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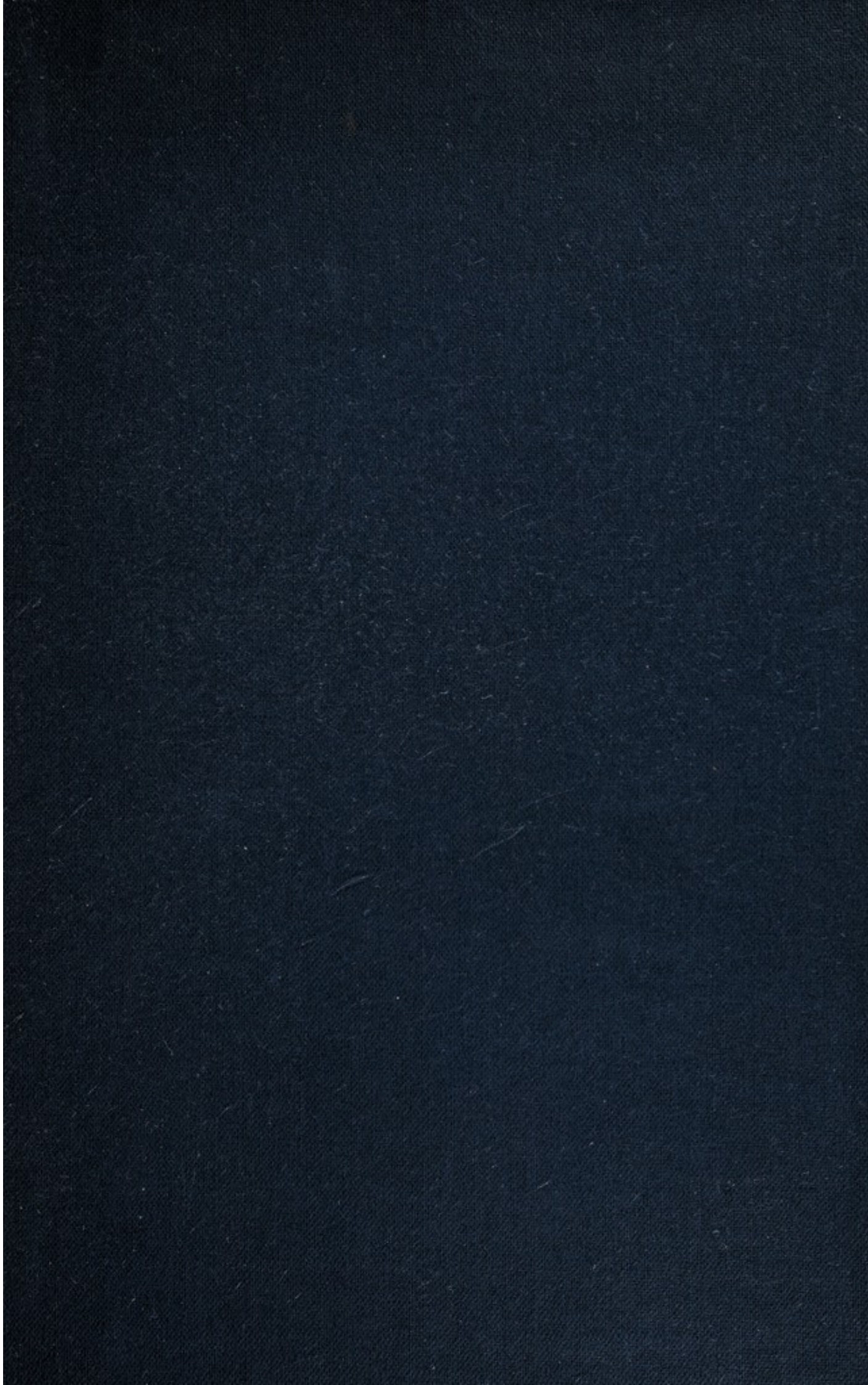
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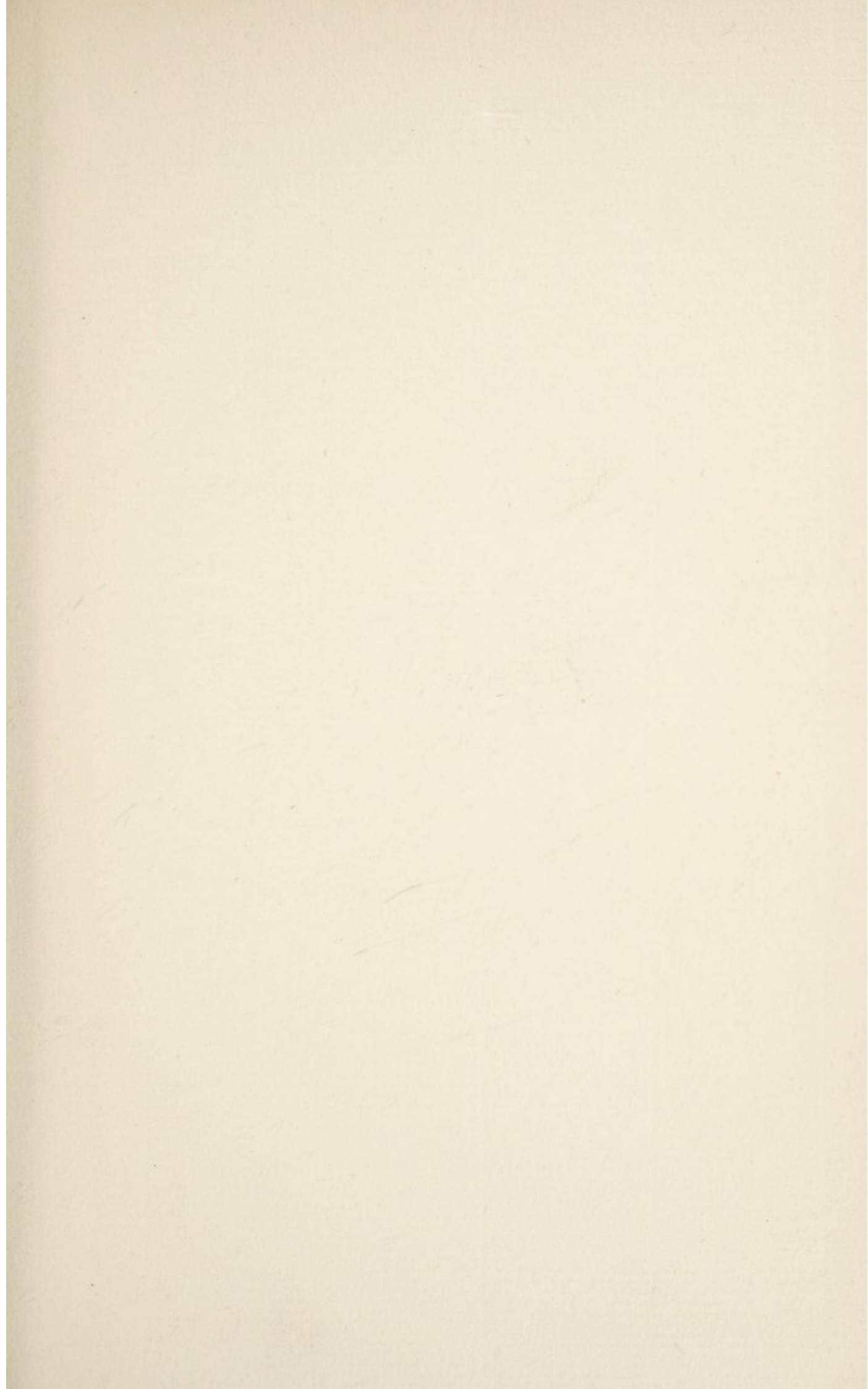


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EPIDEMIOLOGICAL ESSAYS

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BY

F. G. CROOKSHANK

M.D., F.R.C.P.

7/8

If it shall be thought that all these things belong rather to meteorology, it will be admitted, on second thoughts, that the study of the heavenly bodies contributes, not a little, but a very great deal indeed, to Medicine. Thus, with the seasons, men's diseases, like their digestive organs, suffer a change.

HIPPOCRATES : *Airs, Waters and Places.*

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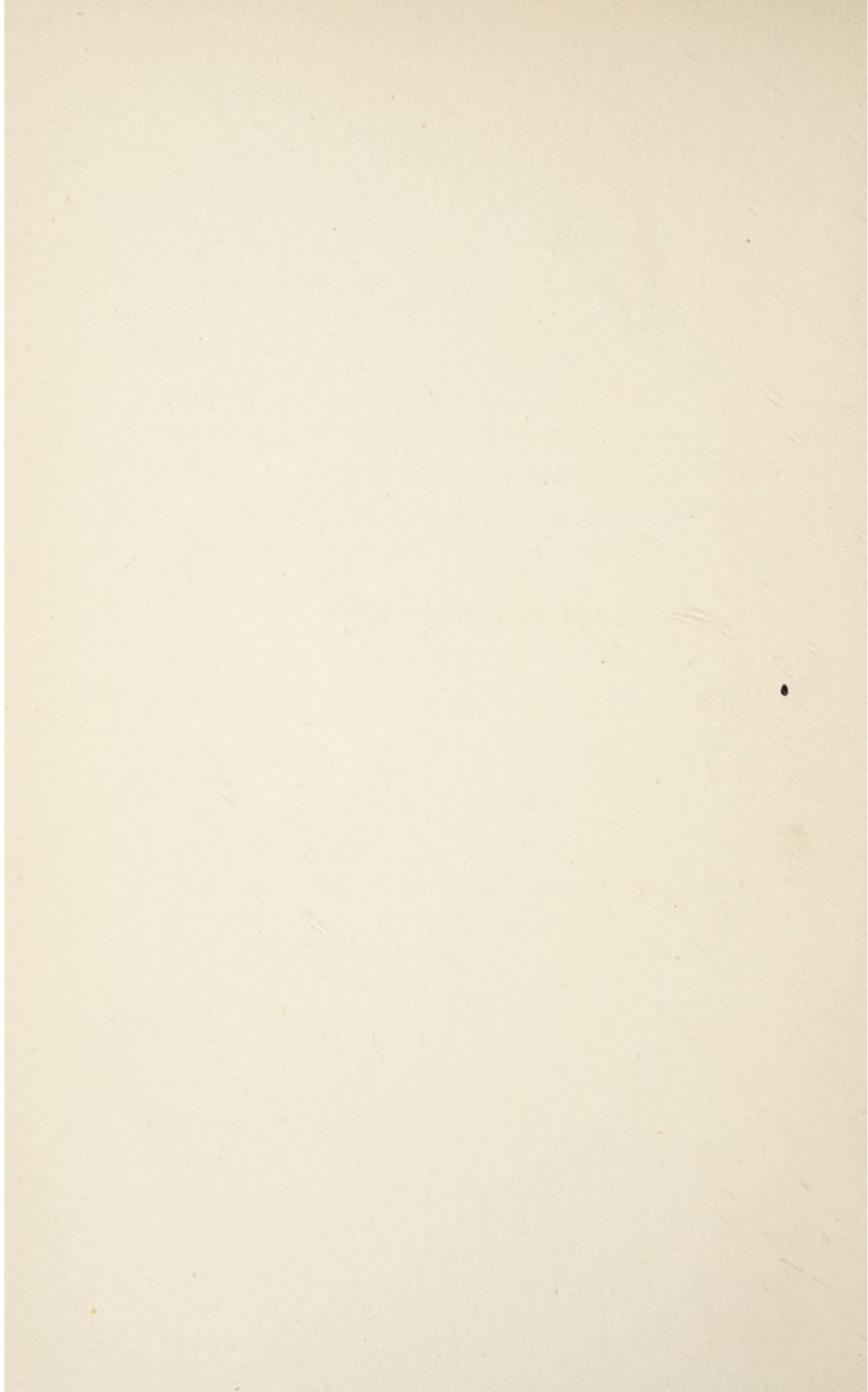
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TO

SIR WILLIAM HAMER, M.D., F.R.C.P.

(sometime Medical Officer of Health and School
Medical Officer for the County
of London)

WITH AFFECTIONATE GRATITUDE



AUTHOR'S NOTE

REVIEWERS in Medicine are always happy to write, of a book before them, that it is merely a collection of papers that have appeared previously. This little volume—a very little one—pretends to be nothing else, but owes its coming into being to the kind demands of many friends, especially from America, who are interested in the epidemiological point of view.

There is a real danger, at the present time, lest the Hippocratic Epidemiology, as developed by Baillou, by Sydenham, by Huxham, and—I will add—by Creighton and by Hamer, be swallowed up in Statistical Science. Statistics may swallow, but can never replace Epidemiology. There is, no doubt, a statistical method of examining and appraising epidemiological data : that is admirable and praiseworthy enough. But Epidemiology—the study of epidemics—has to do primarily, not with figures and numbers, but with Peoples, Times, Seasons, Airs, Waters, and Places. Much the same may be said of the recent application, as it is called, of the Experimental Method to Epidemiology. This is, really, nothing but the practice of observation by analogy, and bears the same relation to true epidemiology as does “Kriegspiel” to Strategy, or attendance at cinemas to the study of Human

Life. Such is the *sermo*, if any, contained in these few Essays.

My best thanks are due to the Editors of the *Lancet*, *Psyche*, the *Military Surgeon*, the *Franco-British Medical Review*, the *Medical Press and Circular*, the *British Medical Journal*, and the *Proceedings of the Royal Society of Medicine*, for courteous permission to reprint. I am more than grateful to Messrs. Kegan Paul, Trench, Trubner and Co., Ltd., whose generosity has made reprinting possible.

F.G.C.

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WHY TIME FLIES¹

Time is that wherein there is opportunity, and opportunity is that wherein there is no great time.

HIPPOCRATES : *Precepts.*

As was justly, though without great originality, remarked by Ephraim Jenkinson, to the Vicar of Wakefield, at Welbridge Fair, the Cosmogony, or Creation of the world, has puzzled philosophers of all ages. We, a little less confident than forty years ago that the Spencerian and Darwinian doctrines summarise all we can know or need think, in the matter of this Cosmogony, have lately turned our attention, a little hesitatingly, towards the examination of some concepts taken for granted by the more ingenuous of the followers of Darwin and Spencer. And perhaps, forty years hence, the special and general theories of Relativity associated with the name of Einstein will be remembered, not so much in respect of any true light they may be thought to have thrown upon the Universe, as by reason of the fact that they once compelled physicists and "men of science" generally, to take an unwonted interest in the psychological and metaphysical problems involved in the examination of what we mean when we speak about Space and Time. To the psychologist, and

¹ *Psyche*, April, 1925, p. 327.

to the metaphysician, Time imports an even more fundamental interest than does Space, and the phrase TIME FLIES is pregnant with implications and with mystery.

Physicists do generally achieve some kind of verbal definition of Space ; and few of us would refuse to admit that, although a little hazy about Infinite Space and its dimensions, we do, nevertheless, carry with us some conception, some conviction, of the reality of a Space in which we live and move and have our being. "Our spatial conceptions depend upon our sensuous perceptive powers" ; to *that* extent our notions concerning Space may be said to have some objective basis.

But no physicist has yet achieved even a verbal definition of Time. One of our best astronomers, in discussing Time, contents himself with telling us that Time is measured by successive phenomena occurring at regular intervals. But, since the *regularity* of successive intervals cannot be ascertained unless we first know how to measure times, the definition—not one of Time, but of the measurement of Time—does not seem helpful, and is certainly less sincere intellectually than is Oscar Wilde's dictum concerning Art : that the mission of Art is to be Art.

Time is, indeed, a conception even more fundamental than is that of Space, and one less susceptible of formal discussion, for it cannot be said to be *entirely* conditioned, as is our conception of Space, by our "sensuous perceptions." At any rate, it is not (as Space may be said to be by those who are not Kantians) entirely conditioned

by our sensuous perceptions of what is external to ourselves.

In our candid moments, perhaps, most of us are inclined to say with St. Augustine : “ If no man ask me the question ‘ What is Time,’ I know ; but, if I pretend to explicate it to anybody, I know not. And yet, of what do we more familiarly and more knowingly seem to speak, than of Time ? ”

Perhaps we may say that for each of us the conception of Time, inexpressible in words though it be, is one which, while partly based upon “ sensuous perceptions,” yet involves an integrating interpretation of these perceptions in unison with data which, whether categorical in the Kantian sense, or physiological in origin, are purely subjective in a manner in which the “ sensuous perceptions ” just spoken of are not. All attempts to explain our conception of Time as *entirely* derived from observational experience of the motions of the heavenly bodies—or, from what is ultimately the same thing, the realisation of *change* around us—break down under examination as decisively as do purely Kantian “ explanations.” They depend, for their fugitive success, on the acceptance of some assurance no more comforting than the astronomer’s statement that we measure Time by the equality of the parts into which we divide it. How do we know that these parts into which we divide it are equal : that these intervals by which we measure it are regular ?

St. Augustine himself exposed very effectively this same fallacy—the fallacy of confounding Time with Motion—and declared, with extraordinary prevision,

that he conceived it as a kind of extension. And, oddly enough, he hinted, not obscurely, what few, if any, in these later days have cared to suggest, that, if we were to think to measure Time by observing motions only, we should be unable to detect the occurrence of what now-a-days would be called a FitzGerald-Lorentz contraction of Time itself. It is indeed quite amusing to speculate on what would be the state of affairs did a FitzGerald-Lorentz contraction of Time actually occur, now and again. Just as for a few moments before Einstein came to the rescue, the results of the famous Michelson-Morley experiment were explained by supposing that solid bodies did, under certain circumstances, contract in a certain dimension, so could we not explain quite many difficulties by supposing that Time does—sometimes—fly much faster than at others? Should such a temporal contraction occur, temporarily, should we be aware of it? Can we, indeed, be sure that such contractions, or expansions, do not occur, and that we are not aware of them?

Let us postulate, what is certainly not inconceivable—since it is almost universally conceived—the existence somewhere in Space of an Absolute Time Recorder. Did the revolutionary motions of the whole Universe, as known to us, and those of each component in the Universe except ourselves, sustain, now and again, a retardation or acceleration, relatively to the Time of this Absolute Time Recorder—to whom, for the moment, we should deny ourselves access—should we become aware of these retardations or accelerations? Probably we should not become aware of them *as such*—nay, certainly we would

not—but we should become aware of some difference in the conditions and circumstances of our lives. And for this reason. Each of us carries within himself what may be called a timepiece—his heart. This beats, normally and relatively to the clock, at a fixed though variable rate; if, that is, a rate may be both fixed and variable. That is to say, between the rising of the sun on one morning, and its rising again on the next, the heart of each one of us will beat some 105,000 times. During the night it will beat, as we say, more slowly: towards the dawn, more slowly still. But, as the day passes, it will beat more quickly, and in the evening more quickly still. During the year there will be variations in the number of heart-beats per day, corresponding to the successively earlier (as we say) or later (as we say) risings of the sun. But, twice in the year, at the moments of equinox, there will be pulse-beat-days of equal length. Now, of these recurrent and rhythmical heart-beats, and of the associated and more variable, though still regularly variable respiratory rhythms, we are and must be subconsciously or unconsciously aware; from time to time, and especially when the rhythm is disturbed, we are consciously aware of them.

To the physiologist, and to the psychologist no less, it should be clear that our conception of Time is derived from, and our appreciation of the passage of Time is based upon, the integration, interpretatively, of these subjectively presented and *not sensuously perceived* (in the ordinarily accepted meaning of the term) data, with appreciation of movements of the heavenly bodies and of

machines like clocks operating in relative concordance with them. The *idea* of regularity seems to arise from appreciation of a certain concordance between these involuntary bodily rhythms and the external rhythms and recurrences that we observe with our "senses"—by our organs of perception. It follows from this that a rupture of this concordance, or a variation in its terms, would lead us to say (as we do in fact often say) that Time is passing more or less slowly, or quickly, than is usual. And the expression is justifiable, if not actually highly commendable. If, as Benedicks says, in a changeless world there could be no Time, and no signification appertaining to the word, are not children and old people, in whom the pulse beat is, relatively to the sun, more frequent than in adults—are not these justified in finding Time to pass more slowly than do adults? And, as the child grows up, and his pulse beats, relatively to the sun, more and more slowly, is he not justified in saying that Time passes more quickly than it did? Again, when we fall sick of a fever, and our heart-beats are, in terms of the clock, more rapid than their wont, does not Time pass on leaden wings? But, when we sit down and read, without regard to the clock, and our attention is quietly fixed on the book, our pulse slowing down withal, are we not, on looking up again, surprised to find how Time has flown? On the other hand, when, under circumstances that provoke strong emotional reaction and acceleration of the heart rate, our attention is entirely fixed on what is passing around us, we are as strikingly convinced, when our attention is released, that the clock should have

advanced more than it has done. One of our most distinguished psychiatrists—Dr. Claye Shaw, till recently still with us—once related to me a very interesting incident. Compelled on one occasion to witness a judicial execution, and having entered the gallows-shed as the clock of St. Sepulchre's commenced to strike Eight, it was with a sense of physical shock that, on passing out, after what seemed a prolonged experience, he observed that the hands stood at but five minutes past the hour! So too, when we leave the theatre and pass into the street after several hours of complete absorption, there is a sense of Time-lapse equivalent to that sustained by any one, to whom holidays are unfamiliar, when he returns home after a week-end that has been charged with emotional experience. Perhaps then, once again, children speak more wisely than the wise men when they say that Time has passed slowly or quickly, and when they refuse to judge of its flight by reference to the fictions and conventions of the clock and the calendar.

Nevertheless, the prime question remains unanswered :—Is Time, as estimated by the revolutions of the heavenly bodies, constant and unvarying in an absolute sense? What if accelerations or retardations, of the which, for lack of an Absolute Time Recorder, we know nothing, do occur? If such do occur, without doubt there would be at least apparent perturbations in the bodily health of at least those communities of individuals not furnished with any compensatory mechanism, or means of adjustment. For them, unless bodily rhythms are linked with astronomical rhythms, a retardation or acceleration of

All astronomical bodies (if harmonious for the astronomical system) would involve a rise or fall, as the case might be, in the pulse and respiration rates of all persons in the unadjusted communities, or for all unadjusted persons in all communities. And doctors would diagnose Influenza, without any doubt! What is curious is this, that, during times of general sickness, or of general commotion and disturbance, it is not uncommon to hear repeated, by different persons, and many times a day:—How quickly, or how slowly, Time is passing! But, how do we know that it is not so passing?

Professor Benedicks, of Stockholm, whose recent book on Space and Time is deserving of close attention, has made some observations concerning Time, strangely disconcerting to the Einsteinian Relativist. Like Heraclitus who, when Thales of Miletus appealed to the fundamental notion of immutability, countered by declaring that ALL THINGS FLOW, Benedicks, while deriving the notion of Space from that of immutability, insists on deriving the notion of Time from that of change. While in advance of the quite materialistic physicists—if such there be—and the many more who are scholastic Realists, in admitting our notions of Time to be of subjective rather than of objective origin, or at any rate not to correspond to anything directly presented in perceptual experience, he yet, rather oddly, seems to ignore the physiological, apart from the perceptual, basis of our conception of Time. A man, blind, deaf and paralysed from birth would, we imagine, have no such conception as we of Space, if indeed any at all. But, would

he not have at any rate some notion of Time? If, as surely we must, we answer Yes—for our paralysed and senseless man would still be sensible, consciously or unconsciously and as an organism, of his bodily rhythms,—we may still accept what Benedicks asserts, that the conception of Time has signification *only* for systems that change. The change need not be external to the organism, for, while there is life there is change, even if not change in extension and so such change as would give rise to any notion of Space. In a sense then, Time is registered by every living creature: by everything that *lives*.

Benedicks, in his criticism of Einstein's Relativity as a theory called forth by the necessity of either rejecting or supporting the undulatory theory of Light, seems to hesitate on the brink of another problem intimately related to that of Time: the question of the Velocity of Light. He insists that the velocity of light is not independent of the motion of its source, as the generalised theory of Relativity requires it to be. He is all for an emission theory of light, in the fashion suggested by Ritz, the brilliant young Swiss, whose premature death in 1909 was so great a loss to science. Acceptance of an emission theory of light renders acceptance of Einstein's theories unnecessary, and relatively inconvenient, since they were devised to support—what is not at all necessary—the undulatory theory of light. But Benedicks does not raise formally, although he suggests incidentally, the question whether we need consider light to have a measurable velocity in the sense in which we commonly employ the phrase.

Benedicks has, however, a theory of simultaneity that suggests what to some physiologists may be a delightful, to others a repugnant, thought. This is that, as we have already hinted, there may be, not independence, but dependence between physiological rhythm and the astronomical or cosmic rhythms. For he thinks of Space as *solid* in such a way that absolute synchronism may be conceived of as existing, in the case of two distant "stations," in such a fashion as is secured between two wheels of a locomotive connected by means of a rigid bar.

The old physicians would have welcomed such a notion, no less eagerly than would have such scholastics as Dante and such mystics as Theresa. For nothing could be more suitable to their ideas of a pre-ordained harmony of the universe, disturbed only by the rebellion of component elements against that *Primum Mobile* by which is "moved equally, as a wheel is rolled, the sun, and the other stars."

At the same time it is possible, if not actually proven, that, as Dr. Smith Ely Jelliffe has suggested, by means of the sympathetic nervous system the physiological rhythms of our heart and other organs are kept in unison (save during illness) with the cosmic rhythms, or the general and Aristotelian motion of the Universe.

Desfosses, who, in the *Presse Médicale*, emits from time to time in the best French style, his divers *Réflexions sur l'Univers et la Vie*, has elegantly touched upon this topic of Time.

Indirectly, while gibing not unkindly at the palæontologists who treat Time, not as an old man with a scythe,

but as an artisan who moulds races and species, he suggests to us that, if we accept the theories of Relativity as giving an account of the universe, the conception of evolution, in a Darwinian sense, loses all historical value and becomes merely an interpretation, that may or may not hold good, of events in a particular frame of reference. More precisely, he asks us with what warrant do geologists and palæontologists assume the changes with which they are concerned to have proceeded at the same rate during the whole of the Time they postulate to have elapsed since—since When ?

In other words : why do they assume that Time has always flown as quickly, or as slowly, as Now ?

Desfosses, with some justice, distinguishes three sorts, or orders of conceptual Time : Psychological Time—perceived by “notre conscience” : Physical Time—judged of by the motions of things ; and Mathematical Time, which results from the importation of the notion of Physical Time into the formulas of the Calculus. Much that he says concerning “Psychological Time” and its blending with “Physical Time,” is worth consideration, no less than is his restatement of what every child has thought when first introduced to the notions of the velocity of light and the distance of the stars. If these notions are TRUE, then, when an astronomer observes the heavens, the apparent positions of the various stars are representations of the relative positions of the respective stars at the various moments when the light rays, now falling together on the astronomer’s retina, departed from the then differently placed and respective stars. “We

receive, on the same photographic plate and at the same moment of Time, a ray of light that left Vega forty-seven years ago, and one that left Lyra twenty-five years ago ! ” Where is, or was, either Vega or Lyra, then, or now ?

Mais où sont les neiges d'antan ?

.....
Mais où est le preux Charlemagne ?

Nevertheless, it is impossible for us not to think of the relative positions of these stars, at any given moment, otherwise than as they would be did light pass instantaneously from them to us ! What if indeed it does so pass ? How was the “ previous question,” that light has, in the ordinary acceptation of the term, a velocity, determined *before* the velocity, now assumed for the purposes of experiment, was measured ? To what extent are the mathematical results, given by the physicists as expressive of the velocity of light, capable of interpretation (on the assumption that light has no velocity, but is instantaneously pervasive) as measurements of the physiological processes involved between the retina and the cortex, and of the intervals necessary between successive stimuli in order that each successive stimulus be separately appreciated by the cerebral cortex ? Did Römer, when he “ proved ” that light has a velocity, pay heed to the subjective and psychological aspects ? Indeed, until these questions are plainly answered by physicists for the benefit of physiologists and psychologists, it would seem, to the latter, that the mathematical status of the velocity of light is not in much better case

than the astronomer's suggestion that Time can be measured by the regularity of the "intervals" of which it is made up. TIME FLIES. Yes: but at what rate?

As Desfosses observes, whether we study the question of Time as physicists, as mathematicians, or as metaphysicians, *on arrive à des abîmes insondables.*

TIME FLIES. "How then can it be measured? We measure neither times future nor times past, nor present times, nor passing times: and yet still do we measure times." To his own question St. Augustine gave part reply, saying, "In thee it is, O my mind, that I do measure my TIMES."

AIRS, WATERS AND PLACES¹

Whoever studies and observes these things may be able to foresee most of the effects which will result from the changes of the seasons.

HIPPOCRATES; *Airs, Waters and Places.*

IT will not, I think, be disputed by those who are interested in Hydrology, in Balneology, in Climatology, and in allied branches of medicine, that two tendencies have lately become manifest at places where what is called spa treatment is carried out. The one is to explain (as it is said) the benefits accruing from treatment at any particular spa, in terms of ions, electrons, radio-activity and other fashionable fictions of the physico-chemists: the other, for particular spas, of malice aforethought, to abandon, submerge or lose their particularities, their individualities, and their character, in a competitive struggle for commercial success.

The prophets of Israel, no longer content with enjoining their clients to bathe seven times and be healed, now supply doubting Naamans with professedly rational explanations of the cure that is about to take place, and, no longer content with insisting that the waters of Jordan-les-Bains are really more efficacious than those of Abana and Pharpar, take pains to advertise the fact that their progressive municipality has recently installed

¹ An Address given to the Section of Balneology and Climatology, Royal Society of Medicine, 1926. *Proc. Roy. Soc. Med.*, 1925-26, xix, Sect. Baln., pp. 17-22.

sumptuous apartments where Abana douches, Pharpar wash-outs and Egyptian coloured lights may be administered in accordance with the directions of the priests of the House of Rimmon.

At any rate, both these tendencies—the tendency to afford “scientific” explanations of the inexplicable, and the tendency to set up artificial substitutes for what is only successful when natural—are the outcome of the notion that it is the mission of science to explain to us what happens in the outside world. Unfortunately, adherence to this doctrine leads many doctors to refuse belief in the occurrence of what they cannot at once explain in terms of current science—so that they are compelled, either to remain sceptical, or to invent some form of words that leads them to think that they *do* understand the workings of the natural world and can even imitate the processes of Nature in all their mystery.

Of course science never did and never will explain aught about anything in the way that once, when medical students, we thought it did. All science can do is to provide us with general statements that are convenient summaries of experience, that lead us on to make fresh observations, and so enlarge our experiences, and amplify our practical resources. At most, science “explains” by referring one set of phenomena to the generalisation or law assumed in respect of some other sets of phenomena. But of the attempts to afford ultimate explanations, and of the assertions made concerning “modes of action,” causation, and the like, by those who use these terms so glibly, the less said the better.

Now, in any text-book of medicine written towards the end of the last century, in those wonderful days when we thought Darwinism had taught us how man came to be, and that physics would shortly let us know all about the universe,—any text-book of medicine, I say, written when we thought that the stethoscope and the post-mortem room, with the aid of the microscope, would teach us all we need know about life and death and disease—spa treatment was only mentioned in terms of depreciation with hints about a “pervading atmosphere of quackery,” and gentle gibes, like those of Sir Clifford Allbutt, about people who believed in the efficacy of the “water chemicals.” This sort of scepticism was natural enough to physicians who really did refuse belief in what they could not, as they said, “understand,” and who, at the time of which I speak, deemed disease itself to be a kind of parasite that invaded particular organs, or else pictured the world of disease as populated by armies of specific organisms going about seeking whom they might devour.

These views have so impressed themselves upon the profession that it will be a difficult struggle before what I may call the functional view-point is re-established, and we come to see that the greater number of diseased states, so far as they are of bacteriological origin, are reactions between the body of the host and organisms which normally are harmless (if not necessary), but which turn “bolshevik” and become mischievous when the functional integrity of the host weakens or is perverted. Be this as it may, however, it is a fact that since the time of

which I speak, medical men, not content with observing the benefits accruing from spa treatment simply, naturally and conscientiously carried out, have seemed to derive some kind of satisfaction from alleging these benefits to arise from radio-activity and what not ; as if such alleged explanations made the matter any more clear ! Of course they do not. In the words of a well-known stage gag, they make it more difficult ! We are indeed no nearer the ultimate understanding of the curative processes initiated at Bath or Harrogate than we were a hundred years ago, when crude chemical analyses were first put forward as explanations. Perhaps we are no nearer than we were 2,000 years ago when the Romans invoked tutelary deities ! But the empirical observations are as true as ever ! Even if we say that the sceptics who declare the whole effect of spa treatment to be "psychological" are right, what is gained, or lost ? Are the results any the less valuable and desirable ? Is the *modus operandi* any more clear ? Suppose that at Bladudville (where, as Mr. Bernard Shaw has discovered, chronic inflammation of the nucal sac speedily disappears), we find that the waters contain distinct traces of lunar emanations in solution. Do we really understand better than before why and how people afflicted in their nucal sacs get better at Bladudville ? All we have done, even when we find that lunar emanations in solution elsewhere, go hand-in-hand with apparent cures of nucal sac disease, is to establish a correlation and to shift the credit from the waters of Bladudville to solutions of lunar emanations. But why and how do these cure ? It is the shift from

the familiar to the unfamiliar that, in an American phrase, makes us "feel good," and that we have really found out something. In the meantime—if analogy counts for anything—we have probably lost a good deal more. Consider our experiences with quinine. A hundred years ago we had accumulated a vast store of experience concerning the cinchona bark, and its virtues were extolled, even in heart disease. Then came a time when chemistry foisted quinine upon us, and we laughed at those who thought bark was a cardiac tonic. We now give the once despised and rejected quinidine with amazing success not only in cases of heart disease, but in certain malarial fevers. Did we not then lose something when, in a moment of arrogance, we scrapped all that 200 years of clinical observation had taught us about "the bark"? And so, perhaps, it is in respect of our latest and supposedly most scientific "explanations" on physico-chemical lines concerning what happens at spas. There is the danger of diverting attention from sequences of experience, from clinical happenings of importance that should be observed and pondered, but which are dismissed, because the latest shibboleths offer "no explanation." As Mr. Bertrand Russell has somewhere said, and with profound truth, "there is nothing in the whole universe really less understood than why one billiard-ball rolls on when struck by another." And there is no quackery to-day more dangerous than the verbal quackery which, flourishing as luxuriantly in Harley Street as at any spa, allows us to pretend that we are nearer the ultimate understanding of life, death and

disease than was Hippocrates : renders us content with the barren stone of false explanations instead of the bread of experience ; and induces us to participate in an attempt to standardise, to industrialise, to commercialise Nature's own methods of cure, or rather to provide in the shopman's phrase : " Something which is quite as good " —but which is not !

Now the proposition that I would make is one that seems to me to embody a conception of which we are in danger of losing sight ; and it is this : that the advantages derived from treatment or residence at any spa, in like manner to the physical and psychical consequences of birth and life in particular regions, are not fairly to be attributed to any isolated factor, but are a function of the *milieu*—in the sense of Auguste Comte. That is to say, they are a function of a totality of exterior circumstances necessary to provoke the characteristic reaction on the part of the individual.

As a matter of fact, the essential truth of this notion (which is, of course, in essence, synthetic rather than analytic) is implicit in two old and well-established aphorisms : the one that when at Rome we should do as the Romans ; the other that when abroad we should drink the wine of the country. And I think that if we pay attention to this point of view we appreciate much that seems at first sight improbable, and we save ourselves much fruitless effort in straining after scientific rainbows. For, once relieved of the necessity of being incredulous as to the truth of what we do not understand, we waste no time in groping after explanations that explain

nothing, and we find opened up before us a whole entrancing field of empirical observation that has been for long closed to those who are afraid of being labelled empiricists, if they observe without theory, and theorists, if what they observe is inconsistent with dogma. We have been far too long under the tyranny of the laboratory theorists who declare that only in a laboratory can experience be gained. Experience in the field is every whit as truly "experimental" as is experience in the laboratory, and, for the physician and epidemiologist, more directly relevant. Yet such is the craze for analogical observation in the laboratory that even epidemiology—the science of disease amongst communities—is now being reduced to an affair of mouse traps.

Indeed, if we wish truly to progress, we must get back—and the sooner the better—to Hippocrates, whose empirical observations and whose few, yet grandly simple synthetic generalisations and inductions remain, and must ever remain, the foundation of all true Medicine based upon the observation and study of Nature, of Nature's ways, and of Nature's remedies.

Nowhere in the Hippocratic *corpus* is the quality that we love to ascribe to Hippocrates better manifested than in the immortal work which we know as *Airs, Waters and Places*, to which I would now make some allusion, first premising that a strong current of thought has lately set in, avowedly based upon the Hippocratic doctrines. To-day in France a new school is engaged in building up a new science of morphology which has little to do with the old, or formal morphology of the Victorian

or Darwinian era, but which considers human form as the expression of human function:—of functional reaction to *milieu*, or environment. This school, now led by MacAuliffe, Arone, and Thooris, derives inspiration from the teaching of Sigaud, Vincent, and Giovanni; and I would remind you that it was Sigaud who achieved the best definition of disease yet formulated: namely, that disease is dissociation of the functional unity of the organism. The object of this school is, then, the study of Living Man: of Man reacting to external influences, and revealing his individuality in his method of reaction; not only in anatomical form but in temperament—that is, functionally, psychically, and chemically, as well as physically.

This new science of morphology, then, seeks to observe man not as a static thing, disjunctive to surroundings, which compel him to life or death as he is or is not fit to survive, but as constituting, with his surroundings, one definite continuity that exhibits a perpetual flux of adjustment and readjustment.

And this is what the *Airs, Waters and Places*—that marvellously concise summary of accurate observation expressed in generalised form—teaches us to do, at the same time that it gives us in outline the general theories of epidemiology, of climatology, of hydrology and of functional anthropology, so set out that there is but little to be added. And, moreover, these theories are theories of the right sort: synthetic statements of the kind which Poincaré says the fruit of right generalisation should ever be—synthetic statements which indicate

belief in the essential simplicity and unity of that nature which, as Bordeu, the French Hippocrates, declared, is yet so much more profound than is the most sublime mathematician or physicist.

Unfortunately, almost without exception, every English translator of Hippocrates has thought fit to employ a peculiar jargon that, however useful to the Greek student, fails to convey, to those who are not classical scholars, the force and directness of the original. The French translation by Littré is, however, beautiful in itself, and may perhaps account for the great appreciation shown in France for the Hippocratic teachings.

“In the beginning,” says the ancient writer, “whoever would wish to pursue properly the science of medicine must in the first place consider the characteristic effects produced by the seasons of the years, remembering that not only does each season in any year differ from the others, but that the same seasons differ in successive years. And then the airs and winds; such qualities as are common to all countries and such as pertain to particular localities. And then the properties and qualities of waters; for, as these differ in their physical characters, so do they differ in their action upon the body. So, too, must be considered the situation of towns, with regard to the prevailing winds, and to the rising sun. And the waters used by the inhabitants: whether marshy and soft, or hard, and from rocks, or salt, and unfit for cooking. And the habits of the inhabitants: their avocations, and whether they be eaters and drinkers to excess, and indolent; or industrious, vigorous, frugal, and abstemious. From these things must he proceed to investigate certain others in particular, so that, when he come into a strange city he will understand the diseases there endemic, and the modifications of common maladies that there obtain.”

Later, after some further detailed discussion of airs and waters, the writer passes on to the enunciation of what

is perhaps the most famous, though the most frequently ignored, observation in epidemiology; namely, that in respect of not only epidemic but other maladies, the most important and dangerous seasons of the year are those of the two solstices, especially the æstival; and the two equinoxes, especially the autumnal. Belief in the accuracy of this observation implies no credulous acceptance of astrology, but recognises an empirical fact, as also the associated induction that fluctuations in weather as well as of health tend to occur at those periods when there is variation and change in the relations of the heavenly bodies, amongst themselves and to us. Even modern science has not gone so far as to dispute the relationship between the spring time and germination, or between autumn and the fall of the leaf! Moreover, we are gradually recognising as a matter of fact that, not only is there a seasonal correlation in respect of influenza, poliomyelitis and encephalitis, but that there are seasonal fluctuations and variations in the incidence and exacerbations of duodenal ulcer and pernicious anæmia—explain them as we may—to say nothing of other diseases!

However—and this is what particularly interests us here to-day—the Hippocratic writer not only recognises the correlation between seasons and times, and those disorders of adjustment to the environment that we call being ill, but a definite correlation between climates, the physical peculiarities of places, and types of mankind. And in the passages in which this view is stated may be found the chief tenets of the school of observation to

which I have alluded, as well as what is the rational foundation of that branch of medicine which makes use of airs, waters, and places for orthopædic and therapeutic purposes.

“For,” says Hippocrates, “where the seasonal variations are most abrupt, there also is the country the most diversified, and the wildest. But, where the seasonal changes are the least marked, there is the countryside the most uniform. And so, when we inquire, is it found to be the case, even with the inhabitants. For as some physical natures are like to the well-wooded and watered landscapes where they occur, so are others to the thin and poor soils; and others again to arid, parched and barren fields, and others to lush meadows and pasturages.”

Of course this is not merely fanciful, as we may be inclined at first sight to think. It embodies an anthropological fact well known to simple observers, even if hidden from the learned. Every schoolboy knows Charles Kingsley's description of Martin the fensman, and Scott never lost an opportunity of instituting comparison between the rugged Highlander and the mountains of Caledonia, stern and wild. Now MacAuliffe and his colleagues have drawn attention to the fact that we can trace, cutting right across all other *differentiæ*, the occurrence throughout the whole of the animal kingdom of distinct types, so that we have rounded, or (chemically) hydrophilous types of men, horses, dogs, fishes, and even insects, as well as (chemically) anhydrophilous or linear types of men, horses, dogs, fishes, and insects. Similar distinctions have been made in respect even of the vegetable kingdom, so that the influence of environment is nowhere better displayed than when, in the arid and

dry countries, we find men, beasts, birds, and plants of one type, and *vice versa*.

For those who seek explanations of the usual kind, one may commend the work of Regnault, who, some years ago, definitely correlated the physical peculiarities of French peasants and agricultural labourers, in different regions, with the local peculiarities of the soil to which they are so much attached. Thus, in the quality of mineralisation of the water, and so of the food, both vegetable and animal, in special districts, we are to see the explanation of the similar quality of mineralisation, and so of physique, of the inhabitants. This question is intimately linked with that of the endocrine glands and their influence on physique, for we are becoming more and more recognisant of the fact that activity of the thyroid, for example, is linked up with iodine in the food and drink: that of the parathyroid with calcium, and so on. We are only just beginning to nibble at this question, I say, but I venture to suggest that one of the advances of the future will be a recognition of the part played by minute traces of silicon, of fluorine, of arsenic, of copper and of other minerals in our food and drink, in their relation with the activities of particular glands and so in the production of physical and perhaps racial types, of one kind and another. At any rate, we are more and more driven to recognise that, as Regnault hinted, environment, while yet an *ensemble*, a unitary fact, is nevertheless of extreme complexity, its full appreciation perhaps involving a reconciliation of much that at present appears opposed in biological and anthropological thought. But these

questions are no less complicated than are the epidemiological questions raised by the airs, waters and places, and it would seem that in both respects we are wiser men when, instead of spending time and energy in an endeavour to isolate this or that specific factor or to secure victory for this or that theory—Darwinism or Lamarckism, miasm or contagium, soil or seed—we seek to balance the results of modern and analytic methods by appeal to the older empiricism with its synthetic judgments and simplifying inductions.

It may be said, however, that Hippocrates does not, in the work of which I have spoken, make any express recommendations of a therapeutic order. True, but the therapeutic usage of airs, waters and places, so far as it is rational, is a direct outcome of the Hippocratic study of the influence of the *milieu* upon the health and character of the inhabitant. It certainly involves, I think, a greater recognition of the thought of Lamarck than it is usual to accord in this country: for, after all, when we send someone away to Bath, to Harrogate, or to the Pyrenees, for the benefit of their health, we are sending them away in order that they may be provoked by the new environment to respond, to adapt, to adjust, in a manner that we think desirable. That is to say, we do so if we are not ourselves misled, by our own jargon, to think we are sending them away in order that some “specific” effect may be produced, by some specific form of electrical or lunar emanation. But if we hold to the Hippocratic tradition and the Lamarckian philosophy, we shall wonder whether the luxuriant complexity of modern life and the

mechanisms of this age of gramophones, cocktails, wireless, evening papers and tinned foods, are not co-operating to destroy what we should earnestly wish to conserve—the local individuality of these environments to which we resort. It seems to me that the local characters in respect of the airs, waters, foods, habits, and so forth, should be far more jealously guarded than they are. If we fail to remember, with Montesquieu and with Rousseau, that the peoples of this world are but as ant-swarms to whom the soil, the *milieu*, has given character, temperament, complexion, habits, form and function, and for whom climates and seasons, sounds and silence, colours, darkness and light, elements, aliments, movements and repose, have all contributed to produce the effects we observe as racial, temperamental, and personal characteristics, then we will find ourselves co-operating in the smoothing out of all those local characteristics—physical, dietetic, hydrological, balneological and the like—which have for centuries been recognised as beneficial, both in varieties of health and in varieties of disease. It is idle to attempt to enlist Nature in a partnership of which the *raison d'être* is the sophistication of Nature's methods. And I am not sure that even the spa physician himself is not more successful—in the right sense—when he, too, is a native and an inhabitant, with local colour and local tradition, rather than a fashionable and fugitive visitor during the high season.

At any rate, the more close is the link between the physicians and the locality, the more valuable will be their contributions to epidemiology and to our knowledge

of the play between airs, waters and places, and states of health and disease. We do more and more need observations of such nature as only the cultivated physician, attached to the soil and observant of Nature and Nature's methods can give us. Dr. Llewellyn, with his important and valuable observations on the relation between rheumatism, temperaments, and the soil, has abundantly illustrated my meaning. Surely, if ever, the riddle of rheumatism will be solved by the co-ordination of such observations as his with laboratory work, rather than by laboratory work alone.

The laboratory *by itself* is bound to fail. Yet there is no reason why we should not prosecute investigations in the laboratory side by side with observations in the field of Nature : no reason perhaps but this, that, when we do prosecute observation in the field of Nature we are not quite so confident of attaining ultimate explanations as are our valued colleagues of the laboratory ! But, even if we are driven, like Hippocrates and like Sydenham, to invoke "occult" and "hidden" forces, that will not mean that we are falling back into superstition and into darkness. It will, on the contrary, perhaps mean that we are adopting a more truly philosophic and scientific attitude than are those who make glib use of the verbal "explanations" that pass current to-day, and that obscure, rather than indicate for us, the operations of Nature and our reactions to that Nature around us, of which we form part.

HOT WATERS¹

Cold is inimical to the bones, the teeth, the nerves, the brain, and the spinal marrow, but heat is beneficial.

HIPPOCRATES: *Aphorisms*, v. 18.

THE late Dr. Vivian Poore, a physician whose sagacity is still remembered by those who loved him, was accustomed, some thirty or forty years ago, to advise his pupils never to be within visiting distance of a spa or health resort without becoming acquainted with its peculiarities and its climate. And he liked also to say that any man of middle life and upwards who proposed to himself a holiday would always do well to consider the spending of that holiday at some suitable watering-place, where the natural resources were advantageous to his constitution or disabilities. So when, late in the summer of 1928, circumstances dictated a short holiday in France, remembrance of Dr. Poore's advice led me to spend a few weeks in the neighbourhood of that wonderful geological formation known as the *massif central*—the just pride of every French man, woman and child. This *massif central* is, of course, the outlying and terminal western spur or bastion of the great Alpine system that extends across Europe from the Carpathians on the east, and it represents geologically the crater or vent of the last of the volcanic activities connected therewith. Geographically, it corresponds closely with the limits of the old

¹ *The Franco-British Medical Review*, January, 1929, p. 85.

province of Auvergne, now comprised for the most part within the departments of Cantal and Puy de Dôme. Historically, politically, and even ethnologically, no less than geologically, these two departments constitute one of the most remarkable regions of a wonderful country, but my immediate *rendezvous* was Chaudesaigues, a village or township on a secondary spur of the great *massif central*, and the *chef-lieu* of a *canton* occupying the greater part of a slip of Auvergne that runs southward, like the salient of a fortress, into the ancient territory of Languedoc. Now, while much of the *massif central* itself is not unfamiliar to the British valetudinarian, by reason of the many watering-places scattered about it—Vichy, Royat, Le Mont Dore, La Bourboule and the rest—Chaudesaigues remains little known, even to Frenchmen.

Anyone who leaves Victoria for Chaudesaigues at ten o'clock of the morning, after an easy journey made in four stages—to Newhaven, to Dieppe, to Paris, and from Paris onwards—will find himself detraining for early coffee on the following day at St. Flour, a still almost medieval city perched upon a rock, whence he sets out in a kind of motor diligence for the fifth, last, and not least exciting stage of his transit. During this run of thirty kilometres, which brings him to his destination in twenty-four hours after leaving London, he may have the unlooked-for pleasure and surprise, when peering into the lovely Gorges de la Truyère from the vertiginous “suspended” road on the famous Côte de Lanau, of seeing an Auvergnat fording the shallow river in the

depths below with a team of chestnut oxen that drag a two-wheeled cart in such a fashion as Cæsar may have seen in Arverna nearly two thousand years ago.

Chaudesaigues itself is a sub-Alpine township made up of two churches and about four hundred houses, built alongside a mountain stream or torrent—the Remontalou—and of which not a bad notion may be received if Friday Street or Holmbury St. Mary, duly magnified, is thought of as at two thousand five hundred instead of two hundred and fifty feet above sea-level, and surrounded, not by Leith Hill, Pitch Hill and Holmbury Hill, but by “montagnes” and “collines” running up to three and four thousand feet with, in the distance, not Blackdown and Hindhead and the South Downs, but the Plomb du Cantal with snow always on its northern crest, and the gaily coloured mountains of the Margeride and of Aubrac. Indeed, our Surrey range from Leith Hill to Hurtwood, occupying perhaps ten square miles, may be regarded as a tiny model of the Chaudesaigues salient which extends over an area perhaps nearly the size of the Isle of Wight.

But the object of this sketch is to give some account of Chaudesaigues itself, whose very name provides at once the clue to its pride and particular interest. The riddle should not be very hard to read for anyone who has ever prescribed *aqua calida*, since the place-name *Chaudesaigues* has certainly come to us from the early medieval Latinity of *Calidæ Aquæ*, by way of the Languedoc or Romance *Caldagues*, even if we may not accept Berthier's disputed dictum that certain Roman references

to *Calentes Aquæ* indicate Chaudesaigues, and not Vichy, or some other *station thermale*. For here, bubbling up steamily from some thirty natural springs, is poured out an unvarying and never-failing supply of the hottest natural water in Europe—unless we reckon Iceland as European!—and of water that is not merely hot but mineralised, radio-active, and pleasantly potable, in spite of a slightly and properly sulphurous flavour. Actually, these Hot Waters issue from fissures in the living rock at a temperature of 180° F.: not quite hot enough to boil an egg, it is true, but too hot at first for shaving comfort, and a very good scalding heat at a temperature of more than that of pasteurisation.

Apart from the medicinal and healing virtues of these waters—of which, presently—their domestic utility is great, while formerly their physical properties were found of service in several industries and handicrafts, notably in connection with wools and leathers.

Indeed, these local industries were once so prosperous that the Chaudesaigues of to-day, despite the recent advent of the motor car and motor lorry, may be said to have dwindled somewhat from her ancient estate. One period of great prosperity and fame was ended by the Hundred Years War—the ancient Pont de Tréboul, built “by the English,” is to be seen twenty kilometres away; a second and later era of even greater activity was determined in the seventeenth century by the Wars of Religion, when differences of opinion between Christians formed, as always, the excuse for destruction and the worst barbarities.

It may well be that, in the future, Chaudesaigues may become one of the most famous spas of Europe, as Alibert declared to be its just meed, but for the present—again despite the advent of the motor lorry and the occasional transit of tourists in a motor car and a whirlwind of dust—it remains simple, unsophisticated and curious, *amusant* in the French sense, thirty kilometres from any railway, and enjoying at one and the same time the conveniences of the telephone—a military necessity in France—and of electric light, provided locally by the “white coal” of the waterfalls—as well as the absence of cinemas, gramophones, loud speakers and evening papers, all of them proper objects of contempt to the hard and thrifty peasants of Cantal. Without doubt these latter and negative advantages are largely due to the odd fact that, as has been acutely pointed out by a local historian, Felgères, when in the past all the world travelled on foot or on the back of a mule, Chaudesaigues was almost as accessible as any other place. Now, the very facilities of modern life have caused Chaudesaigues to become *relatively* isolated and difficult of access. It is perhaps too soon to gauge the effect of the post-war appearance of the motor car in these hill-side districts. Perhaps the full effect has been already attained; possibly there may be future developments. But it is nearly certain that for physical reasons the aeroplane can never be more than an occasional visitant to these rocky and fissured plateaux, ravines and *pics*.

Yet, in spite of the fact that this little town remains, not merely isolated geographically but in some respects

removed from Paris by at least a couple of hundred years in time, it is, paradoxically enough, well abreast of the latest flat in the Champs Elysées in the matter of central heating, by reason of the survival of a system that dates back to a thousand years ago, or earlier. For, not only is almost every house in the village in near proximity to a hot spring—a constant supply of radio-active volcanic water is within a score of yards of most—but the majority of at least the older houses are perpetually heated by an ancient system of “canalisation,” or of hot-water pipes constructed out of pine trees bored from end to end and sometimes lined with lead. And so, even in the winter, fireplaces and stoves are, for heating purposes, a superfluity and a nuisance, and the hard stone basement floors of the humblest cottages remain pleasantly warm. Yet in Chaudesaigues the winter is so cold that a vine will only survive on condition that its roots are bathed in the warm moisture from a hot spring. It is claimed, and I imagine with reason, that this system of hot-water and steam heating was inaugurated by the Romans. It may well be so, and we might even inquire, indeed, whether the very notion of central heating was not first conceived in the neighbourhood of these or like springs. But nowhere else in the world does such a domestic curiosity exist as this system of natural *chauffage centrale* at Chaudesaigues.

The question of the early Roman connection with Chaudesaigues has been hotly disputed, and it cannot be said that the identification with *Calentes Aquae* is altogether satisfactory, though there seems some reason to

admit that Apollinaris Sidonius, Bishop of Arverna (Auvergne) in A.D. 474, had Chaudesaigues in mind when he wrote concerning *Calentes Baiæ* : though again not all authorities are in agreement. However, objects that are indisputably Gallo-Roman are found from time to time in one or two sites, and in the very centre of the town still stands an important mansion, built in the early part of the seventeenth century on much older foundations, where may be seen, a little below the present ground level, a set of baths that must surely be Gallo-Roman, if not actually Roman in every detail. These baths consist of two large apartments, the one containing a stone piscina, or bathing-pool, about ten feet square, while the other is a vaulted chamber with stone benches arranged around a hot spring, so that the rising steam fills the vault with vapour at the same time that the running water fills the piscina. The two chambers are in excellent order, and with a minimum of restoration and modern plumbing could be used to-day for a dozen or twenty persons at a time. It is said that, until a hundred years or so ago, the house where these baths exist formed part of a religious establishment, connected with a monastic order having headquarters elsewhere, and that the baths were used in medieval and even later times for medicinal purposes. Of this there is little clear evidence, but no reasonable person can doubt that from the earliest times the waters of Chaudesaigues were esteemed for their healing virtues as well as used for their economic advantages. We, to-day, may be able to allege physical and chemical facts to "explain" what we call "the action"

of these hot waters, but it is not certain that we employ them to greater purpose than did those whose appreciation was based on what we are pleased to call merely empirical considerations!

At any rate, of one thing I am sure, and that is that these Hot Waters, far hotter than those of our own Bath, do afford the basis of one of the most remarkable water-cures known to me. My personal, although brief experience, and my observations of and talks with a number of persons undergoing the treatment have persuaded me that the claims made by Dr. Brémont and Dr. Besson, the two medical men practising at Chaudesaigues, are in no way overstated. It would, of course, be foolish to expect rheumatism to be cured at Chaudesaigues so long as an active source of sepsis were allowed to add fuel to the fire, but I think it may fairly be said that very many cases of fibrositis, or arthritis, and of osteo-arthritis do obtain great relief, in proportion to the metabolic element recognisable in their causation, while sciaticas and lumbagos, as well as the most intractable of myalgias and neuralgias: in fact, every kind of pain that is aggravated by *chill* or *cold*: will disappear after a week or two's treatment, in a fashion that is really striking. Even a hardened sceptic in the virtues of waters will waver when he sees people who crawl about miserably in their first week set out briskly and jauntily to climb hills in their second, departing, after twenty-one days, firm in their resolve to return in future years!

The phenomena actually observed during the cure are, in the first place, those of sudation, and in the

second, those of diuresis. The diuresis is accompanied by the obvious excretion of pigmentary matters and solids generally, the copious urine becoming high-coloured, aromatic, and loaded with lateritious substances thrown down on cooling. The blood-pressure falls and the heart is steadied; but no one complains of being "lowered," the local explanation given being that the appetite is increased and healthy digestion of simple, natural food is set up. For myself, I will avow that, though following the treatment vigorously and sweating freely with some loss of weight, I walked regularly between twelve and fifteen miles a day without more than agreeable fatigue, and with increasing pleasure enhanced by the fact that neither my food nor my drink was restricted beyond such limits as common sense and appetite imposed.

Actually, the treatment is simple enough. Rising at six, or earlier, and donning flannels or the like—if not already a pensionnaire—one betakes oneself to the *Établissement Thermal* and, after undressing and putting on a bath-robe, passes into a chamber where a pretty hot *douche* is given during a prescribed time with a hose-pipe. Proceeding, the patient passes into an underground chamber, built over actual springs, wherein a dozen *étuves* are arranged in two discreetly separated series of six each; one for the gentlemen and the other for the ladies. These *étuves* are really cupboards—if the expression be allowed—into which the patients are popped, *in puris naturalibus*. The door being closed, a square aperture, like a "judas," is opened and the head is extruded, to be coiffed with one damp cloth and bibbed

with a second. The naked body, inside the cupboard, is thus exposed to the natural steam from the springs, which is suitably directed by three or four leaden or zinc cowls. It all sounds very absurd and very uncomfortable, of course, but, as a matter of fact, the claim made that the *douce chaleur qui monte de cette bouillante et tumultueuse source* is really enjoyable is by no means untrue. Soon *une sueur abondante ruisselle sur tout le corps*—I quote from a local pamphlet—and, after not more than a quarter of an hour, the imprisonment comes to an end. The patient is then wrapped in a towel and conducted to his *couchette* where, skilfully packed, he lies and sweats and sleeps and muses for one, two, or even three hours, being finally released and allowed to dress, when his excellent *petit déjeuner* of coffee and *croissants* is brought to his bedside. Then to sally out, refreshed and free from pain, with supple joints and lissom limbs, to drink a pint of the hot water itself and to enjoy the sunshine and the air and the hills, and the quiet, as well as the drowsing of bees and tinkling of cow-bells and the murmurings of the streams and waterfalls until “big lunch” is ready at noon. Sometimes the cure is varied in its detail, and the doctors must be consulted. Simple hot baths may be ordered, or massage. But the water itself must always be drunk, to replace what is lost in sweating, and to flush away the metabolic *débris* dislodged from the holes and corners of the fibromuscular sheets and masses, and from the joints and the nooks and crannies of the spinal column and its wrappings and stuffings. In these various ways are the natural

resources of Chaudesaigues utilised, but it must be insisted that the bathing establishment, although scrupulously clean and perfectly adequate to the occasion, is simplicity itself. It is, indeed, almost naïve, for no attempt is made to supply any form of treatment other than by direct utilisation of what is already given by the *genius loci*. In this respect Chaudesaigues may teach some of our most vaunted spas a much-needed lesson. Once upon a time, and not too far distant, I sought relief at one of our most celebrated watering-places. I was offered Scotch douches for the outside, Plombières douches for the inside, Vichy douches for the altogether, and Aix massage for everything else. I was given a list of green, blue, red, violet and rainbow-coloured lights from which to choose: I was pressed to undergo radiations and vibrations, high, low, quick and slow: I was told of this, that and the other form of instrumental exercise: I was asked to choose whether I should have baths standing up, upside down or, I think, even inside out; in fine, I was shown a list of one hundred and forty odd available "treatments"! One treatment that I had sought, however, I could not obtain, and it was this—free immersion in the natural waters, without any sophistication, adulteration, electrification or modification whatever. Until, that is, a silver key surreptitiously employed, gave me clandestine access to a disused bathing pool or piscina of Roman provenance, wherein I could swim and move freely at will. And this brings me to mention perhaps the only fault that I find with Chaudesaigues; there is, as yet, no bathing pool, or series of

pools, where (as erstwhile at Bath) the sick and infirm may move about freely in the healing waters. It is to be sincerely hoped that Chaudesaigues will always remain free from all sophistication, from all provision of "stunt" treatment of every kind, and that commercialism will never intervene under cover of "science" to destroy the efficacy of Nature's own remedies, at a profit. But the simple, warm bathing pool, such as is so wisely provided at Droitwich, in Worcestershire, is of incalculable benefit to very many persons, and is always appreciated by those who believe in the value of personal effort during the conduct of a cure.

However, Chaudesaigues is not a wealthy municipality, and for a visitor the cost of living, as well as the cost of treatment, is low, and it may be that increased accommodation will only be provided when the State intervenes helpfully or some capitalist founds a *Société anonyme des Thermes*. Meanwhile, the sufferer from any of the maladies so well treated at Chaudesaigues, unless he can walk about freely, had best, I think, engage a room at the *Établissement Thermal*, still controlled by Madame Veuve Ginisty—one of those wonderful women only to be found in France—who with dignity and matriarchal calm sees to it that the traditions of her *pensionnat* are worthily upheld by the daughters and sons-in-law and granddaughters and great-grandchildren who take part in the activities of the season. He will then obtain his morning treatment without the necessity of leaving the precincts; he will rest after the bath in his own room, and will drink his waters in the courtyard below. For

his two principal repasts—the *grand déjeuner* at noon and the *diner* at seven—he will seek the abundant tables of one or other of the two or three hotels, where he will find, as I did at the Hotel Valette, the famous cabbage soup of Auvergne, and the local *tripou*; or trout from the Truyère, with sometimes a *gigot du Caldaguès*, or a dish of *veau d'Aubrac*, diluting the repast with red or white wine at discretion—vintages from the *caves* if desired, and for cheese, either a slice of the softish *petit bleu d'Auvergne*—very like the blue vinny of Dorset—or the more Cheddar-like *fourme* of Cantal. Of course, if not an invalid and only on holiday bent, he will not stay at the *Établissement* but will be well lodged at one or other hotel, or in more modest houses. But, wherever the traveller may elect to sleep, he will seek his two meals in the ancient fashion at one or other *table d'hôte* in the village. In any case, if an invalid, he will ask the good counsel of Dr. Brémond, at once the Mayor and the senior physician of the canton in which he has so long practised, or of Dr. Besson, his *adjoint* in municipal affairs and his concurrent in practice since the war.

If the visitor, whether invalid or mere tourist, desires distraction, let it be plainly said that there are no facilities for the adult and extraverted Englishman who likes to play at ball. It was proposed at one time to convert a declivitous piece of stony land into a “tennis,” but small boys now play “le fotball” there instead.

On the other hand, even if one rises at six, no day seems long to those who can enjoy the never-ending charm of the hills and the waterfalls, and the lush emerald green

of the natural lawns beside the streams, and the pine woods, and the rocks of white marble and sparkling granite and mica and quartz, with their wealth of insect and reptile life—the lizards and the dragon-flies, and the beetles and spiders. Then there are the ruined and the inhabited castles here and there, and above all the quite incredible Château d'Alleuze, only twenty kilometres away. That such an ogre's castle really exists as this that they showed me I do not believe; it must have been the illusion of a nightmare, bred of a Gothic romance and a picture by Salvator Rosa!

Of the natural marvels within short but perilous motoring distance, and above all, of the Cirque de Malet: or of those created by man, such as Eiffel's Viaduc de Garabit—an airy thing in steel lattice-work painted lavender-grey and hung in the sky like a cobweb at a height of four hundred feet to span a gorge of thrice that width—I will say nothing. But for the mere walker there is the perpetual interest of innumerable miniature ascents and descents, of finding out fresh points of view each day, and of visiting the villages and hamlets, and watching the primitive and laborious husbandry of the farmers and peasants, who wring a scanty livelihood from an ungrateful soil by methods that have been practised since the Age of Bronze. To the ethnologist there is something very fascinating in the peculiarities of the Auvergnat type, which surely perpetuates some brachycephalic Asiatic race, so common is the occurrence of the truly Mongolian *facies*. He may even, if he be lucky, see played La Bourrée, the traditional folk-dance

of Auvergne ; since the war a little out of favour with the young people. If he has the further good luck to see some of the older dab hands take part in it, he will admit at once that some of the figures involving the imitation of animals and the simulation of devils, as when a dancing man suddenly capers on all-fours, must have come down from some far-gone tribal ceremony ! Above all, there is the fine intelligence of the little children to be studied, and the character in the faces of the womenkind, especially those that are old, whose symmetrically carven and harmonic though hardened lineaments are eloquent of continuity in race, in custom, and in thought with the earliest of those Alpine people who reached these rocky backwaters. Then, in returning home after his sojourn, anticipating confidently, as well he may, a *bonne année* after his cure (for such his doctors will promise him), he will ponder again the utter wisdom of Hippocrates, who first declared that he who would study medicine properly must first consider the seasons, the winds, the waters, the airs and the places, and what effects and qualities are peculiar unto each.

SOME PROBLEMS OF INFLUENZA¹

All diseases occur at all seasons of the year, but certain of them are more apt to occur and be exacerbated at certain seasons.

HIPPOCRATES : *Aphorisms*, iii, 19.

THE recrudescences of influenza, and of epidemic encephalomyelitis in one form or another during the last year or two, have not unnaturally provoked fresh interest in the epidemiological, as well as the clinical problems thereby presented.

Even the daily newspapers take a hand, furnishing paragraphs of mysterious provenance that hint at the forthcoming discovery of some unknown and almost unknowable "germ," to be followed at no distant date by the invention of some efficient antitoxin or other infallible means of prevention or cure.

Whilst leaving the bacteriologists to pursue their researches to a successful issue in the seclusion of their own laboratories—for it were impious to believe that persons who ignore clinical and epidemiological data are not the predestined solvers of clinical and epidemiological problems—it may be profitable to discuss briefly two questions that, although nowadays regarded—at any rate in the best medical circles—as hardly worthy of scientific investigation, have nevertheless engaged attention since the earliest ages. These questions are, first, that of the periodicity of the recurrent pestilence we now call

¹*The Military Surgeon*, September, 1926, p. 284; *The Franco-British Medical Review*, April, 1925, p. 165.

influenza, and, secondly, that of the dependence of these recurrences upon, or their correlation with, what used to be called telluric and cosmic influences.

These two problems are hardly separable, and their discussion involves consideration of what is called the doctrine of the epidemic constitution. Some reference to this doctrine is therefore imperative.

It is usual, on the part of those who speak about Sydenham but do not read his writings, to misinterpret this doctrine, and then dismiss it from consideration as "admittedly vague." Not uncommonly it is supposed that Sydenham invoked the epidemic constitution as a kind of mysterious "causal agent" that produced epidemics in the way that poison gas produced casualties amongst our troops in France. Nothing could be more wide of the mark. As I have elsewhere shown (*Influenza : Essays by Several Authors*, p. 507), this misconception is based upon a mistranslation. For Sydenham the cause of many epidemics may have been some atmospheric change or factor obtaining during the course of an epidemic constitution, but the term *epidemic constitution*, as used by him, is merely the epidemiological equivalent of what for the military historian is a campaign, for the art student a "period," and for the viticulturist a vintage-year. The doctrine itself—originated by Hippocrates, revived by Baillou, and expanded by Sydenham—merely implies that, during natural periods of time, the epidemiological happenings in any stated area tend to exhibit peculiarities and particularities that are more or less distinctive, just as during the reigns of particular

kings, fashions, customs, art and literature tend to conform to the spirit of the times, and just as during a protracted war natural periods of activity seem to be marked off as "campaigns." Further—and this is the particular contribution of Sydenham to the formal doctrine—it is believed that observation, if pursued by the historical method over long periods of time, will show that there is a tendency for the periodic recurrence of like *epidemic constitutions*. In similar fashion the historian comments upon the periodic appearance from Italian soil of a Cæsar, a Napoleon, or a Mussolini, and upon the similitude between the recent Russian revolution and that in France in the century before the last. Just so Peirce, the American philosopher, in his remarkable book, *Chance, Love and Logic*, has suggested that there is a tendency for great historical and philosophical periods to exhibit a five-hundred year periodicity. However this may be, if we study the epidemiological records, it becomes clear that Sydenham was right and that epidemiological history can be naturally and simply dealt with in terms of epidemic constitutions exhibiting a tendency to recur, though with irregularities and exceptions, at long intervals of time. The problem of the periodicity of influenza is one detail in the problem of the epidemic constitution. Correlation with telluric and cosmic influences is another and relevant consideration. So also is the empirical fact that epidemiological happenings are not disorderly, but (when looked at from a wide angle) a part of an organised process. The further fact, that observation discerns natural periods in

the epidemiological series, must also be accepted. But such acceptation does not imply either disregard of what bacteriology has to say, or belief in "atmospheric influences" as causes of epidemics.

(I) THE PERIODICITY OF INFLUENZA

At the very foundation of the doctrine of the epidemic constitution lies an observed fact, implicitly or overtly recognised by Hippocrates, by Baillou, and by Sydenham. It is this: that epidemic catarrhal prevalences, and others of the nervous kind now familiar to us as encephalitis lethargica, poliomyelitis, and the like, have always, though irregularly, tended to be manifest chiefly about the times of the equinoxes, and, more particularly, somewhat *before* the vernal equinox—now the twenty-first of March, and *after* the autumnal equinox—now the twenty-third of September.

Certainly there are eccentric variations, but the generalisation holds true, and there is a seasonal incidence, of the kind indicated, in all years.

Now, several years ago, it was suggested by Brownlee—not as a result of historical study, but from investigation of mortality returns from London and certain great towns during recent years—that there is, in any year, a thirty-three week periodicity in respect of influenzal prevalence, with the peculiarity that, when an epidemic falls due between the middle of June and the beginning of November, it does not normally occur, and the maximal incidence is, in any case, between January and

May. (*Lancet*: 1919, ii, 856; 1923, i, 1116.) Brownlee's conclusions have been persistently supported by the medical correspondent of *The Times*, but destructively handled by Spear, who declares that the alleged thirty-three week periodicity has no existence in fact. (*Lancet*: 1920, i, 589.) The truth seems to be that Brownlee has merely re-stated, with the apparent but oftentimes fallacious precision of modern statistics, the older generalisation that these prevalences commonly occur in the late winter *before* the vernal equinox and, though less frequently, *after* the autumnal equinox. In any case, they are most prone to occur about the times of the equinoxes. This seasonal periodicity, thus stated, at once correlates the prevalences with "telluric and cosmic influences." But there is more to be said. In some as yet unpublished Chadwick Lectures given in 1918, I showed, cartographically, from an historical study of the recorded catarrhal and nervous influenzas during four hundred and fifty years, that there is a definite tendency for influenza to appear in epidemic form every ten or eleven years, and a clearly marked though irregular tendency for greater prevalences or pandemics to occur about thrice in a century, or once in every thirty-three years. It would perhaps be more correct to say that, although the manifestations are variable, eleven-year and thirty-three-year periods can be distinguished. Now Stallybrass, in a paper supporting Brownlee's thirty-three week hypothesis, has found evidence of a decennial periodicity as shown by mortality statistics, and his suggestion has apparently been endorsed. (*Lancet*: i,

139, 372.) Stallybrass' decennial periodicity nearly coincides with the eleven-year rhythm that I think historical investigation demonstrates, but, while Brownlee and Stallybrass only deal with death-rates in respect of the respiratory forms of influenza, my own conclusions are based upon epidemiological records. Now, with the great majority of epidemiologists, I consider the nervous prevalences to be a part of influenza and to represent the "other side" of the catarrhal epidemics.

I suggest, then, that there is at any rate a *primâ facie* case for postulating: (1) a seasonal or pre-vernal and post-autumnal equinoctial periodicity: (2) a ten- or eleven-year periodicity and; (3) a thirty-three-year periodicity, of prevalences, epidemics, and pandemics respectively, of what we may call catarrhal and nervous influenza.

(II) TELLURIC AND COSMIC INFLUENCES

If, noting these suggestions, we inquire whether or no there is any evidence of correlation between influenzal prevalences and cosmic or telluric influences, we are at first up against a blank wall. In the very remarkable *Report on Influenza*, issued by the Ministry of Health in 1921, it is stated with the greatest assurance that we may have "complete confidence that the cruder generalisations of the older hypotheses are unsound and that the part played by the meteorological peculiarities of the pandemic years was a minor one" (*loc. cit.*, pp. 150-162).

We are also told that the negative result of exploration of the events of the year 1918 absolves us from need of discussing the telluric theories of Boyle, Arbuthnot and Creighton.

Nothing could be less adequate to the occasion, however, for the explorations conducted by the Ministry into the events of 1918 do not appear to have taken note of the phenomena of atmospheric pressure and magnetic disturbances. To the former subject Dr. C. M. Richter, of San Francisco, has devoted years of study, and his series of papers has been completed by a monograph that appeared in the *Archives of Internal Medicine* for March, 1921, p. 361. Richter has studied the question, not merely in terms of special localities and epidemics, but in those of continents and cycles of years, and has seemingly established that there is correlation between the pandemic periodicity of influenza and cyclic variation in air-pressure, so that, broadly speaking, pandemic and epidemic influenza corresponds with general and local high air-pressure (or anti-cyclonic conditions) and inter-pandemic and epidemic quiescence with low air-pressure conditions. Moreover he declares, and rightly, that while influenzal and pneumonic epidemics and pandemics are a function of anti-cyclonic weather values, only developing during high-pressure periods, the cyclic variations of high- and low-pressure periods harmonise with, and are apparently caused by, cyclic changes in solar activities.

Pace the Ministry of Health, then, there is at least a *prima facie* case in favour of some "cruder generalisations of the older hypotheses."

But can the matter be pursued farther? Yes: if, that is, we care to have recourse to such not very recondite works as *Whitaker's Almanack* (The Earth: Solar System, etc.) and the *Encyclopædia Britannica*, 11th and 12th edition (Climate and Climatology, pp. 525-6: Magnetism, Terrestrial, pp. 376-7-8).

We find it said that, while there is a connection between the solar activity (as shown by the sunspots) and terrestrial magnetism (for the coincidence in time is too marked to be fortuitous), the cyclic variations in respect of terrestrial magnetism and sunspots, as scientifically determined, corresponds very closely with the periodicities and rhythms of influenza as revealed by epidemiological and historical research, and stated above.

To be more precise, Mander has shown that, if the year be divided into three seasons—Winter, from November to February: Summer, from May to August; and Equinox, made up of March, April, September, and October—the maximal disturbances of terrestrial magnetism occur *during the equinoctial season with a tendency to precede the vernal and follow the autumnal equinox*. In other words, the magnetic periodicity is that determined for influenza by Hippocrates, Baillou and Sydenham, and indirectly supported by Brownlee and Stallybrass.

We find, moreover, that, while the seasonal correspondence thus holds good for influenza, for terrestrial magnetism, and for sunspot activity, there is a definite eleven-year periodicity in respect of the last two series of phenomena which corresponds exactly with what I

have suggested for influenza and, very closely, with the ten-yearly influenzal periodicity of Stallybrass.

Nor is this all. Brückner, of Berne, has established a long period climatic oscillation, known as the Brückner cycle, the length of which is approximately thirty-five years, that obviously coincides with Lockyer's thirty-five year cyclic variation in solar activities. The correspondence with the suggested thirty-three-year rhythm of pandemic influenza needs no emphasis, but it should be remembered that E. Richter has found like cycles in the movements of Swiss glaciers, and that the thirty-three-year periodicity of influenza has received such recognition that some epidemiologists have tried to explain it in terms of generations of immunity amongst the population of the world.

Again, it is interesting to observe that the air-pressure variations shown by Richter (of San Francisco) seem to follow the same cyclic necessities, while, just as I noted some time ago that sometimes one influenzal cycle seems to merge into another, so has Brückner declared that his cycles do merge or overlap.

Lest all this should seem merely fanciful, some particular references may be made to recent work. Vallot, Sardou, and Faure, in a communication made to the French Academy of Medicine on July 11, 1922, showed definite correlation to exist between sunspot activity and periods of acute physical disturbance in chronic invalids. The relation between sunspot activity and terrestrial magnetic disturbance was insisted on by Cortie at the Toronto meeting of the British Association

in 1924; and Sir Richard Gregory, when speaking on the same subject in 1925, drew attention to the sunspot activity of 1917. This was the year that gave birth to the last great pandemic influenza, to so much poliomyelitis in America, and to encephalitis lethargica in Austria and elsewhere. Moveux, of the Bourges Observatory, is confident as to the correlation between sunspots and individual and national disturbances of health—physical and mental: predicting renewed disturbances in 1928, when the present sunspot period will culminate. This will be just eleven years from the commencement of the great influenzal disturbances the echoes of which have not yet died away, and seem, indeed, even to be gaining in strength.¹

Our own Meteorological Office issued, in 1923, a Report dealing with variations in the levels of the great African lakes, wherein was emphasised the general correlation between telluric and cosmic changes and disturbances—a subject discussed at greater length by Huntington, of Yale, in his recent work on *Earth and Sun*. Huntington introduces some considerations of enormous epidemiological interest, as when he shows that barometric gradients over the North Atlantic respond in different and opposed ways to sunspots on the two sides of the sun's equator. These and other facts seem to show that, as had already been suspected, there is no simple effect on terrestrial conditions of solar activities, but, as it were, reaction or response that is compensatory,

¹ As a matter of fact, this prediction was amply verified during the winter of 1928-9.

in great part at least, for direct effects. We can thus form some idea of peculiar disturbances arising when this compensation is imperfect, or is interfered with by other influences. Again, the adjudicators for the Adams Prize, at Cambridge, have propounded as a subject for discussion in 1925-6 the question "as to how far the various suspected periodicities of earthquake phenomena, if real, must be attributed to the periodicity of external agents, and how far, if at all, they represent periodicities of free vibrations of the earth itself." Truly, this is a harking back with a vengeance to the "cruder generalisations of the older hypotheses"! For no notion was more familiar to the great thinkers of bygone days than that of the interdependence of conditions of terrestrial life and changes in the physical condition of the earth and the sun, as revealed by earthquakes, eruptions, storms, and the like.

Indeed, there are only too many evidences of the dependence of the rhythm of all living creatures—men, and even microbes—upon the rhythm and changes of the earth and the sun and the other elements of the cosmos.

Sir Arthur Shipley has shown how a sea urchin in the Red Sea "keeps time" with the moon's phases (*Lancet*: 1923, i, p. 479), and Hopkins, of the United States Bureau of Entomology, has formulated a "bioclimatic law" that expresses the response of insect life to climatic conditions. Butler, speaking to the Association of Economic Biologists on November 18, 1921, did much the same in respect of plants and fungi, while, in

the *Lancet*, for March 3, 1923, is an interesting note upon the changes in physiological functions that, even in the laboratory, are found to be correlated with seasonal phenomena. One thing at least is clear. If we take heed of the despised "telluric and cosmic influences" inferred by the older writers and demonstrated by scientific observation in these latter days, we are not thereby under the necessity of dispensing with bacteriological research. Far from it: we may, perhaps, come to a better understanding of those relations between Man and the microbe that, too often, are expressed in terms of plague and pestilence. That is, however, another story. In the meantime, need we hesitate to endorse with Theophilus Thompson, and in respect of influenza, some of the "cruder generalisations of the older hypotheses," and to say that, "where we cannot detect relations of cause and effect, we may yet obtain a glimpse of truth in the study of concurrent series of phenomena"?

THE TREATMENT OF ENCEPHALITIS LETHARGICA¹

Sleep and wakefulness, both of them, when immoderate, constitute disease.

HIPPOCRATES : *Aphorisms*, ii, 3 : vii, 72.

THE comparatively large number of cases recognised, since our last vernal prevalence of influenza (1924), as cases of encephalitis lethargica (or, better, of epidemic encephalitis) has given rise to some disquietude that, not unnaturally, has found expression in the daily papers. To some extent the statistical evidence of increase, as compared with past years, must be discounted. Without any doubt, cases are now diagnosed as "encephalitis lethargica" that, three or four years ago, would have passed into some other, if indeed into any nosological category. When, at the discussion on epidemic encephalitis held at the Royal Society of Medicine in the autumn of 1918, it was suggested by me that many cases widely differing in severity and type from those described by von Economo nevertheless deserved to be placed in one and the same category, very evident amusement was provoked. To-day, cases are called encephalitis lethargica even when no lethargy is present. That is why the term "epidemic encephalitis" is to be preferred.

This shifting of the centre of the spectrum doubtless accounts in part for what is called a change in type and in severity. When only severe cases with marked and

¹ *The Military Surgeon*, October, 1926, p. 439; *The Franco-British Medical Review*, November, 1924, p. 136.

persistent lethargy were diagnosed as encephalitis lethargica, it was correct to assert that the case-mortality was high, even that it was 30 or 40%. Such a figure is now ludicrously inappropriate, in the view of most observers, although of course those who declare that encephalitis lethargica is a malady with a mortality of 40% may feel themselves justified in refusing the name to cases marked only by slight headache, transient diplopia, and an occasional hiccough. At any rate, the finer the mesh of the diagnostic net, and the more widely it is cast into the sea of clinical happenings, the more numerous will be the fish and the less their mean importance.

Such considerations must be borne in mind but, nevertheless, the number of cases recognised, the still serious case-mortality, and the gravity of the too frequent sequelæ seem to warrant the payment of more attention to the question of treatment during the acute stage than is generally accorded. Indeed, it cannot be said that this point has been systematically discussed: certainly, as was recently said in the *Lancet* (May 17, 1924, p. 1011), there is "no recognised treatment." This is true enough but should not be, as is too often the case, an excuse for doing little beyond carrying out the purely mechanical process of notification.

Treatment that really is empirical—and such is not always to be despised—is, as a rule, a kind of professional distillation from what has been brewed among the people. It cannot be said that any basis for a purely empirical treatment of encephalitis lethargica is now acknowledged: experience, whether popular or professional, has not been

transmitted. The happenings have been too discontinuous. A generation has arisen that knows not with what it has to deal, and which refuses the appeal to history.

And so it comes about that the line of treatment now adopted—whether positive or negative—depends very largely upon the personal views held concerning the nature of the “disease.” To that extent it is rational. But, though rational, it is sometimes derived from *a priori* prepossessions, and is only in certain instances based upon inductions made from the available facts. The available facts are largely historical.

Those who have no views—provisional or fixed—concerning the nature of the affection, seem generally content to do either little or nothing—though perhaps inclined to try some drug that someone else says he has found useful—or else, as they say, to proceed “on general principles.” “Proceeding on general principles” is a method that, in practice, seems to have little relation to any principles, general or special, and usually resolves into pure opportunism.

Those who have “views” and who seek a rational method of treatment in accord therewith, fall into two groups.

In the first are those who hold that the cases represent the incidence of a specific disease—as is said, a disease *sui generis* produced by infection with a specific germ that has a specific affinity for the parts of the brain that it “attacks.” Those who think thus are hopefully looking forward to the discovery of some specific anti-toxin

or vaccine that will prove of curative as well as of immunising value. In the meantime, they apparently treat their cases much in the same way as those who have no crystallised views.

In the second group are those who believe that the cases now called cases of encephalitis lethargica are best thought of as constituting the reverse of the medal whose obverse is marked by what we call the respiratory and gastro-intestinal forms of influenza. To use current forms of expression, they regard encephalitis as, clinically and epidemiologically, the nervous form—or one of the nervous forms—of influenza. They think of influenza, in its “ typical ” form as a generalised disturbance of the whole system, but a disturbance atypically “ specialising ” in respiratory, gastro-intestinal, and nervous forms, seen as complications, as special diseases, and epidemically in groups of cases. This point of view, which has been defended by Sir William Hamer, and by the present writer on many occasions, need not be here discussed at length (1, 3).

Its justification, which was at first largely historical, epidemiological and clinical, can now, with increasing confidence, be based upon the results of pathological work undertaken by Tarozzi, Rosenow of the Mayo Clinic, and many others.

This view of the relationship between what we call influenza and what we call epidemic encephalitis, or encephalitis lethargica, goes hand in hand with the twin theses: (1) that no clear line of demarcation other than the topographical can now be maintained between

“ epidemic encephalitis ” and “ epidemic poliomyelitis ” and ; (2) that epidemic poliomyelitis (if not every case or type of “ infantile paralysis ”) stands towards influenza as does epidemic encephalitis or encephalitis lethargica. The topographical distinction between epidemic encephalitis and epidemic poliomyelitis seems as irrational as would be the setting up of right-lung-pneumonia as a disease distinct from left-lung-pneumonia, on the grounds that the one disease attacks the right, and the other the left lung. So, although some say that encephalitis attacks the brain and poliomyelitis the spinal cord, clinical observation shows increasingly that in many cases of “ encephalitis ” there is some cord involvement and that in most cases of “ poliomyelitis ” there is, at the onset, some cerebral implication.

The notion, first set out by Brorström some fifteen years ago, that epidemic poliomyelitis is a nervous form of influenza, has lately received convincing support from investigation of the Tübingen epidemic of 1922, reported by Schall in the *Münch. med. Woch.*, 1923, lxx, p. 763.

The synthetic view, then, is that encephalitis lethargica represents a nervous form of influenza that tends to occur in older, and epidemic poliomyelitis a like nervous form that is more frequent in younger patients.

There are no established pathological, clinical, bacteriological or epidemiological facts inconsistent with this conception, which is otherwise supported by a vast mass of detailed observation. Moreover, it affords a basis for a rational plan of treatment that is very successful in practice.

In order that this basis be apprehended it is necessary to grasp the importance of a theory or explanation, now derided, but once held as a dogma. Certain great physicians of the Renaissance—Fernel, Benedetto and others—following the Greeks, recognised to the full that during certain great outbreaks of disease which we should now call coincident prevalences of influenza, encephalitis and poliomyelitis, the nervous forms, though in liaison with the respiratory and gastro-intestinal catarrhs, and the sudorific fevers, were yet, often enough, in clinical antithesis to them. The nervous and non-nervous forms represented, as I have said, an obverse and a reverse.

In order to “explain” what they observed those who, like Fernel, lived through the great prevalences of the early sixteenth century, conceived the notion that “catarrhs” were the expression of a distillation of humour from the brain and spinal cord. So long as there was expression of this distillation by flux from a mucous membrane, by sweat, or by diuresis, all was well. But when the distillation was repressed, nervous troubles arose—paralyses, convulsions, lethargies, deliriums, psychoses and disturbances of special senses—in fact, the whole gamut of encephalitis lethargica and poliomyelitis. And so they justified treatment of what until lately was called a determinant, revulsive, and derivative, as well as alterative character. And they found it successful. We may laugh at their theory, if we will. But their clinical observation was sound and their treatment was rational. It flowed from their interpretation of carefully observed facts. As a matter of fact, every observant clinician

knows that if a child has measles, and the rash does not "come out," there is danger of convulsion and death. During the sweating sicknesses—indubitable influenzas—it was found that the earlier the sweat the less the danger of the otherwise "inevitable somnolence and stupor." And Sir Thomas Horder, with his clinical acumen, has lately remarked that nervous complications of influenza are to be dreaded less when there is sweating than when the skin remains dry and hot.

An illustration of the principle involved may be borrowed from the story of syphilis. When sudorific and sialogogic methods of treatment were in vogue, lesions of the skin and mucous membranes were common: cerebro-spinal syphilis was rare. To-day, sudorifics and sialogogues are unfashionable, even as adjuncts, except among those of great clinical experience. We see less syphilis of the skin and mucous membranes, but syphilis of the central nervous system is very common. In Eastern countries, where syphilis is rife and sweating cures are popular, general paralysis and tabes are rare.

So it seems as if, no matter what the particular virus may be, there is in certain disorders a sort of *balance* between the vaso-motor conditions of the central nervous system on the one hand and the skin and mucous membranes and viscera on the other. And we are offered the choice between (i) attacking the virus *in situ* and (ii) relieving the congestion in the "site of the lesion" by "drawing off" to the periphery of the body, and by promoting fluxes therefrom. But there is no reason why, if we accept the notions here set out, we should not

combine a judicious frontal attack with a turning movement. It is only those who are obsessed with the idea of a specific malady due to a specific virus that has a specific affinity for specific sites in the brain, who will fail to see reason in the older methods, which have been proved to be useful, and which do not depend for their utility upon the sixteenth century "theory of catarrh."

This theory of catarrh was really the "explanation" invented—as are all explanations—to "describe" what happened. It justified, in the terms of the period, the treatment that had been found useful in experience. And so it is that, by use of the historical method, we can recover the practical experience of bygone physicians that has been lost to us by reason of the discontinuity in the stream of epidemiological happenings.

We have had a similar lesson of late years in another sphere. Those to whom war was a "new disease" of the body politic expected all sorts of consequences and results, advantageous and otherwise, that they would not have expected had they been conversant with the teachings of history. Will the experience now gained be transmitted to the generation after the next, or will that generation again pass through the stage of ignorance and illusion that the present has traversed?

At any rate, those who adopt what may be called the influenzal conception of encephalitis have some basis, both in reason and in experience, on which to found their treatment: some standard by which test can be made.

Early diagnosis is admitted by all to be of paramount importance but, obviously, those who adopt this influenzal concept have this advantage over those who do not, that they, in the first place, will be on the look-out for nervous symptoms occurring during or after what appears to be influenza, and, in the second place, will attach importance to complaints, however trifling, indicating disturbance of the nervous system in patients who have recently had influenza, and when influenza is "about."

For the clinical and epidemiological relation between epidemic encephalitis and influenza is threefold.

Thus: (i) Cases and epidemics of encephalitis appear sometimes as autonomous cases and epidemics, but always in relation in time and space to cases and epidemics of influenza.

(ii) Cases and epidemics of encephalitis occur in persons and in places that have recently suffered from influenza.

(iii) Cases and epidemics that appear to be cases and epidemics of influenza may rapidly, and without break of continuity, become cases and epidemics of encephalitis.

So that, clinically, we find one patient who displays signs of influenza rapidly developing encephalitis: we find others, after recovery from influenza, developing encephalitis; and we find others, again, who, without personal precedent illness, fall ill, more or less suddenly, with encephalitis, at a time when others in the vicinity are suffering from influenza, or catarrh of the respiratory or alimentary system.

In any circumstances, whenever encephalitis lethargica is thought of, the patient should be put to bed—no matter how trivial the symptoms—and kept there, with proper nursing attention.

At the outset, careful cleansing of the mouth and nose should be insisted on: these organs are reservoirs of the “unknown virus.” The use of a paraffin spray, in addition to that of the toothbrush, is generally sufficient, if *properly* directed.

A free evacuation of the bowels, if, as is usual, there is constipation, is imperative. To give a “*casara*” is feeble. An ounce of castor oil or four grains of calomel is the least that will be sufficient, unless there be diarrhœa. Even then, half an ounce of castor oil is a valuable agent.

Next, proper action of the skin and kidneys *must* be secured. Warmth in bed, a warm bath (if practicable) and an old-fashioned diuretic mixture cannot be omitted. Little food is necessary at first. Hot lemonade, fruit juices, broth and beef tea are all that is wanted. The mouth is usually dry and the tongue brown. Milk is heavy and septic and encumbers the alimentary mucosa, as Hippocrates knew.

In addition to these measures I have, since the outbreak in 1918, almost invariably ordered the addition of urotropin, salicylate of soda, and benzoate of ammonia to the diuretic mixture, for those patients who could take liquid medicines. These drugs are helpful: they are antiseptic, and with reason may be said—the one or the other—to attain the cerebro-spinal fluid, the urine, and the bronchial mucosa. Ten grains of each may be given

four-hourly to an adult at first, but the urotropin may be reduced after forty-eight hours if there be fear of renal irritation (2).

A very large number of cases do very well on such a *régime*. But if there be marked lethargy or coma, or the case drags, more active measures are required.

Some French physicians administer pilocarpine to promote sweating: this has done me good service, but if the skin remains dry, hot packs are better. As a derivative, the fixation abscess, much employed by the French, is of great value. One gramme of oil of turpentine is injected into the muscular mass on the outer side of the thigh. At the end of forty-eight hours, if the abscess has "succeeded," it is opened. If not, another injection is made at the same spot. The procedure may be repeated, if necessary or advisable. If the patient be incapable of taking liquid medicines by the mouth, one of the injectable preparations of urotropin may be given repeatedly, intravenously or intramuscularly.

Should the case not show obvious signs of improvement at the end of several days, the old-fashioned exhibition of mercury is called for. Thirty-five or forty years ago, I saw this drug generally given in cases of this sort by Gowers and by Bastian. It is an "alterative": in some way or another it changes circumstances; it is an antiseptic, and it subdues subacute inflammations. Its use should not be confined to the treatment of syphilis. In encephalitis it may be given in several ways: as grey powder, three or four times a day from the first: as the liq. hyd. perchlor., with or without iodide: as an

inunction, to the head, nape, and spine; or, if it is desired, as a mercurial cream, intramuscularly. On the whole, I think the continued use of grey powder and of inunctions the most useful. Even if salivation is produced, one of the very trying and important symptoms—the dryness of the mouth, so often present—is overcome. This dryness of the mouth, leading to overgrowth of bacteria, etc., in the mouth, is a frequent source of inspiration for secondary pneumonia, of which so many patients die.

The question of lumbar puncture is one of moment. The procedure has a value that is in part diagnostic, in part therapeutic. But there is a tendency to perform it, by way of doing something that is impressive, and without any clear appreciation of its usefulness. If diagnostic uncertainty requires it, so much should be done, *once*. Even then, prudence is advisable. I am sure that harm results from its careless performance and its unnecessary repetition, in cases where the spinal cord is involved. And these are more frequent than are thought. The only cases in which I think its repeated performance is advantageous are some of those in which there is a marked meningeal reaction, as shown by: (i) neck stiffness: (ii) a turbid cerebro-spinal fluid and; (iii) a relatively high cell-count therein.

In special cases special drugs are necessary. Bromide does but little good when there is delirium and myoclonus: morphia is dangerous to a degree; but small and repeated doses of paraldehyde—thirty minims every four hours—by mouth, or a large dose—one-half to one

ounce—per rectum is usually successful, and may be continued daily for some time, if need be.

Convalescence must never be hurried. Careful management, feeding, exercise and rest are all required. If there are signs of Parkinsonianism, it is usual to give hyoscyamine. But I have found physostigma, in tincture, in extract, or as eserine, much more useful than this. The dose should be pushed until the effect is produced, though the enormous doses employed by Ringer need not be attained.

It would be foolish to give any “statistical” impression of the results of treatment conducted on these lines, because the personal diagnostic equation is incalculable. But, at two hospitals, my house-physicians and myself have systematically treated all our cases of acute encephalitis and myelitis on these lines since 1918, with a freedom from case-mortality and from sequelæ that does not appear to be secured by those who treat their cases less actively and more “expectantly.”

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THE "TROUSSE-GALANTS" OF 1528-29 AND 1545-46¹

In acute diseases, it is not quite safe to prognosticate either death or recovery.

HIPPOCRATES : *Aphorisms*, ii, 17.

THOSE who are familiar with Hecker's account of the sweating sicknesses may recollect that a malady, known at the time as the trousse-galant, trousse-galland, or trousse-galand, is said to have devastated France in 1528, and that another epidemic, possibly of a like nature, carried the same name in 1545 and 1546.

Hecker did not regard these trousse-galants as manifestations of the sweating sickness, but rather as local prevalences which were a part of the general "epidemic constitution" (to use Sydenham's phrase) of the times during which they occurred, and he left the riddle of their clinical and epidemiological affinities unsolved.

What were these affinities ?

The question is not merely curious ; upon the answer depends the manner in which we shall solve one of the most fascinating problems in historical epidemiography, namely, that of the nature of the sweating sicknesses themselves. And yet, although some reference is occasionally made to the *second* trousse-galant, I do not know

¹ A paper read before the Section of the History of Medicine, Royal Society of Medicine, 1922. *Proc. Roy. Soc. Med.*, 1921-22, xv, Sect. Hist. Med., pp. 27-34.

that the *first* has been discussed, even in France, save incidentally and by Hecker.

The verb *trousser* (whence “trousse-galant”) has many meanings, but its essential significance is expressed by the words *expédier vite*. And, in most modern French dictionaries *trousse-galant* is defined as a popular name formerly given to *une maladie qui emporte le malade en peu de temps*. This is following Littré, who says that the name has also been applied popularly to the *cholera morbus*. Larousse, however, going further, defines *trousse-galant* without more ado as *choléra sporadique*.

Whilst thus the dictionary-makers, there does not appear to be the faintest shred of evidence that either the *trousse-galant* of 1528, or that of 1545-46, was dysenteric or diarrhœal in character, and these epidemics cannot therefore be admitted as choleraic in our sense of the term. How it was that the connotation of cholera became attached to this folk-name I will discuss presently; it is now necessary to state what is positively known concerning the two prevalences mentioned by Hecker.

I.

Hecker's brief and uncritical account of the first of these two prevalences is taken from the writings of Eudes de Mézerai, who flourished in the seventeenth century (1610-83), and obviously cannot be esteemed an original authority. But it is quite clear that de Mézerai derived his information from the earlier work of Guillaume de Paradin—an historical writer little known to

French epidemiographers—who lived during the time of the epidemics in question.

According to Paradin, the years 1525-30 constituted a sort of natural epoch during which the seasons were confounded, the crops failed, and pestilence, war, famine and licence, stalked gauntly through the fair land of France. This statement is extremely important when read in conjunction with citations that I shall make presently from Fernel and from Jordan.

However, what immediately interests us is that, in July and August, 1528, the French army before Naples was broken up by a sudden explosion of epidemic disease (Paradin, Martin du Bellay), and about the same time the crops at home in France failed miserably, so that returning soldiers were confronted with domestic misery and famine. What followed is best described in Paradin's own words :—

“ Il s'engendra de la famine sus-dite, une nouvelle maladie, et inconnue aus Médecins, laquelle outre ce qu'elle estoit très redoutable pour sa nouveauté, soudeinneté et violence, elle estoit si fort contagieuse qu'il n'y avoit celui qui s'en pust sauver de ceus qui avoient tant peu fust communiqué et conversé avec ceus qui en estoient espris.

“ Cette maladie presente et tout mortifère s'attachoient aus hommes avec une grosse et rigoreuse fièvre continue, et les mettoit si bas que la plus grand part en mouroient.

“ Ceus qui par grande aventure en eschappoient se treinoient en grand pitié et langueur six ou sept semaines ne leur estans demeurez cheveus en teste ni ongles en piez et mains et le tout avec si grand desgoustement que toutes viandes leur saisoient mal au cœur.

“ Et ne pardona cette horrible maladie à sexe ni age quel qu'il fust dont il mourut une grande partie du monde.

“ A cette nouvelle maladie fut nouveau nom imposé, et fut nommé *Trousse-galand* pour la contagieuse mort qui l'en ensuivoit avec grande et impourvu soudeinneté.”

All this happened in the autumn of 1528, but later, perhaps early in 1529, there was a second wave of pestilence, for Paradin says, a few pages farther on :—

“ Mais, à fin que rien ne demeurast en la boette de Pandora que la poure esperance, survint une generale et furieuse peste qui redoubla les malheurs precedens.”

Now, while this story given by Paradin is the only original account that I have been able to find of the public health in France in 1528-29 (during which year, say the Teutonic writers, that country completely escaped “ the Sweat ”), there is nevertheless one little piece of indirect evidence not without interest, and, possibly, importance.

Boutiot, who published in 1857 a small book dealing with bygone pestilences at Troyes, states that the malady epidemic in that city in 1529-30 does not appear to have been characterised by the tumours (i.e., buboes) which marked the plague there regnant from 1518-24. Boutiot, who certainly had access to all the municipal archives of Troyes, does not, unfortunately, give any reference or details, but, as he goes on to say that the malady of 1529-30 seems to have been a kind of “ cholera morbus,” it is permissible to believe that his authority made mention of the “ trousse-galant ” which, as we have seen, was considered, at the time Boutiot wrote, to have been a kind of “ cholera morbus.” (Cf. Littré.)

However this may be, Paradin's account clearly permits us to formulate certain definite conclusions concerning the malady of 1528-29:—

(1) It took the doctors by surprise, was esteemed a "new disease" and was given a "new name."

(2) It spread, seemingly by contagion, with extreme rapidity, and, while it affected persons of either sex and every age, was particularly notable for the dramatic suddenness with which it carried off the young and vigorous.

(3) It was not connected with any special stigmata: no rashes, spots, blotches, tokens or fluxes are recorded as distinguishing it.

(4) Death, when it ensued, was after brief illness: convalescence was tedious and attended by marked debility, with alopecia and affections of the nails.

(5) The epidemic was followed by a second wave, even more "furious" than the first.

(6) The whole affair occurred during a period of five years or so, marked, not only in France but elsewhere, by "sudorific fevers" (Fernel: Jordan), as well as by other strange maladies.

All these marks are those of epidemic influenza, much as we have known it, in some of its most striking forms; and it is not a little interesting to note that it was in France and Switzerland that, during the recent epidemics, alopecia and affections of the nails were chiefly noted (Merian).

We may perhaps provisionally conclude then, that the French trousse-galant of 1528-29 was a neurotoxic

influenza, and may leave on one side for the moment the question of its relationship to the contemporaneous "sweating sickness" in England, Germany and elsewhere.

II.

In 1545 and 1546 a peculiar "pestilential fever," characterised by many of the epidemiological features of influenza in its "trailing" epidemic form, spread from Piedmont, through Savoy, into the Auvergne, and, it is said, so far north as Cambrai. This malady corresponded in its geographical range pretty closely with the influenzal encephalitis of 1890-91-92, called "Nonna" in North Italy and in Provence. It is described for us by Sander and by Ambroise Paré, and, like the epidemic of 1528-29, was known as "Trousse-galant." Like it too, it spared neither sex nor age: and all classes of the community were affected; but, as Paré says, *plustot les robustes que les debiles et les riches que les pauvres*. Paré's description, which is more effective than that of Sander, leaves me with little doubt that this trousse-galant, like that of 1528-29, was a severe if peculiar influenza, and marked by neurotoxic phenomena. About the same time, however (in either 1544 or 1545), a sudden pestilence amongst the soldiery broke up our camp at Boulogne and extended rapidly to the French Army. This epidemic, mentioned by de Mézerai (again borrowing from Paradin), is also written of by Hecker as a "trousse-galant," but, so far as I can find, without any strict authority. Nevertheless, the fact is indisputable that

the trousse-galant of 1545-6, described by Paré and by Sanders, was one of a whole congeries of influenzal and encephalitic maladies that, in epidemic guise, trailed through Europe from 1543 onwards, until 1547 or so, and again in 1551.

Some discussion as to the probable influenzal nature of certain epidemics in 1543 will be found in Corradi, and it cannot be denied that *encephalitis lethargica, myoclonica, et convulsiva* was mingled with influenza at Florence during that year (Cardan). According to Hecker "plague and petechial fever" raged in Germany in the same year, but the nature of this plague and spotted fever is not clear.

In Stow's *Survey*, however, there is an account of a perfectly obvious case of "encephalitis lethargica" which, in 1546, attracted much attention in London; and there is also some hint of a "sweat" having afflicted Galway in 1543 (Creighton).

It is suggested therefore that, in considering the nature of this second trousse-galant, attention should be paid to the comparative epidemiography of the times.

III.

After this second affair, the name "trousse-galant" does not appear to have been attached in any specific sense to any particular epidemic, but we owe to Dr. Creighton's learning one very valuable piece of information. In certain parish registers in England there are references to deaths during 1551 (the year of the fifth

and last "Sweat"), from the "Stup-gallant" or "Stoupe! knave, and know thy Master." And Dr. Creighton makes the illuminating remark that these casual references in parish registers indicate a diffusion of the sweat of 1551 "all over England, in the manner of Influenza."

Now Creighton, as we know, never identified the sweating sicknesses as influenzas, but it is impossible, save on grounds of prejudice, to deny that the fifth sweat or "stup-gallant" in England during 1551 *was* an influenza, just as we have seen reason to believe were the earlier trousse-galants in France—that of 1528-29, contemporaneous with the "fourth sweat," and that of 1545-46 which we have just discussed. Again, the comparative record is almost conclusive. The fifth sweat (or stup-gallant) in England was contemporaneous with the *coqueluche*, or influenza in Paris (le Paulmier, Reusner); with a sweat in Flanders (Brassavola); with epidemic pneumonia and fevers in Germany and Switzerland (*cf.* Hecker); with obvious influenza in Swabia (Crusius); with fever and encephalitis in Italy (Corradi); and with epidemic encephalitis in Siberia (Ozanam).

IV.

It may however be said that, if on the one hand the two trousse-galants, and the stup-gallant of 1551, appear to have been influenzas, nevertheless, on the other, their history is inextricably bound up with that of the sweating sickness. This is indeed the case: and the solution of the whole tangle appears to be that the fourth and fifth "sweats" in England (I am not here dealing with the

first, second, and third "sweats") were indeed influenzas. This proposition, as is well known, was first put forward by Sir William Hamer in 1906, but there was something like an obstinate refusal on the part of epidemiographers to weigh the evidence: a refusal in part accounted for by too narrow a conception of epidemic influenza, as a "catarrhal" malady only: in part the result of inadequate acquaintance with the facts; and in part the outcome of a slavish adherence to what may be called the Teutonic theory of the sweating sicknesses.

According to this theory (for which Gruner, Haeser, Hecker, and Hirsch are chiefly responsible), the five classical sweats in England stand for recurrences of a peculiar malady, *sui generis*, bred of the corruption and degradation of the English race in the fifteenth and sixteenth centuries, and which only once—in 1529—spread to the Continent, even then sparing the French, Italians and Spanish, though not the Germanic tribes and Scandinavian peoples.

Hecker and Hirsch both believed that this sweating sickness, *sui generis*, is in some sort represented by the miliary fever of modern times in France and North Italy: Creighton believed that the sweats in England developed from the miliary fever which he thought to have been anciently endemic in Picardy and imported into England by Picard mercenaries just before the battle of Bosworth in 1485.

The crude notion of Gruner and Hirsch has been supported by a cobweb of fantasy in which positive assertions have been founded on negative findings, and

in which one of the chief threads has been the declaration that, in 1528(29) *the English sweat did not invade France*, since, in French history, there are no traces of any such prevalence in France during those years.

The crux of the whole question, then, is that of the nature of this *trousse-galant* of 1528(29) in France, which has been so unaccountably overlooked by almost all who have written on the topic. In a word, if neurotoxic influenza prevailed in France in 1528(29) when the sweating sickness raged in England, Germany, Flanders and Scandinavia, and *ex hypothesi* did not affect France, the odds, in an epidemiological sense, are all in favour, in Lord Herbert of Cherbury's words, of the maladies spread abroad throughout Europe in 1528-29 being "the same contagion of the aire, vary'd according to the clime." That is to say: Europe in 1528-29 was afflicted by a sudorific and neurotoxic, rather than by a catarrhal, respiratory, and gastro-intestinal influenza; an influenza, moreover, which, as in our own times, took on "local colouring" in different places. At any rate, such is the conclusion to which I have come after an honest attempt to review the whole of the evidence, in the light of Sir William Hamer's work—an attempt involving the bold expedient of consulting the original authorities and not merely the inaccurate copyists put forward by so many as original authorities. One or two fresh items of evidence can, in consequence, be adduced, though without much hope of the conversion of a writer in the *British Medical Journal* (1919, ii, p. 386) who does not feel confident that, since the delivery of the Milroy Lectures in 1906, sufficient

new light has been thrown on the historical question to convert what was, in 1906, an arguable proposition into an undoubted fact.

The first items are the account given by Paradin (already cited) and his clear statement of fact showing that, in France, the quinquennium 1525-30 was indeed a true "epidemic period," or "constitution." This is important, for the year of the fourth sweat in England (1528, according to Hecker and Creighton: 1529, according to Hirsch) and of the *Sudor Anglicorum* in Germany and elsewhere (1529) has hitherto been treated, save by Hecker and by Hamer, as a year of isolated epidemiological happenings rather than as a year entering into an epidemiological period or "constitution." Moreover, there are two definite and authoritative passages, in the works of two classical authors, which clearly show that Paradin's statement in respect of the five years 1525-30 in France—namely, that they were marked by a series of pestilential epidemics—is applicable also for the same quinquennium in Britain, in Flanders, and in Germany, during which "sudorific fevers" prevailed. And one, at least, of these passages, as clearly shows that the same "sudorific fevers" prevailed in France during these years as elsewhere. Thus, Fernel declares that "sudorific fevers" inspired great terror "in omnem inferiorem Germaniam, in Galliam Belgicam et in Britanniam" from 1525 to 1530, when, in the autumn, they became widely diffused. The expression *in Galliam Belgicam* may properly be taken to refer to Flanders and not to the kingdom of France as constituted at that day.

Nevertheless, it refutes the foolish statement that French-speaking populations were immune to the sweats, and shows clearly that the great sweat of 1528-29 was the culminating expression of an epidemic genius that had been "brewing up"—as does that of influenza, during a term of years. But Jordan's statement is even more remarkable. It is this:—

“. . . Sudor Britannicus ex Anglia (ubi primum anno salutis 1486 conspectus dicitur, Henrico Septimo rerum illius Insulae potente) vicina littora inficiens. Gallos, Belgas et Germanos ab anno 1525 usque 1530 vastavit et hic morbus denominatione(m) fortitus est.”

It seems then pretty certain that, by whatever names these sweating fevers from 1525 to 1530 were called, the French people did *not* escape them and shared in the general diffusion which in 1528 and 1529 afflicted not only England and Germany, but (as we know from Forest) the Low Countries, Norway, Denmark, Poland and Transylvania; the curious may satisfy themselves, from a careful study of Corradi and Villalba, that neither Italy nor Spain escaped the *thing*, even if the records do not mention *the name*. In the light of this conception of the relation of the *trousse-galant* and "sweats" of 1528-29, as elements of an influenzal constitution, the relation of the *trousse-galant* of 1545-46 to the influenzas and encephalitic maladies that prevailed in Europe from 1543 onwards up to the time of the "stup-gallant," "sweat" and *coqueluche* (or influenza) of 1551, becomes clear.

Incidentally, the contention of the writer in the *British Medical Journal* that no "encephalitis" is shown to have preceded the sweats of 1528(9) and 1551 is disposed of.

Finally, the "catalepsy" mentioned by Forest, as prevalent amongst the soldiers at the siege of Metz (in 1553) may be referred to as illustrating once again how very frequently the nervous form now known as "encephalitis lethargica" has followed in the wake of the generalised and diffused forms of epidemic influenza.

V.

How is it, if the explanations thus given be correct, that the name "trousse-galant" has come to be defined by modern dictionary-makers as standing for a kind of cholera morbus, or, more simply, as *choléra sporadique*? The answer is not difficult to find. Originally, like so many popular names in epidemiological literature, it did not stand for any specific disease-concept, but was the name of one or two like epidemiological happenings. (The sweating sickness, *coqueluche*, "spotted fever," and "sleepy sickness," are all cases in point.) As time went on the name came to be used in a purely attributive sense, as meaning, in Littré's words, *une maladie qui emporte le malade en peu de temps*, without reference to any particular epidemic in the past, or to any particular form of disease. And, since few maladies take off a sick person so quickly as do "ptomaine poisoning," acute gastro-enteritis, and the like, it is not surprising that it thus became used for such diseases, when memory of the old trousse-galants had faded.

In 1643, for example, one Van der Heyden wrote an odd but quite famous little book, in which, describing some form of epidemic disorder of diarrhœal nature, he

says that death frequently and rapidly thereon ensues, *à raison de quoi me semblent mieulx s'accorder avec cette maladie les noms de "Trousse-gallant" et de "Felon."* Here the two folk-names are used purely attributively or abusively, and it is interesting to note that while to-day in remote parts of France a peasant will speak of *charbon*, or anthrax, as *trousse-galant*, our peasants will speak of a poisonous whitlow as a "felon." But nothing could be less like Van der Heyden's malady than either anthrax or a whitlow—from the point of view of the pathologist.

There appears to be some evidence however that in the eighteenth century "trousse-galant" was vulgarly applied much in the sense suggested by Van der Heyden, for in Le Sage's translation of the *Histoire d'Estévanille Gonzales* (edition 1743, p. 55), a widow is made to say, *Un choléra morbus, vulgairement appelé un trousse-galant, emporta mon époux en moins de dix jours.* Perhaps this *choléra morbus* was a dysentery, or typhoid fever.

At any rate, since such was the usage in the eighteenth century we can well see how it was that Boutiot, in 1857, thought the trousse-galant of 1529-30 at Troyes to have been a kind of cholera, and that Littré, about the same time, should have framed his definition in the same sense. Yet there is something more to be said. The extraordinary coincidence in time and place, in Europe between 1832 and 1858, of what was thought to be Asiatic cholera and influenza, had everywhere caused epidemiologists to trace some epidemiological connection between the two types of prevalence, and some confusion came, particularly in France, to exist in the popular mind concerning the

two pestilences. Even when in the autumn of 1918, influenza broke out in Paris, it was suggested that "cholera" had arrived (*Lancet*, 1918, ii, p. 595).

This, however, is not all. During the great influenzal and "choleraic" period, that extended roughly from 1830 to 1860, it was noticed in France that there was an extraordinary series of outbreaks of the trailing form of epidemic disease that has intermittently since 1712 been a part of French rural life, that is known as *la suette des Picards*, *la suette du Midi*, *la suette miliare*, *la fièvre miliare*, and the like, and that has been, as already stated, identified with the sweating sickness of the fifteenth and sixteenth centuries (Hirsch; Netter; Foster). It was further noted that many of the local outbreaks of this miliary fever coincided with outbreaks of what was called cholera, but which may not have been due to the comma bacillus. At any rate, the idea was seriously put forward, obtained great currency, and is not yet dead, that true cholera represents a kind of intestinal sweat, and the sweating fevers are a kind of cutaneous cholera. The miliary, or sweating fever, it was suggested, was a *choléra sudoral*. It is not necessary to say more: the point has been exhaustively discussed by Hirsch. Nevertheless, after all, the definition by Littré of the trousse-galant as a kind of cholera really leads us back to its relation with the sweating sicknesses and with influenza. For the miliary fever, as I have seen it in French villages, is neither more nor less than endemic-epidemic influenza of the type seen in isolated rural districts, and in "closed" communities such as barracks and industrial schools.

The sweating and the rashes are the inevitable accompaniment of a neurotoxic influenza when an unwashed patient is put in a cupboard-bed, is smothered in eider-downs, and is left to stew in the fœtid atmosphere of a one or two-roomed cottage with the windows closed. Certainly, some types of influenza come very easily into the picture even in the absence of such squalid and unwholesome surroundings as those I have indicated, and the blebs of severe "miliary fever" were admirably described by Lord Dawson as seen by him in cases of "influenza" at the Crystal Palace in 1918.

We can, therefore, the more easily understand how it is that, while at Paris in 1918 some cases of influenza were deemed choleraic, in the Midi people spoke of the "miliary fever," a disease which, like influenza, is sometimes despised, sometimes dreaded, and which has even been described by one writer as trivial and by another as a scourge (*La Presse Méd.*, 1918, ii, p. 527). So the circle closes: and, had the influenza of the autumn of 1918 been called a "trousse-galant" there would have been historical justification. At any rate, when in 1918 the brave people of the Midi called the influenza *la suette* they spoke with greater wisdom than did many learned men.

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ACRODYNIA¹

In acute diseases, coldness of the extremities is bad.

HIPPOCRATES : *Aphorisms*, vii, 1.

THERE appears in *The Medical Press and Circular* for November 3rd, 1920 (p. 346) an interesting note on the account, given by Graves, of a curious epidemic, seemingly of peripheral neuritis with œdema, that he witnessed at Paris in 1828.

This epidemic is known to epidemiographers as acrodynia, and, while there are many versions of its history in French literature, that most accessible to British readers is in the second volume of Hirsch's *Geographical and Historical Pathology*. The epidemic, without any evidence other than its supposed affinities to the mythical pandemic "ergotism", was thought due to some form of poisoning, but, as Hirsch says, occurrences of a similar nature were recorded in after years—indeed, so lately as 1866—though many of the accounts show "very material deviations from the type" of 1828-9.

The really important point, however (as usual, missed by Hirsch) is this: that the acrodynia in Paris preceded by only a few months the pandemic influenza of 1830, and that the later, and less extensive outbreaks were, speaking generally, in similar relation to subsequent influenzas.

¹ *The Medical Press and Circular*, December 22, 1920, p. 495.

Acrodynia, indeed, in 1828-9 stood in precisely the same relation to influenza as did the Swedish poliomyelitis of 1887-8-9, as did the encephalitis lethargica of 1917-8-9, and as have done many other epidemics of encephalitis, myelitis, meningitis, and neuritis (Crookshank: *Medical Press and Circular*, November 19th and 26th, December 10th, 1919, pp. 405, 423, 467).

Acrodynia, it may be said, represents a neuritic type of the nervous cases and epidemics that invariably precede, accompany or follow, in some place or places, epidemic and pandemic influenza.

These nervous epidemics have generally differed from each other in respect of their predominant characters: sometimes they have been encephalitic, sometimes myelitic (or "spinal"), sometimes meningeal, sometimes neuritic, and sometimes sympathetic (or "vegetative") in respect of anatomical and physiological incidence.

The symptoms have been sometimes psychical, sometimes paralytic, sometimes myoclonic, sometimes sensory, and sometimes "trophic," "vasomotor," or "sudoral." Sometimes there has been lethargy; sometimes delirium, or mania.

But, in every epidemic all types have been represented, and, for every type described in each epidemic, at least one epidemic characterised by that type can be cited.

In 1918, whilst engaged at the London Hospital, I saw, during the height of the epidemic of "encephalitis lethargica," two cases that I at once connoted with the epidemic. The one, that of a young woman whose illness commenced with fornications, and with blebs on the

hands, proceeding to gangrene, and who died of an ascending central myelitis, recalled forcibly the sixteenth century epidemics which, by a delusion originating in the eighteenth, have come to be regarded as due to ergotism. The young woman of whom I write was, indeed, actually suspected of having been poisoned by ergot.

The other case, that of a child with crural peripheral neuritis and œdema, recalled, with equal force, the accounts of acrodynia.

Recently Hanns (*Progrès Médical*, 1920, xliv, p. 473) has reported in full, as manifesting a "new form of encephalitis lethargica," a case which is even more *à propos*. Hanns' patient displayed all the peculiar classical symptoms of acrodynia—conjunctivitis, facial and peripheral œdema, a curious erythematous rash *en placards*, and so forth.

The curious and interesting fact that the pre-influenzal and post-influenzal epidemics of which I have written should so frequently have been ascribed to food-poisoning deserves attention. Ergot, radishes, and sausages have all been suspected at different times; hence the names ergotismus, raphania, and botulismus. But, although there may have been, and may still occur, small groups of cases due to poisoning by ergotised flour, wild radishes, and bad sausages, there is absolutely no evidence that any one of the widespread nervous prevalences during influenzal periods has been so produced.

The original accounts of the epidemics of this nature that occurred at the end of the sixteenth century,

including those given by Sennert and Horst, are marvels of clinical observation, epidemiological sagacity, and acute reasoning, and avoid entirely the eighteenth and nineteenth century fallacies, of Teutonic origin, that still flourish abundantly to-day in Great Britain, in the United States, and even in France.

The Italians alone have lately seized the essential truths, as witness the recent papers of Maragliano, Re, Zagari and many others.

Since, however, these lines have been written two recent accounts, from the United States, of grouped cases resembling and identified with acrodynia have reached England (Weston, *Archives of Pediatrics*, Sept. 1920, p. 513, and Byfield: *American Journal of Diseases of Children*, 1920, November, p. 347). Byfield regards the cases discussed by him as due to a post-influenzal neuritis. The recently described Haitian epidemic dropsy seems also to call for examination.

“BOTULISM” AND HEINE-MEDIN DISEASE

(NOTES ON THE PRESENT EPIDEMIC)¹

For their species would be almost innumerable, if every symptom . . . were held to constitute a new disease, and receive a different name.

HIPPOCRATES: *On regimen in acute diseases.*

(PREFATORY NOTE: June, 1930. This paper was written and printed during those weeks in May, 1918, when London, in addition to the agonies of the War, was harassed by accounts of a strange epidemic, identified with what has been named Botulism, and thought, in some quarters, to be the result of what we then called “enemy action.” In this paper I tried to set out the reasons, derived from rather unusual clinical experiences I was then enjoying, for considering the epidemic in question to be a manifestation of Heine-Medin disease affecting the brain (or, parts of the brain) rather than the anterior horns of the grey matter of the spinal cord in the fashion then taught in England to be characteristic of what we called *infantile paralysis*. In 1918, the Heine-Medin concept (which we owe so largely to Wickman) of the old *infantile paralysis* or *acute anterior poliomyelitis* representing, so to speak, just one or two notes out of an octave, was little appreciated, at any rate by academic teachers. My own extension of the Heine-Medin concept to embrace those cases of mid- and fore-brain disease now covered by the names *encephalitis lethargica* and *epidemic encephalitis*, met with little support, save from a few authorities of great importance, amongst them the late Sir William Osler. Instead, not every case of brain inflammation then to be seen in London, but certain of such cases, were officially considered to be cases of a “new disease,” *sui generis*, then lately described by von Economo, in Vienna, and named by him *encephalitis lethargica*. The narrow view thus taken has since gradually enlarged: the name *encephalitis lethargica* has been dropped by the wisest, and *epidemic encephalitis* has widely supplanted it. For it has gradually come to be seen that the picture first drawn of *encephalitis lethargica*, was altogether too narrow, and that, as I ventured to say in the beginning, we have to do, really, with a *meningo-encephalo-myelitis*, presenting, in different cases, and in the same case at different times, symptoms of affection of any and every part of the central, peripheral,

¹ *The Lancet*, 1918, May 18th, p. 699.

and sympathetic nervous systems. Even by those who most seriously maintain that *encephalitis lethargica* and *acute poliomyelitis* “ are ” different diseases, the impossibility of giving any clear-cut distinctions is becoming felt so acutely that one eminent observer has been compelled to declare that *encephalitis lethargica* sometimes simulates *poliomyelitis*, and *poliomyelitis, encephalitis lethargica* ! In a word, every criterion that has been proposed, whether clinical, pathological, serological, cytological, epidemiological or immunological, has proved useless, and no better grounds have been made out for distinguishing “ two diseases ” in this respect than could be proposed for distinguishing four kinds of pneumonia—right upper, right lower, left upper, and left lower. Above all, epidemiological, clinical, and pathological studies in Australia and Japan have forced the reluctant conclusion upon those who are informed that, if we admit *encephalitis lethargica* and *poliomyelitis*, as distinguished in England and America, to be “ two diseases ” we are logically driven to admit almost every epidemic that, while like to, yet differs from our epidemic of 1918 and successive years, as a “ new disease.” Hippocrates was right, when he rebuked the Cnidians of his day as he would rebuke those of to-day.

No one has taken a more important share in directing men’s minds towards the larger view than has my friend and colleague, Professor Cruchet, of Bordeaux, who as soon as, if not indeed sooner than, von Economo, recognised, in 1916, that Europe was confronted with an unfamiliar form of inflammation of the central nervous system. He with greater wisdom, did not, like von Economo, confine his attention to the narrow syndrome that was first described as *encephalitis lethargica*, but saw with vision that we had to do with an acute and protean encephalomyelitis, or, as the French now say, a *névraxite*. I dare not claim, as yet, my friend’s support for my thesis that there is *no* good ground for distinguishing Heine-Medin disease (or, *acute anterior poliomyelitis*—if the misnomer is to be perpetuated) from what so many in France call, and with every reason, Cruchet’s disease. But I confess that the maintenance of this distinction seems to me, every year, to be less and less defensible.

But there is another and important question. When, in 1918, I hastened to communicate the conclusions to which I had come concerning the diagnosis of our then epidemic as “ botulism,” to Sir William Hamer, while gratified to find that he agreed with me so far, I was disconcerted to learn that *encephalitis lethargica* and *poliomyelitis* chiefly interested him as manifestations of influenza, in the epidemiological sense.

Rather taken aback, I was utterly amazed when, a day or two later, in discussing the clinical and epidemiological affinity between our *encephalitis lethargica* and its satellites with the American *poliomyelitis* of the preceding year, a very eminent neurologist indeed told me to distrust the American accounts. He had himself studied, he told me, the epidemic of 1917 in New York, and he had found that the cases were *for the most part, influenza*. Here, it seemed to me, was the clue to the mysterious pronouncement by such authorities as Draper and others, that *poliomyelitis* was a general epidemic disease with incidence upon the nervous system in only a certain number of the total cases. I set to work upon some historical studies, which convinced me that Sir William Hamer was right when he asserted the almost universal association, throughout the ages, of influenzal epidemics with epidemic disorders of the central and peripheral and sympathetic nervous systems. Whilst engaged in these studies the great event happened and, first during the summer, and later during the autumn, we renewed acquaintance, after thirty years, with influenza on the grand scale.

There is no need here to say more, concerning what I have already said a great deal. But I am convinced that scepticism as to the essential unity between, on the one hand the great *nervous* prevalences—however we may classify and subdivide them—and on the other, the great *influenzal* prevalences, can only be maintained in ignorance of the records, and is bound to disappear as the relevant observations become known. In the meantime, what may be called the synthetic view makes steady and sure progress. From Finland, from France, from Germany, from Italy, from the Balkans, from India, Australia, and the Orient, there are increasingly evident signs of the interest taken in the synthetic view by thoughtful people, and, as experience is added to experience, the conviction of the essential truth of the notions taught by Hamer in this century, as by Creighton in the last, and by Huxham, Sydenham, and Baillou in the eighteenth, seventeenth, and sixteenth, is deepened. This is not to say that these notions will ever become popular. On the contrary, they are notions which are the outcome of modes of thought and tendencies that have ever been opposed by two massive forces:—the Vulgar, and the Academic.)

It may be asked, without frivolity, whether the use of the word "botulism" in connection with the cases now attracting such general attention is not equivalent to the

traction of an alien and enemy comestible, though not a red herring, across a profitable line of clinical inquiry. Reflection, and the experience of the last two weeks, I think, must have convinced most people that, as has been thought by some of us from the first, the various cases now under observation are clinically indistinguishable from the various “ types ” of polio-encephalitis, or, more accurately, meningo-myelo-encephalitis, that are exemplified in epidemics of the Heine-Medin disease. It is, I believe, not too much to say that no cases now under observation have been without counterparts in Heine-Medin epidemics, and, at the worst, it is probably only premature to say that no “ type ” of Heine-Medin disease is without its compeer in London to-day.

Leaving, however, on one side for the moment the question of clinical identity—or, rather, assuming that there is such clinical identity—the question of pathological identity remains. Certainly, so far as the gross, and I believe also the finer, results of post-mortem examination are concerned, there is no discrepancy; and investigation of the blood and cerebro-spinal fluid reveals no obvious incongruity.

But whatever the presumption we must attend: (1) the application of serum tests: (2) the transmission of the disease to apes: (3) the identification of the Flexner bodies, or perhaps it should be said of Rosenow’s streptococcus, and; (4) the accumulation of blood-count records showing the leucopenia described by Eduard Müller, or the other blood pictures described by American authors.

Moreover, however great may be the apparent certainty that the cases are clinically identical with examples of Heine-Medin disease, unless there is proved to be pathological identity in the completest sense it cannot be said that the suggested diagnosis of botulism is driven off the field completely; if, alternatively, we have not to deal with a "new" disease.

For a simple reason: the authentic accounts of outbreaks of botulism furnish no grounds for saying that, clinically, cases of botulism differ in any way from certain types of Heine-Medin disease.

This statement, which I believe is incontrovertible, leads directly to a question that must needs be propounded. Has it been proved, with bacteriological rigour, that any of the cases called "botulismus" were really due to infection, or rather to intoxication, by the products of the *Bacillus botulinus* described by van Ermengem? Competent authority assures me that, so far as a critical examination of the published records can determine, this has been proved; and I must there leave the point. But it must be said that in America certain outbreaks of disease, clinically resembling certain types of the Heine-Medin disease, have been associated with the commensal ingestion of certain canned vegetable foods, though without in more than a few cases the conviction of *B. botulinus*; while others, in Germany, have been found associated with organisms other than the *B. botulinus* (Much). But again, it cannot be said, merely from an examination of the records, with what rigour the bacteriological work was carried out on which the conclusions

were based. So far as I am aware, however, no bacteriological proof is forthcoming that any of the present cases are due to bacterial infection or to intoxication by the products of bacterial activity, whether associated with food consumption or not.

We are therefore entitled to discuss, on its merits, the question of the clinical identity of the present disease with the Heine-Medin affection, while awaiting the conclusions of pathological and bacteriological investigations. The relation of the present cases to food consumption must first be mentioned. It cannot honestly be said that there is so far any conclusive evidence that any general class or particular examples of food have been implicated. We all eat canned foods, and many, if not all of us, sausages, nowadays; while it does not appear, as yet, that, in more than a few instances, if any, have members of the same family or of the same commensal group been affected by the present epidemic. But, since in epidemics of the Heine-Medin disease each person the subject of marked nervous symptoms is probably but one of a small group of infected persons of whom the majority do not display signs of implication of the nervous centres (or, if so, in only an abortive form), it cannot be said that the absence of evidence that the present disease is due to food or connected with food distribution or consumption really inclines the balance in favour of Heine-Medin disease as opposed to any form of food poisoning; nor, if closer investigation and the discovery in the future of commensal abortive cases does suggest such a connection, will the balance be turned

decisively against the Heine-Medin hypothesis of the present epidemic.

It is perfectly credible, though as yet unproved rather than disproved, that the virus of the Heine-Medin disease (if, as now for the most part believed, it is one and specific) is transmitted, and the disease propagated in more than one manner. Nay, it is even probable; and, moreover, it is probable that the different outbreaks or epidemics are connected in part with differences in modes of propagation.

Consider typhoid fever. Clinically, milk-borne typhoid differs from typhoid transmitted elsehow. It is milder, it is less contagious, it displays a rash less frequently, it is less frequently accompanied by diarrhœa, and it notably affects younger persons. If we knew nothing of the bacteriological basis of typhoid and approached the disease anew, would not some observers recount outbreaks of "milkism" and dispute its identity with, while admitting its resemblance to, "oysterism," or the virulent affection transmitted during the Boer War by flies?

One thing is certain, whilst by no means ignoring the possible connection, at least in part, of the present epidemic with food consumption—whether or no it be indeed Heine-Medin disease—it is of the greatest importance that all deviations from health, on the part of those associated commensally or otherwise with sufferers from marked nervous symptoms, should be studied with care, however irrelevant they may seem to the symptoms that are now attracting public attention. Especially is this true of cases of pulmonary and gastro-intestinal disorder, anomalous in type, or rapidly fatal.

I return to the question of the clinical identity of the present epidemic with the Heine-Medin disease. Heine-Medin disease is the name given to a conception, in part clinical, in part pathological, of a disease constituted by a variety of symptom-groups; sometimes met with, as is said, sporadically; sometimes in widely distributed epidemics. The best known of the “sporadic” symptom groups is infantile paralysis—“acute anterior poliomyelitis.” The epidemic symptom-groups are those of various forms of polio-encephalitis, poliomyelitis, meningo-encephalitis, meningo-myelitis, encephalitis, myelitis, and polyneuritis. The particulate virus is believed to be the same in all cases of the disease. Some writers still dispute the pathological identity of the common sporadic type with the epidemic disease. But, in Römer’s words, no case of sporadic poliomyelitis has been recorded to which a counterpart cannot be found among epidemic cases; and, further, it is doubtful whether so-called sporadic cases are even really sporadic. Abortive associated cases are overlooked, and the “sporadic” homologues of the various epidemic types are diagnosed by the application of other names than that of “Heine-Medin disease.” Just so are “sporadic” cases of laryngeal diphtheria oftentimes diagnosed as forms of “croup” unless a fatal issue or pathological investigation determines their real nature.

Epidemics are necessary for the realisation of the pathological unity of diverse symptom-groups due to the same exogenous virus.

Retrospection at the present moment brings to mind

the fact that during 1917 a not inconsiderable number of cases were seen, in respect of which a diagnosis of "polio-encephalitis" was either actually suggested or might well have been made. These cases fall into several groups:—

1. Ophthalmoplegias: nuclear, acute, and not syphilitic.

2. "Cerebro-spinal meningitis": not due to any ascertained micro-organism, and sometimes terminating favourably.

3. Encephalopathies, principally in children: beginning with headache, vomiting, and perhaps convulsion, and usually recovering, after a period of drowsiness or confusion, without palsy.

Cases belonging to the first group usually pass as nuclear paralysis of undetermined origin; those in the second as "meningismus" or meningitis; those in the third as "convulsions," constipation, acidosis, and so forth.

Some attempt is now being made to trace and classify the records of these and similar cases. Without doubt, however, cases of these seemingly diverse natures became more frequent in the earlier part of this year, though it was not until the last week of March that one was led to declare that we were then apparently on the eve of an outbreak of cases of polio-encephalitis, *clinically indistinguishable from those exemplified in outbreaks of the Heine-Medin disease.*

The cases which led to this conclusion were about ten in number. I exclude one or two dubious examples. Of these ten, two were under my care at the Hampstead General and North-West London Hospital.

One, a healthy girl of 21, was taken suddenly ill and was diagnosed successively by various medical men as suffering from ptomaine poisoning, diphtheria, abscess of the brain, and cerebro-spinal meningitis. She died, unconscious, of bulbar exhaustion, and post mortem it was demonstrated that extensive polio-encephalitis was present (March 7th, 1918).

The other, a boy, exhibited the indications described in the *Lancet* for May 4th, p. 636, by Dr. F. E. Batten and Dr. G. F. Still. He was diagnosed as a case of polio-encephalitis, and recovered after about ten days in hospital.

The remaining cases were seen by me elsewhere ; but were not directly under my charge. Of these four were examples of ophthalmoplegia, two had facial paralysis, and two had diplopia and nystagmus. All suffered from initial headache, several from initial vomiting, two or three were distinctly "mental" for a time ; and all, I believe, have survived.

Since seeing these ten cases I have had the opportunity of seeing at least forty others, of which some have been diagnosed as "botulism," and others have been thought not to be "botulism." *But all correspond to one or other type of the Heine-Medin disease, as seen in epidemics.* Some of these patients have died, others have recovered ; the remainder are under observation, and although it is perfectly true that hardly any two of these cases are, or have been, exactly alike, yet, as has been well said, "they all seem to have something in common." They differ, because in no two cases are the apparent sites of intensity of the neuropathic processes at work precisely the same ;

but they appear to resemble each other in the nature of these neuropathic or neurotoxic processes. A picture seen of a "typical" case is something impossible to construct. The onset of nervous symptoms, though sometimes sudden and sometimes ingravescent, seems usually to be rapid. I believe it may be insidious. Initial pyrexia and headache are frequent, but neither the headache nor the fever always continues; and sometimes there is no pyrexia during the period of precise observation. As in Heine-Medin disease, no age confers immunity, except, perhaps, from diagnosis!

A sudden or rapid onset may be marked by an "attack" or "fit" or a "convulsion," and sometimes, as in cerebrospinal meningitis due to the meningococcus, there may be sudden and persistent unconsciousness. Mentality may be gravely affected, or not at all; but the indications of disturbance of mind range from lack of concentration, emotionalism, confusion, and delirium to apathy, drowsiness, sopor, and coma. Constipation and retention of urine, on the one hand, or total incontinence on the other, may occur. Hypertonia of the limbs passing into extreme rigidity, hypotonia which passes into absolute flaccidity, "cataleptic rigidity," tremors, paresis, and paralysis, are all to be seen in different cases or at different stages of the disease. Apparently there are no nuclei of cranial nerves that always escape, though in particular cases the affection or immunity of particular nuclei may be difficult to demonstrate. It may be pointed out that in Borna's disease of horses (the veterinary analogue of the Heine-Medin disease) the olfactory bulbs are profoundly

affected, and that the dry mouth of the present disease (and of botulism too) is probably the result of arrested mouth secretions (fifth nerve), just as may be the " accumulations " of mucus in the naso-pharynx the result of palatal and pharyngeal paresis. Deafness and tinnitus are not uncommon and nystagmus may occur without diplopia. Some patients complain of " loss of vision " as distinguished from diplopia. This may be cortical or perhaps an indication that optic atrophy will follow later on. Not a few patients complain of pains in the limbs, of tenderness, tinglings, and other paræsthesiæ. In some there are indications of loss of sense of position and an ataxic gait, or at least one that is staggers. The various reflexes may be normal, exaggerated, or abolished. *Some cases are of minimal severity.* Rashes, as in New York, have been observed, and, as noted by Draper, amongst the children affected there is quite a number with separated central incisors.

It is impossible not to admit that the symptomatology of different cases depends on the localisation of the " foyers " of greatest intensity of the morbid processes at work at the time of observation. Hence it is possible to give a kind of topical classification of single clinical pictures, and to speak of encephalic cases, ponto-bulbar cases, spinal cases, meningitic cases, and so forth. But in particular individuals the localisation of *foyers* of maximum intensity itself varies with stages of the disease. Meningeal involvement may be late or early ; mid-brain affections may be, as in the purely ophthalmoplegic cases, apparently the whole basis of the symptoms throughout

the course of the disease ; or, again, but a part of a much more widely spread affair ; and so on. Hence the difficulties in precise clinical classification ; not of pictures, but of cases. Nevertheless, certain composite impressions tend to emerge ; and the more perceivably are the cases watched the more clear is it that, just as the "types" seen in epidemic Heine-Medin disease have sporadic analogues of which a great part at any rate shew the same pathological nature as the epidemic cases, so in the present series of cases are there analogues—I believe, homologues—of the various "types" seen in true epidemic Heine-Medin disease. The parallelism is roughly set out in tabular form below.

Type.	Epidemic Heine-Medin disease.	"Sporadic" analogues.	Present epidemic.
1	Abortive.	"Convulsions," "constipation," "ptomaine poisoning," "acidosis," etc.	Slight cases of various types, meningeal and encephalitic.
2	Spinal, of sporadic type.	Infantile paralysis.	Not recognised (<i>cf.</i> text).
3	"Spinal" of transverse and ascending type.	Some cases (<i>a</i>) of transverse, (<i>b</i>) of "Landry" type.	Transverse myelitis cases, "ascending myelitis" cases, &c.
4	Meningitic cases.	C.-s. meningitis of unproved bacteriological origin.	Meningitic cases.
5	Encephalitic cases.	Polio-encephalitis of Strümpell.	Most of the present cases, including "epidemic stupor."
6	Ponto-bulbar cases.	Polio-encephalitis superior and inferior.	
7 8	Atactic cases and Polyneuritic cases.	Certain cases of toxic polyneuritis. ? Cerebellar cases.	Cases with sensory and atactic manifestations. ? Cerebellar cases.

This table must be regarded as only provisional and illustrative. But it is none the less, I submit, of value. Several points are at once suggested. Why, it may be asked, if the present disease is Heine-Medin, are cases of the “ infantile paralysis ” type not being reported? The answer may be five-fold—

1. In Heine-Medin epidemics spinal cases of the limited “ anterior poliomyelitis ” type are far from being so numerically predominant as may be thought.

2. Unless the present disease is recognised as Heine-Medin, such cases will not be reported.

3. As Heine’s classical description shows, in the acute stage of infantile paralysis—so-called “ anterior poliomyelitis ”—there is generalised disturbance and sometimes cerebral convulsions, headache, and so forth. In other words, infantile paralysis, with permanent lower motor neuron injury, is oftentimes apparently evidence of only such damaged tissue as remains after a widely spread affection of parts of brain and cord. It is apparently only the sites of intensive action that receive permanent injury.

4. If the present disease be indeed Heine-Medin, it is in a few weeks rather than now that we should expect to meet with cases, more numerous than usual, of recent “ infantile paralysis ”—flaccidity with atrophy; and some of these flaccid atrophies will be then found in children whose “ acute ” illness attracted attention by reason, not of spinal, but of *encephalitic* symptoms with “ asthenia,” if at all.

5. Lastly, it must be remembered that it is not proven

that "infantile paralysis" (the result of so-called "acute anterior poliomyelitis") is always the result of a process pathologically identical, in respect of the virus, with the Heine-Medin disease.

The atactic and polyneuritic groups remain for notice. It is admitted by various observers that these groups are spoken of on grounds of convenience rather than from strictly scientific reasons. But already they appear to have had analogues in London to-day, and elsewhere.

The question now forces itself on us: Has sufficient care been taken by the employment of strictly scientific and experimental methods, to make certain that none of the cases of so-called Landry's paralysis—none of the cases of cerebro-spinal meningitis in which no pathogenic organism has been definitely implicated—and none of the cases of "toxic polyneuritis"—which have occurred and still occur amongst troops in the field—are examples of Heine-Medin disease?

It seems certain that it is not warrantable to declare, either on purely clinical grounds or because post-mortem changes of a certain nature are not found in certain parts, that a particular case of Landry's paralysis, of cerebro-spinal meningitis, or of toxic polyneuritis, is not an example of Heine-Medin disease. I am quite aware that distinguished authorities do not agree with this statement, but, given the extreme diversity of the clinical types seen at one time and another in Heine-Medin outbreaks, and given the extreme variability in the intensity and localisation of the changes found post mortem in that disease, it appears to me that the serological and other experimental

methods are the only ones on which reliance can be placed in our efforts to determine the proper content of our conception of the disease.

There are certainly three simple clinical methods which may, and should be, generally employed.

(i.) Eduard Müller insists that in acute Heine-Medin disease leucopenia is sometimes present; while recent American authors give other findings. Some investigations are at present being made on these lines and will be reported later. It appears that in Heine-Medin disease leucopenia may be an early phenomenon, but leucocytosis one that is later.

(ii.) In many of the present cases, as in many of the Heine-Medin epidemic cases, the cerebro-spinal fluid shows an excess of protein and some lymphocytosis. In others, especially those with cloudy cerebro-spinal fluid, as the Americans have found, there is leucocytosis.

My impression is that these latter findings have relation to the degree and extent of meningeal implication present; and I am not surprised that Dr. Batten and Dr. Still do not report such in respect of their cases.

(iii.) Ophthalmoscopic examination is advisable in all cases in which complaint is made of loss of vision.

On the whole, it may, I think, be stated with some confidence that there is at present in London (and elsewhere) an outbreak of cases clinically indistinguishable from examples of Heine-Medin disease. There is, in a word, clinical identity between this outbreak and a Heine-Medin outbreak. Whether there is pathological identity

as well ; whether there is really such a disease as botulism, simulating some types of Heine-Medin disease ; and whether, if so, there are any cases of true botulism now in London—are questions that cannot be answered by appeal to logical, clinical, “ pathological,” or epidemiological methods, but which depend for their solution on bacteriological, serological, and experimental investigations.

Until and unless these questions are so solved, and their solution is one of widespread and far-reaching importance, the problems of immunisation, both prophylactic and curative, cannot be hopefully attempted. But, at the risk of reiteration, it must be said that two points must be borne steadfastly in mind :—

(I.) It is possible that the name “ Heine-Medin disease ” has been applied to outbreaks which, though clinically identical, or nearly so, may yet be capable of differentiation (*a*) biologically, and (*b*) epidemiologically. For (*a*) the Heine-Medin virus may be, biologically, multiple, as is the parasite of malaria, or may occur in diverse strains, and (*b*) the Heine-Medin disease may be propagated in a multiplicity of ways at different times, as is typhoid fever. Hence problems relating to food preparation, food distribution, and food consumption must not be ignored or lightly treated.

(II.) It is possible that outbreaks closely resembling those of classical Heine-Medin disease, or some forms of that disease, may be due to virus that is totally disparate from the virus of that disease.

If—and there seems no reason at present to dispute it—the “ Flexner bodies ” do represent the Heine-Medin

virus, or one phase of it (Rosenow), and botulism is a reality, the latter proposition represents not a possibility, but a fact. It then remains to be determined whether specific infections or intoxications other than botulismus exist, giving rise to similar clinical pictures.

It may be pointed out that only four acute specific diseases essentially affecting the grey matter of the central nervous system—namely, tetanus, rabies, Heine-Medin disease, and botulism, have been shown so far to be connected with a particulate virus ; and, while in certain examples of three of these the clinical pictures may be almost identical, in all four the post-mortem appearances may be so similar *in kind* that for none of them can it be said that they are specific.

A few words on some practical issues may be added.

1. Collective investigation—clinical, pathological, epidemiological, bacteriological, and experimental—both at home and abroad, amongst civilians and amongst the troops in the field, should be instituted at once.

2. Investigation should be retrospective as well, and devoted to inquiry as to the prevalence at home and abroad, during the past year or two, of “ sporadic analogues ” of the epidemic Heine-Medin types.

3. Inquiries should be specially directed to the presence, past and now, of “ sporadic analogues ” amongst troops at home and abroad. Attention should be given to the lines of communication of the present disease, in view of the known fact that the Heine-Medin affection tends to be propagated along lines of traffic.

4. Cases under investigation should be for the present,

if not segregated, at least subjected to rigorous "bed-isolation"; and particular attention should be paid to all which may represent "abortive" types of Heine-Medin, or other disease found to be prevalent.

Cases of "septic broncho-pneumonia" and gastrointestinal disorder should be regarded, if anomalous in type, with grave suspicion, especially if leading to a rapidly fatal result.

In respect of individual treatment the following suggestions are made:—

1. Rest and good nursing are essential.
2. Careful cleansing of the mouth and pharynx; and attention to the state of stomach, bowels, kidneys, and bladder are imperative.
3. There is experimental and clinical evidence in favour of early, and at first intensive, administration of urotropin, which, it is believed, may be combined usefully with salicylate of soda and benzoate of ammonia, in free doses.
4. The employment of strychnine is of use in cases attended by bulbar exhaustion, or by paralysis of the muscles concerned in respiration.
5. The application by an atomiser of liquid paraffin to the mouth, nose, and pharynx alleviates dryness of the passages and, by lessening foulness, diminishes the risk of secondary pulmonary affections. To this end, a drop or two of oil of peppermint, or of izal, may be shaken up with each ounce of liquid paraffin used.
6. Confinement to bed should be prolonged rather than curtailed; and cases "discharged" should be

watched, for at least three months, in order that record be obtained of any sequelæ or permanent disability.

Since these notes were written the position has become more clearly defined. The present cases, it may be asserted, are cases of the Heine-Medin disease. The outbreak is, epidemiologically and clinically, identical with the great epidemic in New York of 1916. It has been “ brewing up ” in London since last autumn. All ages are affected, and the cases are of every grade of severity and of every clinical variety. It is of importance (1) that cases occurring in adult and advanced life should not be regarded as varieties of “ hemiplegia,” “ myelitis,” “ cerebral thrombosis,” “ uræmia,” and so forth, but their true nature recognised; (2) that the profession recognise (a) cases with abortive nervous symptoms, and (b) “ abortive ” cases with *no* nervous symptoms.

The disease has three clinical stages:—

(a) That of initial illness, with, perhaps, sore-throat; respiratory or gastro-intestinal disorder; headache and fever. This stage may be: (1) so trivial that it is overlooked at the time; (2) such as to attract attention and require treatment as “ influenza,” and so forth; (3) so severe that it is fatal in a day or two. Sometimes nervous symptoms occur in this stage.

(b) A latent period that may be very short (24 hours or less), or prolonged to perhaps a fortnight or even more.

(c) The stage of definite nervous symptoms. This stage may be: (1) absent or (2) “ abortive,” (3) rapid or (4) sudden in onset, (5) “ paralytic ” or (6)

“non-paralytic,” (7) so consecutive to (*a*) as to appear clinically a part of it, or (8) so removed from (*a*) (by (*b*)) that connection is not at first considered by the medical man. Abortive nervous cases are quite common, and I have seen some diagnosed as “functional.” But it is very difficult to say (1) how many cases occur without nervous symptoms, and (2) how many immune carriers there be.

The disease is communicable, especially in the stage called “*a*.”

Abundant literature on the subject, for the most part under the caption “poliomyelitis,” is to be found in the American periodicals for 1916-17. There are excellent clinical papers in the *Journal of the American Medical Association*, the *American Journal of the Medical Sciences*, and the *Archives of Pediatrics*. There is a valuable paper with bibliography by Haynes in the issue of the last-named publication for June, 1917; but the latest book available is Draper’s monograph entitled *Acute Poliomyelitis*, and published by Heinemann in 1917.

(POSTSCRIPT: June, 1930. Ultimate difference between this epidemic, and one of Heine-Medin disease, as commonly described, was found in the numerical predominance of cases with cerebral involvement recognised. Thus, in a letter dated June 19th, 1918, the late Sir William Osler wrote to me: “Your paper is the most sensible thing written on the subject. I have seen a great many cases when in U.S., on my last visit. The encephalitic features of *this* outbreak are most interesting, particularly the choreiform movements and inco-ordination, indicating involvement of the basic ganglia and the secondary tracts. . . . In Medin’s original paper there were many cranial nerve cases reported”. An analysis of the cases seen by me during the epidemic of 1918, but by many considered *not* to be cases of encephalitis lethargica, as at first defined, will be found in the *Lancet*, 1918, ii, p. 590, and in the *Proc. Roy. Soc. Med.*, 1918-19, xii, Discussion on Encephalitis Lethargica, Secs. Med. Path. Epid., pp. iv, xii, xiv and xxi.)

THE DEFENSIVE VALUE OF NORMAL MUCUS FORMATION, AND THE THEORY OF CATARRH¹

In the discharges by the bladder, the belly and the skin, if the body has departed slightly from its natural condition, the disease is slight: if much, it is great: if very much, it is mortal.

HIPPOCRATES: *Aphorisms*, vii, 18.

THE interesting note by Mr. Benians, entitled "Air-way Infections," (1) draws attention to some points of physiological, clinical, and epidemiological interest that have not hitherto been accorded sufficient importance.

Certainly, physiologists have not told us much as to the conditions of mucus formation, and, by denying it the right to be called an act of "secretion," have tended to obscure appreciation of the evident purpose which our belief in the economy of nature warrants us in recognising.

It is true that the formation of mucus is held to be an exfoliation, or degeneration, by many writers of the kind who see, in lactation, a form of suppuration—forgetting that there is no inherent distinction between physiological, degenerative, and pathological processes. One and the same process may be (1) physiological when efficient, and conditioned by accustomed stimuli; (2) pathological when tentative, and conditioned by unaccustomed stimuli, and (3) degenerative, when a confession of failure on the part of the organism to respond properly to stimuli within the

¹ *The British Medical Journal*, October 23rd, 1920, p. 627.

range of custom. What is physiological *now* has once been pathological, and may perhaps one day come to be regarded as degenerative.

The normal formation of mucus, then, must be regarded as physiological and purposive; in Samuel Butler's sense, it represents an intelligent act on the part of the organism. What is its purpose, and what are the consequences of failure in its due performance?

It seems pretty clear that a layer of mucoïd matter must, in the first place, protect the delicately vascular underlying membrane from the direct action of atmospheric changes—changes in temperature, humidity, composition, and the like—as well as from the no less direct action—mechanical or chemical—of solids and liquids in contact with it.

Mr. Benians, however, goes further, and suggests that the mucoïd layer may not only prevent the absorption by and inhibit the action on the mucous membrane of bacterial ferments and toxins, but may actually prevent invasion of the body by the organisms themselves. If his hypotheses be well founded, the maintenance of a proper state of mucus formation becomes of great importance; and interference with such a proper state constitutes an etiological factor that cannot be neglected when discussing the origins of certain forms of ill health.

The customary view is, I suppose, that interference with mucus production is a result of bacterial or protozoal attack; the notion suggested by Mr. Benians's paper is that when from one or another cause—whether vasomotor, hormonal, or nervous—mucus production is seriously

disturbed, not only may mechanical, chemical, and other agents do harm, but scope may be given to the activities of organisms which are habitual, occasional, or accidental denizens of the "mucoid layer."

There are not a few clinical facts which seem to me to support Mr. Benians's propositions.

Some years ago, when frequently dealing with cases of typhoid fever, diphtheria, and scarlet fever, I made the practical observation that oily preparations such as paraffin, applied by means of an atomiser, are far more efficacious as cleansers of the tongue, throat, and nose than is either the conventional glycerin of borax or any of the watery solutions used for swabbing and syringing. As I then said (and long clinical experience has confirmed me in the opinion) "such preparations soothe the mucous membranes, prevent breaking the surface, and inhibit the growth of organisms by encapsulating them." (2).

It seems to me that in such cases the paraffin acts by way of taking on the duties of the normal mucus when (as in the worst cases of pharyngeal infection from the point of view of absorption and general invasion) the mucous membrane is obviously "tight," shiny, and glazed.

It is precisely in these cases that old-fashioned demulcent drinks would act as Mr. Benians assures us mucilage in laboratory experiments does act, and that old-fashioned clinicians would prescribe remedies calculated to render free the "secretion" of mucus.

The chemistry of the gastro-intestinal mucus has not as yet been fully elucidated, but certain clinical facts are indisputable; amongst them this, that whilst hyperacidity

is apparently associated with deficiency of mucus, gastric ulcer is rarely met with in cases of "mucous catarrh" or "mucous dyspepsia." According to Sahli (3) gastric "mucus" is held in solution by hydrochloric acid, but is precipitated in cases of hypo-acidity. Mucus, deficiency, then, may well be a factor to be reckoned with in the production of gastric ulcer, by reason of the exposure consequently entailed to mechanical, chemical, peptic, and sometimes bacterial agents.

So far as the intestines are concerned, we have on the one hand the fact that paraffin is at present found to be the best clinical remedy when motions are hard and dry (from absence of mucus, amongst other things), and when the mucous membranes available for inspection appear desiccated rather than succulent; whilst, on the other, the grosser intestinal parasites—tape-worms, round-worms, and thread-worms—best carry on their relatively harmless lives when protected by the excess of mucus invariably present where they are. Indeed, as everyone knows, the best way to get rid of thread-worms is to give repeated doses of those preparations of rhubarb which inhibit excessive secretion of mucus, and of alkalies which loosen what is already there; whilst, until the protecting mucus in which the head is ensconced is cleared away, no dose of male fern will do its helpful work in respect of taenia. In fact, with regard to the grosser parasites, an excess of mucus seems to act just as Mr. Benians thinks normal mucus may act in respect of "carried" bacteria.

The production of mucus in the true dysenteries is pretty clearly pathological, in the sense in which the word

has just been defined, but a fact pointed out by Roger, of Paris, some time ago (4), raises some important issues relative to the pathogeny of certain bowel diseases that should not escape notice. Roger has shown by experiment that the coagulation of mucin into such large masses as those seen in certain cases of "mucous enterocolitis" may be the result of biliary insufficiency, since, *in vitro*, bile inhibits the coagulation of mucin by the normal intestinal ferment called mucinase (5).

Again, the protective effect of mucus is clearly seen in respect of the genital organs of both men and women; for the inhibition, under nervous influences, of the mucoid flow in women is well known, and very probably, like temporarily abnormal conditions of the male urethra due to strong drink, plays a part in facilitating actual invasion of the mucous membrane by organisms of various sorts.

It is, however, perhaps the state of the respiratory mucous membranes that is, in most respects, of greatest interest; and in the light of the hypothesis of the protective importance of the mucoid layer, the rationale of some old-fashioned clinical methods seems clear. I refer to the use of demulcents and of mucilaginous preparations generally, in the acute stages of a catarrh; to the administration, first of medication directed towards the loosening, and later towards the diminution, of mucoid secretion; and to the care necessary to avoid premature inhibition, by opiates, of its secretion and removal.

Certainly we may wish to understand the underlying processes that affect the secretion of mucus, and if, as is perhaps the case, the primary change, in a catarrh, is not

so much one of bacterial infection as a change in the physiological equilibrium of the mucous membrane that allows scope to bacterial activities, it seems likely that, when we do attain such understanding, we may come to appreciate how it is that, during the epidemics we call "catarrhal," there are associated cases and groups of cases, with affection of the cerebro-spinal system, not "catarrhal" in our modern sense, and yet a part of the "epidemic."

To Fernel, Benedetto, Ballonius, and other physicians of the sixteenth century these nervous forms of disease were alternative to those we now call "catarrhal"—whether respiratory or gastro-intestinal—and the epidemiological unity of what we now call encephalitis, myelitis, and the like with what we now call influenza was explained by them in terms of the famous doctrine of "catarrh," whereby an "excretion" of the central nervous system, if "repulsed," set up nervous disease, but, if determined to a mucous membrane, produced bronchitis, diarrhœa, and so forth.

To these physicians, then, a "defluxion" through the ethmoid safeguarded the brain against encephalitis; we, in thought, reverse the process and consider cerebro-spinal meningitis, encephalitis, and poliomyelitis as due to organisms reaching the nervous system by the ethmoid route, after having set up, or failed to set up, a preliminary "catarrh" of the nasopharynx. But we may well consider anew whether the factor which turns the scale to the one side or the other may not be sometimes a condition of the mucous membrane depending on vasomotor or trophic

disturbances, and sometimes an alternative condition of the meningeal and cerebro-spinal structures.

In a word: although the explanations of Fernel and Benedetto may seem to us absurd, were they not right in discerning an essential and epidemiological unity between cases that seem to us, clinically, to be totally antipathetic?

Again, Mr. Benians's notion, that it is possible that a primary alteration in the state of the mucous membranes may allow harmlessly "carried" micro-organisms to gain access to the blood and lymph channels and spaces, is perhaps to be collated with a rather curious passage, in Falconer's celebrated account of the influenza of 1803 (6), that seems to deserve citation. On the occasion of a *post-mortem* examination on a young woman who had died of influenza, upon opening the trachea it was observed that, "on viewing the inner membrane of the trachea with a magnifying glass, ulcers were clearly perceived at the edges of the small holes which supply the trachea with mucus, *to defend it from the acrimony of the air.*"

Now here again is an instance of an observation, worthy of attention, that has been rendered a little ridiculous by the assumption of a particular explanation. It may, however, be put side by side with what is said by Micheli, who in a very valuable paper (7) seeks to explain the "incontestabile unità fondamentale" of the many diverse clinical forms manifested during the present epidemic period, by suggesting that groups of different habitual or occasional microbic habitants of the tracheo-bronchial mucosa become suddenly awakened into, or

permitted virulence, by the supervention of a hypothetical *virus primitivo*.

In Mr. Benians's hypothesis micro-organisms replace Falconer's "acrimony of the air," and an alteration in the state of the mucosa replaces Micheli's *virus primitivo*.

There are obviously, therefore, several points to be considered relating to these matters which would seem not unsuitable for the application of such judicious hypothesis as may stimulate further observation and inquiry. And inquirers might be well advised to adopt as their motto the sufficiently hackneyed but nevertheless still pointed sentence of Baglivi :

"Novi veteribus, non opponendi, sed quoad fieri potest, perpetuo jungendi foedere."

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- (3) *Diagnostic Methods*, second edition, 1918, p. 475.
- (4) *La Presse Médicale*, 1913.
- (5) *Universal Medical Record*, April, 1913, pp. 297-300.
- (6) *Annals of Influenza* (Thompson), 1852, p. 258.
- (7) Sull' Influenza : *Il Policlinico*, 1920, Sez. Med., 2, p. 45.

THE MORTALITY OF PNEUMONIA¹

I would especially commend the physician who, in acute diseases, by which the bulk of mankind are cut off, conducts the treatment better than others.

HIPPOCRATES: *On regimen in acute diseases.*

IN an interesting note in the *Medical Press and Circular*, it is remarked, with truth, that the case mortality of pneumonia has risen greatly during the last two decades, and it is suggested that the influenzal type of pneumonia (of to-day) is much more malignant than was the croupous pneumonia of formerly.

Is there not, however, another possible explanation than that of "change of type"?

When, during the influenza years of 1889-96, pneumonia, however called, was very common, treatment was not what it now is: or, was what it now is not.

Then, a patient was put comfortably and suitably to bed in a woollen garment, was given an initial dose of calomel, was comforted with good poultices, and was fed moderately with broths and beef-tea, while diuretic drinks with good diaphoretic and diuretic medicines were regularly administered. Even in working-class practice, it was possible to obtain homely and sensible nursing and feeding, and, often enough, to bring cases to a happy

¹ A letter published in the *Medical Press and Circular*, January 9th, 1924, p. 29.

termination. In those days there were still, even in London, sensible women who understood their duties as wives and mothers, and who were invaluable in a sick-room.

But now the *mise-en-scène* is altered.

The consulting physician is called, on the fourth or fifth day, to a small household. He finds a feeble and uninstructed wife, whose sole idea of helpfulness is to discuss "specialists" with her relatives, and to take aspirin "to keep her going." He finds a cheery young doctor, who has "been an H.S." and who has a profound contempt for the physician and all his works, and whose mind is filled with much lore about motor-cars and "acute abdomens," and little else.

He finds a couple of nurses, perhaps "hard-faced," perhaps frivolous, but in neither case doing much except busying themselves about charts and reports.

And he finds a drowsy, toxic patient with a foul tongue, and the sands of life running out.

The cheery and surgically-minded young doctor then tells the consultant that he does not suppose "there is anything to be done," but that "the relatives wanted a second opinion." And some such dialogue as this ensues :—

Q.—Are the bowels opened ?

A.—Well, no. I gave a cascara a day or two ago, and nurse was going to give an enema presently.

Q.—Has the patient slept ?

A.—Yes, rather. He has had some morphia two or three times. No harm in that, is there ?

Q.—Poultices ?

A.—No : but I told nurse to put some Gamgee on the chest.

Q.—Food ?

A.—Nurse is seeing to that. I told her he had better stick to plenty of milk and soda, and that sort of thing. It really doesn't make much difference, does it ?

Q.—What medicine has been given ?

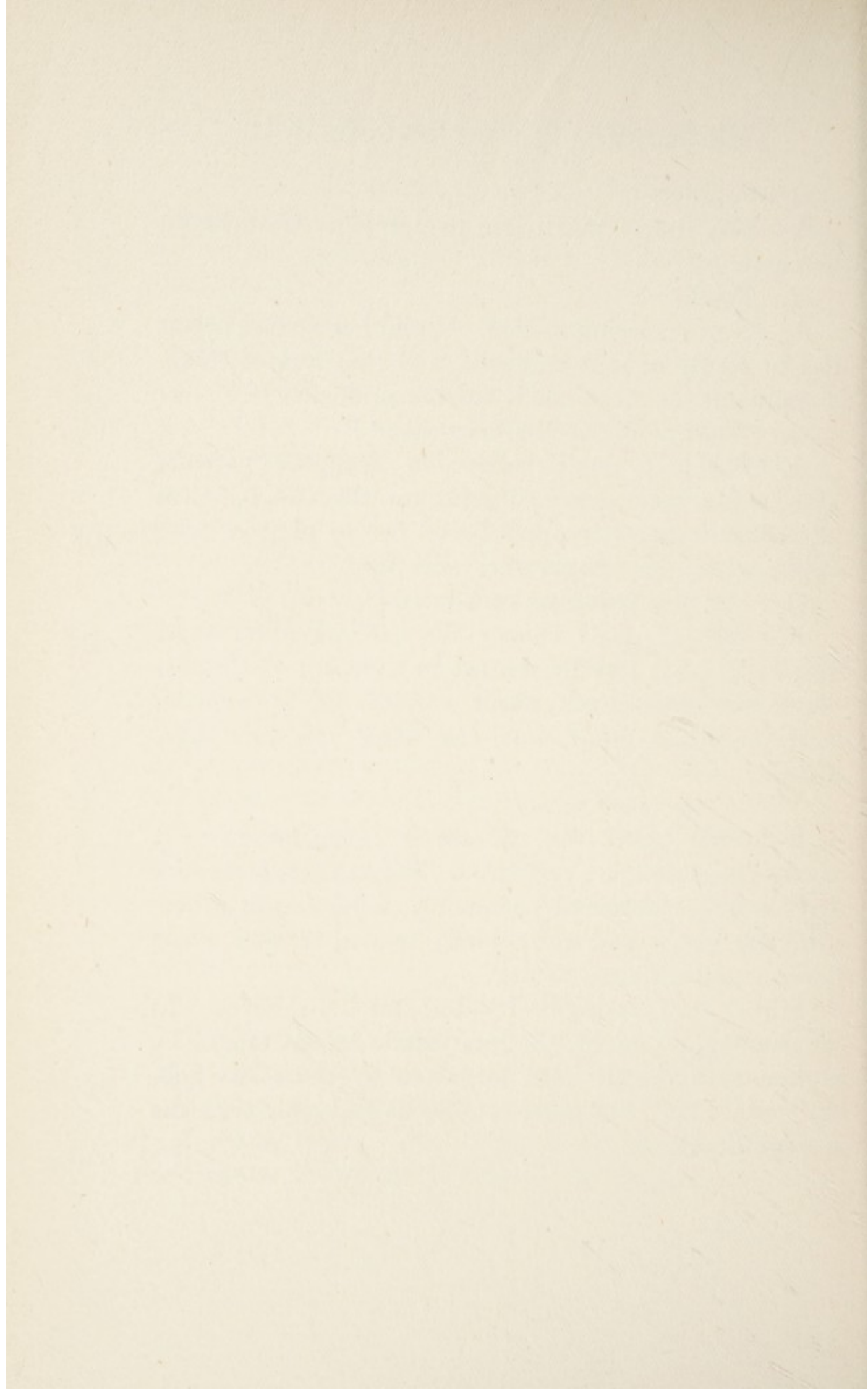
A.—Well : I don't believe in drugging patients myself. He has a heroin linctus for the cough, when nurse finds it necessary, and I told her to plug in some aspirin when the temperature goes up.

Q.—Has any sponging been tried ?

A.—Well, I don't know. We can ask nurse that. But, I say, what I really wanted to know is :—I suppose you don't think much about vaccines in pneumonia, most physicians don't, but *how about this new dope, A.B.C. ?*

I do not think this picture is exaggerated ; but having seen through a cycle from 1889 to 1924, I am sure that the average case of pneumonia, influenzal or otherwise, was better and more wisely treated on the whole thirty years ago than to-day.

Still, I am speaking of London and the suburbs. In the country, no doubt, the inestimable lessons taught by experience have not been forgotten by those less glib, less "educated," but more intelligent and *sensé* than the average Londoner.



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