused by using too much of too weak a pathophane.

It is well-known that the P-K reaction is general for all the idiotoxic sensitivities ; and it is to this reaction and the analysis of it that we owe most of our knowledge of the idioceptor.

Testing the Sensitised Area. How can we demonstrate that the patch of skin to which the sensitive serum has been transferred has become sensitive? We can do it by Prausnitz's original method of subcutaneous injection of the idiotoxin at a neighbouring site. This idiotoxin gets into the blood via the lymphatics, and eventually all of it will have passed through the capillaries of the sensitised patch ; this will become oedematous by serous leak.

Equally, of course, a prick test, or any other dermal test to be described in this chapter, can be made on the artificially sensitive patch and on neighbouring and normal portions of the skin ; it will be found that only the sensitised patch will give any reaction.

Also the idiotoxin can be broadcast in the blood-stream by injecting it directly into a vein. Since the ' guinea-pig ' man is by selection not sensitive, save at the sensitised patch, a vast excess of idiotoxin can be injected without any danger. Here again, only the patch that has received the sensitive serum will show any reaction ; this reaction may be rather of a diffuse nature, resembling more an angioneurotic oedema than a typical skin reaction. The reason for this is that the injected serum, or much of it, tends to seep into the surrounding tissues both laterally and downwards before the idioceptor in it gets fixed to the local cells.

Another method by which the idiotoxin may be brought into contact with the sensitised patch can be demonstrated when this idiotoxin is a food. If the serum is from a person sensitive to, say, egg or fish, then, when the corresponding food is eaten by the ' guinea-pig ' person who has been P-K patched, it will be seen that the idiotoxin gets through into the blood-stream and again gives this localised, but not strongly outlined swelling at the sensitized patches.

Three P-K Contradictions

Food Entry. This phenomenon of specific foods getting through into the blood-stream, which was referred to on p. 227, contradicts the assumption of the physiologists that protein substances are never absorbed as such, but always get through into the body as quite non-specific albumoses, proteoses, and amino-acids. Though most do get through in that way, yet not 100% of protein does so. In no other way can we account for this firing off of the P-K patches by ingesting the specific food ; for the oedema response can only be produced by the specific protein substances concerned.

In early days, a famous research worker in another hospital afforded me a comic example of the way that food can get through the gut wall and into the blood-stream, and

so fire off the patches. Wishing to investigate a case of fish sensitivity, he, at my suggestion, laid down on his own skin a large number of these patches by the intra-dermal injection of this patient's serum.

He intended to experiment with them all next day. Unfortunately, he had never tried this sort of work before, and his wife gave him kippers for breakfast. Just as he was dashing off to his laboratory for his day's research, he felt an intolerable itching; on tearing off his coat, and rolling up his sleeves, he found that all the patches had got touched off through the fish idiotoxin from his breakfast kipper, which had gained access to his blood-stream through the gut wall. If the normal man (Professor Prausnitz) sensitised with Kustner's serum in the original experiment had *eaten* fish, he would have experienced the same thing.

I find that egg or fish gets through from the gut into the blood-stream in about 20 to 30 minutes. The idiotoxin in the blood-stream thus derived from the digestive tract does not disappear until several hours after the ingestion of the food; this can be demonstrated by frequent injection of small quantities of sensitising serum into the skin of the ' guinea-pig ' man from time to time and observing whether or no they produce oedema in about seven minutes by reason of egg or fish protein still circulating in his blood-stream.

As suggested on p. 228, any foreign protein, say from eggs, in the blood stream would be experienced as a loathing for eggs, and a queer feeling of discomfort. This, I think, must be by reason of the egg substances circulating in the central nervous system. If anyone doubts this loathing, let them try eating, say, fifteen eggs at a sitting when they are not accustomed to eggs in that quantity.

E.N. and T. Delusions. This P-K reaction is of value in correcting the claims of the Ear, Nose and Throat Specialists that the noses of hay-fever subjects are necessarily abnormal in anatomy or in histology. I have, for instance, made the inferior turbinate of a normal man highly sensitive to grass pollen by the simple process of injecting these tissues with serum from a strong hay-fever subject. When this man so sensitised was given a pinch of grass pollen as snuff, he sneezed uncontrollably, and soused several handkerchiefs with the nasal discharge, just like a hay-fever patient.

Psychologists' Delusions. Also the more extreme claims of the psychologists are disrupted by the P-K reaction, as was said on p. 206. If the toxic idiopathies are ' all nerves ', i.e. merely somatic reflections of a psychical trauma, have we transferred these ' nerves ' by transferring a ½ c.c. of serum? Clearly we have not; yet we have transferred the disease with the little drop of serum containing the specific idioceptor. If, as I believe, emotions *are* necessary for an idiotoxic response, it is the normal man receiving the hay-fever serum who will supply *normal* emotions! That is what was said in Chapter XI.

Local Fixation of Idioceptor. In the P-K reaction, only the injected area becomes noticeably sensitised. This can be demonstrated by bringing the idiotoxin into contact both with the sensitised patch and also with the normal neighbouring skin in numbers of different ways, of which the most convenient and most convincing is by the intravenous injection of the idiotoxin. It is clear from these experiments that the sensitivity, or at least much of it, remains where the serum has been injected

into the skin. Apparently it remains in this place for quite a long time—certainly for several weeks up till a month or more. Some say it lasts for ever, but my own experience is that the effect gradually weakens, possibly owing to minute and imperceptible quantities of the idiotoxin flowing through the blood-stream, thus over the course of weeks or months gradually using up the sensitivity.

Cell Sensitisation. The fact that the sensitising serum when injected into a piece of skin affects that place particularly, and not nearly so much the rest of the body, shows that the idioceptor element in the serum does not all get into the circulating blood but is fixed locally, i.e. to a stationary cell : we are dealing with a fixed-cell sensitisation, and our tests for this provoke a fixed-cell response. We have seen that very various portions of the hay-fever patient's body can be made to show reactions artificially, or to show them in the course of a toxic idiopathy : the surface of the skin, the eyes, the nose, the throat, the gut, the rectum, the central nervous system—any of these may react to the idiotoxin wandering in the blood-stream ; we are driven to suppose that any portion of the body may so react.

There is one form of tissue which is common to all these areas : blood capillaries are to be found everywhere ; it is difficult, I think it is impossible, to show that any tissue reacts except the epithelial lining of the capillaries.

Sir Thomas Lewis, in his classical experiments on dermographia, has shown the fundamental importance of the cells of the capillary walls. These permit extravasation of fluid between cell and cell by making the cell junctions into minute loop-holes or chinks ; in this way the normal escape of plasma can be increased to produce a waterlogging of the neighbouring tissues with oedema fluid.

Black Swelling. Normally these chinks and crannies in the capillary walls only let through an amount of plasma fluid sufficient for its physiological action ; an outflow sufficient to produce oedema (whether in dermographia, or from a positive prick test, or an attack of toxic idiopathy like urticaria) is only excessive action on the part of the capillary cells.

But in the very intense form of urticaria known as angioneurotic oedema these chinks and crannies can go too far, and let through red and white corpuscles as well as plasma fluid.

This gives rise to what the sufferers call ' Black Swellings '. These resemble bruises both in appearance and structure. They only occur, so far as I have observed, in very persistent and intense angioneurotic oedema.

Perhaps there has been some adjuvant bruising ; if so, the patients have not noticed it, so bruising must have been produced with the minimum of external trauma. It is easy to see how, when the loop-holing of the capillary walls has been carried beyond a certain point, the capillaries must be in a fragile condition : very little ' trauma ', perhaps just the normal contraction of the surrounding muscles, may be enough to permit the escape of the red corpuscles. I have usually seen the black swellings supervene on angioneurotic oedema of the fingers or wrists. These must constantly be subjected to varying strains.

Skin Grafting. Many years ago I thought it would be interesting to attempt a cross skin-grafting between a hay-fever and a non-hayfever subject ; I wished to

see whether the patch of hay-fever skin taken from the hay-fever subject would still react characteristically to pollen in its new environment, and also whether the patch of non-hayfever skin from the normal, i.e. non-hayfever, subject would become sensitised by its new environment. What happened was that only one of these patches of transplanted skins showed any reaction and that not for a long time. I did not realise then, what should be quite clear to us now, that it would be impossible to get any reaction soon after the grafting because the nerve track from the patch of skin to the central nervous system of the old host had inevitably been cut, and in the new hosts had not yet been established. In the end, the patch of normal skin transplanted to the leg of a hay-fever subject began to react, whereas the patch of hay-fever skin transplanted to the leg of a normal subject never reacted. By this time, however, it was difficult to say which parts of the skin were grafted skin and which were ingrowing parts from the host's surrounding skin. In any event, after a considerable lapse of time, normal cells would presumably have become activated from the blood system of the new hay-fever host, while the hay-fever cells would presumably lose their virtue in the normal ' guinea-pig ' host in the way that a P-K transfer of serum gradually loses its virtue.

Appendix Testing. A year or so after this last experiment Prof. Ronald Hare and I attempted to demonstrate the sensitivity to grass pollen of an appendix excised from a hay-fever subject. It sometimes happens that this organ is found after excision to be extremely healthy.

Hare and I waited till in the course of normal surgery a healthy appendix was removed from an intensely sensitive hay-fever subject. This appendix was at once placed in warmed special Ringer solution through which bubbles of oxygen were passing and taken to the laboratory. (The ' special Ringer ' was the physiological solution designed by Sir Henry Dale for his experiments on anaphylaxy with the virgin guinea-pig's uterus.)

After a short while rhythmical contractions began in the appendix muscles, and these were recorded on a revolving drum in the ordinary way. We were disappointed to find that the addition of grass pollen extract to the bath produced no change that we could detect, either in the appearance of the appendix or in the rhythmical contractions as recorded on the drum. This failure we can now attribute to the fact that the idiotoxic response is not the same as the anaphylactic reaction with which Dale was working ; also, of course, the nerves of that appendix to and from the C.N.S. were necessarily cut by the surgeons.

P-K only in Man. Before leaving general statements concerning the P-K reaction, we must note that it seems to be impossible to sensitise the skin of one of the lower animals with human sensitive serum ; it is alleged that monkeys, or at any rate the higher apes, can be so sensitised, but I have not myself made the experiment. This of course falls into line with the fact that the toxic idiopathies, so far as I myself have been able to tell, are not to be found amongst the lower animals. For years I have interviewed veterinary experts, shepherds, huntsmen, and others who have the handling of animals, and have begged them to give me evidence of anything that can be a true toxic idiopathy, but I have been unable

to find a single example. The ' asthmatic ' pug dog and the ' roaring ' horses, with which we are all familiar, have nothing in common with the toxic idiopathies ; and the food-sensitive dogs which one sometimes hears about have, so far as I have been able to tell, nothing in common either. I have written repeated letters of enquiry to such papers as *The Field* : but though I have had many replies, I have never got any direct evidence of a true toxic idiopathy in animals as distinct from ordinary anaphylaxy. Perhaps I have been unlucky.

Anaphylaxy Pro and Con

Anaphylaxy, as I have said, is not the same as a Toxic Idiopathy—is not ' an allergic disorder '. Rabbits or guinea-pigs can of course be made anaphylactic to grass pollen as to any other protein by suitable injections, but the familiar phenomenon of anaphylaxy in the laboratory animals, and especially in guinea-pigs, does not in my opinion tally with the toxic idiopathies, though it has many points in common. Careless writers often use the two terms as elegant variants.

Heredity. Anaphylaxy, as seen in the lower animals, is not at all a heredity disease so far as I know, but is always artificial, e.g. produced by injections of protein material through a syringe. Slight differences do seem to exist in the ease or completeness with which animals can be made sensitive to some protein substance —say guinea-pigs to horse serum, in the business of standardising anti-diphtheritic sera.

It would be an interesting experiment (though hardly worth the labour) to breed from animals found to be most easily made anaphylactic ; in that way, a strain of hyper-sensitive guinea-pigs might be produced, but they don't occur in nature. I am trying this with rats, but don't expect much of the experiment.

Sensitisation, which is so essential a process for anaphylaxy, is not clearly demonstrable with the toxic idiopathies ; it sometimes even seems to be excludable. Thus, a baby born at the end of May is sometimes described by the mother to be almost ' born sneezing ' and to have had hay-fever from birth onwards at every recurring midsummer. Such a baby, at its first midsummer, cannot have previously been in contact with grass pollen ; and, moreover, the mother cannot have been in contact with grass pollen while the baby was developing *in utero* from September to May.

I should put it on record that, as against this difficulty in demonstrating any sensitising process, it certainly seems as if sometimes the prevalence of any potential idiotoxin in the atmosphere does tend to produce a crop of people sensitive to it.

The horse is the scurfiest of animals (see p. 279) so far as I know ; children habitually in the neighbourhood of horses—e.g. living in or near racing stables—also Masters of Hounds and their relatives, do seem to become more often horse-sensitive than the people not living in such horsy surroundings. But, of course, these horse-contacting people would naturally be the first of the population to be aware of this peculiarity of theirs, and so would naturally come first for anti-horse treatment. Certainly the vast majority of people who work with horses don't become in any way horse-sensitive, just as most farmers don't become hay-fever subjects.

The Egg Case. Young mothers of intelligence know exactly when their babies first make contact with any specific food, and they have frequently reported to me that the *first* swallowing of egg substance produced urticaria, vomiting and diarrhoea. It is always possible that unintelligent mothers may overlook such eggy foods as ' Ovaltine ', or even sponge-cakes (see p. 52) or ' Ovaltine Rusks ' ; but I have had reliable evidence that sometimes the very first contact with egg protein produces trouble.

' I tried for the first time to give my baby a tiny quantity of soft-boiled egg with a spoon, but he cried, and refused to swallow any of it at all. After perhaps half-an-hour of fighting with him about it, I noticed that his lips were covered by big blisters ; I naturally thought that he was making all this trouble only because the lips were sore.

So I stopped all spoon-feeding, and waited for a week till the lips were quite normal. On trying carefully again with egg, I found that not only did he refuse to swallow any of it, but wherever the eggy spoon touched his lips a blister appeared in perhaps five minutes.

No, I'm *quite* sure that he had never had egg food in any shape previous to my first feeding him with it as I have described.'

People wedded to the idea of some necessary sensitisation process suggest that babies who have never had an egg before in their food must have become sensitised *in utero* via their mother's blood-stream, which perhaps contained from time to time small quantities of egg (p. 243) ; that seems to me dubious. This sensitisation *in utero* does not explain the babies ' born sneezing ' referred to on the last page.

The Case of Ambrosia Pollen. Ambrosia Trifida—known locally as rag-weed—grows rankly over much of North America apparently ; where it grows its pollen creates havoc in those of the population, and they are not a few, who are sensitive to it. On this side of the Atlantic it does not grow wild at all ; it is in poor supply even in botanical gardens because, being an ugly weed, the gardeners don't cherish it. Apparently as a result of this scarcity, few people in Europe are sensitive to its pollen, and these only slightly so ; it creates no recognised para-hayfevers.

Ambrosia was, I think, the first pollen, other than grass pollen, that I tried on my hay-fever patients ; but, after two or three years searching amongst my comparatively few hay-fever patients of those days, I obtained only one definite positive response to it (on a pharmacist who had never been out of England), so I gave up trying it on patients.

During the last few years Elizabeth Budd and I have been making a more systematic enquiry into sensitiveness to the more easily wind-borne of our composite pollens, and for the sake of its American reputation we added an extract from ambrosia pollen. We have found again that the necessary idioceptors to produce wealings to it are rare in our population.

Fig. 39 shows the relative amounts of total wealing given to eight different compositae pollens by one hundred patients found to be sensitive to one or more of them. These were collected from a thousand or so of patients sent by their practitioners for prophylactic treatment of hay-fever.

It will be seen that, in producing positive skin responses, the Artemisia, Asters, Solidago, Senecio and Dahlias (in that order) come out at the top of the class, with

Ambrosia (in this series) at the bottom. Ambrosia is presumably at or near the top of the list in Canada and the U.S.A. It would be interesting to know about that.

I think we can only account for its low grading of sensitivity in England by the fact that ambrosia does not grow on this side of the Atlantic ; and that means, I suppose, that there must be a process of sensitisation by contact with ambrosia—if not by the patient, at least by his ancestors, or there would be no idiosyncrasy.

FIG. 39

In Chapter XV, when discussing specific response to prick tests, there is a suggestion that a visit of a few years to the U.S.A. might produce a sensitivity to ambrosia pollen. Particularly interesting responses were given, on their return to England, by a mother and daughter (see Fig. 62 *a* and *b* on p. 297) who had been war refugees across the Atlantic Ocean.

I propose next year (1950), in addition to our routine testing of the hay-fever patients to grass pollen and to the compositae group, to try them all with Ambrosia as well ; then ask them if they have ever been to America ; and, if so, for how many autumns have they lived there. That should tell us if there is any correlation between contact with Ambrosia and a specific sensitiveness to it.

No Artificial Toxic Idiopathy. Against the above we can say that, so far as I have tried, a sensitiveness to grass pollen or to horse scurf cannot be produced by the simple process of inoculating a normal person as one would inoculate to produce anaphylaxy in a guinea-pig or rabbit.

I have inoculated myself at various times, both intravenously and subcutaneously, with enough grass pollen to kill a hay-fever subject or make a guinea-pig anaphylactic ; but I have never been able to produce a recordable weal on my skin by a prick test through the pollen pathophane, nor have I ever experienced any symptoms of hay-fever.

Different Symptoms. Anaphylaxy to a given protein, and the corresponding idiotoxic sensitivity to the same protein, can both exist in human beings ; the one is an artefact, and can be produced by deliberate injections ; the other, by heredity. The results to the patient of contact with the specific poison are very different.

Horse asthma, i.e. an idiotoxic sensitiveness to horse scurf, or horse serum, or some horse emanation, is rather common amongst people subject to any toxic idiopathy. So, also, and especially in the first world war, was the repeated therapeutic inoculation of some horse serum for passive immunity into wounded men—for

example, against tetanus for wounds. Horse serum introduced parenterally to normal (i.e. non-horse-sensitive) subjects who have had repeated therapeutic doses of it produced at best the very feeblest anaphylactic responses ; but if we are rash enough to inject horse serum into a horse-asthmatic subject, we can easily kill or half-kill our man. If the two troubles of horse-anaphylaxy and horse-asthma are so very different, it seems wrong to suppose that they have parallel causal mechanisms.

The difference of opinion as to the meaning of the word ' anaphylaxy ' is presumably due to our difference in conception of the causal factors rather than to difference in the symptoms produced. I continue to regard anaphylaxy as essentially different from the toxic idiopathies.

Clear thinking here is important, for there is a move on foot to test idiotoxic pathophanes on factitious anaphylactic animals instead of on our experience of them in an Allergy Clinic. I don't think such tests would be reliable.

Desensitising the P-K Patch. The injected idioceptor will gradually be used up by a sufficiency of the reciprocal idiotoxin ; after this, the sensitised patch will return to the state of the usual non-sensitive skin of the normal man.

This desensitisation can be brought about gradually by a whole number of minute injections, e.g. prick tests conducted on the sensitised area ; or desensitisation can be produced at once by injecting into the blood-stream an excessive dose of the idiotoxin. This will produce the maximum effect that the idioceptor can give, and thereafter the area will be desensitised and normal. The excess of idiotoxin over idioceptor will of course produce no general result on the ' guinea-pig ' man because all the rest of that man beyond the patch is non-sensitive. The excess of pollen extract circulating in the blood-stream is soon eliminated by the body as was the fish or egg protein mentioned on p. 243.

Permanence in the Naturally Sensitive Subject. As contrasted with the P-K patches which we have been considering above, the hay-fever subject or other sensitive person cannot have a patch of his skin desensitised by repeated applications of idiotoxin. A skin test on a hay-fever subject can be made over and over again in one place without desensitising that patch of skin. For example, if you make a prick test on a given area three or four times a day for a month on end, the resulting weals are just as big at the end of that time as they were at the beginning and throughout the experiment.

They will probably fluctuate in size (see p. 261), but there will be no steady trend downwards in size of wealing as the experiment proceeds. We may deduce that the cells of the hay-fever subject are continuously being re-sensitised by the circulating blood fluid, but, however that may be, any given patch of skin will react over and over again without any apparent diminution.

The ' Poison Mixture '. As we have seen, serum from a sensitive subject (e.g. from a hay-fever patient) when injected into a normal skin, will sensitise the patch of skin so injected to the corresponding idiotoxin (i.e. grass pollen) : this sensitiveness can be demonstrated by bringing the idiotoxin into contact with the injected patch of skin in a number of different ways.

Another way of producing a reaction in the normal person is to mix the idiotoxin with some sensitive serum in vitro ; this so-called ' poison mixture ' of idiotoxin and idioceptor when injected into a normal skin will produce the typical weal, and in the usual time.

The Timing of these reactions might now be reviewed. The reaction shows itself in from 5 to 8 minutes, and reaches its maximum in from 12 to 15 minutes. It does not matter whether this is an injection of idiotoxin into a sensitive subject, or whether it is the introduction of idiotoxin into the sensitised skin of a P-K patch on a normal subject, or whether it is the injection of the above-mentioned ' poison mixture ' into a normal man's skin.

It does not matter whether the ' poison mixture ' is injected at once, or whether it is kept, say, for 24 hours, either in the ice-chest or on the bench, after the mixing has taken place.

It seems, therefore, that there is no preliminary binding taking place between the idiotoxin and the idioceptor ; or, if there is binding, it takes no time that can be demonstrated by timing the appearance of the skin reaction. Nor can it be demonstrated that there is any time necessary for the binding of the idioceptor onto the cells to be sensitised, since the ' poison mixture ' works just as quickly on a normal man's skin as idiotoxin will work on the skin of a hay-fever subject or other sensitive. All this suggests that the ' poison mixture ' *is* only a mixture of idiotoxin and idioceptor.

Pollen in the Blood-stream. Pollen gets into the blood of all of us at the time of the flowering of the grasses ; but usually it gets into the blood of the hay-fever subject only to a very limited extent ; he avoids the places where grass pollen is prevalent, whereas the normal man does not. Naturally the normal man gets the bigger dose of it.

There is serious trouble of course when pollen in such large quantities does get into the blood-stream of anyone very sensitive to it.

I have twice heard of small children suffering from hay-fever who had been allowed to lag behind by their negligent mothers when they were following a foot-path through a grass field in full pollination.

In each case, when the miserable and crying child failed to reach the other side of the field, search was made ; in both cases the child was found lying at full length, and quite unconscious, hidden in the tall grass.

Happily, both of these children survived ; but there had been apparently enough pollen in the blood going to the C.N.S. to produce temporary unconsciousness, and probably almost complete cessation of breathing.

It can be demonstrated that a week-end spent among the pollinating grass at midsummer will put into the blood of a normal man enough pollen to produce a slight but definite reaction when some of his serum is injected into the skin of a hay-fever subject. Of course it will produce no symptoms whatever in that normal man, because of the absence of the specific idioceptor in his blood. I have performed this experiment with the blood of a persistent town dweller who had just spent a long week-end amidst the flowering grasses at the pollenarium in June,

but I'm not so sure that the farm hand who lives perpetually amongst the grasses will give the same result.

I have failed to produce this reaction with the blood of a rabbit kept in a hutch among the pollinating grasses. It is conceivable that too much or too persistent pollen leads to its rapid removal from the blood-stream, whereas the pale city dweller has not yet produced the necessary phylactic machinery.

No Antibodies, etc., in Hay-fever. The serum of a hay-fever subject shows no antibodies that can be demonstrated—so far as my own experience goes. Dunbar credited this serum with precipitins and complement fixation ; he also claimed that he could find deflection of complement in inactivated hay-fever serum when using pollen or pollen extract as the antigen. Noon and I were unable to establish these points ; in particular (as was the habit of the laboratory at that time) we attempted phagocytosis experiments. We could find no deflection of complement, no precipitins, no agglutinating action ; nor could we demonstrate any specific opsonic power. All these points have been recently retested by laboratory work and this lack of demonstrable immune bodies has been confirmed. I think Dunbar was mistaken ; Noon and I could demonstrate nothing whatever in the hay-fever patient's blood save this curious capacity for sensitising normal human tissues ; we have found none since.

The Blood of a Desensitised Patient. The hay-fever subject and the normal man are alike in possessing no phylaxis against grass pollen which are demonstrable in vitro by laboratory tests, and the same may be said with regard to all the toxic idiopathy patients in respect of their specific sensitivity. Taking hay-fever as our example, it is disputed whether we can by its P.T.D. treatment produce a precipitin in either a hay-fever case or a normal man, but we can certainly produce a precipitin in laboratory animals, e.g. in a guinea-pig or a rabbit. These animals are therefore made anaphylactic by the treatment as can be demonstrated in the customary way by anaphylactic shock. Man does not show this perceptibly because he is resistant to anaphylaxy, and perhaps because he makes precipitins with difficulty. It is notable, therefore, that when a man should just be showing anaphylaxy after P.T.D. treatment in the hay-fever season, that is just the time when he does not show the idiotoxic response to grass pollen : another demonstration of the difference between anaphylaxy and allergy in Sense II.

Is Desensitisation a Misnomer ? When we give a hay-fever subject a heavy course of inoculation treatment such as is described in Chapter V, we render him no longer sensitive to pollen ; that is to say, he can walk about in a hay-field in June, or he can take pollen as snuff, or he can take into his blood-stream a dose of pollen extract that would previously have given him a terrific shock ; or we can test his skin in the ordinary diagnostic manner but get no response. He *was* sensitive, but has become non-sensitive. But this desensitisation is not of the same nature as the desensitisation against anaphylaxy. It is possible to show that the old sensitising capacity of his blood is still there, but is masked by a newly acquired characteristic in the serum consequent on his inoculation treatment. This newly acquired power masks but does not remove the idioceptor.

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Masking Effect. If the blood, instead of being taken from an untreated hay-fever case, is taken from such a case after P.T.D. treatment, then this serum, if mixed with pollen extract in vitro, will no longer form the normal ' poison mixture ' ; for, when injected into the normal man, it will not give any skin reaction within the statutory 5 to 8 minutes after injection. It would be natural to assume that the old sensitising power of the hay-fever patient's blood has been destroyed, but this is not the case—it has only been masked.

The patch of normal skin which has been thus injected with treated hay-fever serum plus pollen extract gives no reaction at the time of injection, as we have said ; nor will it ever react if it is left untouched. But if after the passage of a few hours some fresh idiotoxin is injected into this area, then a reaction will occur.

I ought to say here that I did not find this out for myself but was told it by Dr. David Harley ; I tried it and found it to be perfectly true.

The explanation seems to be as follows : the old sensitivity of the blood has not been destroyed : it is still there in the serum taken from the ' cured ' case ; but the new protective power created by the inoculation treatment counteracts it in some way. But then the sensitivity factor, i.e. the idioceptor, combines with the cells locally, whereas the protecting or masking substance does not so combine but in due course passes on in the lymph stream and is diluted out of existence by the total blood of the body. Similarly, and even more easily, the idiotoxin in the mixture passes on and is distributed over the whole body. Thus the injected patch of skin has become sensitised but has not been touched off. The amount of idiotoxin that was originally introduced is far too dilute when mixed with the whole blood supply to provoke any reaction, but when fresh or more concentrated idiotoxin is injected into this sensitised patch, then the reaction will occur as in the normal P-K reaction. Of course another way of firing off this patch, which has been left sensitised after the protecting factor, together with the idiotoxin, has receded into the general blood supply, will be to stiffen up the idiotoxin circulating in the blood, e.g. by the injection of a large dose, say of 20,000 units ; then enough of the idiotoxin will be quickly flowing through the capillaries of the sensitised patch to fire off this area in an acute reaction, which will show a typical wealing, swelling, and inflammation. We may deduce from this experiment once more that there is no efficient binding between the idioceptor and the idiotoxin ; nor is there any efficient binding between the idioceptor and the masking quality of the successfully treated case. It is still possible that there might be a binding between the masking substance and the idiotoxin ; that point we must explore by another arrangement as follows :

If in place of a normal person an untreated hay-fever case is injected with the ' would-be poison mixture ' (i.e. serum from a P.T.D. hay-fever case + pollen extract) then this patient does show the characteristic weal, though a small one. There has been no binding between the masking substance and the idiotoxin, therefore the idiotoxin is free to make trouble with the excessive idioceptor of the untreated case.

Artificial Tests for the Toxic Idiopathies

Let us here, where we have more room than we had in Chapter I, consider the general question of these skin tests of sensitivity.

Reliability of Skin Tests. It has been frequently found that the results of the skin tests made by colleagues cannot, unfortunately, be corroborated by us when we see the same patients. This discrepancy may be due to the poor quality or weak strength of the reagents employed—either by us of course, or by the colleague working elsewhere: or it may be due to some extent to changes which have occurred in the patient as detailed on p. 260; more often, however, it seems to depend on the reliability of the skin testing technique employed.

To avoid these discrepancies, it is really very necessary to employ a well thought-out and stereotyped technique.

Disease in Miniature. Tests for our toxic idiopathies have this in common with tests for tuberculosis or diphtheria: we make a safely small amount of the real disease in an observable and safe place to determine what the whole body, or a considerable part of it, might be expected to do if the attack of the disease were more general. So far as we can make it, our test is a test-tube or 'claustrated' experiment, as Almroth Wright used to say.

Artificial production of the toxic idiopathies in miniature is brought about by bringing a minute sample of the idiotoxin into physiological contact with the living cells of the reciprocally sensitive person.

In order to do this, it is clearly of advantage to use, not a crude idiotoxin such as grass pollen grains, but a watery extract made therefrom. In this way, any desired strength of the idiotoxin can be employed for the test.

Such an extract employed for this purpose of detecting the disease we call a 'Pathophane' (Pathos Phanein = to detect disease).

Grass Pollen Pathophane. As usual, hay-fever, our prototype, will serve well for this enquiry into the methods and meanings of skin testing.

On p. 1 we said that in this book hay-fever was to mean:

(*a*) The clinical resultant of the interaction between, on the one hand, the pollen of the grasses, and on the other hand the living tissues of those people who are sensitive to it.

Or, more colloquially, we described hay-fever as:

(*b*) What happens to people who are sensitive to grass pollen when this pollen meets their living tissues.

In the naturally occurring disease of hay-fever, the main classical symptoms (conjunctivitis and rhinorrhoea) are determined by the fact that the idiotoxin is airborne, and that the cells of the internal nares and of the conjunctival sac are the easiest *living* cells for an airborne irritant to reach.

Blackley shook bunches of pollinating grass and sniffed at the resulting pollen cloud. Thus he got the classical symptoms of hay-fever. A little more artificially,

he poked some pollen up his nose on his finger tip. If a minute quantity is put onto the eyeballs, it will produce conjunctivitis and lachrymation, as in the natural disease.

Dunbar put grass pollen up the rectum of a hay-fever subject and caused tenesmus and pruritis ani thereby.

Noon used extremely diluted extracts dropped onto the eyeball and took the resulting conjunctivitis as his test of sensitivity.

Taken as food or drink by a hay-fever subject, it causes a feeling of discomfort and unease in the belly amounting to nausea, vomiting and perhaps diarrhoea.

Just after the 1914 war, a non-hayfever assistant of mine recognised a hay-fever case in the clinic as an old comrade-in-arms, and proposed splitting a bottle of beer with him to celebrate the reunion.

I doctored the beer with grass pollen, and though, of course, my assistant suffered no ill effects (for he was normal) that patient felt very queer and sick for quite a while, and subsequently was sick after running to catch a bus. He was annoyed at this prostitution of the rites of beer-drinking.

The Skin is better. The epidermis has clearly very considerable advantages over the areas we have been considering above as a testing ground for our idiotoxin. There is plenty of room on the skin for many tests, and the results can be clearly seen and roughly measured ; also, considerable oedema in the skin can be produced without unduly frightening or hurting the patient.

The disadvantage of tests on the external skin is that some surgical interference is needed to circumvent the superficial dead and horny layers of the skin which can't react because they are dead. We must introduce our pathophane—grass pollen of course in the case of hay-fever—to the cutis vera beneath ; thus the living cells of the patient can be brought into physical contact with the pathophane, and so may thereupon react by the production of oedema.

The superficial layers of dead skin cells can be circumvented either (i) by making an intradermal injection of the pathophane at the cutis vera level, or (ii) by removing the dead skin cells and then placing the pathophane on the denuded area. Both have their disadvantages. I use only a ' prick test ', to be described in detail later ; this is a useful compromise between the two methods.

There is a third possibility, (iii) allowing the pathophane to seep slowly through the upper horny layers of the skin till at long last it reaches the cutis vera : the ' patch test ' in fact. That has many disadvantages, and need not concern us for the moment ; its use will be discussed later.

The Intradermal Injection of a carefully measured amount of the pathophane would seem to be a very exact and scientific method of reaching the living skin cells ; but there are difficulties. We must consider the amount of idiotoxin to reach the living cells, for on that (other things being equal) depends the intensity of the reaction provoked by it in the sensitive person ; we must also consider the bulk of the inoculum : the strength in units per c.c. of the pathophane must vary inversely with the bulk of the fluid injected, or the amount of acting idiotoxin may easily be too much or too little for our purpose.

Dangers. Too heavy a shot for a particular patient (and he is necessarily an unknown quantity in this respect) may easily cause a general reaction.

I shall never forget a fat boy of six years with a little asthma whom I had been testing for this and that, and generally overhauling, for a report to a G.P. on diagnosis and treatment.

The mother, who had been singularly unhelpful during the long thirty minutes of the interview, said as she was leaving the consulting room with the boy : ' Perhaps I ought to say, Doctor, that he don't seem to like eating up his fish sometimes.'

I had already admitted some men to my consulting room for overhauls, and I was angered with the woman for her procrastination ; so, expecting nothing from it, I injected intradermally into the boy a ' small ' amount of fish pathophane, and told her to go into an empty room next door to wait for any reaction.

I had forgotten about this mother and boy, when her anxious face suddenly appeared through the door saying : ' Oh doctor, doctor, come quick! '

The boy was plum-coloured in the face, unconscious, and apparently not breathing. He had voided faeces and urine, and was drooling saliva from his puffy lips.

Artificial respiration, adrenalin, and oxygen, brought him round ; but I had the biggest scare of my life, and I have never used a diagnostic intradermal injection on an unknown case of asthma since then.

To avoid Danger of giving too much of a pathophane for any diagnostic test, we can either make the pathophane weaker but give a measurable volume, or give a strong pathophane but reduce the injected amount to the minimum. Both ways have disadvantages.

A weak pathophane, but of measurable bulk, must make something of a bleb if injected into the thickness of the skin. Even 0·1 of a c.c. must make a little bleb, and if injected under pressure, it will track into the skin along the lines of least resistance. If a larger quantity of a still weaker pathophane is injected, this mechanical waterlogging of the skin is still more apparent ; it often looks just like a positive response to the test because the watery fluid injected tends to spread into the surrounding skin.

So the decision between a positive and a negative result becomes a question of a lesser or greater spread of oedema fluid. That is very unsatisfactory, for it allows personal bias to influence the judgment.

I have seen half-a-dozen men arguing about a slight but spreading bleb produced by an intradermal injection of too weak and therefore too bulky a pathophane into a hay-fever subject ; opinion was sharply divided as to whether the spread was ' allergic ' or merely mechanical. The deciding factor seemed to me to be the theory previously held by the observer.

A Bad Technique. If, to avoid these difficulties, a ' mere flick ' of fluid—i.e. a minute, but unmeasured and unmeasurable, quantity is injected into the thickness of the skin, then we get all the above troubles simultaneously. We usually, but not always, get a spreading bleb, all accuracy of measurement is thrown away, and the doors are flung wide open to operational bias. The injector, who is convinced that a response *should* be obtained, will see to it that enough is injected to be able to produce it ; on the other hand, he will see to it that not enough is injected to produce too big a bleb in the skin by bulk of the inoculum if an oedema weal is not expected. That is not knavery for it is done quite unconsciously.

To avoid this danger, the diffident but conscientious injector of the mere flick of pathophane is driven to giving it ' blindfold ', i.e. he has to operate with unknown pathophanes in secretly marked syringes—a clumsy business. It was just these errors and ambiguities of technique detailed in the preceding paragraph which were responsible for belief in the ill-starred ' proteose ' theory for the diagnosis and treatment of asthma in the year 1931.

Removal of the Dead Skin to get at the Cutis Vera. The pathophane can be placed right on the cutis vera if the layers of dead and dying cells above it have been removed by the operator. This can be done by scraping the skin as one scrapes it for smallpox vaccination. More simply, this raw area can be reduced to a crimson line on the skin by a single scratch ; this, however, should not be deep enough to draw blood. Onto the scraped area, or the scratched line, the specific pathophane can be superimposed—for hay-fever this will be a grass pollen extract of course. In either case, if the skin cells react, the oedema will begin in perhaps 5 minutes, and be at its height in another 10 minutes, after which it will fade.

If the scratch is long enough, the different pathophanes with which the tests are to be made can be applied at different points along its length. Perhaps this scratched line method is still most often employed by investigators, but to my mind the ' prick method ' about to be described is far preferable, and I have not used the ' scratch method ' as a routine procedure in recent years.

The Prick Test. If a scraped area can be reduced to a scraped line, as above described, this in turn can be reduced to a point—literally a pin-point—by introducing the tip of a fine needle vertically or normally into the skin to the depth of perhaps half a millimetre.

Site Chosen. Usually the prone or flexor surface of the forearm seems the most convenient site for men and women, and for sizeable children ; here, the skin does not vary very much in thickness, and half a millimetre is a fairly safe guide for the depth of the prick. Sometimes, for small children, the upper surface of the thigh is more convenient because there is a larger area ; also, the thigh is sometimes more convenient for the women if, by the vagaries of fashion, their dresses are tight to the wrist. With very small babies, it is often more convenient to put them face downwards on a bed, or on their mother's lap, and make the tests across the shoulders.

The different areas do not necessarily give quite the same size of weal, e.g. the forearm often gives a bigger weal than the front of the thigh. In making comparative tests—as when recording progress in desensitisation during P.T.D. treatment—it is therefore desirable to make all tests for one patient on the same area.

The Prick. The ideal depth necessary depends to some extent on the thickness of the skin at the chosen area. There is in practice seldom any difficulty as to the depth of the prick, unless the skin is pathologically thickened, as, for example, by ichthyosis or by widespread eczema. If this should happen, search must be made for the most normal area of skin.

Introduction of the Pathophane. If the prick is made through a drop of pathophane placed on a selected area of the skin, then a minute trace of this fluid will be carried down to the cutis vera on the point of the needle, and so reach the reacting cells. After the prick has been made, then the drop of pathophane on the skin should be carefully wiped away with clean cotton wool.

I calculate that from ·001 to ·002 (a thousandth to a five-hundredth) of a c.c. is carried down in this way with considerable regularity, and this trace, if the pathophane is strong enough, will give a very satisfactory and measurable weal with, of course, no possible distending bleb of fluid in the skin.

It may be objected that the amount of the pathophane which can operate in this way is a seriously varying quantity, but experience has shown that the weals produced thereby are very comparable in size. For example, a deeper prick, or a more leisurely prick, by no means produces a bigger patch of oedema, though it is probably advisable to use roughly the same sized needle for the various tests. A prick test thus carried out certainly gives a minimum of surgical interference, yet with adequate chemical action if the pathophane inducted is only strong enough.

Strength of the Pathophane. Other factors being equal, the amount of the oedema must depend on the amount of the idiotoxin which reaches the reacting cells, whatever method we may employ in getting it there. If, to avoid distension of the tissues, and to give the minimum of surgical interference, we prefer the minute quantity of, say, a thousandth of a c.c. which we employ in the prick tests, then clearly this minute amount must be very strong, or we shan't get enough idiotoxin to the cells.

I have said above that Prausnitz probably used too weak an extract of grass pollen when he tried his P-K reaction for hay-fever patients.

I employ an extract of grass pollen of a strength of 20,000 units per c.c. for my hay-fever pathophane. If I use a weaker strength than this I get too slight a wealing to be conveniently measurable with my slighter cases of hay-fever. If I use a stronger grass pollen extract than this—say the strong mother-liquor, which has in it 100,000 units per c.c.—I would too often get inconveniently large wealings which would be unpleasant for the patient, and which could not be conveniently recorded on the usual 1 in. by 3 in. microscope slide, so I dilute the mother-liquor five-fold and use the 20,000 unit strength.

Identifying Marks. After a prick has been made through the drop of pathophane, and after this drop has been wiped away off the skin with a clean dry piece of cotton-wool, all that should then be visible is a minute pink dot at most, and generally there should be nothing visible at all. It is necessary, therefore, to make some mark with pencil or pen on the skin with some identifying symbol written alongside so that the place, and the nature of the pathophane, can be known. It is very convenient to employ for this purpose one of the ball pointed pens; an ordinary fountain-pen will serve, but the skin scales and skin grease quickly clog the nib. Also a metal nib tends to scratch the skin unduly, as is shown in Fig. 37 on p. 129, thus setting free any tendencies to dermographia. I make nowadays a

gentle dot-mark with a pen of the ' Biro ' type, or with a grease pencil, to indicate the rear position of a prick hole rather than make a dash with a pen as in that figure in Chapter VIII.

Even these little dot-marks shouldn't be too close to the corresponding pricks, because of this danger of the oedema tracking out to them, thus distorting and exaggerating the size of the weal.

For installing the drops and for subsequently pricking through them into the skin I employ a 1 c.c. syringe with a fairly fine short hypodermic needle ($6/10 \times 16$ mm.). This arrangement is convenient because the 20,000 unit extract of the grass pollen and the normal saline for the control can be drawn off from rubber-capped bottles into different syringes and can be used for a succession of patients : it is as well, of course, to label the syringes.

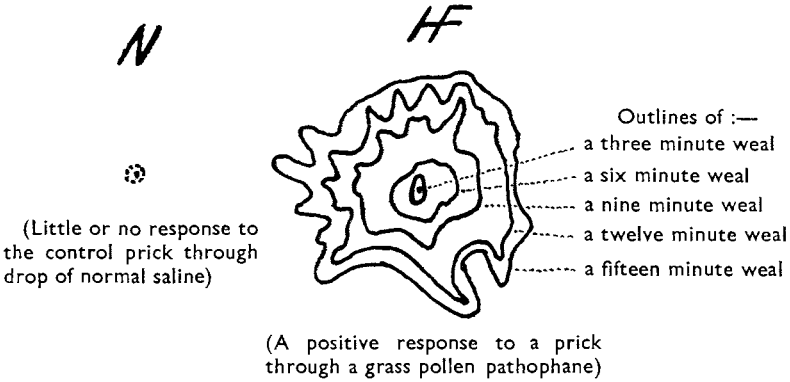


FIG. 40

The outlines show at life size the successive stages of a developing weal. A cine-film was taken of the weal as it came up, and stills were subsequently made from this film at three-minute intervals. The silhouettes from the weals on the stills have been superimposed.

The Diagnostic Reaction. If there is to be a positive response to the pathophane, a slight pouting of the skin round the prick hole is usually visible in from four to five minutes ; this spreads outwards from the central prick hole during the next ten minutes or so, as above described. After this time, the oedema ceases to flow from the central prick : and, as it seeps away into the subjacent and surrounding tissues, the reaction begins to fade.

Shape of Weals. This has no particular pathogenic significance and depends on the texture of the skin. It is very noticeable that, whereas with some people the skin reactions conducted over a space of many years are always circular or perhaps oval, with other people there is a tendency to great irregularity, and sometimes the oedema tracks out into long ' pseudopods '. Silhouettes of weals illustrating these peculiarities are shown in Fig. 41 below. As the oedema fluid naturally tracks out along the line of least resistance, the shape of the resulting weal depends upon the texture of the skin, and the pseudopods are due to potential channels or irregularities in the skin itself.

Beauty Parlours. I commend this method of testing the texture of a person's skin to the beauty-parlour specialists, if not to the dermatologists. All that need be done to test the structure of a skin is to make a prick test through a ' poison mixture ', or better still, through a drop of 1% histamine. The resulting transient oedema will disclose something of the texture of that particular piece of skin. Presumably the more circular and more piled-up oedema-weal will be produced on the more finely and more evenly ' woven ' skins, and vice versa. I feel sure that the beauty-parlourites would like to tell their clients that the texture of the skin must be studied before a ' treatment ' can be recommended for it.

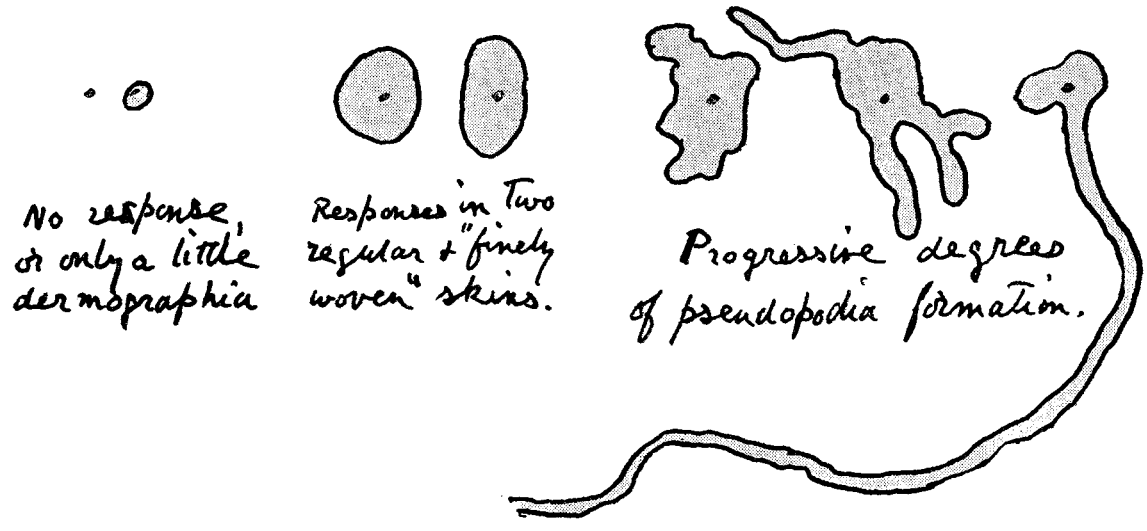


FIG. 41

Prick tests on seven different persons. The exact shape of any weal depends chiefly on the regularity, or otherwise, of the skin structure. This remains constant throughout life apparently.

Size of Weal. The outlined area of the weal may not give a very exact estimate of the amount of oedema. For example, in a very tightly woven skin, the oedema may be piled up high without extending so very far laterally ; and the tense oedema fluid may even be forced through the prick hole out onto the surface of the skin. Sometimes again, and especially at the end of P.T.D. treatment, the oedema weal, besides getting very much smaller in area, becomes also so very faint that it is difficult to see the exact edge of the wealing. In general, however, the size of the weal gives a reasonable guide to the amount of the oedema ; and, of course, it is by far the most easily observed and recorded.

Recording the Weal. What we are taking as our indicator of a reaction is of course the amount of the oedema. A record of the area of the weal, and so a rough estimate of oedema, can be made by superimposing on the weal when it is at its height, i.e. in from 12 to 15 minutes after the prick, a piece of glass—for example, an ordinary microscope slide, as suggested above. With the 20,000 unit per c.c. grass pollen extract, the weals of even very sensitive persons can usually be fitted onto an ordinary microscope slide.

The weal will then appear through the glass as a whitened area with a pink surround. The margin of this whitened area can conveniently be traced on the glass with a fountain-pen. To make the ink ' take ' on the glass, however, we find it convenient to smear white of egg on the glass ; and we mark the smeared side with a gummed label, on which of course can be recorded the name of the patient, the date, etc.

If the glass slide thus recording the silhouette of the weal is placed on an illuminated glass plate, and the case notes are placed above that, then the exact shape of the weal can be readily traced from the glass onto the case notes, dose card, etc. (as on pp. 74 and 75), after which, of course, the glass slide can be cleaned and used again.

Various Factors covering the Size of the Weal. It was said on p. 253 that an artificial wealing caused by introducing a pathophane to the living skin cells is really the disease in miniature—thus, when a hay-fever subject responds to a prick test with grass pollen extract, the weal is an attack of hay-fever in the skin ; it will be governed by precisely the same factors that govern the naturally occurring disease.

These factors, or rather a selected few of them, were listed in Chapter III, and discussed in that and succeeding chapters. Let us consider them afresh in connection with the skin tests which they must modify.

(1) **The Idiotoxin-Idioceptor Couple** is the obvious chief concern of our skin tests, for it is what we are trying to measure by them. It has been under discussion for the last half dozen pages and more, so need not be recapitulated here ; but other, and perhaps less expected, causal factors should be reconsidered briefly.

(2) **Trauma.** The weal will be all the bigger if the skin of the tested area has been previously damaged. We must remember, too, that trauma is never absent from these tests, for the introduction of the pricking needle itself causes unavoidable damage—as is easy to see when a control prick is made on a dermographic subject.

We have just been discussing the care that must be taken to avoid trauma to the skin when using pen or pencil near a diagnostic prick test. Equally, any scratched or bruised area must be avoided for that test.

(3) **Emotion.** If the patient is strongly perturbed at the time of the skin testing, the weal will be the bigger for it ; on the other hand, it is smaller if the patient is feeling quite calm and collected. I have made no very quotable experiment to illustrate this, but it is the impression I have got.

I have repeatedly noticed, for example, that patients usually give a bigger skin response at the *first* visit to the consulting room, and a smaller response perhaps a week or two later with no treatment in the interval. I have always ascribed this to a lessening of mental fluster at the second visit, especially with young women and children who may be apprehensive as to what the awful unknown doctor may be going to do to them.

The prick response cannot be abolished by mere mental calmness, so far as I know, unless, maybe, by practitioners of Yoga. I suppose, for most of us at any rate, the unconscious mind is always sufficiently active during life to furnish some slight emotional thrust towards symptoms.

A patient of mine permitted me to test her sensitivity to grass pollen while under an anaesthetic for appendectomy, and the resulting weal was not noticeably different from those obtained before and after the operation. It would be a question of the depth of the narcosis, I imagine.

(4) **Serous Fluidity.** Any decalcification of the blood will permit serous exudate from the capillaries to flow more freely ; therefore any considerable intake of vegetable acids—e.g. from wine, or fruit, or rhubarb—will tend to make the weals larger. As opposed to this, large intakes of milk, or other vehicle for calcium, will tend to make the wealing smaller. This is notoriously what happens with any tendency to urticaria or dermographia ; it is what happens also in the artificial urticaria of a skin reaction.

(5) **Adjuvant Sensitivities.** The weal will be bigger if, at the time of testing, there are present other general urticarial or dermographic tendencies, e.g. if our hay-fever patient is slightly sensitive to cat scurf or fish mioplasm, and has been affected by either of these idiotoxins recently before the testing.

(6) **Infection.** Bacterial intoxication undoubtedly produces a tendency to oedema in many ways, and so magnifies the diagnostic weal. This point was dealt with very fully when dealing with the effect of bacterial infection on p. 215. See also the anecdote on pp. 212 and 213 concerning the Britannia Girl.

(7) **Site of the Test.** The place selected for the test may make some difference to the size of the wealings. Thus the upper surface of the thigh gives, on the whole, rather smaller responses than the flexor surface of the forearm.

Also any prick made close to the fold of a joint frequently seems to give a rather larger response. This is particularly noticeable on the flexor surface of the forearm. If the uppermost, i.e. most proximal, test is made too close to the bend of the arm, there will tend to be a bigger reaction than if the test was made an inch or two lower down the forearm. It is noticeable that this fold of a joint is precisely the area where people are more likely to suffer eczema. I have (see p. 132) ascribed that to the ' trauma factor ' of the continual bending of the skin. Or it might perhaps be attributed to the better nerve supply to the skin near a joint.

Prick Test Reliability. Bearing in mind the factors numbered 2 to 7 above which help to govern the size of the weal, it is remarkable that the results made on the skin at any one time are so similar in extent and intensity. This leads one to suppose that the actual amount of pathophane introduced into the cutis vera by prick tests must be much more regular than one might think.

To my mind, the main advantage of the prick method of skin testing is its freedom from operational bias ; such a bias leads to far more error than all the impersonal influences mentioned above which affect the size of the diagnostic weal. With a prick test one cannot unconsciously make a bigger or smaller weal to fit a theory ; one cannot, without knowing it, make a false tracing off the skin onto the microscope slide, or off this onto the case notes.

Fluctuating Records. Tests made *simultaneously* on adjacent and comparable sites of the skin of a patient must eliminate most of the factors mentioned above

as liable to control the size of the weal. If, however, in place of making two or three tests simultaneously on a given day, we make those tests daily, we must expect variations because of all the factors (other than the idiotoxin-idioceptor couple) which control the size of the weals.

These fluctuations in the area of the weal may well be by as much as 50%, depending on circumstances; but, once the idioceptor begins to be adequately masked by P.T.D. treatment, these fluctuations are ironed out and the fluctuations come down to zero.

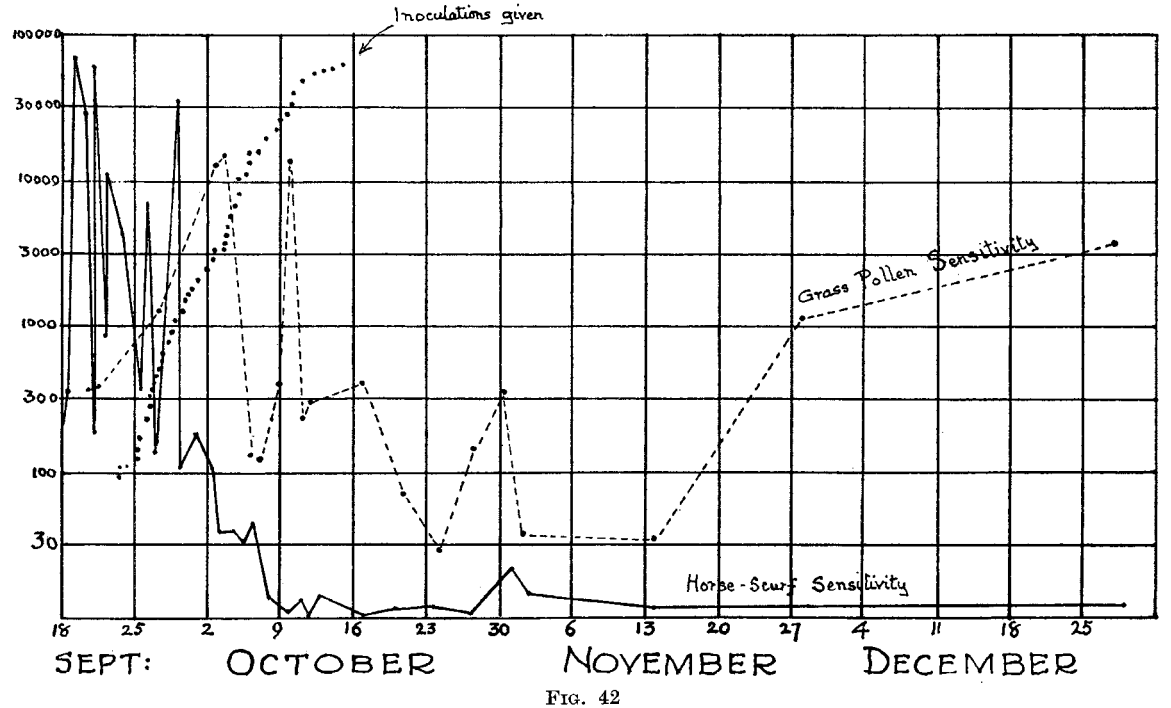


FIG. 42

Shows the violent fluctuations in the size of the weals which may occur if daily records are taken. When the desensitisation process gets to work, these fluctuations are ironed out as in the figure.

No Premonitory Wealings. Will there be any positive wealing on the skin before the patient shows symptoms of hay-fever? May there be positive wealings after symptoms have stopped? In other words, does the period during which a hay-fever patient suffers symptoms in succeeding years at midsummer coincide with the period during which his skin shows a positive wealing to grass pollen?

First: is the idioceptor present in the blood before the symptoms arrive? I have often, at the request of hay-fever mothers, tested for sensitivity the skin of their children to see if they yet showed the presence of grass pollen idioceptor, but I have never found one to react that wasn't at the same time beginning to show symptoms; but, of course, one cannot be sure it never happens. Routine hay-fever testings are made on asthmatic children, or children with urticaria, eczema, etc., and sometimes small positive responses to grass pollen are unexpectedly obtained;

but these are nearly always, if not always, accompanied by an increase of the toxic idiopathy at the height of summer, and are, therefore, really cryptic hay-fever (see pp. 8 and 9).

Persisting Reactions. When hay-fever does gradually peter out and the patient is no longer suffering symptoms, I have usually found that the skin reactions disappear ; but I am bound to admit that in certain cases the skin reaction does *not* disappear so much as would be expected : sometimes it is not very much lessened. I have supposed that the stopping of hay-fever symptoms may be due to a lessening of some other asthmogenic agent—a weakening of some other part of the machinery of causation other than the idioceptor. In that event, I don't see why the miniature hay-fever of the skin response doesn't go too : a tiresome paradox.

A laboratory colleague of mine, whom I see daily, and who was treated for hay-fever when 12 years of age, has lost all hay-fever symptoms for many years now, but he frequently disturbs me by producing on demand a positive skin response to grass pollen.

The Patch Tests. In our clinics we have used these patch tests very little, but it is convenient to round off this subject of diagnostic tests by saying here something about them as we have experienced them.

These tests are made by closely applying the supposedly peccant material to the normal skin of the patient and leaving it there for many hours. In practice the foreign substance under test is usually placed in the centre of a square of lint which is then strapped down tight onto the patient's skin at an area which is conveniently covered by the clothes—sites such as on the thigh, or on the skin over the shoulder blades. There the patch is left till the next day or even the day after ; then the lint is removed to look for a reaction to this treatment. A positive reaction is shown by inflammation, urticaria or even blebbing of the small area under test. The strapping with which the lint is usually fixed onto the skin has an unfortunate way of also causing as much trouble as is found at the patch ; so care must be taken that the two reactions are not mixed up together.

If the suspected material is a fluid, this will of course be soaked into the centre of the lint : if it is a solid it will presumably have no chemical action till at least some of it is dissolved in the patient's sweat or other body fluids : *corpora non agunt nisi soluta.* In either case, the substance, or its derivatives, must seep down through the dead upper layers of the skin till it reaches the cutis vera with its minute blood vessels—whence oedema may be set free.

Now, if grass pollen extract is thus applied to the skin of the hay-fever subject, we should, of course, expect that after a very inconvenient delay we should get some signs of response—just as we saw in Chapter I : wherever pollen lodges firmly on the skin of a hay-fever sufferer it may get dissolved in the sweat, and, if so, may seep through the skin down to the cutis vera and produce very distressing objective and subjective symptoms. That isn't at all a good test in a consulting room ; obviously a prick test is better. In practice, patch tests are only employed where there is, or seems to be, some idiotoxic specificity to a foreign substance which yet refuses to give the customary prick, or other dermal test.

It is significant that many, or most, of these recalcitrant foreign substances are not themselves of a protein nature and, therefore, would not be expected to set going any phylactic action. Such substances may be complicated enough, as, for instance, acetylsalicylic acid, atropine, and so forth ; but these fall a long way below the status of the protein molecule. Positive reactions become stranger still when such very simple substances as potassium iodide or formalin can produce them.

The explanation usually given for these positive patch tests is that the patient is reacting not to formalin or aspirin, but to his own skin tissues which have been denatured, and thereby have been changed into foreign substances, by these drugs ; the patient doesn't react to potassium iodide but to ' iodate of man '. This denaturing process would equally well explain the occasions when a foreign protein gives a positive patch test, but does not give a prick test.

This theory may very well be true at times ; but doubt creeps in when one reflects that, as we found to be the case with bacterial action, the peccant material may also be acting as trauma which localises some pre-existing idiotoxic tendency to the damaged area. It may, in fact, be a case of dermographia with twenty-four hours of ' scratching ' performed by the drugged patch. That suspicion is stiffened by the fact that the strapping itself so often gives a positive response that special care has to be taken to isolate the tested spot from the strapping.

Plant Dermatites

Chrysanthemums, etc. As is well known, the leaves and stems of some plants frequently cause a dermatitis which is indistinguishable from a toxic idiopathy occurring on the skin. I don't want to say much about them, but patients thus suffering have come to us, and we have had to try treatments ; why they are of particular interest to us here is that their action on the skin is usually to be detected by patch tests rather than by prick tests.

The most frequent patients who have come to us for a plant dermatitis have been the professional pickers of the low-grade chrysanthemums which are planted by the acre within easy reach of the London market ; these people come to us in the autumn of course, and suffering from a most intense and even disabling eczema of the skin wherever the stalks and leaves have touched it—chiefly on the hands and forearms of course.

' Good heavens, Mrs. Jones,' I said to a very dignified old lady, ' what have you done to your hands and wrists? ' ' It's them bloody chrysanths, doctor. They say they comes from Japan : I wish to God they'd kept 'em there! '

I have never been able to make an extract from stems, leaves or petals of chrysanthemums which would give any prick test ; but, if a leaf of the plant is strapped down onto a bit of the skin of the patient, very frequently it is found to have caused a patch of dermatitis next day. That result may have been produced by the juices of the plant seeping into the skin and so reaching the cutis vera and the capillaries, but it seems much more likely that the leaf does not act as an idiotoxin or proto-idiotoxin but by trauma of the skin—as does the nettle. This belief

is strengthened when it is found that the undersides of the leaves can cause a positive patch test much more easily than the upper sides.

A gardener told me once that if he takes care that only the upper surfaces of the chrysanthemum leaves touch his arms he gets no rash ; but if the under sides, covered as they are with minute short hairs, touch his forearms then up comes the rash.

Urtica. Returning to the stinging nettle, it seems not to be generally recognised that the urticarial rash they cause differs in intensity with different people ; I have found it to be more intense and more persistent with people of an allergic diathesis in very much the same way that gnat and flea bites show much more on such people.

Why the stinging nettle stings is surely to prevent its being eaten by grazing animals, and I fancy the same purpose was originally served by the rash-producing qualities of chrysanthemums, primulas, etc. Their action, chiefly at least, is by way of trauma and not as idiotoxins.

Treatment. If their action is chiefly by trauma, a desensitisation course with the plant juices would not seem to be very helpful, and I confess it has been a failure in my experience. It would be more logical to try and remove the factors which have made the patient ready to react to trauma. Of these I would put gut sepsis in the forefront, and I think I have had some success along the lines suggested in Chapter XII.

Lily Rash, the occupational disease which affects some of the professional pickers of daffodils in Cornwall and Norfolk, is in rather a different category. The terrible eczema sometimes produced on their hands and arms is undoubtedly produced by the thick slime from the inside of the leaves and flower stalks. I am told that, after an hour or two of picking, the arms get thickly coated with it. Tendency to the disease seems to be hereditary, and in some other respects it falls into line with the true toxic idiopathies.

Some patients do react slightly to a prick test with this slime, and when that is so we may hope to treat the cases successfully by P.T.D. Co-operating with Dr. W. H. Palmer of Helston who lives among these pickers, I have had some success with treatment along these lines.

I have had some failures too, which might have been expected, perhaps, from the fact that the prick test positives are rather poor, while the patch tests (possibly or even probably traumatic in operation) are more considerable. Here again I suppose that the slimy juice of the daffodils is chiefly of service to the plants by keeping the grazing animals (or caterpillars or slugs) from eating them : trauma, in fact.

Much the same is to be said of tulips and other bulb juices, I think, though I haven't had so much experience of them. As for treatment for all such plant illnesses we have choice between a not always successful but always troublesome P.T.D. course, treatment of other possible factors—such as gut sepsis, or change of occupation which is at once surest and cheapest.

Ware Mould Spores. Patients, by the way, sometimes complain that handling the dried bulbs in the autumn gives them eczema or, more often, asthma. I believe

that is chiefly due to moulds and mould spores which grow between the dried outer leaves which cover the bulbs. This is similar to the toxic idiopathies sometimes derived from handling tomato plants in a hot-house; that trouble is usually derived from the moulds growing on their leaves.

Patch Tests have not really been of much use to us in our clinics, or at least have seldom been used, either for drug or for plant idiopathies. Such cases have been comparatively few—perhaps because of our comparative lack of success with them. Also the practitioners have an easier treatment to hand—by suggesting avoidance of the injurious thing.

CHAPTER XIV

VACCINES AND PATHOPHANES FOR THE TOXIC IDIOPATHIES

I want in this chapter to give the practical details, first of collecting some of the substances to which our patients are sensitive (i.e. the idiotoxins); secondly, how we make the extracts therefrom for use as test substances (or pathophanes), and also as desensitising substances (or idiopathic vaccines).

Here, as elsewhere, hay-fever has been the key to most of our knowledge on the subject; so I will begin with a detailed account of how grass pollen is collected, and how we make the extract from it.

Our Standard Grass Pollen Extract

Specificity. The first problem that faced Noon in 1907 was the question of specificity. How many pollen extracts must he make if he wanted to treat hay-fever with success on phylactic lines? Must he have a separate extract from each of the hundred or so of indigenous English grasses? Must he, perhaps, throw in some foreign grass pollens as well?

Briefly, he concluded that pollen from any one grass would serve for diagnosis or treatment of all hay-fever cases: any one grass pollen was polyvalent for all the pollens of the Graminae. Biological problems have a way of not being quite so simple as appears on the surface; as this question of specificity or non-specificity of grass pollen seems to be the point most often challenged by iconoclastic new-comers to hay-fever research, I will take this question of specificity in greater detail in the next chapter.

Noon, however, had the practical answer, and subsequent work has not contradicted him.

Types of Grass. Taking it that any grass pollen would serve our turn, we have in the course of years tried a good many of them to see what selection would be the most convenient.

After a few tentative experiments, Timothy grass (*Phleum pratense*) was Noon's first choice; it was planted at his request by a friendly farmer in the normal broadcasting way; and, while we needed only a gram or two of the pollen for experimental purposes, that single planting sufficed. Timothy was found to have been an admirable choice, and it has been used ever since by us—though not used alone because its flowering season is too short for our collectors.

Staggering the Crop. One strain of grass planted in one field will all pollinate in about a week or ten days—a perilously short harvest if the pollen must be collected by the kilo, as is now found necessary. That harvest time has, therefore,

to be staggered from ten days or so up to the full two midsummer months—if practicable. That could be done by planting on different soils and in different places ; but it is uneconomic to have more than one pollenarium, so our answer has been to plant in one place different grasses and different types of these grasses, and in various ways to spread out the picking of the grass heads as much as possible.

Cocksfoot (*Dactylis glomerata*) yields a heavy crop of pollen, and develops much earlier than Timothy ; so Cocksfoot was soon added : it still is, with Timothy, one of our main sources of supply.

Meadow Foxtail (*Alopecurus pratensis*) develops earlier still and, if it grows well, will yield a heavy crop too. But we have found it a capricious grass, and the soil of our pollen station at Pyrford grows it badly ; so, after trying it off and on for many years, we have now reluctantly given it up as being too uncertain. Sweet Vernal Grass (*Anthoxanthum odoratum*) is also a very early flowering grass ; but we have found it to give a poor yield of pollen and have therefore given it up.

We have tried Lolium, different Fescues, and Rye grasses, but they haven't been satisfactory, though of pedigree seed. Yorkshire Fog (*Holcus lanatus*) we have tried for many years (as will be related in the next chapter when discussing specificity), but we have found it a poor cropper of pollen ; it tends to spread into, and so spoil, the other kinds of grasses. The farmers for some reason dislike it, and call it a ' weed ' ; it never does to gainsay the farmers.

Nowadays we are using only Cocksfoot and Timothy ; but, to keep up, if we can, a steady supply of pollinating heads, we plant the earlier and later flowering varieties of both of them. Also, where the grass in early spring is growing very strongly, and probably in excess of our need, we cut some of the grass heads back to make it flower later than the rest of that planting.

In such ways, we try to fan out the picking of the grass flowers over the two months from the 20th May onwards till mid-July. I wouldn't say that the arrangements we have made are necessarily the best that could have been done ; it is what, for one reason or another, actually has been done ; and it has been successful on the whole.

In general, the farmers have not been able to help us very much with their advice because they are professionally uninterested in the times of pollination. The people who were able to help us most were the workers in the Plant Breeding Research Station at Aberystwyth, and particularly Professors Stapledon and Jenkins. Both these distinguished men came to our pollenarium at Pyrford and helped us in matters of seed and soil.

The Timothy we first grew under Sir George Stapledon's direction had flower-heads sometimes eleven inches long, and looked more like bullrush heads than grass. We have grown none like it since. The farmers complained it was quite useless as grass, and only fit to bed down rhinoceroses on.

Amount of Crop. Roughly speaking, it takes two grams of pollen to furnish enough of the extract to complete a full P.T.D. treatment for one person, as described in Chapters V and VI ; and, now that we count our patients by thousands, sufficient grass pollen to carry us through a season has to be measured in kilos—not in grams.

From the three acres of ground which Lord Iveagh has put at our disposal, the thirteen kilos gathered in 1946 is a good crop ; the five kilos gathered in the abnormal summer of 1947 is a very poor one. Like all farming operations, the result, in spite of our carefully arranged plans, depends largely upon rain and sunshine occurring in appropriate amounts and at appropriate times.

The Planting. The grass seeds are planted in drills two feet apart. This gives us much stronger grass plants with bigger flower heads ; and it is easier to keep the ground clear of weeds than if the seed was broadcast.

FIG. 43
Field workers picking the grass heads for the pollen harvest. Note that the spaces between the drills can be clearly seen in the foreground.

It will be obvious that it enormously lessens the fatigue of picking the grass-heads subsequently, if neighbouring portions of the field all produce the grass-flowers simultaneously—i.e. if the seed is pure-bred, and all grass is of one type in any given spot.

Collecting the Pollen. Many suggestions have been made as to how the pollen should best be collected. In the grass-breeding stations they tie paper bags over the grass-heads, and the pollen forms within these bags. The grass breeders only

need minute quantities of pollen ; but, for the relatively enormous quantities that we need, the labour of fixing on the paper bags would be quite prohibitive.

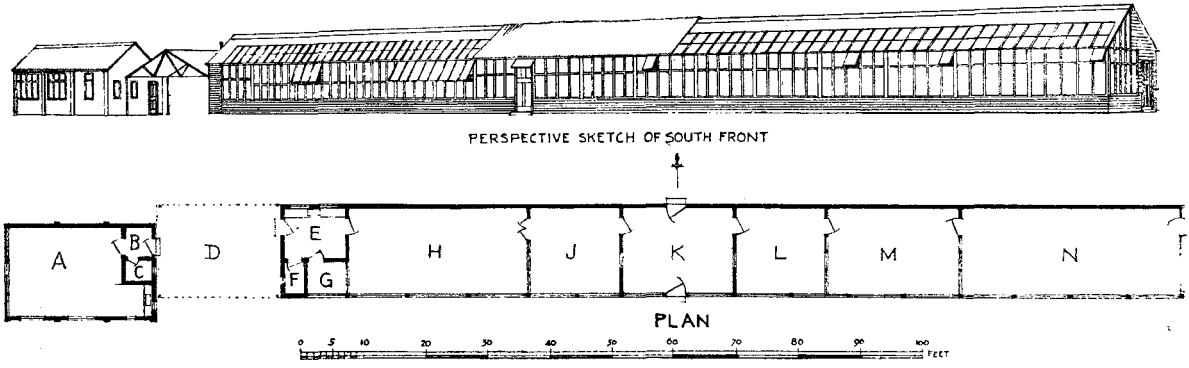


FIG. 44

Pollenarium in Elevation and Plan from the drawings of the architect, Mr. J. A. Hale. The letters on the plan signify as follows : (A) Land Girls' Canteen. (B) Lobby. (C) Girls' W.C. (D) Covered Car Park. (E) Scullery and Lobby. (F) Staff W.C. (G) Compositae Room. (H) West Developing Room. (J) Middle West Developing Room. (K) Office. (L) Drying Room. (M) Middle East Developing Room. (N) East Developing Room.

I have often played with the idea, and people have often suggested it to me, that we might employ an electrically-driven vacuum cleaner to collect pollen off the grass-heads in the open ; but when the area to be collected from runs into acres,

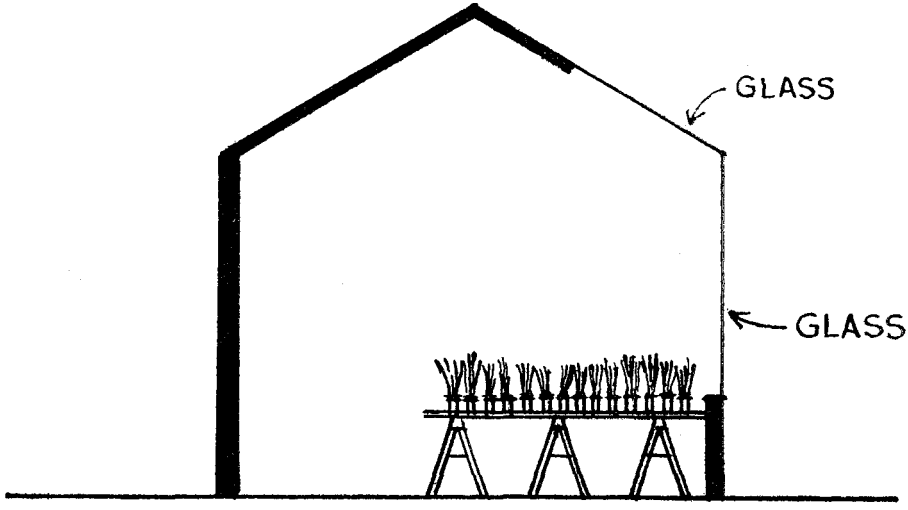


FIG. 45

Diagram showing position of grass heads relative to the window and sky-light.

there are obvious mechanical difficulties : more would be lost on the wind than we should get into the bag. Also, I believe, we should not get the pollen very clean.

Pollination Indoors. The plan devised by Leonard and Dorothy Noon in 1907 still seems to us by far the best.

The plan is to pick the grass-heads directly the anthers begin to form, to cut the stems to a uniform length of approximately 10 inches, and to carry them indoors—placing them in bunches of fifty heads, in a 2 lb. jam-jar filled with water. If they are placed in a sunny room, the anthers will first form ; then later the pollen develops on the anthers, coating them with bright yellow.

The Pollenarium. At first, the jars filled with grass-heads were set to develop in a window of some village hall or country house ; but it soon became apparent

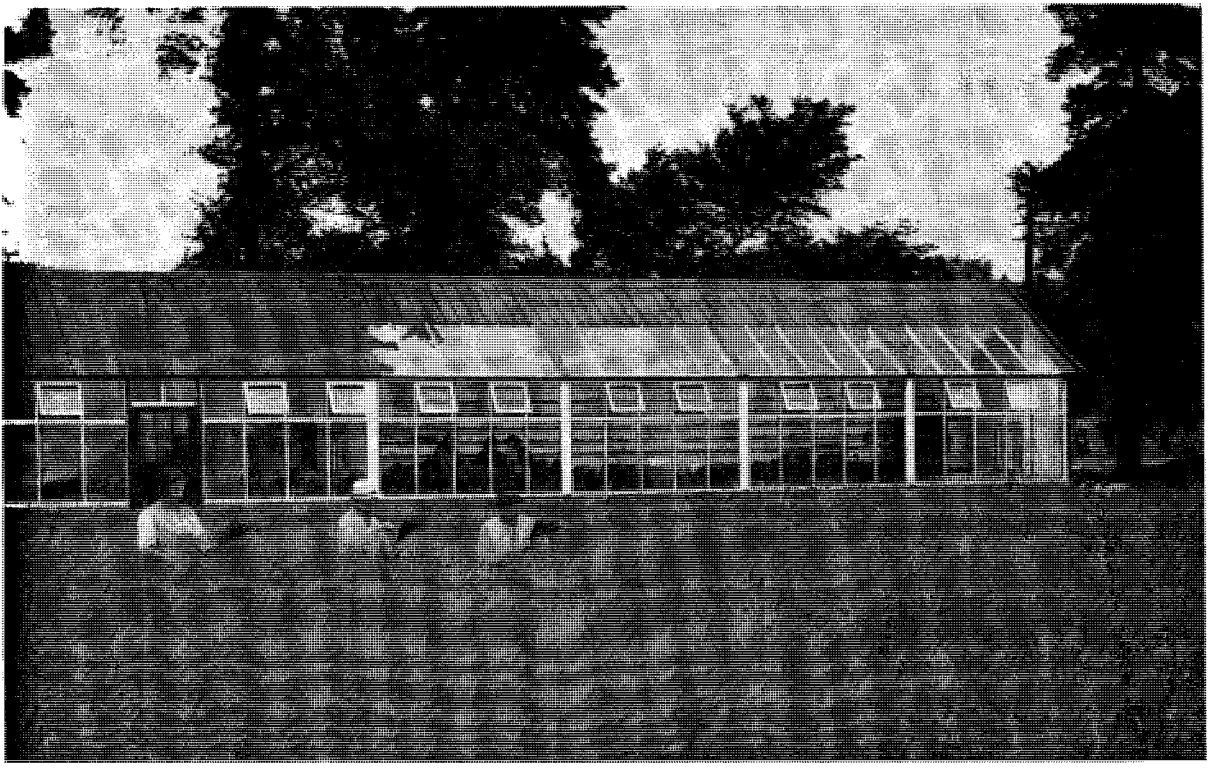


FIG. 46

Field workers picking the grass heads in front of the Pollenarium. The jars containing the grass heads can be seen within the building.

that the amount of pollen produced from the various jars was very unequal. Thus, on a table put into an ordinary domestic window facing south, and covered with jars containing the grass-heads, more pollen was produced from the row of jars next to the window-pane than from the jars on the rest of the table all put together. To get the maximum crop, we clearly needed a special building—and hence our pollenarium built on the Experimental Dairy Farm of Lord Iveagh at Pyrford, near Woking.

Fig. 44 shows an architect's sketch of the front of the pollenarium, and below it the ground plan to show how the various rooms, etc., are arranged in it.

The building, of course, faces south with a glass front above the level of the tables,

and also glass above the tables in the front portion of the roof; see the sectional drawing, Fig. 45, showing the position of the tables relative to the glass windows.

Fig. 46 shows the photograph of the east end of the pollenarium, and Fig. 47 shows the inside of the same developing room. This last photograph shows the way that the jam-jars containing the grass-heads are arranged on the tables. Note that in this photograph the windows of this developing room are open because the anthers are still forming.

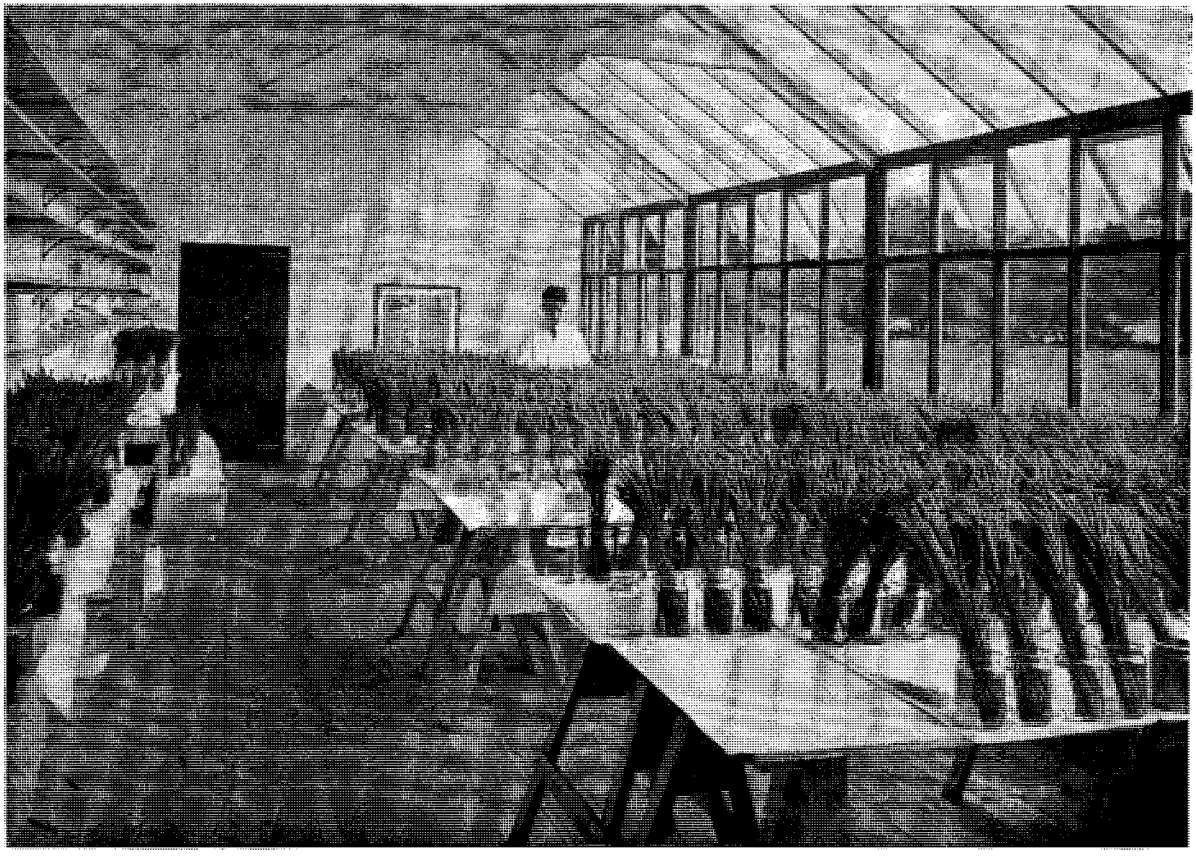


FIG. 47

Shows the inside of a Developing Room. Note that the windows are open and therefore the anthers are only just forming.

Heat and Light. It is very noticeable that in rainy and cloudy weather the pollen develops very badly, or may not develop at all. This we might expect, of course, from the comparatively slight development of pollen in the jars at any distance from a normal window. We made experiments by suspending above the grass-heads lamps which distributed to them three kinds of light: ultra-violet light, white light, and infra-red light. These experiments showed that it was radiant heat, rather than white or ultra-violet light, which favoured the grass-heads.

During a rainy summer—e.g. in 1946—in order not to lose a large part of the crop, we found it an advantage to arrange that radiant heat from electric radiators could be applied to the grass-heads from radiators suspended above them. In cloudy, damp weather this may easily make a difference of a two- or three-fold yield of pollen, as compared with the yield from the unradiated tables. Apparently it is a question of dryness; the pollen is not formed to fly off into the surrounding atmosphere unless this atmosphere is dry at the time.

Milking. When a good crop of the yellow pollen has formed on the anthers of the grass-heads, all the windows and doors are rigorously closed, and the bunches of grass-heads in the jars are gently patted with the hand. The pollen thus detached produces a cloud which hangs suspended above the tables in the still air. After all the bunches of grass-heads have been thus ' milked ', the milker steals quietly out and locks the door, perhaps to continue the process in another of the developing rooms.

After the pollen cloud has settled onto the greaseproof paper of a table, i.e. after an hour or two, the jars with the grass-heads are removed—taking great care not to spill a drop of water—and placed on a neighbouring empty table which has already been prepared with greaseproof paper.

The uncovered sheets of paper, with all that has fallen on them from the milked flower-heads, are manipulated as described in the next section.

After this the jars are replaced, and replenished with water if necessary, again taking great care not to spill a drop. The flower-heads can be milked in this way for perhaps three days in succession, and then, if there is a good supply of fresh grass-heads coming on in the fields, and if their anthers are beginning to develop well, the jars and the denuded grass-heads are removed from the room, leaving on the waxed paper all the pollen which has settled on it from the final milking—mixed up, of course, with a great many fallen anthers, bits of grass, and insects.

The Shed Pollen. The pollen, etc., is manipulated into a pile in the centre of each sheet of waxed paper by lifting the corners or sides of the paper and gently tapping it. It is noticeable in this and the following manoeuvres with the pollen that, once it has settled in a mass, it does not easily become a cloud again. There seems to be some agglutination or attraction between grain and grain, so that it pours almost like a fluid.

Apparently the small insects don't like being piled into the middle of the paper, and with one accord they begin to trek out from the central heap to the periphery of the paper, and so most of them may be removed. The sheets of paper bearing this pile of pollen are then carried from the developing room into the drying room (see plan in Fig. 44).

Drying the Pollen. For storage purposes we must get the pollen bone-dry. Of necessity the developing rooms have hundreds of jars of water in them, and the air must consequently be somewhat damp. The drying room, on the contrary, should have no standing water in it at all ; if necessary, i.e. in damp or cold weather, the

drying capacity of the air is heightened artificially by warming this room with electric radiators.

Fresh sheets of the waxed paper are placed in shallow wooden trays, into each of which a sheet of the paper fits snugly; onto these sheets the pollen is sieved through a fine wire sieve with three thousand or more meshes to the square inch. This allows all the pollen to pass through, but removes the fallen anthers, remaining insects, bits of grass, etc.—also any damp and therefore clogged pollen.

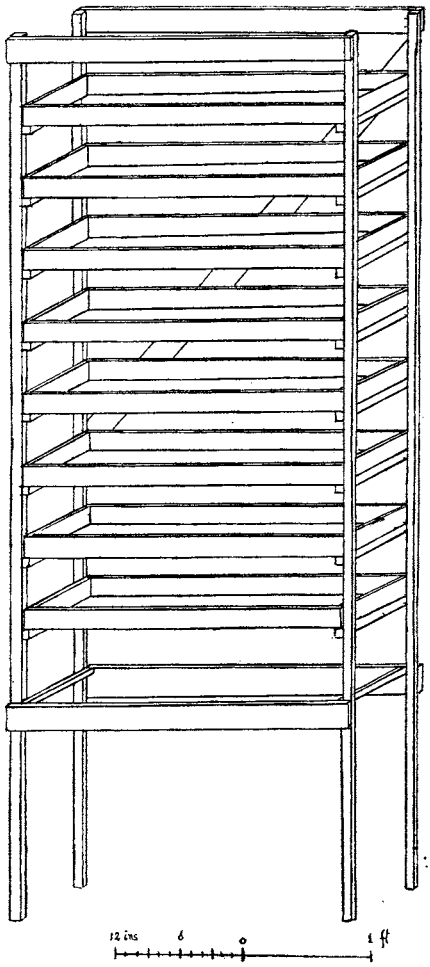


FIG. 48
Diagrammatic representation of a rack filled with drying trays.

When the pollen is thus distributed in a thin layer all over the tray, this is placed in a rack with similar shallow pollen-covered trays, allowing plenty of air to circulate between tray and tray (see Fig. 48).

If the weather is dry, the pollen may be left for perhaps three days before sieving once more. If all is then satisfactory, i.e. if the pollen pours like a fluid, and without the slightest trace of clogging, it is weighed; 30 grams of it are poured into a bone-dry half-pint U.D. milk bottle, which it about half fills. The bottle is carefully corked with a clean cork covered with greaseproof paper.

If the weather is not so dry, the pollen may have to be sieved every day to look for early evidence of clogging, and to give it a better chance of drying thoroughly.

It is necessary to have the bottles only partly filled in order to verify that the pollen is not beginning to clog. This can be done by turning the bottle upside down and seeing that the pollen continues to flow like so much fluid. If there is the slightest suspicion of lumpiness, it is probable that the bottle of pollen will go mouldy; to save it, the pollen must be soused with acetone, and perhaps half an inch of the acetone left standing over the pollen in the bottle.

Transport to London. As these bottles of collected pollen, either bone-dry or soused in acetone, accumulate in the drying-room cupboards, they are transported to the Pathophane Laboratory in the Inoculation Department of St. Mary's Hospital, where the attendants continue to watch for dangerous clogging by means

of daily examinations. This process is continued until the entire crop of pollen for the year is ready for extraction.

A good year, such as 1946, should give us between four and five hundred of such bottles of grass pollen.

A Continuous Process. As soon as the tables in a developing room have been cleared of the water jars, grass-heads, and wax paper carrying the pollen heaps, the room is quickly arranged for the development of a fresh batch of grass flower-heads which have been in the meantime picked in the fields and brought into the pollenarium. This business is arranged as far as is practicable so that the process is continuous in the four developing rooms. If possible, one room should be refilling; another room or two should be used for developing and milking, and yet another room should be having the deposited pollen removed from it and carried into the drying room and put onto the racks.

Extraction

Wax. The pollen grains are waterproofed by nature with bees-wax. Apiarists ascribe to their little pets the virtue of secreting this wax; but as the pollen grains are covered with good bees-wax in our pollenarium without the bees ever having got to them, credit for the wax construction should clearly go to the grass-heads and not to the bees.

This wax is removed by washing with acetone. The acetone containing the dissolved wax is decanted or run off from the pollen grains and cleared of the wax by distillation; then the distillate of acetone can of course be used again for a further extraction.

Apparently the acetone takes away nothing from the pollen but this bees-wax and some colouring; and when all the acetone is distilled away from it, we have left behind a lump of wax about as big as one's fist from a year's harvest of pollen, but with no grass pollen idiotoxin in it.

Dewaxing. This process we used to carry out in the milk bottles themselves, decanting the various washes of acetone off from the pollen grains. Latterly, however, we have used percolators, and run the acetone through the pollen grains to dissolve off the wax from them.

Five kilos of pollen are placed in a big percolator and covered with acetone. The tap below the percolator is then opened until the waxy acetone begins to drip from it. The tap is then closed, and the acetone is allowed to stand for two hours on, and in amongst, the pollen grains to extract the wax completely.

The tap is then opened again; and, as the waxy acetone drips through, fresh acetone is added above the pollen in the percolator. This process is continued till the percolate on evaporation leaves no greasy stain. This needs perhaps 25 litres of acetone for 5 kilos of pollen.

The addition of a vacuum pump to the percolator will of course help the acetone to come through more quickly; and after the dewaxing is judged to be complete

(by the greasy-stain test), warm dry air can be pulled through the acetone-wet grains in the percolator for 5 hours to dry them as far as possible.

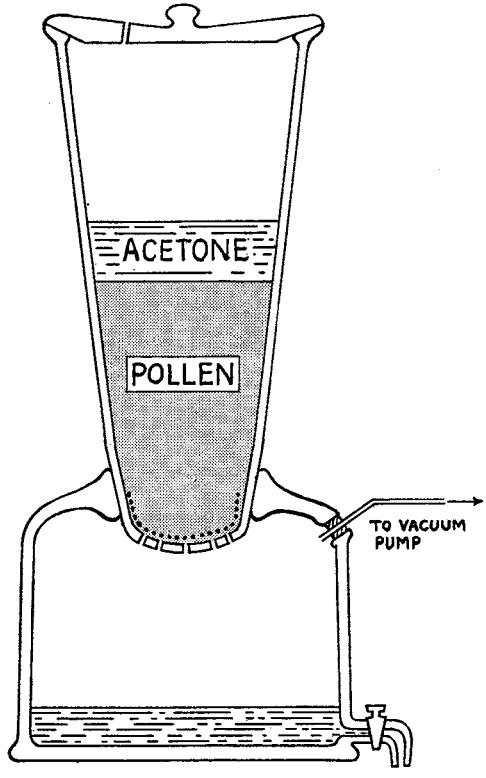


FIG. 49
A Percolator showing the pollen with supernatant acetone.

The pollen is then transferred to shallow trays, e.g. photographic developing dishes, and left in a warm room to dry out thoroughly.

When thoroughly dry, the now dewaxed pollen will again pour like liquid, and without caking or clogging.

Watery Extraction. The extraction of protein substances from the grains, which is to give us the material for idiotoxic vaccines and pathophanes for hay-fever, is of course made in a watery fluid. I don't think the exact formula for this matters very much, but we must have something approaching physiological saline. To each litre of distilled water I add nine grams of sodium chloride and three grams of sodium bicarbonate. Whenever I have made slight differences in the extracting fluid, I have not noticed much difference in the potency of the final extract nor in its keeping qualities.

The main difficulty in the extraction is, not so much getting the necessary proteins out into the fluid, as in getting this fluid out from the pollen grains which soak it up like so much blotting paper. By heavy centrifuging, and by sucking air

through the damp grains on a Büchner filter we can get off a good deal, but we calculate that out of 100 c.c. of extracting fluid on 30 grams of de-waxed pollen, 30 c.c. of fluid are irretrievable. The obvious solution is to extract in a succession of washes, so that, if a considerable proportion of the last washing is lost, it will only be very weak stuff. That is a good plan if only a kilo of pollen is to be handled, and that is how we did it till the labour involved became prohibitive.

The Old Way. It must be remembered that each gram of the pollen is to be extracted finally into 10 c.c. of fluid ; therefore the 30 grams of the pollen in a bottle must in the end be extracted into 300 c.c. of fluid.

To cut down the unavoidable loss of idiotoxin in that part of the extract which at the end must stick in amongst the pollen grains, this extraction is done with at least three successive washes.

Our routine is, or was, to add to those 30 grams of de-waxed pollen 120 c.c. of the extracting fluid for the *first* wash. The mush thus made is stirred up well and the recorked bottle is put into the refrigerator overnight. Next day this bottle is again stirred and, with three similar bottles, is swung on a centrifuge, the pans of which are built to take four half-pint ' United Dairy ' bottles.

We find, by the way, that it is considerably cheaper to build the centrifuge to fit these standard-sized bottles rather than to order several hundred receptacles to be made to fit the standard centrifuge.

The supernatant fluid is decanted into a large glass mixing-bin, and a 100 c.c. more of the extracting fluid is added to the centrifuged deposit of wet grains at the bottom of the bottle for the second wash ; the resulting sludge is well stirred up, and again put into the refrigerator for another twenty-four hours. Then for the second time, after the sludge is well stirred up, the bottle is swung on the centrifuge, and the supernatant fluid is decanted into the mixing-bin to join the extraction of the previous day.

Lastly, the remaining 80 c.c. of the extracting fluid is added. Once more the sludge is well stirred up, and once more it is left to stand in the refrigerator overnight and then re-stirred. The extracting fluid is recovered by hard centrifuging from this third washing and joins the common stock as before.

Compromise Method. If we are extracting from 10 kilos or more of pollen, as we have to do nowadays, 30 grams into each half-pint bottle would mean at least 300 bottles, and the necessary processes detailed in the last few paragraphs would take a month or two to perform. We have therefore compromised : we extract in 15-litre jars with a kilogram of de-waxed pollen in each jar ; we use two washes for each kilo of pollen—each wash being of 5 litres of extracting fluid. Each wash is centrifuged in a Sharples super-centrifuge. We certainly lose more extract in this way, and I wouldn't regard it as nearly so satisfactory as the older method—but it saves time.

Clarification. Whatever the method of extraction, all the washings are mixed well together. It will contain a good deal of particulate matter, and this is got rid of by putting the crude extract through a clarifying Seitz K filter ; it is run through either by gravity or by suction.

Sterilisation. This clarified extract is then put through a Seitz E-K filter, which will completely sterilise it by removing the bodies of any contained bacteria ; though, as a matter of fact, we have found that if the previous processes have been carried out in a careful fashion, there will be very little, if any, bacterial implant.

This sterilisation must, of course, be verified by making cultural tests both aerobically and anaerobically. After this, sufficient carbolic in normal saline is added to bring the total strength of the carbolic acid up to 0·5% of the total extract. It may be objected that this will be exceeding the 10 c.c. of fluid per gram of the pollen ; this is allowed for by the irrecoverable very dilute extract which is left sticking amongst the pollen grains after the third washing.

Noon-Units. Originally we took the extract from a gram of crude pollen as our unit. Leonard Noon was a considerable mathematician ; he could handle fractions with big denominators, or figures running to five or six places of decimals, with equanimity : this was to my complete mental confusion on occasion. I therefore proposed to him that we should, for the sake of weaker brethren like myself, keep to whole figures by using a sufficiently small unit. The unit I proposed was the amount of extract we could get out of a millionth of a gram of grass pollen ; in other words, a gram of grass pollen should furnish us with a million units if extracted as completely as we could manage it. I argued that we were never likely to split up such a small unit as that, for we had agreed that an extract from three-millionths of a gram of pollen was the smallest amount that could produce an effect on the pollen-sensitive eye. This millionth of a gram is of course the ' Noon Unit ', which is employed to this day.

Strength of Mother Liquor. If 10 c.c. of fluid are used for the extraction of each gram of the pollen, then, according to the above plan of calculating Noon units, 10 c.c. must together contain one million units, and each c.c. would, therefore, contain 100,000 units. This, then, is the strength of our mother liquor.

Dilutions of the Extract. In practice, we make nine dilutions of the mother liquor which are as follows :

(1) 100 units per c.c.
(2) 200 units per c.c.
(3) 500 units per c.c.
(4) 1,000 units per c.c.
(5) 2,000 units per c.c.
(6) 5,000 units per c.c.
(7) 10,000 units per c.c.
(8) 20,000 units per c.c.
(9) 50,000 units per c.c.

and finally, the mother liquor itself :

(10) 100,000 units per c.c.

These ten dilutions, it will be remembered, we met in Chapters V and VI when discussing the technique for the P.T.D. treatment for hay-fever. They are contained in the equipment with which we provide our self-inoculating patients.

It will be recognised also that the eighth dilution on the list—i.e. 20,000 units per c.c.—is the strength we have found most convenient as the pathophane when

making our prick test for the diagnosis of hay-fever and for the control of the P.T.D. treatment.

Keeping Qualities. This extract keeps fairly well, but it does gradually diminish in potency. I had by me till recently some extract made by Noon in 1908, and this forty-year-old stuff still showed the specific grass pollen reaction to the skin, but has by now grown very weak.

Judging by the skin reactions, the new yearly brew of extract is always stronger than the extract of the year before; and, therefore, though the two years old extract can quite well be used for a prophylactic treatment, yet the confusion caused by mixing the two vintages in the treatment of one single patient would be so great that for many years now we have used for every patient only the most recent extract.

If, for example, the patient has started to use 1946 extract and then, half-way through the course, has switched to the 1947 vintage, he would not be increasing by 15% at the switch over, but by something like 50% in all probability; he would, therefore, run the risk of an unpleasant reaction.

Post-Dating. For prophylactic treatment given from February to May we must, of course, use the pollen collected the year before, for the pollen of that year is not yet grown: we must collect in June and July of one year the pollen which is to be used prophylactically from February to May of the succeeding year.

So many unthinking people wrote every year to ask why they must use in the late winter and spring the pollen which had been collected during the midsummer of the year before that I have been driven to labelling the pollen extract according to the year on which it was to be used by patients, and not by the year in which it was collected at the pollenarium. This small deception has saved me from writing hundreds of explanatory letters every year.

Other Idiotoxins

The grass pollen extractions have been our chief preoccupation. Other extractions have been based on this very largely, and the strength in units has been calculated in precisely the same way.

A word or two may be said about some of them.

Horses. As recorded in Chapter IV, horses early appeared as possible causes of para-hayfever, and ' horse-asthma ' is still, perhaps, the commonest of the toxic idiopathies derived from any animal scurf.

It is the skin scales rather than the hair keratin that functions as the idiotoxin. The horse is an exceedingly scurfy animal, and consequently this scurf is easy to collect; a man, while grooming horses, can easily fill a jam pot with it in an afternoon.

As with all the extracts, it is highly necessary to put the fluid through a bacterial Seitz filter, as has been described above for the grass pollen. This will cut out all the bacteria and moulds, but will not remove preformed endo- or exo-toxins. It is these toxins, from streptococci and staphylococci, I suspect, which sometimes give horse-scurf extract a slight general toxic effect upon both patients' and normal

skins ; but the oedema from this is very slight and is easy to distinguish from a genuine horse response.

Cow- and bull-scurf can be collected fairly readily, also, where these animals are regularly groomed. I find young bulls are much scurfier than cows.

Goat-scurf similarly presents no difficulty.

Dog-scurf is less easy to collect. I find a good plan is to stand a friendly dog in the middle of a double sheet of yesterday's *Times* newspaper and then curry-comb him onto that. It is quite easy to manoeuvre the fallen scales into a heap in the centre of the paper (as was done with the grass pollen above) and thence into a bottle.

As I have said before, the scurf of *any* dog will serve ; but some are scurfier than others. Short-haired dogs are, as a rule, the easier to collect scurf from.

' **Fox-Dogs** '. Somebody told me that well-cared-for fox-hounds are groomed regularly by the kennelman, and advised me to write to a parson M.F.H. of his acquaintance, asking him to get his kennelman to collect some of the *dog*-scurf for me.

I wrote as politely as I could, giving rather full details, and explaining my patients' need for this scurf. I added that I would gladly give some small honorarium to the kennelman to pay for his trouble.

That sporting parson sent back no scurf ; but he wrote a long and kindly letter explaining that he hunted with hounds, not dogs! ' I propped your letter up against the coffee-pot at breakfast-time, and read it aloud to my wife ; we laughed and laughed at your idea that we used *dogs*.'

In reply, I said I had been several times ceremoniously introduced to the One Great Hunting Joke ; but, as fox-hounds were in fact dogs, I would be very glad indeed to have some of their scurf.

I got no answer ; but fortunately both my secretary and my head technician had large and good-tempered dogs.

Cats are difficult to get scurf from, I find—perhaps because they clean their fur so constantly. Also, they rather object to being curry-combed, while other animals seem to like it. If any considerable quantity of scurf is needed, it is generally best to kill with chloroform some unwanted cat, and then stretch its skin on a board for very vigorous scraping. Cats, as I have said elsewhere, chiefly leave their skin scales behind them where they have slept, e.g. on the hearth-rug. Their scurf will certainly be called ' dust ' the next time that hearth-rug is shaken.

Pigs, on the contrary, positively *like* to be scratched, and so are fairly easy to deal with. A clean and friendly pig will allow a boy to sit astride it and scrape the skin thoroughly with the edge of a blunt knife. All these scurfs are processed in the same way as horse scurf.

Feathers are accused of causing asthma almost as a matter of course ; but, as was explained on p. 54, they are often very dubious idiotoxins.

Where they really function in this way, I think the dust from the ledges in the henhouses (consisting as it does chiefly of the skin scales from the birds) gives as potent an extract as one is likely to get. The actual keratin of the featherets doesn't get extracted.

Flower Juices, too, are often dubious idiotoxins, as was explained in the last chapter on p. 265. Where it does seem to work that way, and that is, I think, when the juice will give a positive prick test on the sensitive subject, it is quite easy to express the juice from the fresh plants—e.g. from daffodil stalks and leaves. The slimy juice has water added to it to dilute it tenfold, and is then sterilised in the ordinary way by passing the mother liquor through a Seitz E-K filter.

Mould Spores, like grass pollen, are created to be airborne. They may, therefore, cause a para-hayfever, asthma, eczema, etc., if they land from the air on some mould-sensitive person.

If the habitat of the moulds can't be avoided, the ideal course would be to culture the peccant mould and collect the spores for diagnosis and treatment. On the advice of Professor Storm van Leeuwen, I grew *Aspergillus fumigatus* because he told me that this mould served as a test for all the mould spores. Two or three grams of the slate-grey spores which I collected at that time didn't corroborate this polyvalency of *Aspergillus* for all moulds. My own observations have tended in the direction of a considerable specificity as between mould and mould ; but the whole question needs further work.

Particularly should the spores of the various tomato moulds be worked at ; asthma from this source almost amounts to an occupational disease amongst the tomato-growers. All this work we are now attempting, but it is easier to collect grass pollen by the gram-weight of it than it is to collect mould spores.

Flours. Other substances that can readily be borne on the air are the flours of the various grains. There is clearly no difficulty in collecting material here for extraction ; P.T.D. treatment with it can be used with confidence once one is quite sure that the patient is exclusively sensitive to it.

Sawdust is often airborne in a joiner's shop, and this in spite of mechanical dust removers. I have found teak, mahogany, and English birch and oak, occasionally functioning as idiotoxins.

Here again there is no difficulty in collecting the material for extraction, but one should be as sure as possible that the patient is specifically and exclusively sensitive to the sawdust before persuading him to undergo the labours of a P.T.D. treatment. Usually, however, though the sawdusts may be strongly suspected, they give no specific skin reaction on extraction, and it seems probable that these fragments of wood generally act as traumata, like the dust of the chaff-cutting machine (see p. 283).

Food Idiotoxins are all of them easy enough to come by for extraction. At the time of writing, I count 108 such extracts in my cabinet before me, all made in our pathophane laboratory ; but actually very few of them get used on the average patient. I suppose a dozen such extracts would be enough for most purposes. I find the foods to which skin response is most often given are milk, eggs, fish, oatmeal, and wheat in that order.

Milk is, of course, first swung in the centrifuge to get rid of the butter fat ; white of egg is all ready for dilution.

I find that nearly all people are more sensitive to the white of egg than to the yolk, but occasionally it is the other way about. Some people are sensitive to duck's egg and not to hen's egg, or vice versa ; usually I find them equally sensitive or insensitive to both.

As for the meats, including fish, the myoplasm can be easily squeezed out in a meat-press, and then, like the egg white, merely needs dilution before it is filtered.

Cooked Food. All these foods I have usually left uncooked for the extraction. Patients frequently say that they can eat food if it is cooked very hard, but cannot tolerate it if it is raw or only lightly cooked.

On p. 52 under the heading of ' Flat Eggs ' I told how an egg-sensitive boy could tolerate eggs which had been first fried hard on one side, and then turned and fried hard again on the other. He was scarcely sensitive to egg proteins if they had been thus thoroughly denatured by heat i.e. chemically changed.

I know of no particular reason why people shouldn't be sensitive to food denatured by heating, but not sensitive to the uncooked food. Where food is normally cooked, as with meat, it is from the cooked food, naturally, that they will be getting their symptoms ; but I have heard of cases, for example with fish, where patients can tolerate the uncooked food but can't tolerate it when it is cooked.

I have only got chance evidence to back my opinion and no systematic research ; but the general rule seems to be that the harder the food is cooked the more easily will it be tolerated by the patient. Rightly or wrongly, I still make my food pathophanes and idiotoxic food vaccines from uncooked foods.

Dubious Idiotoxins. On pp. 54 to 57 we spoke of substances generally credited with idiotoxic action, but which exert their asthmogenic influence largely or entirely in other ways. A pathophane made from them could produce little or no response on the skin, and an idiotoxic vaccine therefrom would not help the patient, save possibly by non-specific desensitisation or shock therapy : treatments not to be recommended.

Dust, beside being dubious as an idiotoxin, is necessarily of such chance composition that it doesn't seem legitimate to make a stock dust extract either for pathophane or vaccine purposes.

That, of course, does not prevent a specified dust from containing genuine idiotoxins mixed in with it : grass pollen is in any dust in June, cat scurf is to be found on most dusty hearth-rugs in winter, horse scurf is thick in the dust of a circus—and so forth. But if the patient is sensitive to any of these things, that had far better be detected by their specific skin test ; then, if appropriate, they can be treated by the specific idiotoxic vaccine.

For a case in point see the story on p. 57 of a young woman reputedly sensitive to ' dust ', who was really only sensitive to her father's dogs. A stock dust extract could hardly help her.

Autogenous Dust extract is most often justified when mould spores are strongly suspected from the history or locality of the attacks ; but this is only so because our knowledge of the specificity of mould spores and our capacity for making useful specific vaccines from them lags behind.

If any such ' autogenous ' dust extract is to be made, it is processed like so much pollen or horse scurf.

Patients asked to collect their house dust usually take very little trouble to collect authentic dust. They send, instead, mud from their boots or fluff from their carpets. I urge them to collect with a vacuum cleaner the fine particulate matter which has settled on cornices, picture rails, the tops of books or of cupboards, etc. As stated elsewhere, I usually find the extract from such dust to be non-specific, and usually poisonous to everyone.

I have here dealt with dust at full length because there is still a movement on foot to consider dust as a definite idiotoxin after elaborate chemical analysis of a mixture of dusts collected from a large number of homes. People on the look-out for ' cures ' which need no investigation or thought are tempted to regard such a vast collection as the longed-for ' polyvalent idiotoxin '.

Chaff-cutting. I think the most clearly mechanical action of dust to be found in England is from the chaff-cutting machines ; I have never been able to deduce any idiotoxin action here at all, though I have had many people sent to me to be treated for it, and have made many specific extracts for the testing of them.

I believe, therefore, that the usual action of any dust extract is by trauma ; where there is any idiotoxin action it is derived from some chance protein.

Drugs. There is another class of substances which are not true idiotoxins, but which produce attacks with great regularity. These are the non-protein drugs such as aspirin, formalin, potassium iodide, and so forth—drugs which have already been spoken of in Chapter XIII when speaking of the patch test.

Such drugs haven't concerned us much in the matter of treatment save that they had better be avoided ; they will certainly not make useful idiotoxic vaccines.

Bacteria. We have seen that bacterial infections can play a great part in causing the toxic idiopathies ; but certainly their role as idiotoxins is very anomalous. As we saw in Chapter XII, they may act in several other ways ; if they are idiotoxins at all we can't make a useful diagnosis with a bacterial pathophane, and we apparently can't give a regulation P.T.D. course with an extract of them.

If we are ever to use them as *idiotoxic* vaccines I fancy we must first eliminate the infective factor, the pathogenic quality as infective agents, before we can use their extracts for P.T.D. treatment. But we should be able to demonstrate idiotoxic specificity on that particular patient before using their extracts as so much vegetable juice.

CHAPTER XV

SPECIFICITY OF IDIOTOXIC ANTIGENS

It was recorded on p. 267 that in 1907 Noon found the practical answer to the question of antigenic specificity as between the pollens of one and another of the grass-heads. He decided that, for purposes of diagnosis or of treatment, these pollens were identical in *quality* : if a man was not sensitive to one grass pollen he would not be sensitive to any of them ; if he was sensitive to the one, he would be sensitive to all. That, I believe, still holds true.

As this is a point which has been repeatedly challenged in the past—chiefly by people beginning to work at the subject, and as the general question of idiotoxic specificity is of interest transcending hay-fever and its treatment, it will be well to examine the whole question afresh ; but let us begin with hay-fever, as Noon did forty years ago.

Noon's Original Experiment. He took four different grass pollens—those of *Phleum pratense*, *Poa trivialis*, *Holcus lanatus*, and *Agropyrum caninum*. Extracts in normal saline were made from all these four pollens, and all of them were tested on the eyes of a number of normal (i.e. non-hayfever) persons ; also they were each of them tested on the eyes of six hay-fever subjects who had grass pollen sensitiveness to a varying degree. The tests were made on that area of the body which in hay-fever subjects usually gives the most distressing symptoms—the eyeball. He found, of course, that, if he was to avoid hurting his patients, he had to make very considerable dilutions of each extract, and proceed from the very weak to stronger dilutions. As he had expected, none of the normal people showed any response to any of his four pollen extracts : in comparison with this, *all* the six hay-fever subjects reacted to *all* of his four extracts by developing within a few minutes a red eye (or at least an inflamed inner caruncle), and very considerable itching and irritation in that eye.

By starting with an extreme dilution of an extract, and then dropping onto a patient's eyeball a gradually increasing strength of pollen extract, he determined what was the weakest dilution which would just produce a positive inflammatory response on the conjunctiva ; thus he could estimate the sensitiveness of that particular patient to that particular grass pollen. The conjunctival sac takes some time to cool down after a positive reaction, but by continuing his experiments for many days, he could make this estimate on all of the half-a-dozen patients with all four of his chosen grass pollens.

He found, as was to be expected, that his four extracts differed in strength, i.e. differed in their capacity for producing an inflammatory response and itching when applied to any one hay-fever subject ; also, because his six patients had had different degrees of hay-fever, they reacted to any one of the pollen extracts in

differing degrees of dilution, i.e. one man proved more sensitive than another. But the six patients kept the same order of sensitivity whichever of the four extracts they were tested with ; the four extracts kept the same order of potency whichever of the six patients they were tested on.

Thus, the *Phleum* extract proved to be the strongest with all six patients, while *Poa* proved the weakest. Similarly, Mr. A was the most affected by all four extracts, Miss B was rather less affected, Master C still less by all—and so forth.

Both the extracts and the patients kept station. There was no such anomaly as that Mr. A was most affected by *Phleum*, while Miss B was most affected by *Poa* or *Holcus*. Noon and I took that to mean, in the language of this book, that roughly speaking there was the same idiotoxin in each of the four pollens, though present in different amounts in our extracts made therefrom ; also that there was the same idioceptor in each of the six patients, though again present to a different degree.

Noon deduced that any one of his four grass pollens could be used for the diagnosis or treatment of any one of his six patients ; he also concluded that though any grass pollen would serve for any true hay-fever patient, yet some pollens (e.g. *Phleum*) might be more convenient for use. By and large, this deduction has been found to be correct.

That, I affirm, is the sound practical conclusion ; but in the light of what is to be said at the end of this chapter it might perhaps have been more accurate to say that each of his six test patients had possessed a very similar group of idioceptors, and that this group had affinities for a very similar assortment of proteins to be found in all four of his test grass pollen extracts.

Doubts. Later on, people beginning to investigate hay-fever problems queried this question of general grass pollen ' specificity '. Indeed, some of them used to declare that it was necessary to see what grasses were growing near the patient's home in order to determine against which of them he must be protected. The futility of this procedure should have been apparent from the work of Blackley, who had demonstrated thirty or forty years earlier that grass pollen would fly for many miles on light summer breezes and that on an easterly gale it would even fly from Norway to Yorkshire, a distance of three or four hundred miles.

Others tested many different grass pollens on their hay-fever patients' skins, and selected the pollen which gave the biggest oedema weal as being the chief mischiefmaker and, therefore, the pollen against which the patient had the greatest need of protection. This unwarranted assumption is risky in view of the various factors governing the size of the weals given on pp. 260 to 262 ; it is impracticable because of the great number of English grasses—to say nothing of the exotics.

One such misguided investigator caused much amusement amongst the botanists at Kew by pillorying, as the cause of hay-fever for one of them, a grass which was only to be found in America. He had, of course, imported his reagents from the U.S.A.—hence his unfortunate mistake.

More Tests. These critics were obviously correct in supposing that if the different grass pollen extracts were sufficiently different antigenically from one another, it would be necessary to use different extracts for different cases of hay-

fever. It might be objected, too, that Noon in his experiment had tested an insufficient number of pollens. I therefore repeated Noon's original experiment, using many different grasses ; but I always came to the same conclusion : if a man is proved sensitive to one, he will be found sensitive to all ; and if he is not sensitive to any one, he is not sensitive to any of them.

Still More Tests. It was still maintained by some that they had found great antigenic differences between the pollens of different tribes of grasses. When one thinks of all the factors which influence the size of a weal, and some of the very defective tests that can be employed, this need not surprise us.

I asked, however, the chief English protagonist of the different grass pollen idea to name three certainly different antigenic types : and he gave me *Phleum*, *Dactylis* and *Holcus*, which he said he had found to be markedly different from one another. Two of these grasses (*Phleum* and *Holcus*) were, it will be noted, in Noon's original selection of four grasses for testing. I had plentiful extracts made one year from the pollens of these three named grasses, and in the succeeding year I tested these extracts on some 200 patients. It so happened that my three extracts proved of very equal strength, judged by the level wealing they provoked on the skin of my hay-fever subjects ; as usual, they provoked no wealing at all on the normal people who served as negative controls. With regard to those patients giving a positive response, I found that if a patient was sensitive to any one, he would of necessity be sensitive to the other two.

This similarity of pathophane response between these three pollen extracts justified me, I considered, in using any of the three for a P.T.D. treatment. I found that if I desensitised with a vaccine made from any one of the three, the patients who received this treatment were equally and progressively desensitised to the other two. It made no difference that I could detect whether I used *Phleum*, *Dactylis* or *Holcus* alone as the idiotoxic vaccine, or any two of them blended, or used a mixture of all three ; the wealings produced by prick tests of each of these three pollens disappeared *pari passu* during the P.T.D. treatment, whichever extract or combination of extracts I employed for it. Where I attained complete obliteration of pollen reactions (i.e. in the majority of cases treated) it was complete for all three pollens.

A Fortiori. If there were any noteworthy differences between the various pollens of the Graminae we should be more likely to find it between the most divergent types. To put it the other way round : if no difference can be found, or only a negligible difference, between such exotic ' grasses ' as maize, bamboo and sugar-cane on the one hand, and Timothy grass on the other, then *a fortiori* we are not very likely to find profound differences between the various English meadow grasses.

Maize. On one or two occasions in the past we had got only a poor and possibly insufficient crop of pollen from the various grasses at midsummer ; so, as a last resource, we had collected pollen from the maize plants growing near our pollenarium on Lord Iveagh's farms, for maize flowers in England a month or so later than the grasses do. We were delighted to find that there seemed no *qualitative* difference between the pollens of maize and the pollens of the various meadow grasses when

we tested them on the skins of hay-fever patients. The trouble was the *quantitative* difference : the husks of the maize pollen are far heavier than those of Timothy or of *Dactylis*, but they don't yield an increased extract in proportion to their increased weight. Therefore, the Noon-units as calculated from the weight of maize are much weaker than our customary units, and we felt justified in writing the strength of the maize extract down by 50% before we mixed it in with the main supply for that year.

Our cases taking P.T.D. treatment with this augmented pollen extract did as well as usual—so far as we could observe.

Bamboo, though so different in appearance, is really a member of the great grass family. My amateur collectors in the tropics didn't know how to dry the pollen properly, so for many years it arrived mouldy and useless from all over the world.

But at last I got a fine dry sample—enough for testing skins, though not enough for treatment. One spring I tested many hundreds of true hay-fever cases with bamboo pollen extract, and also, of course, with English meadow grass pollens : the results were identical. No normal, i.e. non-hayfever, person was sensitive to either ; all hay-fever persons were sensitive to both meadow grass and bamboo pollens without exception, and roughly to the same degree.

Sugar-Cane. I heard from a hay-fever patient of mine that he had had an acute attack of typical hay-fever in Natal, in South Africa, when picnicking under the lee of a sugar-cane brake which was visibly pollinating ; sugar-cane is a grass, though a strange one.

Some years later, the botanist hero of the anecdote told on p. 285 was in the Barbados on a botanical mission ; he took the opportunity of collecting for me three or four ounces of beautifully dry pollen from the cultivated sugar-canes. (He collected also an equal quantity from the wild canes found in the island, but I found that pollens from these two sources might be taken as identical.) I soon found that skin responses to the sugar-cane pollen ran very closely indeed with those from English grasses, though not quite so closely as did the bamboo pollen : there were one or two discrepancies in the reactions, but not more than 3% which I couldn't account for.

There was plenty of this sugar-cane pollen for desensitisation purposes ; so, when an undoubted case of hay-fever in a schoolgirl (who had never been out of England in her life) had to be treated by a rush desensitisation in hospital, I treated the child exclusively with an extract of the sugar-cane pollen. She had given equal skin reactions to Timothy and sugar-cane and, as the treatment proceeded, the parallel wealings disappeared *pari passu*, till she left the hospital insensitive to either of the pollen extracts. She experienced that summer a 100% relief of her hay-fever symptoms, in spite of the fact that she had been inoculated only with pollen from this exotic ' grass '.

' Polyvalency '. There is sometimes a demand from doctors that the grass pollen extracts, which they are to use on their patients or which their patients are to use on themselves, must be ' polyvalent '. Polyvalency has, I am afraid, become

to the clinicians little more than a trade term meaning that the extract desired must be a hotch-potch derived from many different grass pollens—the more the better : a little of everything you can think of.

In view of the above-mentioned experiments, my conviction still is that any one grass pollen *is* polyvalent for all grasses for all practical purposes. It is not only impracticable to put into the extract a sample of every grass, but also quite unnecessary.

Heresies Persist. I have discussed *ad nauseam* this question of antigenic similarity to be found among the varying pollens of the Graminae ; but I still hear of the necessity for testing the pollens of maybe a dozen different grasses. I still get reports that patients are ' + + ' to this grass, ' + + + ' to that grass, and ' + ' to a third, and maybe ' − ' to one or two others. Sometimes I get reports from the doctors sending their patients to the hay-fever clinics that they are sensitive to the ' A ' group of grasses and not to the ' B ' group, or vice versa. I protest that all these refinements are unnecessary.

If they really are true hay-fever cases, I find that people bringing these queer reports all react to my standard grass pollen extract of the year. What is more, I find that they are all benefited to a gratifying extent by P.T.D. treatment from one and the same grass pollen extract.

Para-Hayfevers. We may take it that, for the practical purpose of testing and desensitising hay-fever subjects, there is a marked homogeneity amongst grass pollens ; the question then arises—what antigenic relationship exists between any grass pollen and those non-grass pollens which may produce a para-hayfever.

The reason why there is such a thing as hay-fever at all is, we must suppose, the botanical accident that the grasses which bulk so large in the plant population are wind-fertilised. What are the other plants which share with grass this (to us) dangerous habit?

They are chiefly the low-grade composites of the autumn and the wind-pollinated trees of the early spring and summer ; for us the composites are much the more important of the two, though a long way behind the grasses in this respect.

Grasses v. Composites. A composite para-hayfever, as distinct from true hay-fever, is usually detected by the fact that this ' hay-fever ' is reported as occurring from the end of July up to September or so. Sometimes the trouble only begins at mid-July, and then a composite sensitisation only is to be suspected. Sometimes the trouble begins in late May or early June in the orthodox hay-fever way, but goes on without any perceptible break till well into the autumn ; then the double sensitisation to both grass and composite pollens is to be suspected. Skin tests with grass and composite pathophanes will usually confirm these suspicions.

The chief exception to this occurs when a chronic bacterial infection of the upper air passages has dragged out the hay-fever season beyond its ordinary limits. That was fully explained in Chapter XII, p. 214, Reason (d).

These two ways of detecting a composite para-hayfever, the seasonal timing and the diagnostic skin tests, really answer the question of idiotoxic specificity as

between the pollens of the grasses and of the composites : for practical purposes, they are antigenically different.

Usually a P.T.D. desensitisation to one and not to the other will confirm this difference. Often in the past we have disregarded the patient's statement that his ' hay-fever ' does go on into August and September, and so have treated thoroughly for the true hay-fever only, with the result that the symptoms did not begin as hitherto, in May or June, because the hay-fever part had been abolished by treatment ; they only began towards the end of July. When such a patient came back to us with gratitude for the midsummer relief, but complaints about the autumn persistence of trouble, we generally found on skin testing that we had indeed removed the grass pollen sensitiveness, but found a persisting sensitiveness to the composites.

Fig. 50 shows such a case where the patient was treated with grass pollen only, with the result that the hay-fever was removed but the autumn para-hayfever remained—as the patient complained later on.

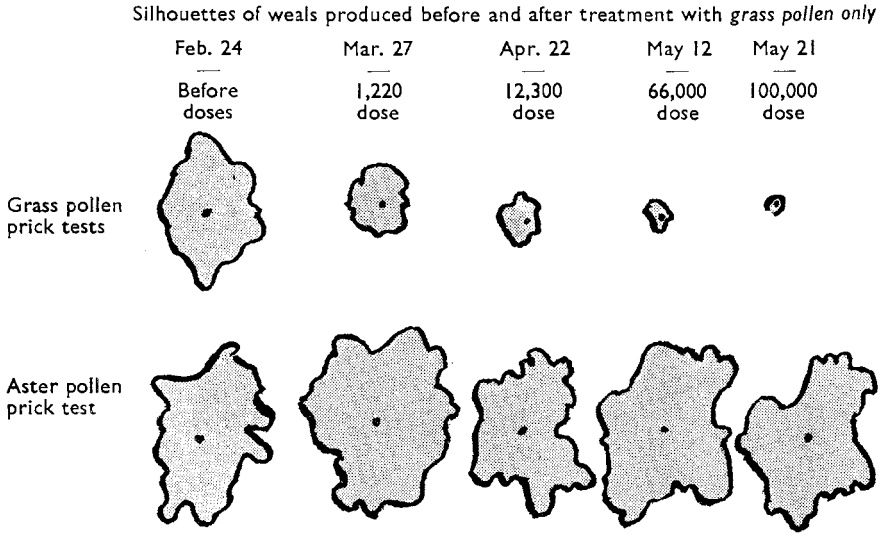


FIG. 50

The negative control tests have not been shown in this and in other similar figures for the sake of simplicity. Controls with normal saline have of course always to be made with all prick tests—as shown in Fig. 40 on p. 258.

It is clear from the above figure that the grass pollen moiety dwindled to nothing under grass pollen treatment ; but the aster pollen moiety remained unaltered (so far as the vagaries of the test permitted).

Phylactic Spread. The above result, or something like it, is the general rule ; but often the specificity for one type of pollen is not so exact. There may even be good evidence of some non-specific effect on the compositae sensitivities from the grass pollen doses ; indeed, sometimes the compositae wealings dwindle as quickly as do the homologous wealings under the influence of grass pollen vaccine.

In Fig. 51 it is clear that the grass pollen sensitiveness, i.e. the hay-fever, disappeared according to plan ; but the sensitiveness of the heterologous compositae pollens also dwindled, though not quite so quickly.

This dwindling of the compositae sensitiveness suggests that part at least of the ' antigenic structure ' of the grass-pollen vaccine may be shared by many of the

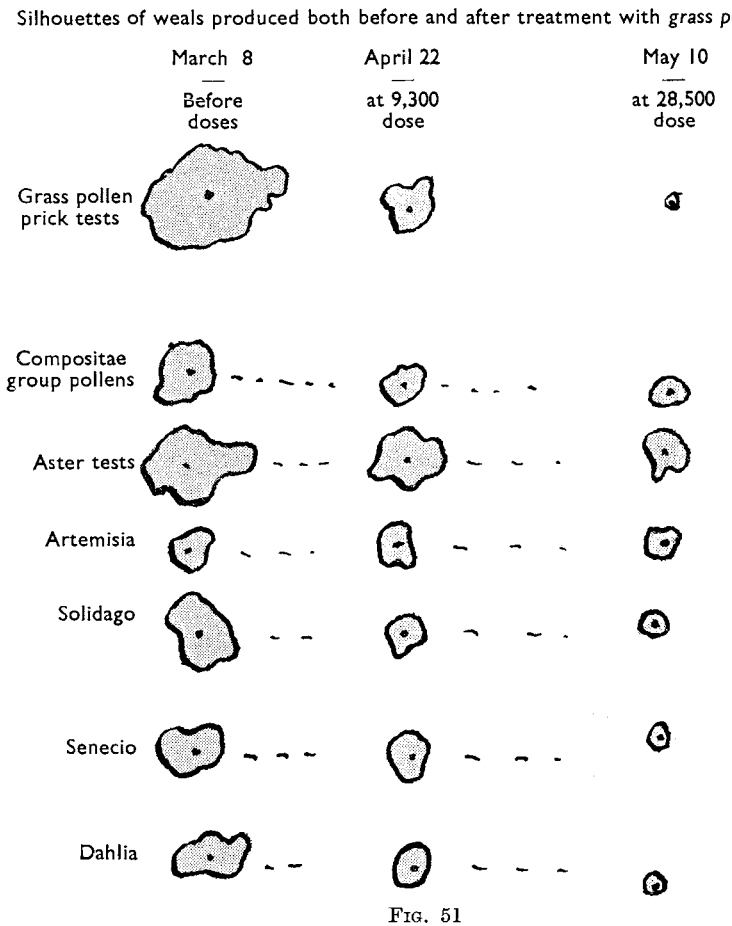


FIG. 51

compositae pollens. It is perhaps more accurate to say that, out of the very mixed proteins necessarily to be found in all pollen extracts, some of our patients find considerable idioceptor affinity with some of these existing in both grass and compositae pollens ; some find less affinity or none : the difference is clearly in the patients.

Fig. 52 shows much the same result, but here the dwindling of the hay-fever skin responses is much less complete ; in fact the heterologous artemisia diminution is about as good as the homologous grass pollen results.

The above figure suggests that with this particular patient those parts of the idiotoxic vaccine which were functioning, i.e. had found idioceptor affinities in

the patient, were largely to be found not only in the grass pollen but also in the pollens of the compositae. Perhaps it would be more accurate to put it the other way round and say that the idioceptors present in the blood of that particular patient had found substances to reciprocate with them both in grass and in compositae pollen extracts.

We commonly speak of a double sensitiveness ; but it seems more reasonable, especially in view of what is to follow when analysing the compositae and tree pollen sensitivities, to envisage a very complicated assortment of idioceptors which often differ widely in our different patients. Usually hay-fever subjects find idiotoxin affinities almost exclusively in grass pollen extracts, but sometimes, as in Figs. 51 and 52, their idioceptors are allied to proteins common to both grass and compositae pollens.

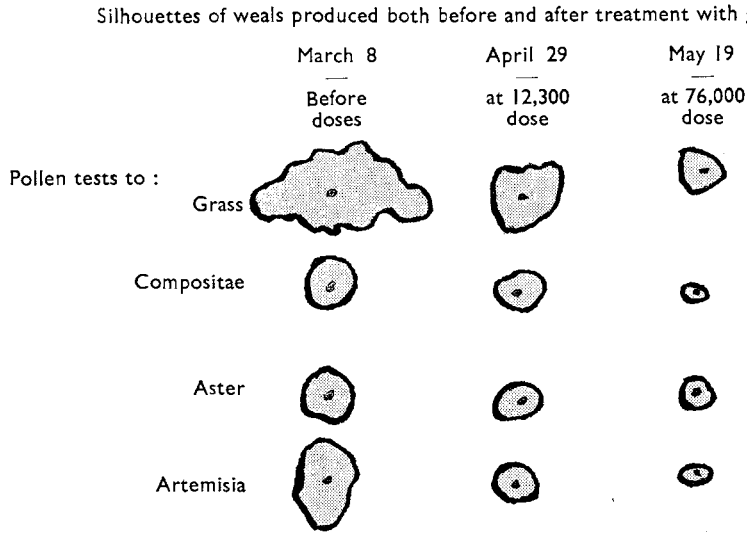


FIG. 52

In view of the generally sharp differentiation between grass and composite pollens, these common protein factors must be only part, and presumably only a small part, of the protein structure of either of the extracts. That may well account for the comparatively poor desensitising effect of our standard grass pollen extract shown in Fig. 52 above : only a small part of the idiotoxic vaccine was functioning. The Noon-units of grass pollen given in the figure are misleading, therefore, and the extract (for that particular person) was really rather weak.

That is why, I take it, we find that if we are to obtain uniformly good results with all our hay-fever patients we have to give a very high nominal dosage : 54 inoculations ascending from 40 units to 100,000 units.

If I'm correct about this, and if we knew more of our individual patients' idioceptor make-up, we might be able to say that the range of dosage for Mr. A should be from 20 units up to, say, 1,500 Noon-units, while for the much less sensitive Miss B the range had better be from 200 up to 100,000 Noon-units. I am

not advising this differential diagnosis as between one hay-fever patient and another ; we do not as yet know enough of our patients' idioceptor make-up : if (as is probable with our present knowledge) we made mistakes as between Mr. A and Miss B, we should either be damned for inefficiency of treatment, or cursed for the unpleasantness of it.

The Compositae. When we come to consider what antigenic differences or similarities there may be between one composite pollen and another, we find this group of plants to be much more divergent than the grasses were. Yet there is occasionally some antigenic unity between them—as witness the general responses of the compositae shown in Figs. 51 and 52 above. We must remember, however, that these cases and others like them had come to us for hay-fever treatment ; it was generally only as a side-show that they were found to be also sensitive to the Compositae Group pathophane, and were then tested with the constituent pollens of that group.

Compositae pollens in general certainly do not ' keep station ' in the way that Noon and I had found all the grass pollens to do. If they did, then all the patients sensitive to them would give sensitivity patterns similar to Fig. 39 on p. 248 whenever tested with those particular eight composite pollens.

Usually they don't conform at all to Fig. 39, and particularly if they are found to give considerable symptoms on their own account—as was the case with the patient in Fig. 50.

Individual Patterns. The composite-sensitive patient who gave the biggest weal responded to all the seven compositae pollens which were tried on him. The pattern is given as Fig. 53.

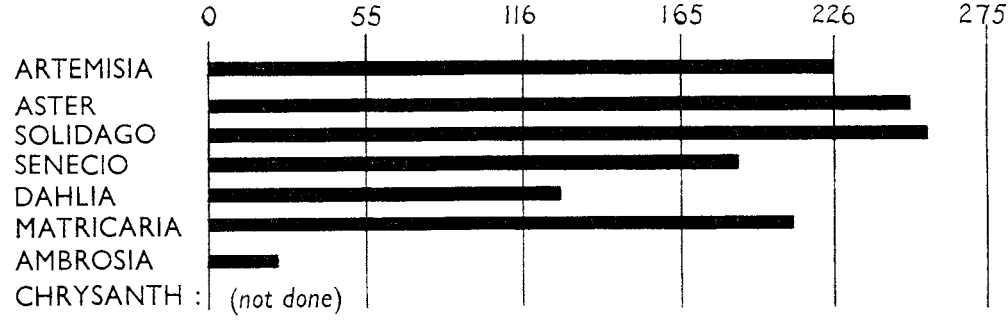


FIG. 53

Here the size of the wealings has to be plotted on a much smaller scale than is used for the other patterns—as indicated by the vertical lines which show the areas of weals in square millimetres. But even the record response to senecio is not so very big as compared with quite ordinary weals produced by grass pollen on hay-fever subjects ; the senecio weal covers an area of two hundred and sixty square millimetres, which is represented by a circle of 18·2 mm. diameter. Such a circle is made by tracing with a lead pencil round the edge of a silver threepenny bit laid flat.

Inaccuracies ? We can perhaps regard the average sensitivity patterns in Figs. 39 and 63 as being substantially correct, because small accidental discrepancies will have been averaged out of them. In judging of the individual patterns we must bear in mind that the prick-test method of estimating relative degrees of sensitivity is not so very exact : we must not read too much into slight differences of oedema from the prick.

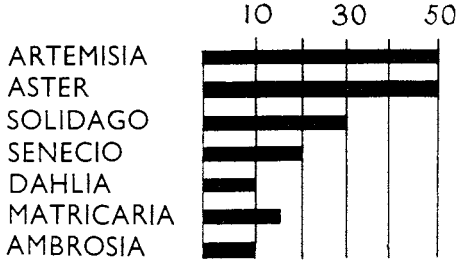


FIG. 54

However, most of the extraneous influences (i.e. other than the idiotoxin-idioceptor couple) which are given at length on pp. 260 to 262 will not affect the ratio between them : they will affect the size of the sensitivity pattern but not its relative proportions.

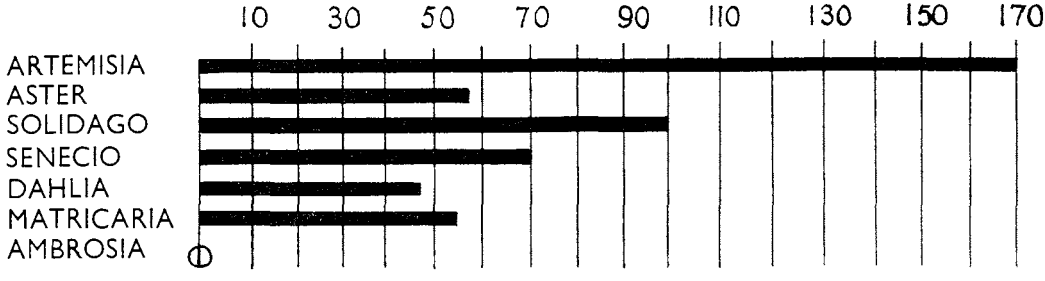


FIG. 55

The next four patterns given here have been picked out as conforming fairly closely with the average ; Fig. 54, though the wealings here are all rather small, is uncommonly close to it. Figs. 55, 56 and 57 are not so close, but still are more or less of the average pattern.

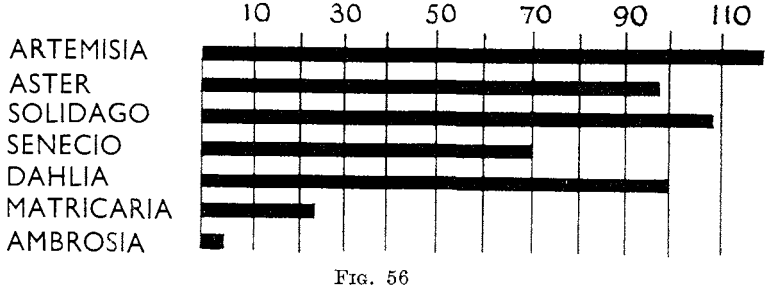


FIG. 56

We can, by stressing heavily the possible inaccuracies of the prick tests, account for some of these departures from our average, though the deficiency of aster in

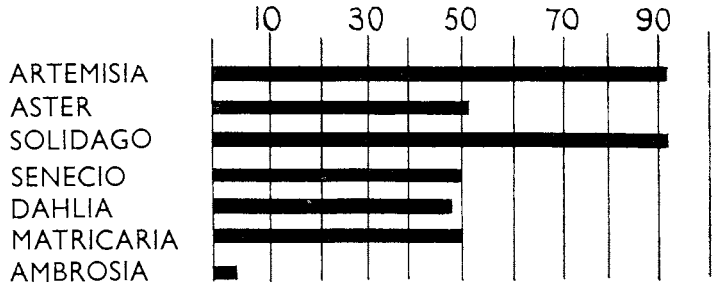


FIG. 57

Figs. 55 and 57, the excess of dahlia in Fig. 56 and of solidago in Fig. 55 can hardly be accounted for in this way—to mention no other discrepancies.

Bigger Aberrations. When we do not select those sensitivity patterns which conform more or less to the average, we soon find that there is a great diversity of

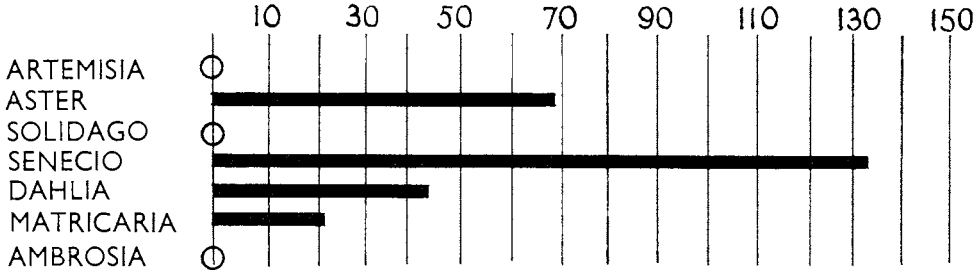


FIG. 58

pattern—a diversity which is inconsistent with the belief that we have one single arrangement of idioceptors in our patients which have mutual affinities to be found

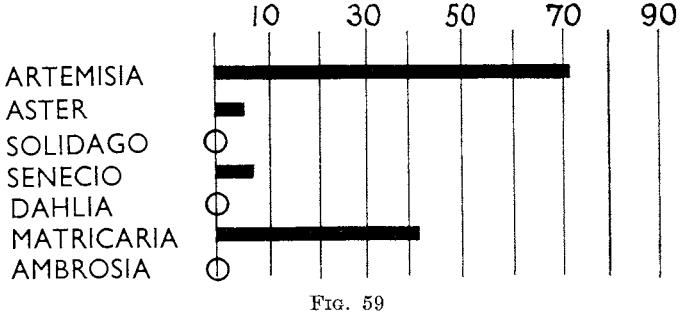


FIG. 59

chiefly or only in our compositae pollen extracts. We may, for example, find responses to some of these pollens to be completely lacking or very nearly so. Figs. 58 and 59 below clearly suggest a very different arrangement of idioceptors.

Very often we find perhaps only one of the pollen extracts to produce a wealing. These singletons show clearly that there is sometimes a marked specificity as between the various compositae pollen extracts.

We have found singleton response for each one of the pollens under experiment —also, of course, for many more compositae pollens than have been figured in the examples of individual pattern given here for this particular 160-patient experiment.

Some pollens, however, give singleton response more frequently than others ; in our experiments artemisia, the most potent of all as shown by the average pattern, is pre-eminent as an individualist. Fig. 60 gives an artemisia singleton as shown by one of our patients.

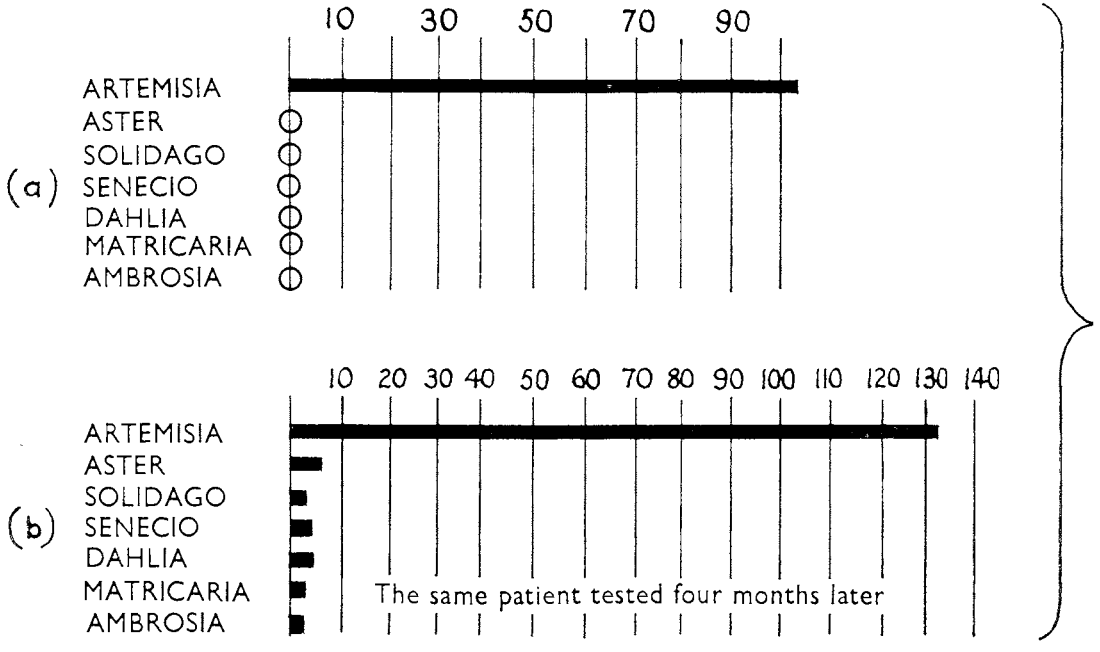


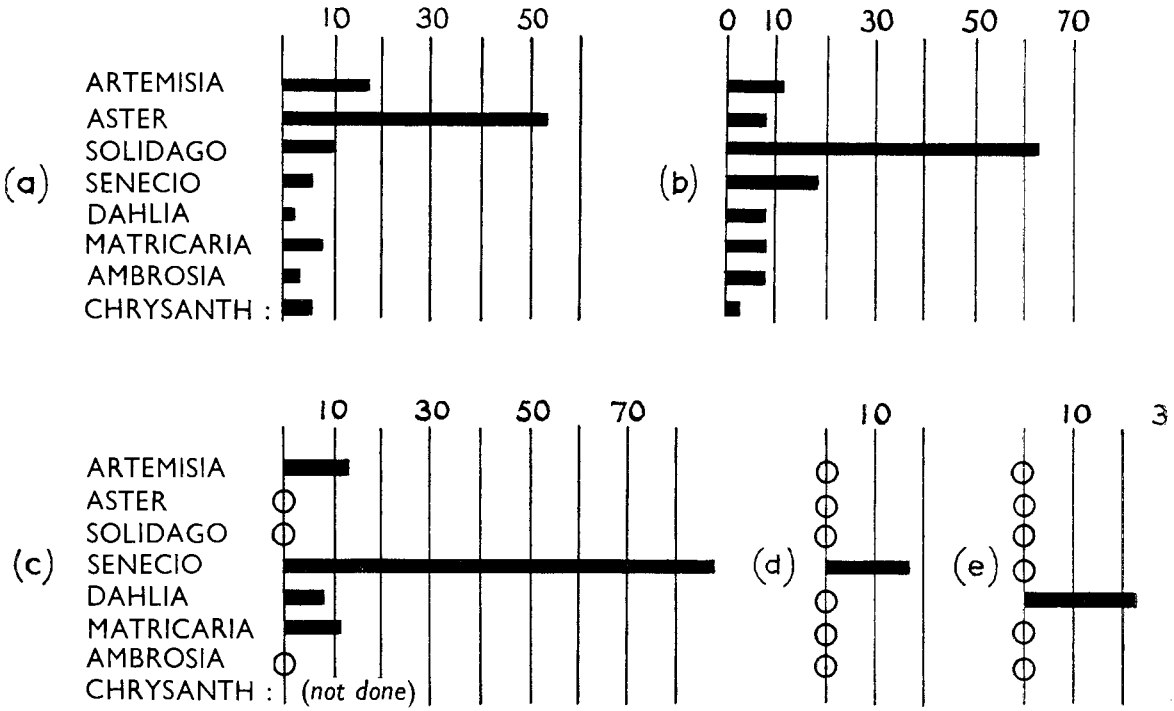
FIG. 60

This patient was tested twice at an interval of four months. The second test confirms the first as to the singleton nature of the artemisia response ; it also shows that at the second visit the patient was in a more responding condition—probably through temporary emotional or bacterial influence. Something has put him into a quasi-dermographic state so that he not only responds more strongly to the prick test with artemisia, but he now gives minute responses to aster, solidago, etc., pollens to which he had given no response whatever four months earlier. These little responses are hardly significant so far as idiotoxin diagnosis is concerned, though they may be telling us something about the state of his gut or temper.

It should be mentioned that, in spite of this ' quasi-dermographic ' state, the saline control for Fig. 60 (*b*) was completely negative—as negative as it was with all the other sensitivity patterns given here.

Figs. 61 (*a*, *b* and *c*) show almost singleton responses to aster, solidago and senecio respectively, but none of them is quite clear of minor responses to other pollens.

Most of the *complete* singletons (excluding artemisia) are small, e.g. Fig. 61 (*d* and *e*) showing senecio and dahlia respectively. One must suppose, I think, that the patients giving these two responses were far away from a dermographic state, hence, perhaps, the smallness of their singleton responses ; whereas the patients showing the patterns Fig. 61 (*a*, *b* and *c*) were only just sub-dermographic.



FIGS. 61

The two tests on the patient tested at four months interval (*a* and *b* in Fig. 60) show clearly the two states—the far away from dermographic and the just sub-dermographic.

Ambrosia, like the other six pollens that I have used in the 160-patient experiment (and like many other compositae pollens which have not been so thoroughly examined), can show a singleton response—not very big, but big for ambrosia. For example, mother and daughter came for treatment : the daughter responded only to ambrosia out of the ten pollens tested ; the mother also responded slightly to artemisia and aster (see Fig. 62, *a* and *b*). The point of interest here is that both mother and daughter had recently returned from North America where they had gone as refugees during the war.

Sometimes, and I think significantly often, the patients who give a considerable ambrosia wealing have a history of a recent visit to Canada or the U.S.A. On the other hand there have been a few patients giving a very distinct response to ambrosia who have never been out of England.

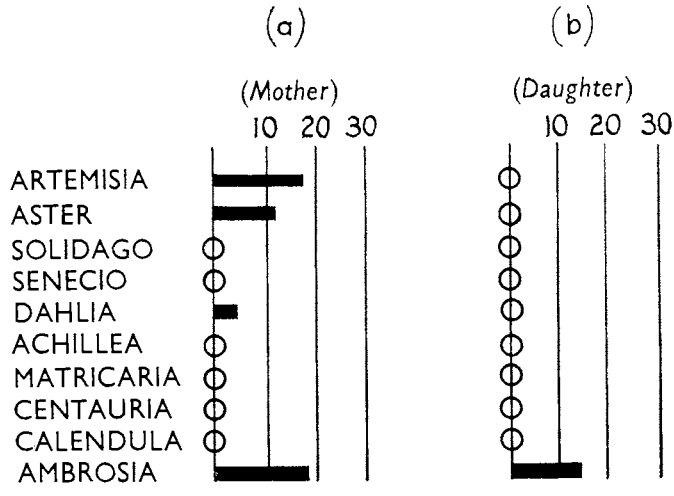


FIG. 62

The Conclusion to be reached from all these, and other, testings of compositae pollen extracts is that we are certainly not dealing with one idioceptor, or group of idioceptors. The compositae form a big and rather heterogeneous group of plant pollens. Some seem to be exclusively wind-pollinated such as artemisia or ambrosia, some partially wind-pollinated like the asters, and some hardly wind-pollinated like the chrysanthemums (ox-eye daisies); some are in concentrated supply at least locally, like the michaelmas daisies in a garden, some in scant and very local supply, and some in practically no supply at all— like ambrosia which exists only in botanical gardens on this side of the Atlantic.

If we are to accept a necessary sensitisation process (and it here seems hard to escape it) then all these differences between the pollens will affect degree and frequency of sensitisation. Whatever the cause may be, it seems clear (and far more so than we found it with the grasses in general) that our patients are not all affected by the same factor or factors in the various extracts; and some of these factors are individual to one particular pollen, some are common to several or many of them, while we shall see that some minute portion of these antigenic factors may be common to a wide range of the protein substances of the world, and perhaps to all of them.

Compositae Idiotoxic Vaccines. The immediately practical use of all this discussion of specificity of idioceptors towards the various compositae pollens is, as was the case with grass pollen forty years ago, to know if one such antigen would serve for all, or whether we should have a number of them. For the last fifteen years or so we have used michaelmas daisy (aster) pollen almost exclusively for the

treatment of the autumn compositae para-hayfevers—and we have used it both as pathophane and as desensitising vaccine.

Occasionally we have experimented with other pollens, notably ambrosia pollen about thirty years ago because of the notorious North American fall ' hay-fever '. I gave it up because practically nobody here was sensitive to it. In the last two years or so we have gone systematically to work, with far more suitable patients to test, and with more botanical knowledge ; we have obtained the results detailed above.

If we are confined to one such pollen, then michaelmas daisy appears to be a good choice. This has been verified by a handsome percentage of our autumn compositae fever cases who have derived complete or considerable relief from a P.T.D. course of it. But we have had a few outstanding failures, and the sensitivity patterns shown in Figs. 59 and 60 seem to explain why michaelmas daisy pollen could not always serve our turn.

The plan we have adopted is to test all cases sent labelled ' hay-fever ', and all cases suspected of autumn pollen fever, with compositae group pathophane, i.e. a mixture of the five pollens giving the most important responses. A positive response of any considerable size to the group test was followed by an analysis into its component pollens ; then, by the sensitivity pattern thus formed, the decision can be made as to which if any of the pollens, or what mixtures of them, should be employed for desensitisation.

Tree Pollens

Many trees are wind-fertilised, especially those flowering in the late winter and early spring when there are few insects about to do this fertilisation work for them. These trees are a very disparate botanical group—if they can be called a group ; and, as one would expect, their pollens show a greater individuality as antigens than even the compositae pollens have shown, and far greater, of course, than the grass pollens.

The people in our clinics who suffer any considerable symptoms from tree pollens have been very few in number as compared with true hay-fever cases ; they are even far scantier than the compositae para-hayfevers—themselves not so very numerous. Therefore, in considering antigenic spread versus specificity amongst tree pollens we have had to work with only small groups of patients.

In one experiment, out of a list of fifteen people sensitive to some of seven tree pollens, thirteen were also sensitive to grass pollen ; and, of these thirteen, six were found to be sensitive to the compositae group pathophane also. Of the two remaining patients, one was sensitive to the compositae group but not to grass pollen, and the other was sensitive neither to grass nor compositae.

These facts by themselves would suggest considerable diversity of idioceptor affinities in those fifteen patients under experiment.

Fig. 63 shows the *average* wealings of seven tree pollens on those fifteen patients. The figure has been constructed, as was Fig. 39 on p. 248, for the compositae. As in that figure, the average responses have been arranged from above downwards in order of potency ; therefore the two patterns are more or less alike.

As before, the vertical lines show the areas of the wealings in square millimetres : these averages only attain small figures just because they *are* only averages

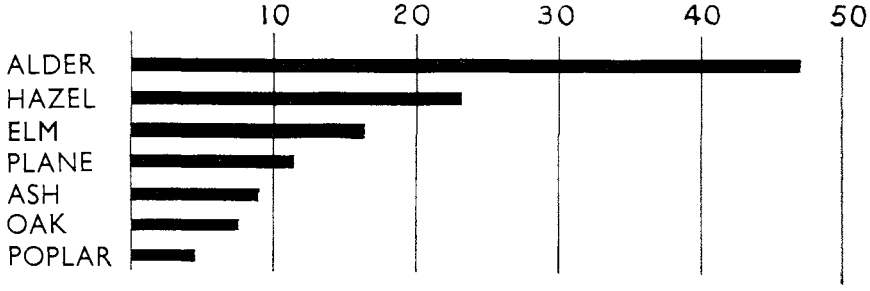


FIG. 63

This figure shows the average wealings taken from a small group of patients who are sensitive to one or more of the anemophylous tree pollens.

of very discrepant wealings. Alder pollen easily heads this small class of pollens as deduced from these fifteen people.

Individual Patterns show that even the biggest of them do not give very big wealings as compared with those given by grass pollen in hay-fever.

Fig. 64 below shows the record weal of this tree series—appropriately made by the champion, alder pollen. By chance this record weal of alder pollen is of the same size as the record weal made by solidago in Fig. 53 ; they are both 260 sq. mm., so even this ' vast ' alder pollen reaction above only amounts to that quite

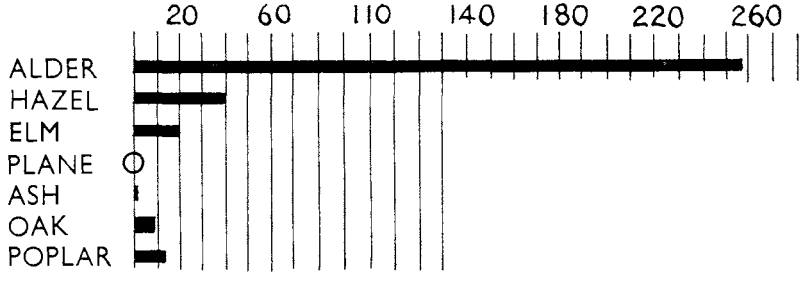


FIG. 64

moderate circle made by drawing with a lead pencil round a silver threepence laid flat.

If we look in the individual patterns for close approximation to the average, we don't find them. Perhaps (*a*) in Fig. 65 is as good as any, unless the rather deficient Fig. 64 or (*b*) in 65 is thought more alike.

Of dissimilarities there are plenty. Alder often occurs as a semi-singleton, as in the two patterns just referred to ; but, as we see in these patterns it is nearly always associated with lesser responses to either hazel or to oak, though usually to both. Indeed it is remarkable that in this short series of fifteen patients tested with seven pollens, alder is three times exclusively associated with hazel and oak,

sensitivity was treated with the single plane idiotoxic vaccine with complete relief of symptoms.

Trees : Conclusions. We get, I think, from the above experiments (as we did from the compositae para-hayfevers) one practical and one more academic conclusion.

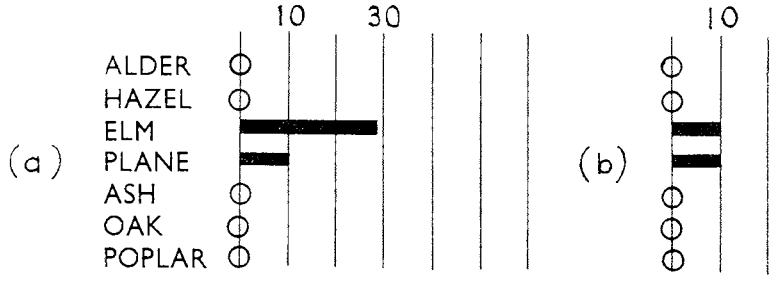


FIG. 68

What Idiotoxic Vaccine ? The investigation was forced on us by the appearance in our hay-fever clinics of patients who, both by timing of their symptoms and by the skin tests they gave, were suffering either entirely or considerably from some of the tree pollens. This is a very small and so a clinically unimportant group compared with the true hay-fevers, whom we reckon by the thousand ; they are

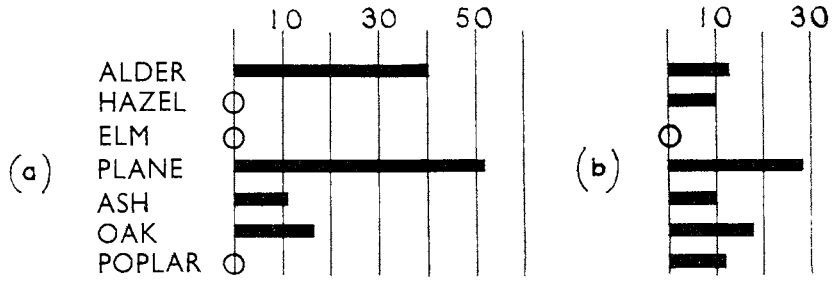


FIG. 69

even much rarer than compositae para-hayfevers, whom we reckon by the hundred ; on this scale the tree pollen para-hayfevers have been reckoned by us only by tens.

It is useful, and especially when there are ' hay-fever ' symptoms too early for the grass pollen to account for them, to test with a tree-group pathophane ; if there is any considerable response to this, we can analyse it with tests of the constituent pollens. If there is anything like a singleton response, that particular pollen can clearly be used as an idiotoxic vaccine. If there is a general response we incline to a mixed tree vaccine.

If the patient is also sensitive to grass pollen, and is, therefore, a hay-fever subject, then frequently an ordinary P.T.D. treatment with grass pollen alone will do all the patient requires ; usually we have preferred to try this single grass pollen vaccine first, as it is almost certainly by far the more important of the two—so far as symptoms are concerned. Also, of course, the residual grass pollen, traces of

which are to be found nearly all the year round (grass pollen was caught on the roof of St. Mary's, London, in January), though insufficient to produce recognisable symptoms by themselves, will certainly step up the symptoms of any para-hayfever which may be operating at the moment—just as we found bacterial infection to do (see p. 214).

Antigenic Specificity, or the reverse, amongst the tree pollens which are wind-borne is also part of our findings from these somewhat tentative experiments. I again get the impression that with any single pollen extract the number and complexity of the proteins extracted must be very great ; so when any two patients are sensitive to, say, alder pollen, they may not necessarily be sensitive to the same parts of that extract, i.e. their sets of idioceptors with affinities in that extract may not be the same. This perhaps comes back to the old medical dictum that there are no two patients quite alike.

Again there are presumably some chemical similarities to be found amongst the proteins to be extracted from all pollens, whether grass, tree or daisy : remember that out of fifteen tree pollen sensitives of early spring, thirteen were demonstrably sensitive to our standard grass pollen pathophane also, and seven to the autumn compositae. Only one tree-pollen para-hayfever case in this particular series was sensitive neither to grass nor composite pollen.

Specificity, then, is clearly a question of degree. If only a few protein elements in a given extract can find the corresponding idioceptor pattern in a patient, then presumably that patient will only be slightly affected by that particular pollen, and only give a slight skin response to an extract of it. Also, if that extract is used as an idiotoxic vaccine, only a small fraction of it will be effective for desensitisation ; and, as we measure our dose-units by the total weight of pollen extracted, that patient is likely to be grossly under-dosed.

It looks as if we may need smaller and fewer doses where our patient has serious trouble from his hay-fever or para-hayfever, or when he gives a big response on the skin to the diagnostic pathophane ; bigger or more doses would be needed for the slight case—if it is thought necessary to treat it. This squares very well with the impression I and my colleagues have got that the really gratifying case to treat is one of severe, yet isolated, sensitisation. Idiotoxin elements and idioceptor elements match better, and therefore the doses are in effect stronger.

If we must have a standard method of treatment it is, therefore, necessary to begin with minute doses (40 units or even less) but continue these up to very big doses, as we do in our P.T.D. course.

That, I think, accounts for the unusual success of our fifty-four or more gradually mounting doses of P.T.D. With some we begin with too small a dose perhaps, and with some we may go on with the doses unnecessarily long ; but on the whole we manage to accommodate the patients with many and with few affinities between their idioceptors and the idiotoxin elements of their chosen vaccine.

Heterologous Idiotoxins. If we test that idea further and use an extract for which the patient can have at best very few idioceptor affinities because there is no notable response on the skin with it—if we use for desensitisation the *wrong* extract

in fact—what happens? As we might expect, nothing happens, at first at any rate. According to the above argument, my explanation would be that there were too few affinities between the patient's idioceptor groups and the mixed proteins of the idiotoxic vaccines to make ordinary doses effective : that vaccine for that patient was in fact too weak in all the ingredients that mattered—though there may possibly have been some few of these present.

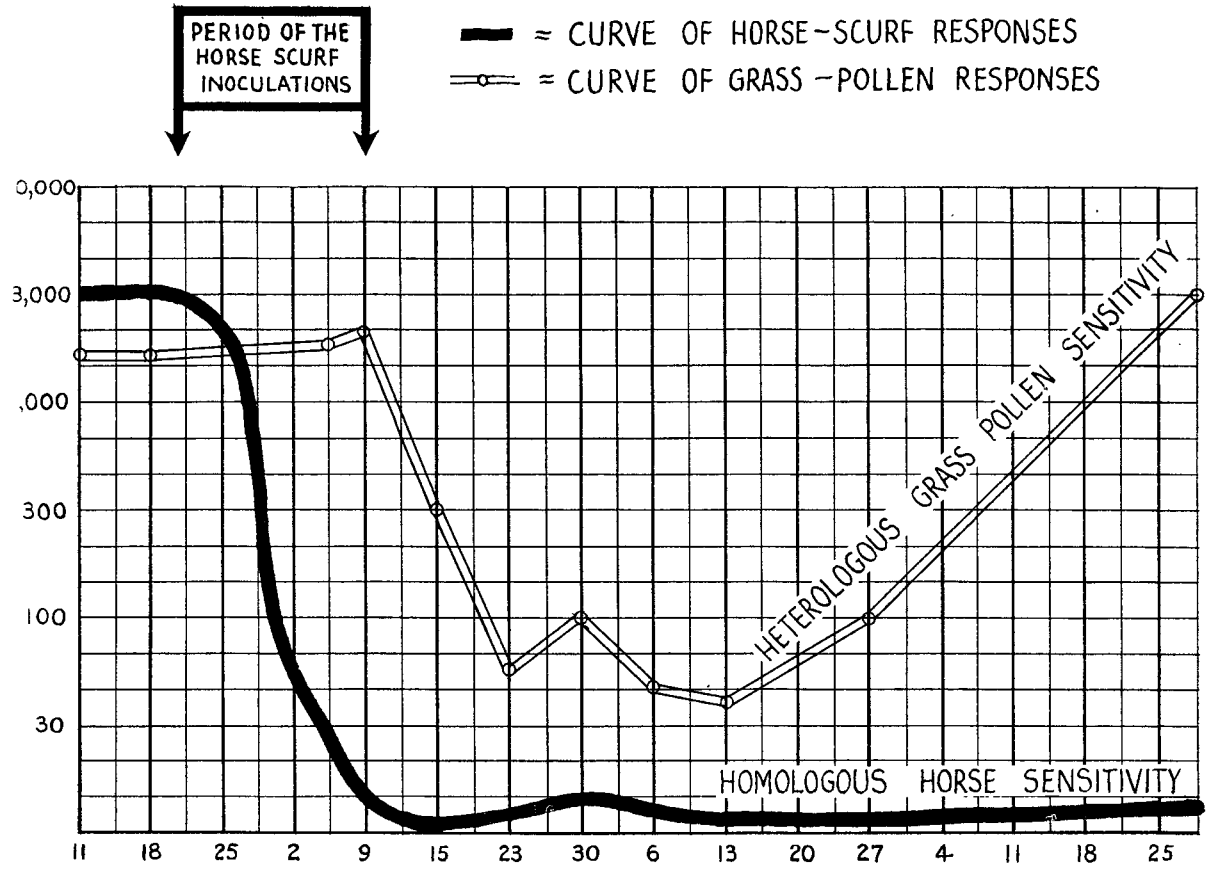


FIG. 70

A diagrammatic representation of the very complicated curve shown as Fig. 42 on p. 262 ; this simplification shows what happens when you press the doses of a heterologous vaccine very hard. The response to it is delayed, is only partial, and is soon lost.

I argue thus because I have been surprised on one or two occasions to get a slight, belated and transient effect, with markedly heterologous vaccines, or perhaps I should say—on the wrong sensitivities. I have got this result when treating only one of two sensitivities very thoroughly, at the same time observing the skin effects on both.

We can't expect heterologous desensitisation always to happen if we believe, as I am inclined to do, that the idioceptor capacities of each patient are very varied, and that no two patients are quite alike in this respect—even among those rated as genuine and ' uncomplicated ' hay-fever cases.

When this experiment is tried on people sensitive to both grass pollen and, let us say, aster pollen, we might expect that many idiotoxic qualities are held in common by the two pollens because they are fairly close biologically ; but even with these we may find little or no heterologous desensitisation with some patients, as Fig. 50 shows. It would have been interesting if it had been practicable for this patient to continue for, say, a month longer his big grass pollen doses ; that would have meant, however, inoculating with grass pollen during the hay-fever season—which couldn't be done.

In 1933 it chanced that I had to inoculate only for horse para-hayfever a patient who was also strongly sensitive to grass pollen.

Lady X was a noted rider to hounds ; she had no use for the summer months when there was no fox-hunting to be had, so hay-fever didn't concern her. She was very insistent, however, that her horse sensitivity should be removed at all costs, and as quickly as might be.

After verifying as far as possible that there were no complications to her strong sensitivities to grass pollen and horse scurf, I put her into a nursing home and gave a thorough rush course to horse, and not to the pollen ; but I made frequent skin tests to both. After I had reached the dose of 1,000 units of horse scurf her horse sensitivity quickly began to fall to zero and remained there for nearly a year.

The heterologous sensitivity, i.e. to the grass pollen, was unaffected by the horse vaccine till after the horse sensitivity had reached zero. After that it began unmistakably to swing lower and lower ; but it never reached zero and it relapsed completely after about six weeks. I suppose that some few of her hay-fever idioceptors found affinities with horse scurf, and when the dose of that got really high this was enough to desensitise her a little and for a short while to grass pollen.

Another case of the same kind was concerned with a boy sensitive to both white of egg and fish myoplasm.

A clinical colleague, whose case the boy was, wished to treat him with big doses of cod-liver oil, but couldn't do so because whenever the boy swallowed any of this he got asthma or came up in a crop of nettle rash ; so I was asked to desensitise to fish.

This was managed very thoroughly by a semi-rush course in hospital, and soon the boy gave no response whatever on the skin to the fish pathophane. As the boy was taking the fish doses regularly and without any symptoms I continued them, in spite of the fact that he was now having cod-liver oil by the mouth. I then noticed to my surprise, and rather to my disgust as orthodox immunologist, that the responses to the egg protein were beginning to dwindle also, though the boy was receiving no egg vaccine.

The egg sensitivity relapsed to its previous state soon after I stopped the fish treatment but the fish sensitivity remained at zero for as long as I had the boy under observation.

Judging by the avidity with which the boy was reported to consume his cod-liver oil, the fish sensitivity remained down for a long time—and perhaps for good.

A comic, though perhaps significant, feature of the affair was that the mother was compelled by her son to come all the way across London to get the Hospital cod-liver oil (which, to me, had an excruciatingly fishy taste) because the boy flatly refused to swallow the more expensive and almost tasteless brands of oil to be bought nearer home.

I can find no figures to illustrate this second case unfortunately, though I remember very distinctly my annoyance at the anomalous temporary reduction of the ' wrong ' sensitivity (i.e. to egg). Perhaps this line of investigation should have been followed up further ; at the time it seemed of little practical

application to see what a 'wrong' vaccine might not do if only it were pressed hard enough.

It is easy to show in routine skin tests in clinics that patients are sensitive to one but not to another of our pathophanes ; every clinic furnishes us with half-a-dozen singletons, say, to dog, cat or horse scurf. That is, of course, just like our finding singletons to aster, artemisia or alder pollens which we were discussing a page or two back.

Where we have in a patient a double sensitivity—say to cat and horse, we can explain it by saying (*a*) that the patient in question has two distinct sets of idioceptors for cat and horse respectively, or (*b*) that this patient has idioceptors which find affinity with those protein factors which are common to both cat and horse, but don't exist in man.

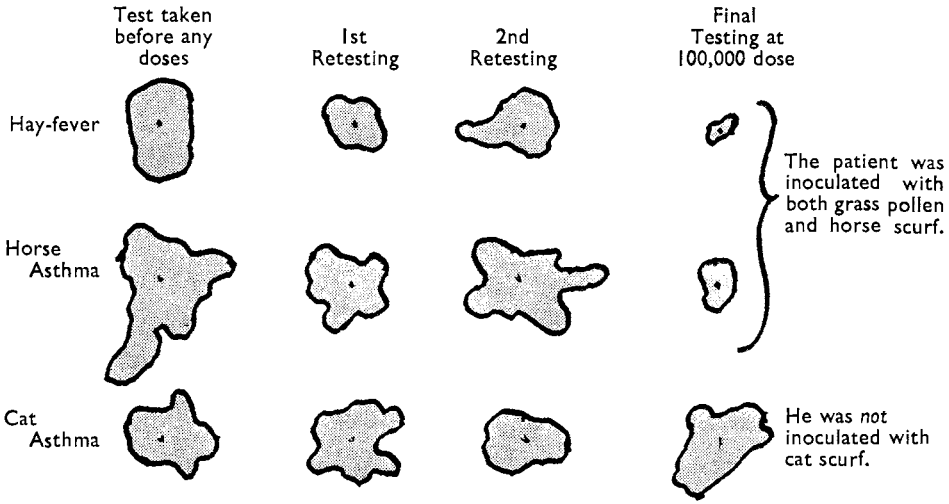


FIG. 71

Result of treatment : no hay-fever or trouble from horses, but cat scurf continued to give symptoms whenever encountered.

The distinction as between (*a*) and (*b*) explanations can only be attempted by desensitising with one animal scurf and then seeing what happens to the heterologous sensitivity. The general rule seems to be that the heterologous sensitivity is unaffected or at least much less affected, and I'd say that Fig. 71, though not a show specimen, gives a very fair rendering of what happens.

My notes are disappointingly empty of good illustrations showing the results of heterologous desensitisation because with two big animal sensitations I have usually inoculated against both of them ; where I have not done so the patients have not come back for the necessary re-testings or I haven't taken them.

In conclusion, I think we find that though patients differ more than we thought them to do in their idioceptor make-up even to one clear-cut disorder like hay-fever, yet there is a very general idiotoxin specificity on a biological basis. Dogs, foxes and wolves and even hyenas are more alike to each other than any of them are to cats or horses—as judged by skin tests or by selective desensitisation.

At the same time, if only one looks hard enough for it, there are similarities, even between such disparate groups as the felidae, the canines and the equidae. But these similarities do not justify us in using the wrong idiotoxic vaccine. The apparent success obtained by shock therapy is unlikely to be due to a desensitisation process.

Well, that is about all. I don't hope that this chapter of odds and ends can throw a flood of light on the causal mechanism of the Allergic Disorders ; but it is possible that some research worker, when picking over the debris, may be beckoned along a fresh line of investigation—to the benefit, eventually, of our patients.

' After me cometh a Builder.'

APPENDIX

INSTRUCTIONS FOR PROPHYLACTIC THOROUGHGOING DESENSITISATION (OR P.T.D.)[1]

(e.g., as with grass pollen vaccine, or "pollaccine," for Hay-Fever subjects)

The Desensitisation Set contains :

I. A card giving on its first page a list of doses and how they may be measured. Within are spaces on which to record the doses together with the date of each injection.

II. A cubic centimetre syringe and needle—sterile in tube. This centimetre space is divided into tenths and hundredths of a c.c.

III. A bottle of surgical spirit marked ' Alcohol ' is for sterilising the *outside* of the needle (while still attached to the syringe).

IV. A suitable series of ten different strengths of the desensitising vaccine.

V. A bottle of adrenalin : antidote against unwelcome reactions.

I. **A Card giving List of Doses.** Take this dose card out and look at it. The left-hand column of figures on the front page gives the successive doses—ranging from 40 units to 100,000 units. This is a longish way to go, and necessitates over 50 inoculations as shown on that *left hand column* ; each dose prepares the patient for the stronger dose, to follow, and they are all so calculated that no step is harder to take than the others, e.g. the high doses, at the end, give no more trouble than the earlier ones.

The third column of figures (i.e. the column on the right side of the front page) tells us the bottle out of which it is most convenient to take any particular dose ; thus the first dose of all (i.e. of 40 units) comes most conveniently out of the 100 unit per c.c. bottle, while the twenty-fifth dose of 1,840 units comes most conveniently out of the 2,000 unit per c.c. bottle. When about to measure out any particular dose on the dose card, look to the extreme right of the same line ; there is indicated the bottle out of which that dose should come. The ten bottles in the set will be found to tally in strength with the ten bottles listed in that right-hand column.

The centre column of the front page shows the amount to be taken into the syringe from this correct bottle to get the particular dose which is to be given. As

[1] *Note :* This technique is designed for *self*-inoculation, i.e. for use by the patient him- or herself. It is found to be quite practicable for intelligent children over 8 years of age ; in no case would the apparatus be issued to a patient unless it is quite safe for this particular person. During the last 15 years, self-inoculation has been used with increasing frequency, and it has been found that perhaps the easiest people to teach are boys and girls in their early ' teens '. It is, however, highly necessary that a medical man should with every case make the correct and exclusive diagnosis ; then he should see that the patient has been taught the following method sufficiently well for complete safety before being trusted with the desensitising set.

Several more diagnostic skin tests should be made during treatment to verify that the response on the skin is dwindling, and at the end disappearing—according to plan.

the syringe is divided into tenths and hundredths of a c.c., this central column also gives the amounts in tenths and hundredths, i.e. to one or two places of decimals.

On the three remaining pages of the dose card there is to the extreme left of each page a space for the date, and, next to it, a space for the dose in units : please note : in recording the doses, do *not* put down either the bottle employed (third column of first page) or the amount of fluid taken into the syringe (central column) but the *Dose*, i.e. the correct number in the left-hand column of first page. The blank space headed ' Skin Reactions ' is for use by the doctor when recording the size of the skin tests made both before and during the P.T.D. course of treatment.

II. **The Syringe** (in a glass tube) is already sterilised on its inside and fit for use. It should be employed only for this desensitisation treatment and should be kept preferably in the patient's bedroom. If not taken to bits, and if used only as instructed, the syringe itself need not be re-sterilised ; but the *outside* of the needle must be re-sterilised each time before use.

III. **The Little Bottle of Alcohol** is provided just for this needle sterilisation. Thrust the hypodermic needle (still fixed firmly on to the syringe of course) through the centre of the rubber cap on the bottle marked ' Alcohol ' and leave it so for 20 seconds or more. The Inoculator (presumably the patient) should not draw any of the alcohol into the barrel of the syringe during this manoeuvre ; if any gets in it should be carefully ejected. On withdrawing the needle from the rubber cap, leave the alcohol bottle lying on the table for the final check-up. The syringe is now ready for measuring out the dose.

IV. **The Bottles** containing dilutions of the desensitising vaccine are ten in number. These range from 100 units per cubic centimetre (the weakest) up to 100,000 units per c.c. (the strongest) ; as said above, their strengths tally with the strength of bottles given in the third or right-hand column on the dose-card, and are designed to make the measuring out of the exact dose as easy as possible. The correct bottle to be used in measuring any particular dose is given at the extreme right of the line on which the dose is printed.

The dose to be inoculated can be found by looking within the dose card to see what was the *last* dose recorded ; the next below on the left-hand column of the dose list will, of course, be the next dose to be taken.

Measuring the First Dose. If no dose has yet been recorded within the dose card if, in fact, the treatment is just commencing, then clearly the top dose of 40 units will be the dose to be given ; at the right of the top line of print will be found listed the 100 per c.c. bottle ; take the corresponding bottle out of the set for immediate use.

The amount to be taken from this selected bottle is given in the centre column of the dose card ; if we are measuring the first dose of the treatment, the centre of the top line (see card) shows the correct amount of fluid to be 0·4 c.c. Take the syringe, the needle of which has just been removed from the alcohol, and pull the plunger back till it rests at the 0·4 mark ; there is now in the syringe 0·4 c.c. of air.

The needle is now thrust through the cap of the 100-unit per c.c. vaccine bottle, and the air in the syringe is squirted into the bottle ; then, with the cap still impaled by the needle, hold the bottle and syringe vertically with the bottle on top and upside down, and the needle pointing upwards and projecting into the fluid. Now withdraw the plunger to the required mark—in this case to 0·4, taking care that no considerable bubble[1] in the syringe is falsifying the measurement of the vaccine. When exactly the correct amount has been taken into the syringe, quickly withdraw the syringe needle from the cap of the bottle and place the vaccine bottle and the syringe containing the 40-unit dose of vaccine down on the table for the check-up to follow.

Now check up, taking in turn the four objects (*a*, *b*, *c* and *d*) on the table before you.

(*a*) *The Alcohol.*—Did I sterilise the outside of the needle? Yes, because the alcohol bottle is out on the table : so put it away in the box.

(*b*) *The Card.*—What dose am I aiming at? From a scrutiny of the dose card the dose to be given is the first, i.e. 40 units.

(*c*) *The Card and the Vaccine Bottle.*—What bottle *should* it come from? The dose card says it should be from the 100-unit per c.c. bottle. Was it? Yes, it was, judging by the label on the bottle out on the table : so put that bottle away in its place in the box.

(*d*) *The Card and the Charged Syringe.*—What amount should I have taken from that bottle to get the required dose of 40 units? The card says in the central column 0·4 c.c. of fluid. *Did* I take that? The markings on the syringe and the absence of a big bubble shows that I did. Good : the syringe is now loaded with the correct dose and is ready for the act of inoculation.

[V. The use of the adrenalin bottle—' antidote against unwelcome reactions '—may be left over till the general question of Reactions, and the causes for them, is discussed.]

The Act of Inoculating. The syringe is before us on the table, loaded with the correct dose of the vaccine ; as this has been carefully checked in all respects as to accuracy, give the measurement no further thought.

The dose is to be injected beneath the skin, but above the muscles—or, as the doctors say, sub-cutaneously. This can be done at any convenient place, but it is more convenient where the skin is fairly loose so that a fold of it can be picked up between the finger and thumb of the left hand ; the possession of only two hands naturally excludes the arm as a possible site for the self-inoculator. Perhaps the upper surface of either thigh (as the self-inoculator sits on chair or bed) is the most obvious place ; the skin over the lower anterior abdominal wall, i.e. in front of the belly, is very convenient ; and the chest a few inches below the collar-bone is, with the help of a looking-glass, an easy place to reach. If the inoculator is not the patient (if, for example, a small child is being inoculated by its parent), then the

[1] There is no danger from the injection of air bubbles under the skin ; the sole objection to them is that they complicate exact measurement.

back of the body is accessible, and it is convenient to place the patient face downwards on a bed ; the upper edge of the buttock or the skin at the outer edge of the shoulder-blade are both convenient sites.

No antiseptic whatever is to be applied to the skin : this is a wise precaution, and *not* an added risk. If anything, let the skin be fairly recently washed, but avoid anything like a ritualistic cleansing.

To get beneath the skin, yet above the muscle, a fold of skin is picked up with the left hand between the finger and thumb. The syringe is then held by the glass barrel between fingers and thumb of the right hand (*not* with a finger over the piston rod) and the needle is thrust quietly, but relentlessly, completely through the skin ; and remember that on an adult the skin may be up to a quarter of an inch thick.

To get the needle through the skin painlessly there must be no hesitation whatever, but there should be no hurry : do not slam it in, and above all, do *not* screw up the courage, take sighting shots, or dawdle. The self-inoculator should, in fact, put that needle in with exactly the same movements, at about the same speed, and so far as practicable, with the same thoughts of the inevitableness of the act, as he would employ when pushing a pin into a potato or into a cushion. (If the inoculator cannot visualise such an act easily, let him turn to ' pincushion ' drill (p. 7). It doesn't much matter how the needle on the syringe is put through the skin so far as safety and treatment are concerned, but if the act is carried out as directed, the self-inoculator will be rewarded by not feeling the pain of the needle piercing his skin.

After the needle is well beneath the skin, but above the muscles, let go of the fold of skin with the *left* hand and shift the finger and thumb to the front end of the barrel of the syringe, i.e. just behind the butt of the needle. When that is firmly held, transfer the finger and thumb of the *right* hand to the piston-rod of the syringe and, before pushing in the dose, try, as it were, to draw something from the punctured area of the body back into the syringe by attempting to pull the piston *out*. Nothing will come in all probability, or at most a bubble or two of air ; but if the point of the needle is inadvertently in a blood-vessel—a chance of perhaps one in a thousand—then this will be disclosed by red blood coming back into the syringe and so mixing with the vaccine. In such an event withdraw the needle from the skin without injecting the dose, squirt out the contents and start afresh. If, as is much the more probable, no blood comes back but only a little air or froth, then quietly inject the dose by pushing home the plunger. On withdrawing the needle see that none of the vaccine leaks out by massaging the track of the needle with the forefinger of the left hand.

The inoculation has been given ; so now record the date and exact dose in numbers of units on the space provided on the dose card ; re-pack the syringe (in its tube) and also the dose card, and put the set away till next time.

Intervals between Inoculating. For the self-inoculator, it is preferable to inoculate every night on going to bed ; but if he has commenced betimes—say in February or early March—it will soon become clear that he will finish the course long before the hay-fever season is due ; this is sometime in the latter half of May

in the south of England. If he is going ahead too fast he should slow down to, say, a dose every other night, or perhaps even twice weekly ; if he finds that he is now dawdling too much he must quicken up again. The aim is to be at the very top of form when the grass pollen cloud begins at the end of May.

Then why start so early as February? Because there may be good reasons (e.g. an attack of 'flu or breaking the syringe) for postponing the dose for perhaps a week or more. The supervising doctor should suggest to the patient just how the doses should be arranged.

Visits to Doctor during the P.T.D. treatment : the skin response to the diagnostic prick test will have been outlined on the dose card in the blank space reserved for this purpose at the top of the second page. The size of the original wealing will gradually diminish as the P.T.D. treatment proceeds. But no two patients are exactly alike, so it is desirable that they should come back to the doctor two or three times to verify that desensitisation is proceeding according to plan. Suitable times are at about the 1,060 unit dose, at the 10,700 unit dose, and when nearly at the end of treatment.

What about Reactions?

Anything at all unpleasant or undesirable occurring after the inoculation gets called a ' reaction '. This may be at and around the place of injection, and so is called a Local Reaction ; or it may be experienced at distant parts of the body, and then would be called a General Reaction. With reasonable care both kinds of reaction should be avoided ; on the other hand excessive concern about them and constant peering at the skin to see if anything is showing certainly induces the frame of mind which will make them more likely to occur. It is quite usual to hear self-inoculators say towards the end of the season : ' The first two or three doses made my leg swell a bit, but after that I forgot to look, and it doesn't seem to happen now.'

Local Reactions. If the dose is given into the thickness of the skin, and not beneath it as it should be, then the skin will be uncomfortable for quite a time ; the remedy is obvious. Put the dose into a place where there is plenty of accommodation for a little swelling, i.e. where the skin pulls up easily.

The best treatment for a local swelling or tenderness is calculated disregard ; or, perhaps, calcium lactate or milk beforehand. Buy a little calcium lactate at the chemists' and put of this as much as a piled up shilling will lift into a quarter of a tumbler of water ; better still (if you can get it) is a tumbler of milk : in either case it should be drunk half an hour before the inoculation. Best of all—don't take anything, and don't fuss about it.

General Reactions. If the self-inoculator has nothing the matter with him beyond his hay-fever, he is unlikely to get reactions ; if a number of other asthmogenic factors are operating, then he might do so—and should be careful.

General reactions are perhaps more alarming than the local effects, because more mysterious. They occur at places distant from the inoculation site and may take the form of nettlerash over the body, headache, asthma, etc. They come up in,

say, 15 to 30 minutes after the dose, and may last an hour or so. They will come up especially quickly and severely if the rare accident occurs of putting the dose into a vein ; so never omit the precautions given above at the top of page 5 about pulling *back* the plunger before inoculating.

If the proper dose has been given and in the proper way, why should anyone get a ' reaction ' at all? Because, unfortunately, the *patient* may change ; he may not be as inoculable on Tuesday as he had been on the Monday. The patient must be reasonably healthy in mind and body if he is to be fit to receive the full dose ; if he is *not* fit, the inoculation should be postponed. If, for example, a person has a bad sore throat with a temperature, if he has a bad attack of ' flu ', if he has eaten some bad fish and has got diarrhoea in consequence, then he is perhaps already three-quarters of the way towards an attack of nettlerash, and any extra strain, as from a vaccine injection, will take him the rest of the way. The remedy is, as said above, to postpone treatment till these inflictions have passed away ; and that is why it is useful to have a little margin of time so that these delays may not upset the treatment. Do not repeat previous doses if the interval has not been longer than, say, 10 days, but proceed straight away with the next dose.

An emotional shock which causes the patient to dither mentally will have this same effect of tending to produce a reaction. If you have had a row with your best friend and called him (or her) all the names you could think of, and rather wish you hadn't, you will be in a trembly, jumpy state, and that is not the moment at which to inoculate ; put it off till next day.

If these postponements are indulged in merely because the puncture is being funked, then go and confess this minor sin to the doctor in charge and he will probably be able to stiffen your morale and correct your technique. The best way to do this is by pincushion drill ; see note below.

Treatment of Reactions. If a general reaction *has* occurred, don't get into a fluster : you can either treat the business with disregard and wait for it to pass, or, if it is beyond a joke, take adrenalin from the flat amber bottle in the set. It is to be given in exactly the same way as a dose, and the amount to be given can be from 0·3 up to half a c.c., and this amount should be repeated every 15 minutes for an hour or so—in fact, for as long as the general reaction lasts. By the way, Adrenalin doesn't undo any of the good of the doses.

Naturally, after a severe reaction there should be some consideration as to why it has occurred, and it would perhaps be necessary to wait for a day or two till the conditions were normal again. *Note* : It is of no particular use to cut down the dose—which is what people sometimes want to do.

Recapitulation

These instructions are lengthy, but the actual operations are few and simple :

(1) Take the syringe with needle attached from its case and sterilise the outside of the needle. Time, 30 seconds.

(2) Draw into the syringe the exact dose to be inoculated. After the first half-dozen occasions, time probably 10 seconds at most.

(3) Pick up a fold of the skin over the selected area, e.g. the upper surface of the thigh, puncture the skin ; make sure that you are *not* in a vein by attempting to withdraw the piston ; then squirt the contents of the syringe well into the centre of the fold : time, 5 seconds upwards. There will be little or no pain if the puncture is made with absolute ruthlessness —yet without any hurry or hesitation.

(4) Place the syringe carefully in the case provided. Time, 10 seconds.

(5) Record the date and dose on the space provided, and put the box away. Time, 30 seconds.

After the emotional interest in self-inoculation has subsided, i.e. after the first few doses, the whole business can be done carefully, without hurry and painlessly, in a minute to a minute-and-a-half.

Pincushion Drill—For the Faint-hearted

If the patient has temporarily lost his or her nerve, and in consequence the punctures through the skin are beginning to hurt more than is readily tolerable, then a little pin-cushion drill is strongly recommended. It is performed as follows :

I. Take a pin-cushion, or similar puncturable object on to your lap, and lay an ordinary long darning needle on to the table before you. The darning-needle is to be imagined to be the syringe holding the correctly measured dose.

II. With the left-hand forefinger and thumb pinch up a portion of the pin-cushion. With the right-hand pick up the darning-needle, holding it in the middle of its length, and keeping it more or less horizontal.

III. Don't take any preliminary runs with the darning-needle at the pin-cushion, and don't do any hesitation wobbles : with one single slowish movement thrust the point of the needle into the cushion in the part you have pinched up by the left hand. Just put it straightaway in.

IV. Then let go of the pin-cushion, and with that left hand take hold of the needle just where it disappears into the cushion. With the right forefinger and thumb pretend to draw back a plunger of the imaginary syringe (to make sure, of course, that the needle is not in a vein—page 5). Satisfied on that point, pretend to press the plunger home and so inject the imaginary dose into the pin-cushion.

V. Withdraw the darning-needle with the right hand, and simultaneously put the forefinger of the left hand on to the puncture hole and give it a little gentle massage to make sure the imaginary dose does not leak back along the track of the needle and out on to the surface.

VI. Place the darning-needle back on to the table in the original position, and the deed is done.

Do this several times till hand-perfect at it. Do not omit any of the above movements (e.g. the imaginary pulling back of the plunger) otherwise they are liable to be omitted when actually inoculating yourself.

If this drill is carried out punctiliously, say a dozen to twenty times, then even if the head doesn't know exactly how an inoculation should be done, at any rate the hands will know.

Postscript. When the desensitisation course is quite finished, wash the inside of the syringe with water or with extract in one of the weaker bottles to get it clear of the sticky strongest vaccine before putting the set away for any prolonged period : or, if you prefer it, remove the piston from within the syringe. Otherwise the piston may become glued-up in the glass barrel and the syringe will be useless when next wanted.

For hay-fever patients in the south of England the need for a fresh desensitisation course should be considered in the following February or March. To start in April, except for Old Hands at the business, may mean rather a hustle to get things done in time.

Great care is taken that this Self-Inoculation Set does not get into the hands of the lay public, save under medical supervision.

INDEX

HAY-FEVER
A KEY
TO THE ALLERGIC DISORDERS

JOHN FREEMAN, D.M.(OXON)

Toxic Idiopathies considered in the volume include :

HAY-FEVER
THE PARA-HAYFEVERS

• • •

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DYSIDROSIS
URTICARIA
ANGIONEUROTIC OEDEMA
GENERALISED SWELLINGS
INSECT BITES
STINGS OF ANIMALS
STINGS OF PLANTS
DERMOGRAPHIA

• • •

PAROXYSMAL HYDRARTHROSIS

• • •

IDIOTOXIC ALBUMENURIA

• • •

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IDIOTOXIC ENTERITIS

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