

## Reviews.

### THE GENESIS OF EPIDEMICS.

ALTHOUGH the literature of epidemiology is large, the number of modern writers who have devoted separate volumes to the general theory of epidemics is relatively small; even, if we confine ourselves to those who have had wide practical experience of epidemic happenings, absolutely small. Colonel C. A. GILL, I.M.S., falls within the last-mentioned category. He has had extensive field experience and has already won an honourable reputation as a scientific investigator. His book on *The Genesis of Epidemics*<sup>1</sup> is the fruit of long study and contains much valuable information. In the first part of the book, after submitting some definitions and briefly reviewing doctrines and methods of inquiry, the author propounds the hypothesis that in the genesis of an epidemic four factors are involved—namely, (1) a reservoir of infection, (2) a parasite, (3) an immunity factor, (4) a transmission factor. This he calls the "quantum" theory of epidemicity. In the second part the known facts respecting malaria, influenza, and plague are described. Those respecting malaria are set out in considerable detail, and this section of the book, altogether apart from any matters of theory, is a valuable summary of research work, a good deal of it the author's own. The third part of the book discusses epidemics more at large, but with frequent references to the detail of the second part. Part 4 treats of the bionomics of disease, and Part 5 is a short summary of some conclusions.

In the reviewer's opinion Colonel Gill's objective studies, particularly his account of what actually happens during the progress of an epidemic of malaria and of the means by which the state of the population at risk (measure of intensity of infection, of degree of immunity, etc.), and the changes of that state, may be assessed, are important contributions to epidemiological knowledge, and particularly welcome to those epidemiologists without first-hand knowledge of the facts. The same remark applies to Colonel Gill's meteorological and climatological studies, although it is possible that specialists in physical meteorology and certain that expert statisticians will find lacunae in his knowledge. Nobody, however, who winces before the criticism of the specialist would write on epidemiology, and no specialist worth his salt would press hardly on inevitable defects.

As an exponent of theory, Colonel Gill, partly owing to literary disadvantages—his style is too reminiscent of the official "minute," wherein "I think" becomes "it is thought," to be attractive—is not wholly successful. Accounts of his theory are rather redundant and sometimes so expressed that the theory is reduced to a truism. That is evidently the case with our own brief summary above. The four factors named include *every* theory which has ever been propounded, and, of course, their co-operation will "explain" every epidemic, but only in the sense that a dormitative virtue explains the properties of opium. Colonel Gill is not, of course, really preaching so empty a doctrine. What we take to be his serious thesis is that the genesis of epidemics can be satisfactorily described in terms of *quantitative* variations of measurable factors, and that in particular it is not necessary to suppose that *qualitative* changes in the parasite factor are involved. Thus, to take a fairly clear-cut example, in the report upon the pandemic of influenza, 1918-19, published by the Ministry of Health, data collected in different towns and institutions showed the flattest contradictions in respect of the immunity conferred by passing through an attack of influenza. In Leicester it appeared that only 4.9 per cent. of those attacked in the summer were again attacked in the autumn, while 15.4 per cent. of those not previously attacked took influenza in the autumn, but in Manchester the autumn attack rates upon those attacked or not attacked in the summer were nearly the same—namely, 9.9 per cent. and 10.1 per cent. The authors of the report cast about for a means of reconciling these con-

tradictions, and suggested that the immunizing properties of the strains involved in different local epidemics might be different. They produced no direct evidence of this, and Colonel Gill has no use at all for suggestions of this kind. Most readers are likely to find Colonel Gill more successful in showing the weakness of the support facts afford for hypotheses of biological variation in the parasite factor than in setting up a rival hypothesis. Thus one of the remarkable features of the great pandemic of influenza was the *abrupt* change in the age distribution of deaths which characterized it. Why was it abrupt? If the *relative* sparing of the aged were due to the greater proportion of survivors in the older age groups who had been immunized by previous attack, why was there no uniform change in the proportions between 1890 and June, 1918? As the Registrar-General said, "It may be doubted whether so sudden and so complete a change of age incidence can be paralleled in the history of any other disease." Finding no answer to these conundrums, many have given up the problem in disgust. Colonel Gill seems to treat these difficulties rather lightly:

"It is thus assumed that, prior to the onset of the summer epidemic in 1918, old and debilitated persons, who would otherwise have constituted the most susceptible age group, had been removed by death, and that a considerable proportion of the population belonging to the higher age groups had acquired, as the result of the epidemics during the years 1890-1918, a *relatively* high degree of immunity.

"If these surmises be correct, it is clear that they afford an explanation of the relatively high death rate amongst the aged in the pandemic of 1890, and of the relatively low death rate in the higher age groups during the years 1918 and 1919."

The italics in the last-quoted paragraph are ours. We think the devil's advocate would say, first, that there is really no evidence that the surmises are correct, and, secondly, that even if they were they would leave the remarkable discontinuity as perplexing as ever. If Colonel Gill were to retort that no man could say precisely when the equilibrium of the immunity "quantum" and the infection "quantum" would be upset, the devil's advocate would rejoin that that was only a slightly involved way of saying that epidemics in fact happen, and perhaps even quote the author's remark early in his treatise that "it would not serve any useful purpose to recount the innumerable forms assumed by the *deistic theory* of the origin of epidemics."

It is, however, neither fair to the author nor to his subject to stress what, in the reviewer's opinion, are weaknesses. We are, indeed, of opinion that Colonel Gill's standard of what constitutes an adequate *theoretical* description, or—in ordinary language—*explanation*, is not very high and that he sometimes pays himself in words, but we are also of opinion that his book deserves to be widely studied, and is worthy of the traditions of his service.

M. G.

### RECENT ADVANCES IN SURGERY.

MR. W. H. OGILVIE has undertaken a difficult task, but has discharged it so well that *Recent Advances in Surgery*<sup>2</sup> is worth its place in any surgeon's library. The author has been assisted by Mr. J. F. Carter Braine, Mr. T. P. Kilner, Mr. Grant Massie, and Mr. V. E. Lloyd, who contribute chapters which add greatly to the value and interest of the book. Mr. Ogilvie, in his preface, points out the difficulties of deciding what is recent, what is an advance, and even what is surgery, but he and his collaborators have struck a very happy mean, and are to be congratulated on the judicial attitude adopted throughout the book. If circumstances seem to justify a clear verdict, or if "non-proven" sums up the present state of knowledge, they are equally ready to say so. The book is well illustrated with photographs and diagrams, and a good list of references follows each chapter.

Tannic acid in the treatment of burns is a notable absentee, and we hope to see some mention of this very remarkable advance in the next edition. Perhaps, too, there should be some mention of radium as a serious competitor to excision of the carcinomatous rectum. None the less, omissions are few, and the range of the book is

<sup>1</sup> *The Genesis of Epidemics*. By Clifford Alchin Gill, Lieutenant-Colonel I.M.S., D.P.H. London: Baillière, Tindall and Cox. 1928. (Med. 8vo, pp. xxvi + 550; illustrated. 21s. net.)

<sup>2</sup> *Recent Advances in Surgery*. By W. Hencag Ogilvie, M.D., M.Ch. Oxon., F.R.C.S. Eng. London: J. and A. Churchill. 1928. (5½ × 8½, pp. vii + 461; 108 figures. 15s.)

wide. Interest is well sustained, and it is pleasant to encounter an occasional aphorism. "A muscle cannot recover if it has too much work, and will never recover if it has none," is the signpost to salvation for poliomyelitis. "Every operation for [gastric and duodenal] ulcers appears to be a success until it is found out" is equally refreshing. American mass statistics have been freely used, and the unrivalled numbers of the Mayo Clinic are often helpful in arriving at conclusions. None the less it is cheering to see so many British references, and to realize that this country is contributing its full quota to surgical advance.

Mr. Ogilvie rightly emphasizes the present tendency towards simplification and standardization of operations. He agrees with Sir Berkeley Moynihan that operative ingenuity has reached its peak. Erichsen said much the same thing, and time proved him wrong. Mr. Ogilvie, however, with commendable caution, adds the proviso, "except possibly in the direction of the transplantation of organs." His section on rejuvenation touches on this subject—in fact, there are few surgical subjects upon which he does not touch. There are many old friends in the book, and some new acquaintances to us, such as Sever's and Kienböck's diseases. If we are not to be allowed to invent new operations, we can still look forward to discovering plenty of diseases and giving them euphonious names.

#### ACIDOSIS AND ALKALOSIS.

THE French workers LABBÉ and NEPVEUX have recently published, under the title of *Acidosis and Alkalosis*,<sup>3</sup> a book which summarizes their own clinical and laboratory experiences of these conditions, as well as those of other workers. This publication will undoubtedly serve as a very useful book of reference. Its interest is mainly centred in the discussion of the ketosis of diabetes, but many other pathological experimental conditions are dealt with, and a large bibliography appears at the end of each chapter. One very important feature of the book is the care and detail with which the necessary laboratory methods are described. It is possible to learn from it, for instance, methods for determining the pH of the blood, the alkali reserve of the blood, the tension of alveolar CO<sub>2</sub>, and the amounts of ammonia, of acids, and of acetone bodies excreted in the urine. The physiological mechanisms by which the acid-base equilibrium of the blood is kept at the normal level, and the significance of departures from this normal equilibrium, are very clearly discussed. When attempting to follow the author's theories regarding the causes of the onset of ketosis in diabetes the reader may feel handicapped by the fact that the distinction between the terms "acidosis" and "ketosis" is not kept sufficiently clear. Indeed, no mention is made of the fact that, whereas acidosis does not necessarily involve ketosis, alkalosis can lead to definite ketosis, and also to carbohydrate intolerance and diminished oxidation of carbohydrate, thus imitating the conditions found in diabetes. The omission of these facts leaves the section of the book which deals with alkalosis incomplete, and the general discussions of problems of diabetes would perhaps be more helpful if the authors had them more definitely in mind.

#### THE EXTRA-OCULAR MUSCLES.

ANOTHER addition has been made to the already large literature on the subject of the movements of the eyes and squint by Professor PETER of Pennsylvania in a handbook on *The Extra-ocular Muscles*.<sup>4</sup> The volume is essentially clinical in character and professedly limits itself to practical problems rather than theoretical discussions. It is divided into five parts. The first of these deals with the anatomy of the ocular muscles and the physiology of their associated movements. This is handled in the orthodox fashion; in the anatomical section the researches

of Whitnall on the orbit have been utilized to good advantage, and the physiology is described with care and discrimination, a sound balance being struck between the classical views and the large amount of recent work which has been directed to this complex problem. The second part, dealing with heterophoria, suffers somewhat from confusion introduced by the large number of clinical tests described, a fault which is universal in every book on the subject. The elaboration of new methods for detecting and estimating errors of muscle balance offers a fertile field for the exploitation of clinical ingenuity, and the matter has been certainly overdone. The teaching in these pages, however, is sound on the whole, though many would consider it impracticable to advise the use of prisms so powerful as is recommended; the employment of 10 prism dioptres in hyperphoria, for example, seems excessive, and likely to give rise to more discomfort than relief. In this defect of muscle balance, also, the correction of small deviations of less than 1.5 prism dioptres is probably rather more important and beneficial in its effects than is suggested here. In the third part, which deals with squint, a very great deal of importance is attached to the part played by the "fusion faculty" in the determination of concomitant strabismus; and in discussing the operative treatment of this condition it is noteworthy that, while advancements or resections are favoured before other procedures, tenotomy is banned and recession is advised. Even so, however, it is thought advisable to hold recession in reserve as a supplementary procedure in all cases to increase the effect of single or bilateral advancements. The fourth part deals with paralytic squint, and there is appended a note on nystagmus.

#### STERILITY IN WOMEN.

THE successful treatment of *Sterility in Women*,<sup>5</sup> according to Dr. SIDNEY FORSDIKE, "confers greater happiness upon the people concerned than any other form of surgical procedure." The newer methods of investigation and treatment by inflating the uterus and tubes, and by  $\alpha$ -ray examination after the injection of opaque material such as lipiodol, have tempted him to give his experience of them. The methods he employs, both for inflation and for injection of lipiodol, are described in detail, and illustrated by seventeen plates of  $\alpha$ -ray photographs, or what he calls "hystero-grams." His results are given and discussed fairly and judiciously. The new methods were devised primarily for diagnosis, but Rubin claimed 10 per cent. of pregnancies following inflation, and Forsdike finds his own results show this figure to be rather an under-estimate than an over-estimate. In 100 cases inflated 15 pregnancies followed, and out of 67 in which lipiodol was injected the tubes were found patent in 41, and of these 14 (that is, 34 per cent.) became pregnant. The difficulty in deciding between *post hoc* and *propter hoc* in these cases is acknowledged, and no explanation is attempted as to why the injection of an opaque substance such as lipiodol to enable an  $\alpha$ -ray plate to be taken should also increase the chances of pregnancy—but there are the figures. Chapters are included on the causes of sterility and on other methods of examination and treatment, and the book can be recommended as giving a reasonable account of the clinical features of sterility in women under present-day conditions. The clinical are much better than the scientific portions, in which there are many errors—for example, that ovulation and menstruation occur about the same time, and that fibroids cause sterility when "the fibrous tissue is so widely prevalent in the uterine wall as to produce a condition of atrophy of the endometrium." There are also loose statements on the cessation of "ovarian function," by which term apparently "ovulation" alone is implied. Another defect is the number of generalizations that are either incorrect or at least impossible to prove. Far too many conditions are mentioned as "invariably the cause of" or "invariably associated with" sterility or dysmenorrhoea or other symptoms. Finally, the book is marred by careless use of words, so that the meaning is often to seek.

<sup>3</sup> *Acidose et Alcalose*. Par Marcel Labbé et Floride Nepveux. Paris: Masson et Cie. 1928. (Med. 8vo, pp. 296; 54 figures, 32 fr. sans majoration.)

<sup>4</sup> *The Extra-ocular Muscles*. By Luther C. Peter, A.M., M.D., Sc.D. London: H. Kimpton. 1928. (6 × 9½, pp. viii + 294; 98 figures, 5 plates. 18s. net.)

<sup>5</sup> *Sterility in Women*. By Sidney Forsdike, M.D., B.S. Lond., F.R.C.S. Ed. and Eng. London: H. K. Lewis and Co., Ltd. 1928. (Demy 8vo, pp. viii + 133; 25 figures, including 17 plates. 9s. net.)