THE GOULSTONIAN LECTURES ON PUERPERAL FEVER.

Delivered at the Royal College of Physicians, London.

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LECTURE I.

MR. PRESIDENT,—The disease generally known as puerperal fever is distinguished by characters which render it peculiarly suitable as a subject for the consideration of such an audience as I have the honour of addressing on the present occasion.

We can trace its history in the literature of medical science more or less continuously from the present time to periods so remote, that we possess now only scanty records of them. In the progressive stages of historical inquiry, we find that puerperal fever, though comparatively a modern introduction into nosological classification, has retained a degree of exactness in its nature which can be claimed by few other diseases, which enables us to recognise it without difficulty in the descriptive works of various authors. It has shared the same fate—or perhaps we ought to say, has enjoyed the same privilege—as those to which I refer, in that it has afforded the student of history the means of determining the character of mind, the method of investigation, the degree of accuracy in observation, and the practical skill which were possessed and exhibited at any particular point of time within the compass of historical records.

Lastly, it may be regarded as the most recent and not least grateful recipient of the favour and advantage which every branch of our science and practice derives from the progress of physical knowledge.

If puerperal fever be thus distinguished, it would seem an easy task to entertain you without fatigue in any one of the lines of research which I have indicated. I am obliged, however, to request that the standard which I regard with deep respect, and you with satisfaction, may be reduced to meet an unusual demand for judgment and caution in dealing with materials so abundantly afforded by past history and recent important researches as to make the satisfactory treatment of them a matter of more than common difficulty.

There is a particular mental attitude, if I may use the expression, which we must assume, in order to arrive at just conclusions regarding the value of the contributions of others to our stores of knowledge of any particular branch of science. It is not easy to avoid a tendency to become partisans of a theory under the influence of evidence which rests upon data beyond our immediate power of verification. To observe and record with accuracy the phenomena of disease, both clinical and pathological, requires much more than ordinary caution and patience when those observations are made for the express purpose of testing the credibility of the opinions of others.

It is desirable to keep in view the general principle that those facts are most valuable which contribute to the practical treatment of any malady, including of course in the term treatment the knowledge of etiology and the best means of prevention. It may be necessary to mention some of the various methods of treatment which have been at times in general repute, though with few exceptions I shall omit those which were of empirical character and ephemeral duration, and cannot be classed with remedies rationally suggested by etiological and pathological considerations, and fairly tested by experience.

It shall be my aim, therefore, when perusing the history of this fever, to prefer those records which contain practical information, and which tend to the elucidation of the origin and real nature of the disease. And, while pursuing this plan, we may with advantage notice instances in which contemporary neglect apparently rewarded most important researches. The effect of thus ascertaining the reception which was accorded to new and original views will be to render us less liable to the disparaging charges which posterity may make against us when present time has become the past—a time which, there are good reasons for believing, will prove not the least distinguished epoch in the future history of the progress of medical science.

I shall attempt to place before you such evidence as is necessary to show that puerperal fever existed very much in the same form as in the present day, at that period which we generally term “classical”, when

nosological distinctions were based upon symptoms, and the modern science of pathology had not been developed, or was entirely unknown.

I may assume, for the sake of convenience, the date of the foundation of this college (A.D. 1518) as the commencement of the modern era, through which we shall trace the successive additions which were made to the general and special knowledge of our subject.

Within the last few years, very considerable modifications have been brought about in our views of contagious agents, in which of course those on puerperal fever have to some extent participated. It is to the chemical investigator and the scientific physiologist that we are chiefly indebted for a series of observations and suggestions which have thrown more light upon the real nature of the fever than we should ever have obtained from ordinary clinical and pathological methods. In fact, it may be said that those sources of information were exhausted. As the conclusions deduced from the observations alluded to are of great practical value, and are receiving constant additions now that so many are engaged in research, it will be necessary for us to examine with patient care the facts presented by the most reliable authorities, and endeavour to ascertain whether they will bear the test of general experience, and how far they afford satisfactory explanations of the causes which produce the phenomena which characterise the disease.

There is one point which must be noticed when we compare in detail the descriptions which we possess of the fever as given by some authors, with the general symptoms which it usually presents in the routine of daily practice. It is the fact that the disease has often appeared in a more acute form, and has been attended by more serious results, than we observe now. Those epidemics, as they were called, which occurred in some of the large cities of Europe and in this country, particularly in the middle and latter part of last century, afford us considerable assistance in explaining these apparent discrepancies, though a complete solution is only to be supplied by an accurate determination of the etiology of the fever. It may seem paradoxical to assert that the disease is a definite one, and yet that we are to be prepared to meet with various descriptions by authors of its symptomatic and pathological characters. If it were not for the fact that many have formed their own ideas of its nature from personal experience alone, and have obtained only a limited view of the many phenomena which it exhibits, it would have been unnecessary for me to direct attention to the reason why we should make some allowance for the difference in details and in opinions which I allude to. There is one way in which we may yield in this respect without going too far, and that is by separating the points of resemblance from those of difference, under the guidance of experience; and by not insisting too strongly on the introduction of the latter into the terms of a definition, we may agree that, both in a scientific and practical point of view, there is sufficient positive evidence to justify us in regarding the disease as one of specific nature.

The Hippocratic treatise known as the *Περὶ Γυναικείων* contains a general description of puerperal fever, which has been quoted by most writers on the subject. We may supplement it by references to the *De Morbis Vulgaribus*, or *Epidemics*, in which a few cases are related with such clearness and detail as to afford the evidence we require to prove the point in question.

Hippocrates says: “If the lochia do not occur in the puerperal condition, it is very probable that fever and abdominal swelling will supervene. The whole system, but particularly the abdomen, will be highly sensitive to touch. Sometimes there will be intense abdominal and lumbar pain, dislike of food, insomnia, and irritation of the surface of the body. On the fifth or seventh day after these symptoms, the patient will be attacked with disturbance of the alvine and urinary secretions. If the lochial discharge return, the patient may with proper care soon recover; but, if not, there is great danger that diarrhoea will ensue, and the lochia be arrested. The arterial pulse is weak, though sometimes quick and varying in force. Thus the early stage of the disease is characterised. After a time, the hollows of the face become flushed. In this condition, the diet must be light. If there be delirium, laxatives must be given to drink, and other remedies, according to the indications of bilious or mucous secretions. Fomentations and poultices are to be applied daily. If, however, the lochial purgation appear to affect the head, thorax, and lungs, for that may happen, the patient always very soon succumbs.” (Hippocratis *Opera*, Ed. Foes., p. 603, secs. 40, *et seq.*)

This general description applies, as we see, to one particular form of puerperal fever, and, though it is followed by an account of some other forms of disease which we know to be nearly allied to it, and to depend upon the same cause; yet there is no strong indication that they were associated for any reason of that kind in the treatise referred to.

To illustrate the above, in the first book of the *Epidemics*, the history is related of the case of the wife of Philinus, who was seized on the

fourteenth day after delivery with pain in the hypogastric and pubic regions, consequent on the cessation of the lochia. The first symptom mentioned is rigor, or in the original: "She was seized with fever attended with rigors." On the eighth day after this, the rigor returned; on the fourteenth day, she had subsultus and incoherent wanderings, and on the twentieth day she died. (*Op. cit.*, p. 976.)

This case is followed by one still more accurately described. "The wife of Dromeades, having been delivered of a female child, and all other matters progressing favourably, on the second day after was seized with rigor and acute fever. She began to have pain in the hypochondrium and nausea; was incoherent and sleepless for many hours; respiration was long and irregular, and she suffered from thirst, cold sweats, and epistaxis. On the sixth day, she expired." The third case is that of a woman who "was seized with fever soon after the birth of a son. At first, there was thirst, nausea, and cardialgia; the tongue was dry, the bowels disordered; she had slight rigor, acute fever, and a faint cold sweat about the head. On the third day, she was in great pain, and there was diarrhoea; on the fourth day, she had rigors again, and all the symptoms were increased in severity. They continued with slight variation till she died with epistaxis on the fourteenth day." (*Op. cit.*, p. 987.)

Some remarks on the conditions of the atmosphere which existed at the time of the occurrence of this case contain the observation that erysipelas, in a malignant and non-malignant form, was prevalent. There is generally some reason for the introduction of such a fact as this, which is left to the reader to explain as he pleases. The way in which Hippocrates quietly mentions it without remark, still leaves the impression that he had noticed a connection between atmospheric conditions and the occurrence of certain diseases which he could not explain.

There is another important case, which is compared by the commentators with that of the wife of Philinus. The symptoms are regarded as very typical in both instances of the consequences of arrested lochia: a theory, I need hardly mention, which satisfied the minds of the physicians of that day, and indeed for centuries later.

But I do not mention this case, which commences *ἐν Θάσῳ τὴν κατακείμενην*, etc. (*op. cit.*, p. 1096), for any other reason beyond the fact that, during the period of eighty days of illness terminating in death, we have those symptoms detailed which clearly mark the case as one of phlebitis or inflammation of the cellular tissue of the right limb. The woman was attacked on the third day after delivery with the usual symptoms of puerperal fever, such as were mentioned in the other cases I have related. On or a little after the twentieth day, the symptoms of phlebitis commenced, and the febrile variations which are so commonly met with in this form of disease are simply and clearly stated; they were of a wandering and paroxysmal character (*πεπλανημένως δὲ παροξύνουσαι*), which exactly describes them.

Before disposing of the Hippocratic works, it may be allowable to refer briefly to two cases of abortion (*op. cit.*, pp. 1078-79)—cases, at least, in which febrile symptoms followed immediately on that occurrence. It is right to mention that they are referred to in the annotations of the edition of Foesius as instances of fever producing abortion. There is, however, this peculiarity in the account of the cases which we can hardly regard as without intention. It is, that they both commence somewhat thus:—"This is a case in which acute fever attacked a woman immediately after abortion," etc.; or, "This is another case in which acute fever followed abortion at the fifth month," etc.; and both cases terminated fatally on the seventh day. As it is not the purpose of Hippocrates, in his clinical histories, to connect cause and effect, but to leave the simple facts to speak for themselves, we are not obliged to submit to the conclusions of commentators who have looked at them through the medium of a theory. We are told of the events and symptoms in order of time, and we are permitted to consider the occurrence of abortion related in those cases as certainly presenting a different aspect in connection with the symptoms than if it had been mentioned later in the history.

Independently of these cases, we have the distinct statement, that abortion might be followed by puerperal fever; for we find, in the treatise *De Morbis Mulierum*, such remarks as the following:—"This (puerperal fever) may happen after abortion"; or, again, "Abortions are more dangerous than ordinary parturition, for the uterus is liable to ulceration or inflammation—a condition most serious". It is from such extracts as these and others that most modern authors satisfy themselves of the existence of puerperal fever among the maladies of ancient times. In a treatise by Hulme in 1772, the point seems to have been settled in the opinion of the authors of last century, and his concluding remarks are interesting enough to be quoted as the general expression of opinion at the time referred to. "We have here", he says, "a pretty accurate and plain description of the

puerperal fever, as observed by Hippocrates, which in most parts exactly corresponds with what has been said of it in the first part of this treatise, inasmuch that, had both descriptions been written about the same time, it might have been disputed which author had borrowed from the other. But the truth is, neither of them in the least tittle copied from the other, but from Nature alone. Hence it is similarly arises; from which also we may learn that the operations of Nature are the same in Britain as in Greece, and continue the same in this day as they were above two thousand years ago. This is likewise a clear proof of the immutability of the puerperal fever, that it is an original disease, and hath been prevalent at all times and in all climates, and yielded to the same method of cure as hath been prescribed above." (Page 95.)

We may assume, then, that there is no great difficulty in establishing the first question of identity in all important particulars. There are one or two points deserving of further notice. It may be observed that Hippocrates omits all mention of treatment in his clinical examples, so that we are unable to determine the extent to which he practised the instructions contained in his general treatise, nor do we feel at all certain that we understand the views which were taken of the essential nature of this fever. It is asserted by his commentators that Hippocrates referred puerperal fever to an arrest of the lochia, the grounds for that assertion being his statement that this is the first symptom of the disease in the puerperal state. There is no doubt that he did entertain some such idea; for he remarks that, "if they"—that is, the lochia—"return, it is a favourable symptom". But the question of the cause on which the local and general disturbances depend is not limited by any means, as some suppose, to the arrest of the lochia alone. There are distinct pathological conditions assigned as causes in a certain number of cases; namely, inflammation and ulceration of the uterus, and retention of placental structures. There is no point more carefully set forth than this, and there is no hesitation in referring the symptoms which follow such retention to the effects of decomposition. "There may be ulcerations in the uterus from the decomposition of the lochia (*Γίνεται δὲ καὶ ἑλκεα ἐν τῇσι μήτρῃσιν οὐα τῶν λοχείων σαπέντων*)" certainly implies this. The word here used we have found necessary to introduce into our own language when we wish to indicate in a special manner the process of decomposition, though it appears to have been commonly used by the Greeks: as, for example, when Hermes reported of Hector's corpse:

δαδεκάτῃ δὲ οἱ ἡὼς
Κείμενος οὐδὲ τι οἱ χρῶς σήπεται, οὐδὲ μιν εὐλαί
ἔσθουσι.

"This the twelfth evening since he rested there,
Untouched by worms, untainted by the air."

(*Iliad*, xxiv, 414, Pope's Translation.)

We shall see that the principle involved in our modern term "septic", though clearly appreciated by the ancients, was to some extent lost sight of till almost the present day.

So far as the arrest of the lochia was considered one principal cause of puerperal fever, the theory was naturally applied to the explanation of the various constitutional symptoms noticed in the disease; that is to say, they were simply regarded as of metastatic origin, and their cessation on the return of the lochia was explained by and supported that idea.

Only one other circumstance remains to be mentioned under which the symptoms of puerperal fever may occur, and that is the possibility of inflammation at the ordinary period of menstruation. From what has been extracted thus briefly from the Hippocratic works, we may conclude that several causes were recognised by Hippocrates as capable of producing the series of symptoms which we term puerperal fever, and that amongst those there is evidently a clearly conceived notion of infective agency.

It is not probable, it is quite certain, that the more carefully we scrutinise the classic authors, the more they appear to have known of the symptoms of disease; and their accuracy and acuteness of observation, and, above all, their singular simplicity and truth, command our respect and attention. It is only by some such general expression of appreciation that we can fairly excuse ourselves from a more elaborate analysis than I have offered above. The same kind of sensation steals over the mind in perusing those authors which the works of art of that period produce upon the senses. They exhibit a subtleness of intellect and perception which so astonish and delight, as to make comparison with contemporary efforts never occur to our thoughts, and which lull all feelings of envy into tranquil admiration.

It is not unprofitable, you will agree, to cherish the feeling I have described. It seems long since the Hippocratic writings were seriously studied in this country. We have perhaps been advancing, or at least moving so rapidly, that we have not cared to look back. Yet there

never was a time, I believe, since the period of Grecian excellence, when such true appreciation of its wonderful productions would be developed by a more intimate acquaintance with them than at the present day. Though the knowledge of the phenomena of disease may never have been so complete, their origin and cause so profoundly investigated, as now; though we have the assistance of mechanical, optical, and chemical science to aid in a thousand ways, and may be too much occupied with the present to think of the past—yet we may learn much by studying the too few relics we possess of ancient wisdom and knowledge, as in an equal degree we may acquire taste by contemplating those ruined monuments—silent proofs of genius and skill.

In stating that we are in a position to judge of their merits, I mean to imply that there does not appear to have been any fair critical power exhibited even by the commentators of comparatively modern time. The annotations in the edition by Foesius of Hippocrates's works published in 1621, enable us to ascertain the exact state of knowledge during the long interval; and I may be excused for omitting to make any mention of a period in which science suffered at the hands of her enemies, and was adorned, consistently with Gothic feelings and ideas, in most unbecoming and uncomfortably restricted garments.

From the quotation in Dr. Hulme's treatise, and from those cases to which he omits to refer, it is clear that, so far as symptoms are concerned, all the important phases of puerperal fever were so well described by Hippocrates as to admit of no considerable addition or improvement. It is to the progress of morbid anatomy and pathology that we are chiefly indebted for recent information on the value of symptoms, and their relation to, or origin in, pathological processes. As examples of this, we are able to explain the various local and constitutional symptoms produced by those particular forms of the disease and their occasional complications, which we are accustomed to designate by distinct terms, *e.g.*, phlegmasia dolens, phlebitis, metritis, thrombosis, and others. When I said that we might assume the date of the institution of this College as an approximately correct and convenient starting-point of the modern era, it was intended to fix the attention upon one or two illustrious names which will always in this country be associated with anatomical and physiological science, and which retain their influence in the preservation of our national character of scientific thought and practical purpose.

For the present, we may limit our inquiries to the gradual evolution of the theory now daily gaining ground, that there is a particular cause for the various forms of puerperal fever; viz., the existence of a specific poison in the human system, either introduced or self-developed. It matters not for our purpose what the nature of the evidence was which first led to this belief, provided only that, so far as they went, the data were strictly true and the deductions logical. Our knowledge of the general principle, that we are all more or less under the influence of theories in our practical views of disease, enables us to discern to some extent from the method of treatment the particular theory which any individual maintains on any particular subject, without knowing directly from other sources what that theory may be. Supposing that we have to determine the view entertained by some author of reputation on the nature of puerperal fever, we may ascertain from his directions as to treatment what his theoretical views were, whether chemical, mechanical, physiological, or pathological. This is not perhaps true of most of us, as we rather follow example than theory, and are more under the influence of education than those to whom we are indebted for original ideas. But, as we find there is often antagonism in the mind between the suggestions of a theory and the influence of habit, it becomes necessary to apply the test of treatment to determine how far any individual believes in his theory. In this question of the origin of the belief that puerperal fever depends on a specific poison, we find that the idea is by no means so recent as might be imagined; but, as it was regarded in such a doubtful way as not to influence treatment to any beneficial extent, we cannot admit that it deserves such serious notice as when we meet with those who have entertained the belief so strongly as to have used every endeavour in their power to arrest the influence of the poison, and to prevent its contagious effects. This is the reason for the above remarks. Time has proved that many explanations of the cause of puerperal fever have been erroneous, and that the belief in a specific poison is entitled to general acceptance. Now, if we limit ourselves to tracing its development, which has been sufficiently gradual, we shall be better occupied than in the criticism of erroneous doctrine, and we can perceive that, with such a limited object in view, we may unravel the tangles of history, and, by following one thread, do better than by attempting to begin without an end in hand on this or that knotted loop of conjecture.

In its present form, our idea of the specific nature of the poison which produces puerperal fever is somewhat different from that which

was involved in the "*σῆψις*" of the Greeks; but to what extent it differs, and why, depends of course on our knowledge of the properties of the poison, and that point we need not consider at present. We are not obliged to meet the request to define what we mean by contagious; we should not allow an argument on the meaning of words to interrupt our attention to facts.

I now propose to examine how far the contagious theory of puerperal fever is supported, or contradicted, or neglected by the authorities of the seventeenth century. The theory of a poison generated in the process of the decomposition of organic matter involves the assumption that, *pari passu* with the process of decomposition, there is a simultaneous and equivalent one of generation. This, perhaps, may be going a little too far; but there is no doubt that decomposition and disintegration have always been clearly distinguished. To those who do not reflect upon such questions, the contrast would not seem so striking as to the mind of the scientific inquirer. It is the one, however, which called forth such a request as the following from the pen of Harvey, whom we may regard as the exponent of the most advanced opinions of his day.

"What shall we say of the animalcules which are engendered in our bodies, and which no one doubts are ruled and made to vegetate by a peculiar vital principle? Of this kind are lumbrici, ascarides, lice, mites, syrones, acari," etc.; and then quoting from Aristotle: "For in almost all dry things growing moist, or moist things becoming dry, an animal is engendered." (Page 282, Trans. Harvey: *On Generation* (Sydenham Society's Works).)

In another place, he exclaims: "Let physicians, therefore, cease to wonder at what always excites their astonishment; namely, the manner in which epidemic, contagious, and pestilential diseases scatter their seeds, and are propagated to a distance through the air, or by some *fomes* producing diseases like themselves, in bodies of a different nature, and, in a hidden fashion, silently multiplying themselves by a kind of generation, until they become so fatal, and, with the permission of the Deity, spread destruction far and wide among man and beast" (p. 322, *op. cit.*). There can be no doubt of the views of the author of these passages regarding the development of minute independent organisms in the human body; and, though we have made considerable progress in our powers of demonstrating forms of life of minute dimensions since his time, yet, for practical purposes, he had so clear an idea of the probability of the existence of almost atomic germs, that he would not only have believed the statements of Professor Tyndall, but have followed that distinguished experimentalist to the extreme limits of imagination. He would have agreed with the description of the poet:

"Unfinished things one knows not what to call,
Their generation's so equivocal:
To tell them would an hundred tongues require,
Or one vain wit's, that might an hundred tire."

Essay on Criticism, part i.

There is all the proof which we require of Harvey's belief in the effects of a poison generated by decomposition to be found in a subsequent passage in his work on *Generation*; and, as it is an important one in many ways, particularly in a practical point of view, I shall with your permission quote it at length.

"Women, as they alone have a menstruous, so have they alone a lochial, discharge; added to which, they are exposed to disorders and perils immediately after birth, either from the uterus, through feebleness contracting too soon, or from the lochia becoming vitiated or suppressed. For it often happens, especially in delicate women, that foul and putrid lochia set up fevers and other violent symptoms; because the uterus, torn and injured by the separation of the placenta, especially if any violence have been used, resembles a vast internal ulcer, and is cleansed and purified by the free discharge of the lochia. Therefore, do we conclude as to the favourable or unfavourable state of the puerperal woman from the character of these excretions.

"For, if any part of the placenta adhere to the uterus, the lochial discharge becomes fetid, green, and putrid; and sometimes the powers of the uterus are so reduced, that gangrene is the result, and the woman is destroyed. If clots of blood, or any other foreign matter remain in the uterine cavity after delivery, the uterus does not retract nor close its orifice, but the cervix is found soft and open. This I ascertained in a woman, who, when labouring under a malignant fever, with great prostration of strength, miscarried of a fetus exhibiting no marks of decomposition, and who afterwards lay in an apparently dying state, with a pulse scarcely to be counted, and cold sweats. Finding the uterine orifice soft and open, and the lochia very offensive, I suspected that something was undergoing decomposition within. Whereupon I introduced the fingers, and brought away a mole. . . . The woman was immediately freed from her symptoms, and in a short time recovered." (*Op. cit.*, p. 544.)

On the principle advocated by Harvey, that we do well to treat with respect the opinions of the past, I have made these extracts from his work on *Generation*. He observed that "respect for our predecessors and for antiquity at large inclines us to defend their conclusions to the extent that love of truth will allow. Nor do I think it becoming in us to neglect and make little of their labours and conclusions, who bore the torch that has lightened us to the shrine of philosophy." (*Op. cit.*, p. 432.)

In order to ascertain how far the ideas of Harvey on the connection between puerperal fever and decomposition were entertained generally, we cannot refer to a better authority than the celebrated French obstetrician François Mauriceau. In his remarks on the suppression of the lochia, and the symptoms which are produced by it, he says that "those putrefying humours, by reason of their prolonged detention in the cavity of the uterus, produce for certain considerable inflammation, so that the suppression of the lochia may be one of the most dangerous symptoms which can happen in the puerperal condition". (Ed. 1681, p. 234.)

The most important pathological effects of inflammation are described by Mauriceau, nor does he omit to mention the symptoms of puerperal phlebitis, attributing that also to the suppression of the lochia. This idea led him to depend upon bleeding as the chief remedy for the disease; so that we can hardly say he had any conception of the existence of a poison generated in the process of decomposition as the prime cause of these symptoms. As a clinical observer, however, he mentions in detail the most minute as well as the leading symptoms of nearly all the forms of puerperal fever with which we are acquainted; at least of those which occur in the puerperal condition, properly speaking. It is rather in his character of a practical physician that we must regard him than in that of a scientific pathologist.

I have already indicated to what extent simple clinical observation is able to explain the etiology of puerperal fever; and, for the reason that pathological analysis had not been brought to bear upon it till a century or more after Mauriceau's time, we have no important addition to notice in the works of intervening authors.

Changes had taken place, however, in the general doctrines of medical science during that period, and these necessarily more or less influenced general opinion on the etiology of our subject. I have used the term *pathology* in the sense in which we employ it at the present time, or at least in that sense which we attach to the term *morbid anatomy*. It had a different signification, however, a century ago, when Cullen said, in his lectures, "The pathology that I am to deliver is properly to be referred to two heads—that of causes and that of symptoms;" and again, "When we speak of the pathology of a disease in the institutions or in the practice of medicine, the disease is considered in its symptoms, causes, and effects." The term thus may be seen to have included the etiology as well as all the phenomena of disease.

As far as puerperal fever is concerned, we have only to notice in Cullen's works certain views which he entertained on the general question of the difference of fevers and their causes, and which were intended to apply to puerperal fever amongst others of contagious nature. It is his theory of contagion that we are induced to examine, and not any additions to the morbid anatomy or the symptoms of puerperal fever by himself or his immediate predecessors, that deserve our attention. The term *fomes* was used by Harvey to describe a minute organic particle, one of the products of decomposition, possessing very active properties, though certainly indefinite in its character. This term is also used by Cullen in much the same sense. He assumes the existence of different kinds of fomites, of different specific nature, and suggests the employment of the term *effluvia* "rather than the general ones of contagion and miasma". By *effluvia* he meant the products of the decomposition of any animal matter, as distinguished from contagion and miasma; miasma being the product of the decomposition of vegetable matter, and contagious being a term used to distinguish such diseases as scarlet fever, measles, and others, not originating in a decomposing process. Such I conceive to have been the meaning of *effluvia*, contagion, and miasma: "Substances", in his own words, "thus imbued with an active and infectious matter may be called fomites; and it appears to me probable that contagions, as they arise from fomites, are more powerful than as they arise immediately from the human body."

"The miasma", he says, "so universally the cause of fever is that which arises from marshes or moist ground acted upon by heat;" the word fever, as arising from miasmata, being restricted to the intermittent species. He defines "contagions as arising directly or originally from the body of a man under a particular disease, and exciting the same kind of disease in the body of the person to whom they are applied." (*Præticæ*, vol. i, p. 80, *et seq.*) From these passages, it is not

difficult to understand Cullen's idea of fomites, though, as far as having proved the existence of such particles, he was not the least more advanced than his predecessors; that is to say, his reasons for believing in such germs of disease rested entirely upon clinical observation.

He found some difficulty, however, in dealing with a class of fevers or pyrexia known as "continued", jail, or hospital fevers, and in this class he included puerperal fever. He attributes their origin to a common cause. On this point, it will be seen that there was a definite opinion regarding the effects which may be produced by the effluvia from the living human body, which, "if long retained in the same place without being diffused in the atmosphere, acquire a singular virulence, and, in that state being applied to the bodies of men, become the cause of a fever which is highly contagious. The existence of such a cause is proved by the late observations on jail and hospital fevers, and that the same virulent matter may be produced in many other places must be sufficiently obvious." With respect to these contagions, Cullen observes in another place: "Though we have spoken of them as a matter floating in the atmosphere, it is proper to observe that they are never found to act but when they are near to the sources from whence they arise; that is, either near to the bodies of men, from which they immediately issue, or near to some substances which, as having been near to the bodies of men, are imbued with their effluvia, and in which substances these effluvia are sometimes retained in an active state for a very long time."

With such views of the origin of what we now term septic causes of disease, we may expect that the treatment recommended by Cullen was consistent with those views. There is so little empiricism in his admirable works, and he always leads up to the subject of treatment in the most logical manner after a careful analysis of the symptoms and probable nature of any disease, that we are not surprised to find that he avoids those discussions which many of his contemporaries indulged in. It was not a question for him whether to bleed or not to bleed was right, nor would he have recognised any statistical contributions to the determination of that question as of much value. While as for special remedies, unless they were known by experience to produce such effects as would theoretically assist in the consistent working of his idea of the cause of the fever, would he have paid attention to them.

Cullen was not the man to be deceived by the representations of success which were reported to have followed the use of ipecacuanha in puerperal fever, in a memoir by Dr. Doucet of Paris, the publication of which was made the year before Cullen's last edition was issued. His own directions for treatment, which we will notice presently, were based upon the view, "that the cause of death in fevers may be a poison; that is, a power capable of destroying the vital principle; and this poison may be either the miasma or contagion which was the remote cause of the fever, or it may be a putrid matter generated in the course of the fever. In both cases, the operation of such a power appears either as acting chiefly on the nervous system, inducing the symptoms of debility, or as acting on the fluids of the body, inducing a putrescent state in them." (Section 101, *op. cit.*) In other words, a "strong tendency to putrefaction in the fluids" was "the direct cause of all the phenomena of these fevers." (Section 102, *id.*)

Thus far I have advanced by rather rapid strides in the history of the most important part of our subject. I have referred to original sources for information, and have not attempted, under the influence of any feeling, to make it appear that the authorities referred to knew more or less than they could fairly claim to have known. I have omitted almost all mention of a great deal that has been slowly dismissed from that total of data which we feel is more essential to correct conclusions than the assistance of the most powerful logical reasoning. We have arrived at a time when our modern system of medical science was beginning to develop itself; when attention was being directed to those new and unexplored regions which the researches of the morbid anatomist and pathologist have since exposed to our view. To what extent those new methods of inquiry modified the general ideas concerning the etiology of puerperal fever, I shall proceed to consider in the next lecture.

ANIMAL VACCINATION.—Since 1872, there has existed in the *abattoirs* of Bâle a vaccine establishment, headed by a veterinary surgeon, which is used by all classes of society. Bulls are inoculated with vaccine from children, and in their turn serve as foci for vaccination. After having shaved the posterior face of the testicles of these animals, about twenty superficial incisions, from two to three *centimètres* long, are made with the instrument charged with vaccine matter. If done carefully, the operation always succeeds, and, at the end of the sixth day, it is possible to collect vaccine, which, mixed with glycerine in the tubes, preserves its properties longer than human vaccine.