

Section of Epidemiology and State Medicine.

President—Dr. E. W. GOODALL.

Sydenham as an Epidemiologist.¹

By M. GREENWOOD.

ALTHOUGH the name of Sydenham is as well known as that of any medical man recorded in history, it would be an affectation to pretend that his writings are now studied by any considerable number of those who read without intending themselves to add to the bulk of printed matter; to the generality of medical men, the "English Hippocrates" is hardly more than the shadow of a name; at most his works perform the service rendered by Virgil and Horace to the eighteenth century parliamentarian, that of furnishing more or less appropriate quotations.

That Sydenham performs even this office is evidence that he was a man of mark; quotation, even second-hand quotation, after two hundred years is a tribute seldom paid to mediocrity. Still it is opportune to inquire whether we may derive other benefits from the labours of the illustrious defunct than a choice of mottoes; whether there is a body of doctrine, first formulated by Sydenham, or bearing the impress of his personality, still capable of either guiding our researches or warning us what we should avoid. It is peculiarly opportune to initiate such a discussion now because I hope that, at the next meeting, our President will submit results well adapted to test certain of Sydenham's theories.

The object of this communication is, therefore, to make readily available such of Sydenham's ideas as are concerned with our special branch of medicine. I am precluded from a discussion of his strictly clinical work and I do not propose to embark upon a voyage into the

¹ At a meeting of the Section, held May 9, 1919.

seas of history and pure scholarship which are navigated by our colleagues in another Section. Even thus limited, the object of the paper is more difficult of attainment than might have been anticipated; it is, indeed, hard to come to close quarters with the mind of Sydenham.

Sydenham is parted from us not merely by the gulf of two centuries, but one personality must and—unless we choose to make use of an instrument which is no longer an obligatory item in the educational kit of the student—two personalities may intervene between us and him. The whole of Sydenham's published works are in Latin, but the evidence was strong enough to convince a distinguished former member of this Section, Payne, and is probably sufficient to convince most of us that the Latin is not Sydenham's. When Latham executed the Sydenham Society's translation of the works, this opinion touching the authorship of the Latin version was not universally accepted, but there was enough in its favour to leave doubts in the mind of Latham which may have led him to take more liberties with the text than might have been thought respectful in the case of, say, Celsus.

It would ill become one whose Latin verse exercises at school sounded depths of infamy which are painful to recall, to venture a judgment upon nice points of classical scholarship. That Latham's book frequently seems to avoid complexities of construction in which I have entangled myself is no proof that Latham was not a faithful translator; a great scholar once said that if he failed to comprehend a passage he translated it literally. We all remember, too, how Scaliger reconstructed a lost Greek original from a Latin paraphrase; so that a free English version might be more faithful to the thought of Sydenham than the Latin text itself or its literal translation. But it is always possible that Latham was not a Scaliger; this possibility and a natural obstinacy have induced me to struggle with the Latin text, but I think very seriously over any interpretation of my own which differs substantially from that of Latham.

Now when we grapple with the Latin text we are not, or, to speak more modestly, I am not, so much delighted with its elegance as sundry authorities assured us would be the case. There are two ways of writing Latin if one does not happen to be a Roman. One is to treat it as a living language and not to boggle at expressions which would have given serious annoyance to Cicero or even to Apuleius; this was the method of Erasmus, of Bacon and indeed of most scientific men writing between the Renaissance and the nineteenth century; it is, or was, the custom of the Pathology Section of this Society.

The second method is to play the sedulous ape to the classical stylists and forms a branch of literary art of which one of the most remarkable exponents, in prose, was Marc Antoine Muret. The practitioners of this art were not men of science. The translator of Sydenham's works was a literary artist whose model was Cicero and it is certainly easier to reproduce the prolixity of Tully than any other features of his style.

One can certainly choose passages from Sydenham, such as the eulogy of Hippocrates in the preface to the Medical Observations, which are really impressive, but the general effect of the long and involved sentences is wearisome and calls to mind Macaulay's gibe at the Man of Arpinum himself—viz., that a parallel to many of his speeches would be afforded by a barrister prosecuting a rioter who remarked that the occasion was a good one for instructing the younger auditors in the public gallery in the true meaning of the Bill of Rights.

But there is a further barrier against sympathetic understanding, that of temperament. A hasty reading of Sydenham would lead one to suppose that a large proportion of the inhabitants of London who died between 1660 and 1675 were the victims of our author's incompetent colleagues, and one is surprised that our forefathers left a window of the College of Physicians unbroken, but not at all surprised that Sydenham never presided over that illustrious corporation. After a few score pages, this portrayal of the one just man in a generation of charlatans is nearly as annoying as in the leading case of Aristides, and it is quite impossible not to notice with malicious satisfaction that the one just man who derided subtle speculations and traditional doctrines himself often adopted as axioms some of the most conspicuously feeble relics of the Galenical tradition, and, as a pathologist, differed very little from Dr. Caius or Chaucer's Doctor of Physik.

I mention these obstacles because they must be surmounted by anyone who desires to become acquainted with the thought of Sydenham, and it is no real service to the cause of epidemiology to pretend that its classics are more inviting than they really are. A medical Gilbert Murray might attract a few more readers to the works of Sydenham, but the Medical Observations will never secure admission, as did Harvey's treatise, to Everyman's Library. Sydenham's general doctrine of epidemics is contained in a small compass—viz., in the two concluding sections of the First Chapter of Book I of the Medical Observations and in the following chapter. It will be advantageous to give here a reasonably strict translation of the operative passages:—

“As to the acute diseases (which I now purpose to discuss), some are generated by a secret and inexplicable alteration of the air infecting the bodies of mankind and are only conditioned by a peculiar crisis of the blood and humours in so far as the occult atmospheric influence shall have impressed the crisis upon the said bodies. Such diseases only ravage while the hidden constitution of the atmosphere endures and appear at no other time; they are termed epidemics.

“Other acute diseases being conditioned by some individual anomaly and not the resultants of general causes do not simultaneously assail many persons. Acute diseases of this kind, with certain exceptions to be considered when we discuss this genus, are independent of years and seasons. Such acute diseases I term Intercurrents or Sporadics; because they may accompany any Epidemics. I shall deal first with the Epidemics, giving pride of place to their general history.

“Nothing, I suppose, has more astonished the student of medicine than the protean character of epidemic diseases, not so much as referable to differences of weather as to differences of epidemic constitution in different years upon which they depend.

“This very evident diversity of the diseases in question is seen both in respect of symptomatology and of necessary treatment. From which it plainly appears that diseases which to the inattentive observer may seem congruous both in respect of external features and symptoms will be found by a judicious scrutiny to differ as chalk from cheese. I do not indeed know whether a sedulous examination (for properly carrying this out the brief space of man's life were hardly sufficient) might not teach us that certain Epidemics succeed one another in a series, forming as it were a circle, or alternatively that owing to an occult diathesis of the atmosphere and a mysterious succession they attack us indiscriminately. This only, fortified by a multitude of exact observations, I do confidently hold, that the aforesaid species of disease, in particular the continued fevers, may vary so enormously that you may kill your patient at the end of the year by the method which cured sufferers at the beginning of it; and so when by good luck I have hit upon the proper treatment of a fever of this kind, I can, under God's providence, nearly always reach my end by aiming at the same goal, respect being had to the age and temperament of the patient and such like matters; until that particular species becoming extinct and another emerging, I am again puzzled how to help my patients; and it is only by dint of the greatest caution and using all my wits that I can avoid, indeed, I cannot always avoid, risking the lives of one or two of my clients, until continuous observation leads at length to comprehension and I again steadily and intrepidly advance to conquest.

“Now although I have attended as diligently as possible to the more apparent diversities of atmospheric conditions in different years with the object of reaching an explanation of the vicissitudes of epidemics, I am fain to confess that I have made no progress at all; I very clearly perceive that years perfectly agreeing in their obvious meteorological characters may be utterly dis-

parate in the matter of diseases and conversely. This is the state of the case. There are different constitutions of years due to some hidden inexplicable change in the bowels of the earth when the air is contaminated by such effluvia as predispose and determine the bodies of men towards some disease or other; heat or cold, dryness or moisture, are not the causes; this state of affairs endures so long as the particular constitution is dominant and then yields its place to another. Each of these constitutions is characterized by a particular kind of fever, not seen under other circumstances, and fevers of this class I term *Stationaries*.

“In addition there are, if I may use the term, particular *crases* of the same year in which, owing to obvious meteorological factors, fevers following the general constitution of the year are more or less epidemical or arise earlier or later. Above all, however, such fevers as occur indifferently in all years (and I therefore term *Intercurrents*) trace their origin to some obvious character of the atmosphere; such are pleurisy, angina and similar diseases, which prevail when warmth suddenly replaces long and intense cold. Perhaps the sensible qualities of the atmosphere may operate in the production of such fevers as appear in any constitution, although this they cannot do for fevers special to any one constitution; but I must admit that the aforesaid atmospheric characters do more or less predispose our bodies to the generation of this or that Epidemic, which may also be said of an error in the six Non-Naturals.”¹

The above is a full translation of the first six sections, which are fundamental; it will be enough to summarize the remainder.

Some epidemics are perfectly uniform in their evolution and these ought to form the basis of an epidemiological history; others are variable from constitution to constitution, while, worse still, the same disease may vary within the same constitution (sections 7 to 9).

All epidemics fall into two groups, the vernal and the autumnal, but this division is to be taken broadly for “atmospheric conditions may play into the hands of some epidemic helping it to ravage prematurely, while on the other hand a want of correspondence may lead to the epidemic getting to work on the predisposed subject only late” (section 10).

Vernal epidemics ending by midsummer are Measles and the spring Tertians; Small-pox and Plague begin and end later. Cholera morbus is autumnal and beginning in August is over within a month, dysentery, quartans and autumnal tertians last longer. “As to fevers specially, they have always been named from symptoms. But since nearly every constitution, over and above the fevers it breeds, is prone to set going some one or more of the famous epidemics, such as plague, small-pox, dysentery, &c., I do not see why these fevers should not rather take

¹ Air, Meat and Drink, Motion and Rest, Sleep and Wakefulness, Mental Emotions; the *Secreta* and *Retenta*.—Greenhill’s note.

their names from the constitution, since this favours the production of some one of these diseases at the time of their emergence,¹ than from any particular alteration of the blood or symptom which may be found equally well in specifically distinct fevers" (section 13).

In the next section, it is pointed out that the form of intermittents may be assimilated to that of continued fevers, and that the true state of the case is only revealed as the influence of the prevailing constitution wanes.

The fifteenth and sixteenth sections I give in full:—

"This is to be carefully noticed, that when several of these diseases infest the same year, one of them gets the mastery, as it increases the rest decrease, when it wanes they once more wax, and so they plague humanity in turns as the genius of the year and the sensible qualities of the atmosphere give one or other assistance. But it will be found that whatever disease rages most furiously and causes most havoc round about the autumnal equinox, gives its name to the constitution of the whole year, whatever disease takes the lead then will be found to have the mastery of the others the whole year through, assimilating the qualities of contemporaneous epidemics to its own so far as their nature will allow. For instance, when small-pox is prevalent, the fever appearing sporadically during the whole year partakes of that inflammatory type which begets variolæ. Each disease originates in a similar way, there is a great affinity between the characteristic symptoms of each (excepting in the variolous eruption and its accessories) as plainly appears in the tendency of both towards spontaneous diaphoresis and towards salivation."

I omit the second illustration, that of dysentery.

The eighteenth section is as follows:—

"Lastly, I must observe that when any constitution generates various species of epidemics these are essentially different from those bearing the same name but produced by another constitution. However, many distinct species may occur in one and the same constitution, they have in common one general factor, that derivative from the peculiar diathesis of the atmosphere; hence however distinct they may be in type and specific form, the constitution common to all so moulds the substance of each that the principal symptoms, other than those referable to the particular type of evacuation, are alike in all; the several maladies in this too, agreeing that they all at nearly the same time increase and remit their severity. It is further to be remarked that in the years in which these various species prevail at the same time, they all agree in the manner of onset and initial symptoms."

¹ The words are "Quatenus horum morborum alterutri producendo favet eodem illo tempore quo comparebant." Latham translates: "That being determined by the particular disease which they usher in." I suggest that the version in the text conveys better what I take to be the thought—viz., not that the fever determines the constitution but the constitution the fever.

I do not think I have omitted any important statement of Sydenham's general doctrine, and now attempt to ascertain its meaning. There is no difficulty about the theory of intercurrents; it is that these diseases (amongst which he included scarlet fever) are independent of whatever influences generate an epidemic constitution. But it is not—to me at least—clear what the relation is between an “epidemic” and a “stationary fever.” Are we to understand that *the* characteristic product of an epidemic constitution is its stationary fever, that it is in the type of stationary fever that one constitution differs from another? If so, is the stationary fever an independent phenomenon or an epiphenomenon of the “epidemic”? Is the “epidemic” determined by laws other than those describing the genesis of the “constitution”? It seems to me that Sydenham was not consistent, that he sometimes regards the stationary fever as an epiphenomenon of all “epidemics” occurring during a constitution, sometimes as a separate entity.

Take, for instance, the epidemic constitution of 1665-66. Here we have plague proper, and in addition a pestilential fever, which fever is held to be at bottom a pleurisy upon which the pestilential character has been grafted by, presumably, the constitution. Now we may suppose that this suggested the question whether plague proper were not in the same case—viz., a special graft (i.e., the stationary fever of the constitution) upon some intercurrent stock—and that if so the pestilential fever had just as good a right to be called plague as *the* plague. Anyhow, Sydenham *does* ask this question, and contents himself with the shy answer—“Febri autem illi, de qua modo loquebar, an *pestis* appellatio attribui mereatur, non ausim definito pronuntiare.” So far as appears, we can suppose that the doctrine of constitutions is expressible in the symbolic form, thus: during any epoch the manifestations of acute diseases are the resultants of two components ($a + C$), ($b + C$), ($c + C$), &c., a , b , c , &c., denoting the individual (intercurrent) bases, and C the common addition (stationary) contributed by the prevailing constitution. But this formula will not cover such a description as that of the two variolous constitutions, the regular of 1667 *et seq.*, the irregular of 1674. As regards the former period, we are told that a continued fever prevailed which, being engendered by the variolous constitution was identical with small-pox save in the matter of the exanthem, and any signs and symptoms essentially dependent upon the exanthem. There is no suggestion that this variolous fever was an intercurrent (like pleurisy) upon which a constitutional stationary was grafted, but rather that it and small-pox were sisters, both daughters of

the constitution. Hence the representation would be that only one component is involved in the manifestation of acute diseases, which are $a, b, c, \&c.$, under one constitution, $a', b', c', \&c.$, under another. But then for the years 1670-72, we are told of an anomalous small-pox due to a constitution tending to the hotter and more inflammatory type, which constitution also engendered bilious colic; this time the account is more consistent with the two component interpretations.

I do not profess to have a settled opinion, but it seems to me that on the whole the more probable view is that Sydenham inclined to the first-mentioned interpretation of the doctrine of constitutions—viz., that of superposition upon distinct types of a common external form or group of allied forms. In this way, we can understand his separate characterization of the secular evolution of diseases, so that the complete epidemiological theory of Sydenham is, as I have elsewhere suggested,¹ to be expressed in the following terms: “(1) There is a process of secular or long period modification in virtue of which a specific type becomes dominant in a particular epoch and then gradually or suddenly gives place to a rival. (2) There is another set of factors producing short period oscillations in the epidemicity of a given disease leading to the phenomena of seasonal prevalence. (3) We can connect these two trains of ideas by the conception of an epidemic constitution in virtue of which certain types of epidemic or certain features of morbidity tend to prevail at a given time to the exclusion of other types or symptoms. We may say that the amplitude of the first kind of wave is measured in centuries, that of the second in months, and that of the last in years.”

We are now concerned merely with the doctrine of an epidemic constitution, so that I may expand my interpretation of it a little in order to give scope for criticism.

The complete morbid process of an epidemic disease is made up of two parts; the first specific (the $a, b, c, \&c.$, of the formula), subject to secular modification and also to short period oscillations—i.e., it is a doubly periodic function of the time. The second part is generic, common to all species of epidemic diseases and a function of some terrestrial conditions included under the term “Epidemic Constitution.” Consequently, the total effect upon the community at any instant of small-pox is the resultant of two terms $f(t) + A.F(t)$, the contemporaneous effect of scarlet fever is $f'(t) + B.F(t)$, and similarly for all other diseases. The actual effect of the second member of each

¹ Seventeenth International Congress of Medicine, Lond., 1913, Sect. XVIII, Discussion No. 3.

expression is different (A and B are supposed to be constants), but for each disease one and the same functional term $F(t)$, is included; this is the product of the epidemic constitution.

I am alive to the dangers of anachronism in rationalizing (perhaps in the unfavourable Freudian sense of that word) an historical doctrine. Perhaps, what I have just written would have been repudiated by Sydenham with as much energy as the barons of Runnymede might have shown in rejecting the interpretation of the Great Charter that we learned at school. Indeed, one can quote numerous passages which are consistent with the belief that Sydenham did not realize the existence of any morbid specificity, in fact, regarded all acute diseases as variants of a fundamental inflammatory process (his *pathology* of inflammation was traditional in the worst sense, the mere journalese of Galenism). Still, the interpretation I have put upon the doctrine of an epidemic constitution at least divests it of the mysticism which became a cloak in the eighteenth century, and was not wholly in tatters at Montpellier in more recent times. The theory as stated may be false, but is neither trivial nor incomprehensible. Let us then discuss it.

So far as certain diseases are concerned, nobody doubts that there have been changes of clinical type, scarlet fever is the classical instance. This fact, however, makes neither for nor against the theory of an epidemic constitution. If, in my nomenclature, a is the specific element of scarlet fever and C the constitutional epiphenomenon, the type of scarlet fever at epochs 1 and 2 might differ, because at 1 we have $a + C_1$ and at 2, $a + C_2$ agreeably to the doctrine of constitutions; but we should also have a disparity if a itself varied, without invoking a C at all. The only crucial test would be to find C attached to a , b , c , &c., during one epoch, and C' attached to the same a , b , c , &c., during another epoch. In other words, we should have specifically distinct maladies converging towards common clinical forms, the point of convergence varying with time. Not much light is shed on this by modern mortality or morbidity statistics, for the trend of official classification is towards primary divisions; we shall not discover from them if pulmonary complications of typhoid are more frequent in one year than another. A judgment on this point can only be pronounced by those whose clinical experience of zymotics extends over many years and whose memories or notes are trustworthy.

It is to be regretted that the eighteenth century annalists, like Huxham, have had few successors. Painful though the confession must be to a statistician, I do confess that tabular matter is not a perfect substitute for faithful annals.

The most plausible evidence in support of the theory of epidemic constitutions is that provided by the bacteriologists, who are not usually supposed to entertain much reverence for Sydenham. In the late epidemics of influenza a clinical convergence towards a grave type of pneumonia has been uniform, but the primary bacteriological findings have been multiform. If we are to define primary disease in bacteriological terms, there have been whole alphabets of *a*'s, *b*'s, *c*'s, &c., but a common C has been involved, the product of what Sydenham might have termed the peripneumonic constitution of the years 1918-19. Perhaps he would even have sketched a phrenetic constitution for some areas in 1915-16, and have similarly interpreted the simultaneous and successive occurrence of typhoid, paratyphoid A and paratyphoid B, admitting a specific factor of each (the *vera causa* of the bacteriologist) but asserting that this was complemented by a constitutional element.

I am not equally sure that our distinguished colleague Dr. Hamer is a whole-hearted supporter of Sydenham. Dr. Hamer, I think, would at any rate reject my formula, and not agree that the gross epidemiological phenomenon which we call "influenza" is $a + C$, $b + C$, $d + C$, &c., the complex termed "epidemic cerebro-spinal meningitis," $a + C'$, $b + C'$, $c + C'$, &c., the small letters being specific factors, the large letters generic or epidemic constitutional factors. He seems inclined to postulate a single mutating *vera causa*, and this does not appear to me to be the doctrine of Sydenham, although, as stated above, I recognize the essential ambiguity of much Sydenham advanced.

Returning to my attempted rationalization of Sydenham's doctrine, I suggest that it is really a very important one. If it is true that some common factor C is the divinity which shapes the ends rough hewn by different specific factors, the *veræ causæ* of the bacteriologists, we perceive that a very large part of the campaign against epidemic diseases must be directed against the general factor; because, by hypothesis, the tracking down and elimination (when practicable, as by specific immunization) of *a*, *b*, *c*, will still leave it open to the general factor C to complement *c*'s, *d*'s, and *e*'s, whole alphabets of small letters, yet undeciphered.

Of course if we adopt Sydenham's own theory of the epidemic constitutional factor C, this is a mere counsel of despair—much as was that form of the doctrine which presented itself to the mind of Watt in 1814 as the principle of substitution. But it no longer seems that we should

regard the basis of an epidemic constitution as beyond the compass of human intellect.

We have naturally very little patience in this Section with the appeal to some strange god high Sanitation popularly identified with well-flushed water-closets, and invariably invoked by the opponents of serum therapy and experimental medicine. But we are, perhaps, a little too complaisant towards the advocates of a millennium attained by specific conquests of the alphabets of small letters.

Professor Gay, a distinguished American bacteriologist, in his recent monograph on "Typhoid Fever," begins his discussion by remarking that "it will, we believe, be evident that all significant information concerning the nature of the disease itself and its method of dissemination, as well as all effective means that have been devised to prevent and cure it, have depended on laboratory data and are based on the recognition of the single bacterial causative factor."¹

But within a few pages, our bacteriological colleague is to be found remarking that "all subsequent information, extended and elaborate as it is, has not supplanted the explanation of Budd (respecting the transmission of typhoid), and has for practical purposes added little to it."² Yet Budd lived and worked some time ago. The moral is that a general consideration of the facts of human life, the slow changes of normal social evolution, the drastic changes enforced by recent events may be of as much importance as an intense scrutiny of the specific *veræ causæ*, the *a, b, c* of the bacteriologist.

In making these remarks, I am, I fear, digressing beyond the limits marked out for my contribution to the discussion. I do not wish to assume the rôle, rather a foolish one, of an advocate for seeking salvation by a return to the standpoint of the seventeenth century. But I am concerned to show that the teaching of Sydenham, greatly over-praised by a few, ignored by most, contains some ideas which, true or false, may still usefully be discussed.

¹ "Typhoid Fever," by F. P. Gay, New York, 1918, p. 10.

² *Ibid.*, p. 42.

DISCUSSION.

The PRESIDENT: Sydenham is rightly looked upon as the founder of the modern science of epidemiology. It is doubtful, however, whether he had more than an inkling of what his work was destined to be the forerunner. In our time the epidemiologist stretches his view far beyond the "history of epidemic diseases." If we take those words in the sense in which Sydenham uses them—viz., a description of these diseases, it will have nothing to do with their cure, so far as the treatment of individual cases is concerned. The only cure he thinks of is the preventive, and I doubt whether the epidemiologist, pure and simple, cares even about that. His history of epidemics is not the history of the cases of which epidemics are made up, but of the epidemics themselves, from one season to another, from one year to another, from one age to another. And as he finds that in different seasons, years and ages, there are epidemics of different diseases and that the same epidemic diseases behave differently, so he tries to find out the causes of these differences, that is to say he is led on to study their ætiology, a thing with which Sydenham would have nothing to do, because of its difficulty. But Sydenham is the founder of our modern science only by accident. What was the object of his work? He has answered this question himself very plainly. The first edition of the treatise which deals with the subject with which we are now concerned was published in 1666 under the title of "*Methodus curandi febris.*" In the preface to that edition he tells us that that method is founded upon his own observations, and that in the belief that his method would be beneficial to his fellow sufferers, he makes it common property. Be it noted, however, that his observations were made solely with the object of establishing a mode of cure. By the time the third edition appeared in 1676 the work and its title had both expanded, the latter to "*Observationes medicæ circa morborum acutorum historiam et curationem.*" A history of these diseases is now added to an account of the method of curing them. In the dedicatory epistle to this edition, addressed to Dr. John Mapletoft, Sydenham writes as follows—(I quote from Latham's translation of Greenhill's edition, and shall use this translation throughout)—"The more I observed the facts of this science [medicine] with an attentive eye, and the more I studied them with due and proper diligence, the more I became confirmed in the opinion which I have held up to the present time, viz., that the art of medicine was to be properly learned only from its practice and its exercise; and that, in all probability, he would be the best skilled in the detection of the true and genuine indications of treatment who had the most diligently and the most accurately attended to the natural phenomena of disease. . . . I directed myself to the close observation of fevers and . . . at length hit upon a mode of curing them." He then goes on to say that since publishing the first edition he had observed several new forms of fever and that his experience had been

much extended. Lastly, in section I, chap. ii, par. 3, of the "Medical Observations," he distinctly states that his close investigation of epidemics was undertaken solely for the purpose of finding out the proper method of curing individual cases.

Sydenham's observation was made chiefly upon the epidemics of the fifteen years 1661 to 1676. As a result of his studies he noticed several facts of a strictly epidemiological nature. First, he noticed the seasonal variations of diseases (I, ii, 2); secondly, he was aware that the characters of epidemics of the same disease varied in different years (II, ii, 2); thirdly, he recognized that infection might be spread not only by infected persons but by infected things, *fomites* (II, ii, 4); fourthly, he observed that after an epidemic of any disease, cases of that disease might still continue to crop up sporadically; fifthly, he mentions the prevalence of flies in connexion with certain abdominal diseases; sixthly, he pointed out that epidemics came to an end not because the food upon which they fed was exhausted, but for some other reason, an atmospheric condition; and lastly, he noticed that if cases of a given epidemic disease began to appear at an earlier time than was to be expected from an experience of former epidemics of that disease, then the epidemic was likely to be an extensive and severe one. But besides these epidemiological facts, Sydenham gives us descriptions of the epidemic diseases prevalent in his time. From a careful perusal of his writings I have no doubt that he saw small-pox, chicken-pox, measles, scarlet-fever, plague, typhus, relapsing fever, enteric fever, dysentery, autumnal diarrhoea, ague, influenza, cerebro-spinal fever, and epidemic encephalitis. His descriptions of some of these diseases is absolutely clear, especially of small-pox and plague. Indeed I doubt whether any writer of modern times has added a single important observation to his admirable and detailed description of natural small-pox. But of many of the others his accounts are vague. It is greatly to be pitied that he deliberately refrained from swelling out his pages with histories of particular cases and confined himself to general expression because he feared that repetition would be vain and wearisome. And here I may add that Sydenham's habit of thought led him to differentiate between diseases—to separate out new diseases. Of late years there have been some among us whose idea seems to have been to merge what most of us believe to be distinct affections into one and the same disease. To such I would recommend Sydenham's observation that although these epidemic disorders "may to a certain degree, both in their external characters and in several symptoms common to many of them, appear to the careless observer to coincide, they are, in reality, if we attend closely, of wholly different characters, as little like one another as coins and counters."

But it is by his doctrine of epidemic constitutions that Sydenham is most remembered, at any rate by the epidemiologist. I suppose that there can be little doubt that he derived the idea from Hippocrates. Yet there is a very great difference between the ideas on this subject as put forward by these two great physicians. So far as I understand them, there is nothing mysterious about the constitutions of Hippocrates. They were frankly determined by

weather conditions. With Sydenham, however, so far as an epidemic constitution is concerned, the weather plays quite a secondary part. It is by no means clear, however, exactly what Sydenham meant by the phrase, "An epidemic constitution," for there is more than one epidemic constitution; Sydenham found five in fifteen consecutive years, and saw no reason why there should not be more—an epidemic constitution is a constitution of the atmosphere. It is not the same, however, as the manifest qualities of the atmosphere, wind, rain, &c. There is something mysterious, inscrutable, occult, obscure, peculiar, particular, and unknown about it. It is due to or connected with mysterious changes in and vapour from the bowels of the earth on the one hand or, on the other, skiey influences, such as the conjunctions of the heavenly bodies. But these mysterious unknowns, looming in the background, were the cause, each at a different time, of various epidemic diseases; yet by no means of all epidemic diseases. The five constitutions to which I have alluded above were those of the intermittent fevers, plague, small-pox, dysentery, and the comatose fever. In subsequent writings Sydenham added the constitution of the depuratory fever and the new continued fever. Now these were the most striking and prevalent epidemic diseases of his time, and according to him they were caused each by a special epidemic constitution of the atmosphere. The diseases which were thus caused were termed by Sydenham *stationary fevers*; other epidemic diseases, due to other and usually (at any rate in Sydenham's opinion) more or less obvious causes, such as the manifest, sensible and appreciable qualities of the air, wind, rain, heat and cold, &c., were called *intercurrent* or *sporadic*. Scarlet fever (and what Sydenham describes as scarlet fever was, in my opinion, that disease and not rubella)—scarlet fever was one of them. But then it was not an affection which bulked largely in the epidemics of that period. If it had, doubtless we should have heard of a scarlatinal constitution. While an epidemic constitution was the direct cause only of the stationary fever, it could and did influence the intercurrents. That was shown by the fact that there was a certain set of symptoms, such as natural sweating and salivation, which were common to all the concurrent diseases. In fact, Sydenham's hypothesis on this subject led him to differentiate certain fevers which, though they were not stationary fevers, yet bore an impress (without, however, the specific marks) of certain stationary fevers prevalent at the time. I refer to the variolous, the dysenteric, the pestilential, and the morbillous fevers. Personally I am of the opinion that in his desire to discover something new and in his belief in the influence of the epidemic constitution, he was led astray, and described unexistent "fourth" diseases. While the stationary fevers are due each to its own particular epidemic constitution of the atmosphere, to such an extent that without the constitution you cannot have the disease as an epidemic, yet these fevers are influenced also by the manifest qualities of the air; hence seasonal prevalences and variations in character of epidemics. Further, the manifest qualities of the air may exert some influence on the epidemic constitution, to the extent of admitting or excluding a stationary fever. Such influence is, however, only temporary.

I have stated above that Sydenham writes of the epidemic constitution of the atmosphere as being of a most mysterious nature. He does so frequently. Yet I am led to believe that at the back of his mind there was an almost materialistic idea of it. In one passage he writes of "the *particles* which are mixed with the atmosphere, which war against health and which determine the epidemic constitution." And there is one curious passage as follows: "We must consider, not that any particular diathesis is to be assumed from the atmosphere itself (*nulla ejusmodi diathesis in ipso aëre supponi debere*), by which, whilst we have one epidemic propagated in one place, we may have another, wholly different, elsewhere and at no great distance—if such were the case every movement of wind would (as it sometimes does) invariably diffuse a constitution—but that each particular tract of the atmosphere is filled with the effluvia of some mineral fermentation, that these contaminate the air through which they pass by their particles, that these particles are differently destructive to different animals, and that they propagate diseases appropriate to the different affections of the soil, until the whole mine of such subterranean vapours be exhausted."¹ The interesting admission here to my mind is the one that an epidemic constitution can be shifted by the wind.

Sydenham *appears* to be very inconsistent at times. He frequently tells us that the epidemic constitution is the cause of certain epidemic diseases, and leads his reader to consider that it is the sole cause. What, then, are we to think when we come across the following passage: "Much as these two forms [i.e., the stationary and the intercurrent diseases] may differ from one another in respect to their origin in atmospheric influences, they agree in respect to several of their external and predisposing causes. Laying contagion out of the account, which occasionally gives origin to the stationary form of fever, and laying also out of the account intemperance, which is the mother of both forms, the commonest external and evident cause of most fevers is either premature change of dress or exposure to cold after exercise." In another passage Sydenham writes of the "coughs helping the constitution in producing the fever." It is clear from these and other passages which could be quoted that the epidemic constitution was not the *sole* cause of an epidemic disease. There were exciting and adjuvant causes. There can be no doubt, indeed, that he had misgivings as to the efficiency of the epidemic constitution as a cause of epidemics. Plague was a disease which greatly puzzled him from this point of view. He expresses grave suspicion that the mere atmospheric constitution, however much *λοιμώδης*, was by no means sufficient, in and of itself, to originate plague. "Either the disease itself must continue to survive in some secret quarter, or else either from some *fomes* or from the introduction from pestilential localities of an infected person, it must have become extruded." But he goes on to invoke the favourable atmospheric diathesis to explain the outbreak of an epidemic. Now it is clear from his explanation of the result of the action of the Grand Duke Ferdinand II, when the Duke effectually stopped the plague from invading Tuscany by cutting off its communication with the

¹ *Observ. Med.*, V, iv, 3.

surrounding districts, that in Sydenham's opinion even epidemic constitution could be very local. It seems to me that Sydenham is not very clear on the question of causes very largely because he is not really interested in investigating them. Not for him "Felix qui potuit rerum cognoscere causas." He defends himself from blame for not having attempted to pierce the *penetralia*, and writes "Ætiology is a difficult and perhaps an inexplicable affair, and I chose to keep my hands clear of it." Moreover, according to him, we need not be troubled by ignorance of causes when we are seeking for cures of diseases; for it is experience and not knowledge of the cause that will prove the right guide.

But in spite of these seeming contradictions, I think we can form a fairly correct idea of what Sydenham really had in his mind in respect of the causation of epidemic diseases. While there were certain obvious causes such as the various states of the weather, contagion, intemperance, ill-advised changes of raiment, and the like, which could give rise to isolated cases, they were not sufficient to account at any rate for any but the less frequent and less severe forms of epidemic (intercurrents). In the causation of the more important epidemics some other factor was necessary. He was quite ignorant what it was. But he was fairly sure that its presence was necessary, for he states that you may have sporadic cases of plague, for instance, lingering perhaps from a previous epidemic, kept alive by some smouldering fumes, but there will be no epidemic in the absence of the pestilential constitution of the atmosphere. At the same time it was not necessarily the sole cause; it was a very important causative factor, overshadowing, but after some time capable of being influenced to a slight degree by the lesser, though more obvious, factors. This unknown factor waxed and waned, like the epidemic it produced: the waxing and waning of the epidemic was indeed the visible sign of the mutability of the constitution. No two or more constitutions could exist together. Certain characteristic symptoms were the manifest evidence of any particular constitution; some of these were confined to the particular stationary fever which was engendered by the particular constitution; others would be found not only in the particular stationary fever but also in the intercurrent diseases prevalent at the time. In answer to Mr. Greenwood's questions I should reply that the characteristic product of an epidemic constitution is its stationary fever, and that it is the type of stationary fever that one constitution differs from another; that the stationary fever is a phenomenon; and that while the epidemic is determined mainly by the laws which govern the genesis of the "constitution," it is also subject to other laws, as also is the constitution.

How far have we advanced in our conceptions of the causation of epidemics since Sydenham's day? Not very far, I fear, as regards what he called the epidemic constitution. I am of the opinion that we must still admit that there is a very important factor, or there are very important factors, still unknown, in the causation of epidemics. My conception of the causation of epidemics is that there are several causes at work, varying in number and

importance for different epidemics and at different times; that an epidemic is the sum of several factors. Of recent years factors unknown to Sydenham have been brought to light. We know more about the influence of the ages of the persons exposed to attack, of their surroundings, of the seasons, of the part played by insects and animals, and so forth. We also have added to our stock of knowledge the whole of the bacteriological evidence. I am not sure, indeed, that there are not some bacteriologists who would not claim that Sydenham's epidemic constitution has been explained by the germ theory of disease; that the unknown factor he recognized was the micro-organism. I am sure, however, that no epidemiologist will admit that claim. Admitting a micro-organism as a factor, and a very important factor, in the causation of disease, we still are driven in most instances to explaining the causation of the epidemicity of the micro-organism; and in most instances, if not in all, we are very far from having attained that object. Sydenham recognized a few obvious causes of epidemics and epidemic diseases, more especially of the latter. But he was also well aware that other causes, which he believed to be the most important, were still unknown to him, and especially those connected with the more important epidemics. To speak more correctly, all we have done has been to reduce the amount of the contents of this large magazine of unknown factors by withdrawing from it certain factors which we have been able to name, and transferring them to the store of known factors.

Dr. CHARLES SINGER: As a professed historian I have listened with much interest to Mr. Greenwood's paper because, as it appears to me, he has been doing what we very seldom see done, and he has been using history for its proper purpose. For the last half century it has been generally recognized that ideas, like other organic products, cannot be fully understood until their history is known. Ideas, like species of animals, have their history, and betray their history in their structure. It should be the function of the medical historian to trace that history as a continuous whole, and so to play his part in the illumination of medical ideas. Now the doctrine of epidemics, like other ideas, has had its history; and a history, moreover, for the writing of which we are provided with unusually abundant material. The history of the doctrine of epidemics may be summed up in one sentence, as a struggle between the ideas of *miasma* and *contagion*. Among early peoples all evil is contagious: misfortune, ritual, uncleanness, wickedness, and, of course, disease. The point might be illustrated from a thousand passages, and is to be found in the Bible. It is a test of how far Hippocrates had left primæval superstition behind, that he rejects the whole doctrine of contagion and is a believer only in *miasma*. The struggle of the doctrines of *miasma* and *contagion* can be traced through the ages, the doctrine of contagion gradually coming more and more to the fore until about the middle of the sixteenth century. It was three years after modern science had made its stately entry with the work of Copernicus and of Vesalius, the natural historians respectively of the macrocosm and the microcosm, that—in the year 1546—a work of the highest

epidemiological importance appeared; the "De Contagionibus" of Jerome Fracastor. In this work, for the first time, the doctrine that epidemics were due to the spread of infection by minute parasitic organisms of specific nature, was for the first time scientifically set forth and philosophically maintained. During the century and a half that followed this very striking work the sound views of Fracastor were very widely held. It was a misfortune that the authority of Sydenham, who went back to the old and unexplained view of "epidemic" constitution, prevented the further spread of the theory of Fracastor and caused his views to remain practically forgotten until quite modern times. I feel therefore that, on the whole, Sydenham's influence on epidemiology proper was reactionary. It is rather, I think, in his work as a pure clinician and a describer of the natural history of infectious disease in the *individual* that his true greatness is to be sought.

Dr. HAMER: This most interesting paper calls for much more thorough study than most of us have presumably so far been able to give to it. As regards the barriers against the "sympathetic understanding" of Sydenham, I have never been troubled as to the "sympathetic," for, like Dr. Payne, I greatly admire the old Puritan rebel, but the "understanding"—well, that is a difficulty. But to come to Mr. Greenwood's most interesting analysis of Sydenham's work, I confess that though I had long recognized, as did the man who went to see Hamlet, that here was an author who made use of a large number of more or less appropriate quotations, it was not until 1915 that I began to entertain a hope of having a first very imperfect inkling as to what Sydenham might perhaps be driving at with his epidemic constitutions. In the course of inquiry in that year into the outbreak of cerebro-spinal fever in London, it transpired that the symptom-complex in question occurred in close relation with those of influenza, bronchitis, and pneumonia, and it was then further found that Sydenham had described the same phenomenon in his account of the new fever of 1685. Then, later, Colonel Dorgan worked out the same problem in military camps, and Dr. Crookshank found a like correlation between influenza and the Heine-Medin symptom-complex. Looking backwards it became clear that, examined year by year, there was abundant evidence in the records of these changes of type in influenzal diseases, and it was realized that London was responding in 1915-18 to a "constitution" closely resembling that of nearly two hundred and fifty years ago; this "constitution" was traced year by year, through the outbreak so fully described in New York in 1916 and our own epidemics of 1917 and 1918. According to this conception, epidemic constitution represents something that appertains not to one year but to a series of five, six, or more years; then it dies away almost to the point of disappearance to rise into prominence again at a later time.*

These inquiries led to closer study of the time relations generally of the outbreaks of the influenzal group of diseases, and incidentally to further examination of Sydenham's constitutions, and so ultimately to the conclusion (expressed at the meeting held here last October) that Creighton had found

the solution of the puzzle in his formula that there was "something more than accident in the association between epidemics of influenza and epidemics of ague." Examination of Creighton's "History of Epidemics" showed quite clearly, it was submitted, that the remarkable related "agues" which occur in the years round about all the great "posting" epidemics of influenza, throughout the whole of the recorded history, "are nothing more than those very gastro-intestinal, pulmonary, and nervous manifestations which actually constitute, as every epidemiologist realizes, part and parcel of the influenzal prevalences themselves." I have recently gone over the ground again and hope to be able to supply, some day, the detailed evidence that Sydenham's constitutions of the years round about 1661, 1675, 1679, and 1688 were influenzal constitutions.

There was a discussion here on November 13, 1918, in which Sir Arthur Newsholme referred to my "seductive hypothesis," but he clearly inclined to the view that the nexus which binds together the catarrhal group of diseases (including cerebro-spinal fever, encephalitis lethargica, and polio-myelitis and polio-encephalitis) was merely predisposing (telluric, climatic, &c.) and not an actual causal influence exerted by one and the same infecting agency operating in all of them. I have already submitted to the Section the main argument against this view. Well, "the bearing of these observations lies in the application of them" to the present paper. First as to Sydenham and the bacteriologists. In some coalitions credos do not count so much as other considerations, but in this particular instance the credos are strongly held. There are thus really only two doubts, and Mr. Greenwood is alive to both of them. There is the question as to what the bacteriologists may say, and the further question as to what Sydenham would say. I can only submit that if any timorous epidemiologist could ask him "Alas, my master, how shall we do?" he would tell him to lift up his eyes and behold the horses and chariots of fire, saying, "Fear not, for they that be with us are more than they that be with them." I am much disturbed, however, to find that Mr. Greenwood thinks I am not a true disciple of Sydenham. I accept, of course, all Mr. Greenwood says about me, but is he right in hinting that Sydenham appears to rule out a mutating *vera causa*? I sometimes wonder whether in the fullness of time, when epidemiologists, bacteriologists, protozoologists, and statisticians have worked out in complete detail all the available knowledge concerning the various rôles in epidemic disease assumed, on the one hand, by ultraviolet viruses mutating within the cycles of their multiannual or seasonal periodicities, and assumed on the other hand by the varying reactions exhibited by communities more or less immunized or sensitized; and when the whole thing has been expressed in the proper differential equations, and these have been triumphantly solved by the Brownlees, Greenwoods, Rosses, and Yules of the period, whether something approaching prodigiously close to Sydenham's conception of epidemic constitutions will not be the result. The people of that advanced era will be able to exclaim with Ecclesiasticus, "Let us praise famous men and our fathers that begat us," and perhaps will also say with Ecclesiastes, "There is nothing new under the sun."

Dr. G. C. PEACHEY: Before offering a biographical and bibliographical note, I desire to record my dissent from the assertion just made by Dr. Singer that "the history of medicine is a history of ideas and that biography is only of value in so far as it bears on ideas": indeed, what I have to say goes some way towards disproving his statement. In my recent sketch of the life of John Pechey, Licentiate of the College of Physicians (1654-1718), I drew attention to the fact, not previously recognized, that besides being the author of "The Whole Works of Sydenham," of which eleven editions were published between 1696 and 1740, and which has hitherto been regarded as the earliest English translation of Sydenham, he had previously published, in 1686 the first part, and in 1688 the second and third parts of a work entitled "Collections of Acute Diseases," which consist entirely of extracts from Sydenham's writings. That this work was unknown to Latham is plain from a statement in his preface, in which he says that "the previous version of Pechey represents in point of style and language neither the English of Sydenham's time nor that of our own" (he is here referring to Pechey's "Whole Works of Sydenham," the first edition of which appeared in 1696); and he adds later that he realizes the difficulty of reconstituting Sydenham's words in what may be supposed to have been the form in which they would have originally appeared (had they appeared in English at all) during the lifetime of the author. But Sydenham did not die till December 29, 1689, three years after the appearance of the first part of Pechey's "Collections," and, as I have shown in my memoir, there is some reason to suppose that this latter publication was known to Sydenham himself. That this was unrecognized by Latham, and indeed by all Sydenham's biographers, is due primarily to the extreme rarity of Pechey's "Collections," only two copies of which have come to my knowledge after exhaustive inquiries in Great Britain, France and America (one of these being in the British Museum and the other, which wants Part I, in the Bodleian): and secondarily to the fact that the title-page of Pechey's "Collections," in five parts, dated 1691, does not state that the work consists of a translation of extracts from Sydenham's writings. No copy of the original title of the first part, dated 1686, is forthcoming; but this is to be found in the Michaelmas Term Catalogue, No 24, December 13, 1686, in which the ascription to Sydenham is duly recorded. The practical value of my note lies in the suggestion that students of Sydenham may look upon Pechey's translations, which Sir Norman Moore has described as "vigorous and idiomatic," as contemporary with, and probably known to, Sydenham himself; for the text of the "Collections" corresponds almost verbatim with that of Pechey's "Whole Works of Sydenham" (1696), which latter remained unaltered in all the subsequent editions to 1740. The subject is discussed at length in my paper entitled "The Two John Peacheys, Seventeenth Century Physicians: Their Lives and Times" (Janus, 1918, xxiii, 121), and the results of my further study throw some new light upon the still vexed question of Sydenham's Latinity.

Mr. M. GREENWOOD (in reply): In our endeavours to master the secret of Sydenham's doctrine, we need the help of our historical colleagues and the scholarly contributions to the discussion made by Dr. Singer and Dr. Peachey were very welcome. I regret never having studied the early translation of John Peachey whose contentious life has been so charmingly told by his namesake.

Dr. Goodall's weighty observations have added what was necessary to my string of quotations. No doubt it is Utopian to expect that any medical student or even any newly qualified medical man will peruse the *Proceedings* of our Section; but were any such eccentric youth to escape from the prison house of his curriculum, I believe that a study of this afternoon's debate would be of service to him. There are not unimportant aspects of preventive medicine hidden from the writers of elementary text-books and not fully revealed even to the medical correspondent of *The Times*.

I am not sure that the chasm which separates me from Dr. Hamer is a very deep one, but I am afraid there *is* a chasm. Perhaps it is addiction to those algebraical methods at which Dr. Hamer (although himself under the gravest suspicion of being at least a carrier of the mathematical virus) pokes gentle fun, which urges me to attempt—no doubt with small success—to reduce any proposed epidemiological theory to terms capable of precise discussion. I cannot be satisfied with merely praising famous men and our fathers that begat us; these lyrical strains always make me doubt with Charles Fox whether anyone *could* be as wise as Thurlow looked. Dr. Hamer, as I think, still shrinks from a close scrutiny of his, or of Sydenham's, doctrine of an epidemic constitution. Does he believe that cerebro-spinal fever, encephalitis lethargica, poliomyelitis and influenza are mutants of one underlying cause, or does he believe that some common factor modifies all diseases prevalent during a particular epoch, long or short—confers upon fundamentally disparate diseases a common clinical and epidemiological type? If my interpretation and Dr. Goodall's interpretation of Sydenham's doctrine is correct, does Dr. Hamer regard influenza as the stationary fever of 1918-19 and cerebro-spinal fever as the stationary of 1915? I do not think this is a mere verbal dispute.

What separates us epidemiologists from the bacteriologists is, as Dr. Goodall remarked, that we do not think the problem of disease as a mass phenomenon so simple a one even in theory as the bacteriologist would have us think it. The bacteriologist—or, at least, his popular exponent—holds that all diseases can be attacked and conquered *seriatim* by specific measures; isolate the organism, "stamp it out" or procure an artificial immunity and that disease is "conquered." But if my interpretation of Sydenham's constitutions is correct, all that can be achieved in this way is either to reduce the number of stocks upon which a stationary fever may be grafted or to reduce the number of stationary fevers, leaving the stocks upon which the remainder may be grafted untouched. *Naturam expellas furca, tamen usque recurret*, hence Watt's theory of substitution, and Sydenham's constitutions.

So the problem of preventive medicine becomes wider and we are led to ask whether what is wrong with the unhealthy may not be not infection with this or that germ but—unhealthiness, even as the chief trouble of the poor is not addiction to public houses but just plain poverty. But if Dr. Hamer's mutating *vera causa* is the secret, why, the future of the bacteriologist is a roseate one, the meningococcus, Pfeiffer's bacillus, the parabacilli and even the filter-passers may be impostors; but the bacteriologists, *Hamer duce et auspice Hamer*, can advance with confidence; one day a single blow will place in their power the *vera causa* of at least five diseases and the stamping out should be vigorous indeed!