OLIVER WENDELL HOLMES

AND THE

CONTAGIOUSNESS OF PUERPERAL FEVER

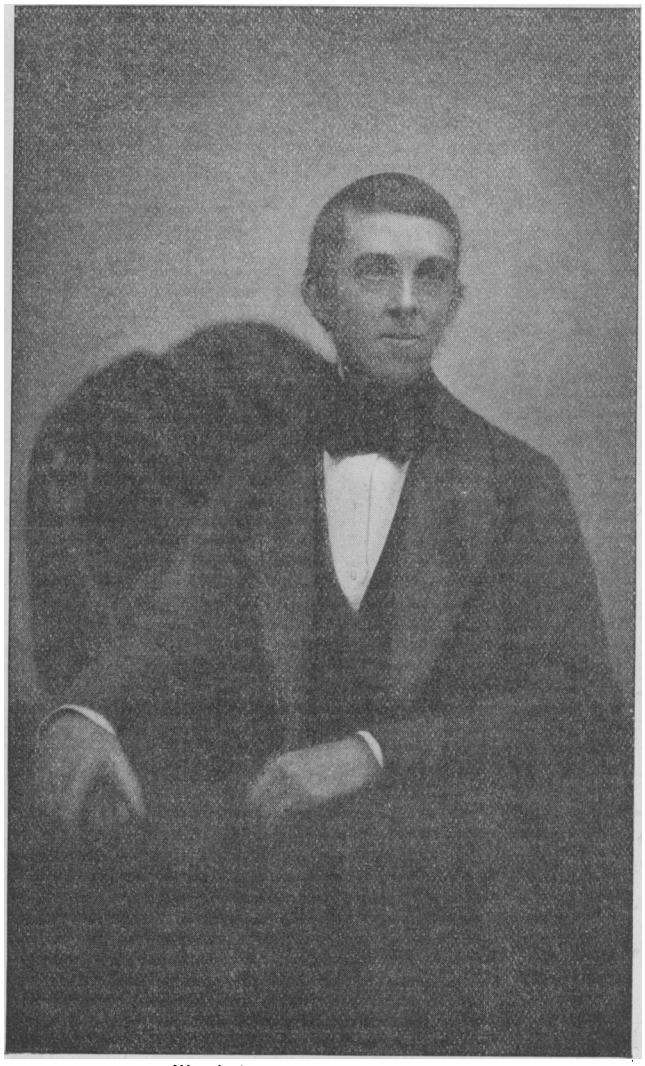
An Address delivered to the Trowbridge Division of the Bath and Bristol Branch of the British Medical Association.

By CHARLES J. CULLINGWORTH, M.D., F.R.C.P.

Some three or four months ago, your President, Dr. Haydon, my friend and former house-physician, expressed a wish that I would give an address to this Division some time during his occupancy of the chair. This request I felt it impossible to decline. The end of October was mentioned as a suitable time, and here I am.

Turning over in my mind how I could best utilize such an opportunity, it appeared to me that here at last had come my chance to fulfil an intention I had long cherished of reminding my professional brethren of the debt we owe to Oliver Wendell Holmes for his powerful, but I am afraid largely forgotten, essay on the contagiousness of puerperal fever.

No one who is acquainted with my teaching or writings or who remembers the active part I took some ten or twelve years ago as one of the Secretaries of the Semmelweis Memorial Fund in this country will suspect me of failing to recognize the great work, accomplished "with tears and travail," of that illustrious man.* It is possible, however, to do justice to Semmelweis and yet to be ready to acknowledge the work done by others in a similar direction.



Oliver Wendell Holmes (taken between 1845-50)

A Memorable Essay.

I purpose to-day to call attention afresh to Holmes's memorable essay, and to try to do something towards removing the reproach—not, I fear, entirely undeserved—that we English-speaking people on both sides the Atlantic,† whilst giving abundant honour to Semmelweis, have been in danger of forgetting the earlier and equally remarkable contribution to our knowledge of puerperal fever that we owe to one of our own kith and kin. This forgetfulness has been due, not so much, perhaps, to the overshadowing of Holmes's work by that of Semmelweis, as to the fact that the medical reputation of Holmes himself was put into the shade by the brilliant success he achieved in later years in general literature, and that his own profession came to be proud of him as the autocrat, professor, and poet at the breakfast-table, rather than as the fearless and outspoken defender of the life and health of the parturient woman. Holmes has himself left on record an account of the origin of the essay of which I am about to speak. At a meeting of the Boston (U.S.A.) Society for Medical Improvement, of which Wendell Holmes was a member, the stated business having been concluded somewhat early, one of the members, in order to fill up the time, reported an incident that had recently occurred in the neighbourhood. A physician had made a *post-mortem* examination of the body of a puerperal fever patient, and had himself died in less than a week apparently in consequence of a wound received at the examination. In the meantime, he had attended several women in confinement, all of whom were said to have been attacked with puerperal fever. The discussion that followedthe relation of this case made it clear to Holmes that "a fuller knowledge of the facts relating to the subject" was much needed, and he therefore felt that it would be doing a good service "to learn what experience had to teach in the matter." . . . He embodied the results of his inquiry in an essay which he read before the Society and which, at the Society's request, he subsequently published in the *New England Quarterly Journal of Medicine and Surgery* for April, 1843. As this Journal had only a very restricted circulation and died a natural death when it was but a year old, the essay was practically buried, and until its reissue, with additions, in 1855, cannot, as Holmes himself acknowledged, be said to have been brought fully before the profession.

The Evidence it Contains.

Let me try to give an epitome of the evidence contained in this essay and of the arguments founded upon it, and I will do so as far as possible in the author's own words. The essay commences, then, with the statement that the present inquiry has not been undertaken because of any doubt in the minds of well-informed members of the medical profession "as to the fact that puerperal fever is sometimes communicated from one person to another, both directly and indirectly." Such doubts the author would consider "merely as a proof that the sceptic had either not examined the evidence, or, having examined it, had refused to accept its plain and unavoidable consequences." "It signifies nothing," he says, "that wise and experienced practitioners have sometimes doubted the reality of the danger in question; no man has the right to doubt it any longer. No negative facts, no opposing opinions, be they what they may or whose they may, can form any answer to the series of cases now within the reach of all who choose to explore the records of medical science." In regard to the relevancy of negative facts he reminds his readers that "children that walk in calico before open fires are not always burnt to death." The instances to the contrary may indeed, like other truths, be "worth recording"; but not "if they are to be used as arguments against woollen frocks and high fenders."

There is another criticism that he foresees and forestalls. "It may be said that the facts are too generally known and acknowledged to require any formal argument or exposition." This objection he sweeps aside by a reference to two of the

* The credit of having been the first to make known in Great Britain the work that Semmelweis was doing in Vienna belongs to Dr. C. H. F. Routh, who, on his return in 1848 from a visit to that city, read a paper on the subject before the Royal Medical and Chirurgical Society of London (see *Med. Chir. Trans.*, 1849). In 1886 Dr. Theodore Duka, a fellow countryman of Semmelweis, contributed to the *Lancet* a most interesting and sympathetic account of his life and labours, founded on the more elaborate work of Bruck, which had then recently been published. Dr. Duka's paper was afterwards reissued in the form of a pamphlet.

† Two of the latest and most important American textbooks of obstetric medicine do not even mention him.

leading American obstetric treatises of the time: In one of these, *Dewees on the Diseases of Females*, a book at that time of very wide repute throughout the United States, it was expressly stated, in the very latest edition, that so far as that country was concerned, puerperal fever had not hitherto appeared under any circumstances that afforded "the slightest ground for the belief that it is contagious." In the other, *The Philadelphia Practice of Midwifery*, a work of scarcely less authority than Dewees, not one word could be found "in the chapter devoted to this disease, which would lead the reader to suspect that the idea of contagion had ever been entertained."

Having thus shown the need for the inquiry, he proceeds to formulate the thesis which, at its close, he finds himself in a position to defend. This thesis he presents in the following words: "*The disease known as puerperal fever is so far contagious as to be frequently carried from patient to patient by physicians and nurses.*"

Before submitting direct evidence in support of this statement he rids himself of certain *impedimenta* in the shape of side issues. For example, he at once admits the possibility that what is called puerperal fever may not be equally contagious in all its forms. He refuses to "enter into any dispute about the particular *mode* of infection, whether it be by the atmosphere the physician carries about him into the sick-chamber, or by the direct application of the virus to the absorbing surfaces with which his hand comes in contact."

He points out that it is not necessary in order to prove his thesis that "the contagion of puerperal fever must always be followed by the disease," it being "true of all contagious diseases that they frequently spare those who appear to be fully submitted to their influence." He grants, more readily perhaps than we should do nowadays, "that the disease may be produced and variously modified by many causes besides contagion, and more especially by epidemic and endemic influences."

And having thus cleared the air, he marshals his witnesses and proceeds to build up a mass of evidence, which, though its cogency was doubted at the time, as we shall presently see, has now long been acknowledged to be far beyond the reach of refutation. Let us very briefly pass this evidence in review. Some of the witnesses are familiar to us. Of these, most, if not all, are our own countrymen. But there are also witnesses—mostly American—whose evidence, though equally striking, has not found its way into our treatises and textbooks, and has not, therefore, in the same sense become common property. We will take, first, as the writer himself does, certain British witnesses, beginning with that fine old eighteenth-century surgeon, Charles White of Manchester.

The Witnesses Cited.

"I am acquainted," says White (1773), "with two gentlemen in another town, where the whole business of midwifery is divided betwixt them, and it is very remarkable that one of them loses several patients every year of the puerperal fever, and the other never so much as meets with the disorder." That the full significance of this circumstance was not perceived at the time, even by White himself, does not diminish its value as a piece of evidence. Then comes the more personal testimony of Dr. Gordon of Aberdeen,* published in 1795, which, though it has been quoted over and over again, is still worth repeating, not only because of its early date, but also because, as Holmes points out, Gordon's "expressions are so clear," and "his experience is given with such manly distinctness and disinterested honesty." "This disease," he says, "seized such women only as were visited or delivered by a practitioner, or taken care of by a nurse, who had previously attended patients affected with the disease." "I had evident proofs of its infectious nature and that the infection was as readily communicated as that of the small-pox or measles and operated more speedily than any other infection with which I am acquainted." "I had evident proofs that every person who had been with a patient in the puerperal fever became charged with an atmosphere of infection which was communicated to every pregnant woman who happened to come within its sphere." "It is a disagreeable declaration," he adds, "for me to mention that I myself was the means of carrying the infection to a great number of women." He cites a number of instances in which the disease was conveyed by midwives and others, and says: "These facts fully prove that the cause of the puerperal fever . . . was a specific contagion, or infection, altogether unconnected with a noxious condition of the atmosphere." But the most startling of his statements is still to come. "I arrived," he says, "at that certainty in the matter, that I could venture to foretell what women would be affected with the disease upon hearing by what midwife they were to be delivered, or by what nurse they were to be attended, during their lying-in; and almost in every instance my prediction was verified," a piece of evidence so terrible that Holmes printed the quotation in capital letters.

Armstrong of Sunderland.

The next witness summoned is Dr. Armstrong, of Sunderland, the author of a well-known essay on puerperal fever, in which he describes 43 cases as having occurred in Sunderland between January 1st and October 1st, 1813, of which number 40 were the patients of a single practitioner, and were all attended by himself and his assistant, whilst the remaining 3 were distributed amongst the same number of accoucheurs. The practitioner in whose practice the fever made such ravages had no hesitation in asserting that the disease, as it appeared in his practice, was highly contagious, and was communicable from one puerperal

* The present Professor of Midwifery in the University of Aberdeen—Dr W. Stephenson has caused the following inscription to be painted on the wall of his class room: "The infectious nature of puerperal fever was first demonstrated by Dr. Alexr. Gordon. Aberdeen, 1795." A short notice of Gordon, by his grandson, the late Professor of Materia Medica in Aberdeen, will be found in one of the volumes of the (old) Sydenham Society, entitled *Essays on the Puerperal Fever and other Diseases Peculiar to Women; Selected from the Writings of British Authors Previous to the Close of the Eighteenth Century.* Edited by Fleetwood Churchill. London, 1849. This volume also contains a reprint of Gordon's pamphlet.

woman to another. Like Dr. Gordon, he scorns to avail himself of any sheltering theory about epidemics or drains, and boldly and manfully makes his confession "However painful to my feelings," he writes, "I must in candour declare that it is very probable the contagion was conveyed in some instances by myself, though I took every possible care to prevent such a thing from happening the moment that I ascertained the distemper was infectious."

Other Evidence.

It would be tedious to give in detail the testimony of all the British witnesses much as I should like to do so. Let it suffice to say that statements of a precisely similar character to those already given are quoted from Davies, Gooch, Ramsbotham, Roberton, Blundell, Hutchinson, King, and Lee. The account by the second William Hey, of Leeds, of an outbreak which occurred in that town and its vicinity in the years 1809-12, Holmes does not appear to have seen.

"The recurrence," he says, "of long series of cases like those I have cited, reported by those most interested to disbelieve in contagion, scattered along through an interval of half a century might have been thought sufficient to satisfy the minds of all inquirers that here was something more than a singular coincidence. But if, on a more extended observation, it should be found that the same ominous groups of cases clustering about individual practitioners were observed in a remote country, at different times, and in widely separated regions, it would seem incredible that any should be found too prejudiced or indolent to accept the solemn truth knelled into their ears by the funeral bells from both sides of the ocean, the plain conclusion that the physician and the disease entered, hand in hand, into the chamber of the unsuspecting patient."

That such series of cases *had* been observed in the United States and even in his own immediate neighbourhood, he then proceeds to show. "Certainly nothing," he says, "can be more open and explicit than the account given by Dr. Peirson of Salem of the cases seen by him. In the first nineteen days of January, 1829, he had five consecutive cases of puerperal fever, every patient he attended being attacked, and the first three cases proving fatal. In March of the same year he had two moderate cases, in June another case, and in July another, which [last] proved fatal. 'Up to this period,' he remarks, 'I am not informed that a single case occurred in the practice of any other physician.'" It appears that Dr. Peirson had altogether in his practice 20 cases of puerperal fever, of which 4 were fatal.

In the year 1842 the attention of the College of Physicians of Philadelphia was called to the prevalence of puerperal fever of a peculiarly insidious and malignant character in the practice of one of the Fellows of the College. Every woman that this physician had attended during several weeks past was said to have been attacked by the fever, while no instance of the disease had occurred amongst the patients of any other accoucheur practising within the same district.

Dr. Condie, who reported the circumstances, although disposed to be sceptical as to the contagious nature of many so-called contagious diseases, declared himself convinced by the facts that had come under his notice that the puerperal fever in the particular form it had on this occasion assumed was certainly communicable. The physician referred to (Dr. Rutter) stated that a succession of cases having occurred in his practice he went away for a week, but that he could not readily believe in the transmissibility of the disease from patient to patient or from physician to patient, inasmuch as one of the very first patients he attended after his return was attacked by the fever and died, notwithstanding that he had not used on that occasion any article of clothing he had used before.

These remarks were made on May 3rd, 1842, and in a letter dated December 20th in the same year, Dr. Rutter speaks of having had a personal experience of nearly seventy of these "horrible cases," all of them within the past twelve months.* The obstetric physician to whom this letter was addressed suggests that this experience of Dr. Rutter's was in all probability simply due to the fact that the midwifery practice of the district was largely in his hands.

At another meeting of the same college "Dr. Warrington stated that a few days after assisting at an autopsy in a case of puerperal peritonitis in which he ladled out the contents of the abdominal cavity with his hands, he was called upon to deliver three women in rapid succession." All were attacked with puerperal fever. Soon afterwards "he saw two other patients, both on the same day, with the same disease. Of these five patients two died."

At the same meeting Dr. West related that Dr. Samuel Jackson, whilst practising in Northumberland County (U.S.A.) attended, in rapid succession, seven women who were all attacked with puerperal fever and of whom five died. "Women," said Dr. Jackson, "who had expected me to attend upon them, now becoming alarmed, removed out of my reach, whilst others sent for a physician residing several miles off. These women, as well as those attended by midwives, all did well." Only two other cases of death in childbed were known to have occurred during the same period within a radius of 50 miles, and both these deaths were afterwards ascertained to have been caused by other diseases. Dr. Jackson "underwent, as he thought, a thorough purification, and still his next patient was attacked with the disease and died. He was led to suspect that the contagion might have been carried in the gloves which he had worn in attendance upon the previous cases. Two months or more after this he had two other cases. He could find nothing to account for these unless it was the instruments for giving enemata, which had been used in one or two former cases, and were employed by these patients. When the first case occurred he was attending and dressing a limb extensively mortified from erysipelas, and he went immediately to the accouchement with his clothes and gloves most thoroughly imbued with its effluvia."

Up to this point the evidence adduced has been gathered entirely from published records. The next three items of evidence were now made public for the first time. All the cases referred to occurred in the State of Massachusetts, and two of the three series "in Boston and its immediate vicinity."

The history of the first of these three series is as follows:

On March 19th, 1842, a physician, Dr. C, made a *post-mortem* examination in the case of a man who had died after an illness of only forty-eight hours "with oedema of the thigh and gangrene extending from a little above the ankle into the cavity of the abdomen." Whilst conducting the autopsy Dr. C wounded himself slightly in the right hand. During the night immediately following he attended a patient in labour, the wounded hand being at the time very painful. The woman died of puerperal fever on the 24th. Dr. C. was unable to visit her after the confinement, being himself ill from the wound in his hand and unable to leave the house until April 3rd. On April 9th he delivered another patient, who died of childbed fever on the 14th. On April 10th and 11th he delivered two other patients, both of whom died, one on April 14th and the other on the 18th. On April 27th he attended a fifth patient, who died on May 3rd. On April 28th he delivered another woman who developed symptoms of puerperal fever, but recovered. He now left town for a few days, but on May 8th he again attended a confinement. This patient, the seventh of the series, became ill, but recovered. About July 1st this same doctor was called to deliver a patient in a neighbouring village. The woman died in two or three days. No other cases occurred at the time in the practice of any of the physicians in the town or vicinity. The nurse who laid out the body of the third patient was seized the same evening with sore throat and erysipelas from which she died in ten days. The nurse who had laid out the body of the fourth patient was taken ill the following day, and died in a week. Several cases of erysipelas occurred in the house where the autopsy mentioned above took place, soon after the examination. It may be mentioned as a remarkable circumstance that Dr. C.'s partner, who assisted at the autopsy and was in attendance upon all the cases of erysipelas that followed it, had 12 midwifery cases between March 26th and April 12th, the patients all making a normal recovery.

The second series of cases that Holmes now for the first time placed on record occurred in the year 1830.

A doctor attended a patient on February 4th, and she died on the 12th. During the following month the same physician delivered eight women, all of whom did well with one exception. This patient was confined on February 28th and died on March 8th. The doctor inspected the body on the 9th, and the night after he attended a lady, who became ill, and died on the 16th. On the 10th he attended another patient, who sickened but recovered. On the 16th he went direct from this patient's room to another confinement. The lady became ill and died on the 21st. On the 17th he made an autopsy in one of the fatal cases. On the 19th he delivered another lady, who sickened, and died on the 22nd. This made 6 cases with only one recovery. He now refused to attend any labour until April 21st, when, having thoroughly cleansed himself, he resumed his practice, and had no more puerperal fever.

He had had some previous experience of puerperal fever in his practice, but until now he had not entertained any suspicion that the disease could be communicated. When a few years afterwards he met with another series of cases, he tells his friend Dr. Storer, that he was so fully convinced of the communicability of the disease, that whilst in attendance on the cases he changed his clothes, and washed his hands in a solution of chlorinated lime* after each visit.

The third series of original cases mentioned in Holmes's

* *Medical Examiner*, Philadelphia, January 21st, 1843.

* It will be remembered that solution of chlorinated lime was the disinfectant used by Semmelweis.

essay occurred in his own city of Boston in the summer of 1842. A physician lost five cases in succession from puerperal fever between May 7th and June 17th. For two weeks previous to his first case he had been attending a severe case of erysipelas.

"This long catalogue of melancholy histories," says Holmes, "assumes a still darker aspect when we remember how kindly Nature deals with the parturient female when she is not immersed in the virulent atmosphere of an impure lying-in hospital, or poisoned in her chamber by the unsuspected breath of contagion."

He next goes on to "mention a few instances in which the disease appears to have been conveyed by the process of direct inoculation." He refers, for example, to the well-known story of Dr. Campbell of Edinburgh, who, in October, 1821, assisted at a *post-mortem* examination in a case of puerperal fever, carrying the pelvic viscera in his pocket to the class-room. The same evening he attended a woman in labour, and he was called to a second the following morning. Both these patients died, as well as three others whom he delivered within the next few weeks. Many others of his patients were seized with puerperal fever, and though they escaped with their lives, they did so only after a long illness.

This was not Dr. Campbell's only experience of the kind. "In June, 1823, he assisted some of his pupils at the autopsy of a case of puerperal fever. He was unable to wash his hands with proper care, for want of the necessary accommodation." On reaching home he found that two midwifery patients had sent for him. He went to their assistance without washing his hands or changing his clothes; "both these patients died with puerperal fever."

Further instances of a similar character are quoted from Roberton, Ingleby, Rigby, Merriman and others.

In an unsigned article in the *British and Foreign Medical Review* for January, 1842, said to have been from the pen of no less an authority than Dr. Rigby, there is narrated the following case:

> A young practitioner, contrary to advice, examined the body of a patient who had died from puerperal fever; there was no epidemic at the time; the case appeared to be purely sporadic. He delivered three other women shortly afterwards; they all died with puerperal fever, the symptoms of which broke out very soon after labour. He assisted to remove some coagula from the uterus of one of the patients of his colleague, and this woman was attacked in the same manner and died, whilst all the rest of his colleague's patients did well.

The case quoted from Dr. Merriman was related by him at a meeting of the Royal Medical and Chirurgical Society of London, and is worth briefly reproducing as it illustrates the risks an obstetrician runs of infecting his patients even if he is merely present at the *post-mortem* examination in a case of puerperal fever without taking any part in the manipulations. "Dr. Merriman was at the examination of a case of puerperal fever at two o'clock in the afternoon. *He took care not to touch the body.* At nine o'clock the same evening he attended a woman in labour; she was so nearly delivered that he had scarcely anything to do. The next morning she had severe rigors, and in forty-eight hours she was a corpse. Her infant had erysipelas and died in two days."

". . . add to all this," says Holmes, "the undisputed fact that within the walls of lying-in hospitals there is often generated a miasm, palpable as the chlorine used to destroy it, tenacious so as in some cases almost to defy extirpation, deadly in some institutions as the plague, which has killed women in a private hospital in London so fast that they were buried two in one coffin to conceal its horrors . . . which has led Dr. Lee to express his deliberate conviction that the loss of life occasioned by these institutions completely defeats the objects of their founders; and out of this train of cumulative evidence, the multiplied groups of cases clustering about individuals, the deadly results of autopsies, the inoculation by fluids from the living patient, the murderous poison of hospitals—does there not result a conclusion that laughs all sophistry to scorn, and renders all argument an insult?"

"It is true," he goes on to say, "that some of the historians of the disease . . . profess not to have found puerperal fever contagious. At the most, they give us mere negative facts, worthless against an extent of evidence which now overlaps the widest range of doubt, and doubles upon itself in the redundancy of superfluous demonstration."

The President of one of the principal American life insurance companies, on being asked his views as to insuring the life of the next patient of a doctor who had had a succession of ten, five, three, even two fatal cases in his practice, naturally replied that he would require a very large extra premium, if indeed he consented to take the risk at all. His examination of the recorded facts called forth just such expressions of indignation as might be expected, and just such as would soon have swelled into a general cry of horror had the hideous catalogue of cases, as Wendell Holmes says, ever been fully brought to the knowledge of the public.

THE EFFECT OF THE ESSAY.

It is not to be wondered at that such an *exposé* should rouse the active opposition of those members of the medical profession who were still unconvinced as to the contagiousness of puerperal fever. We owe, indeed, to the fact that such opposition did arise, and from very influential quarters, the republication of the essay in 1855, twelve years after its first appearance. Two widely-known professors in two of the largest medical schools of the United States had recently expressed their disbelief of the doctrine that it was the object of Holmes's essay to enforce. One of these was Professor Hodge, who held the Chair of Obstetrics in the University of Pennsylvania. The other was Dr. Meigs, Professor of Midwifery and the Diseases of Women and Children in Jefferson Medical College, Philadelphia.*

The former of these in an introductory lecture "On the Non-contagious Character of Puerperal Fever," delivered at Philadelphia on October 11th, 1852, summed up as follows:

> The result of the whole discussion will, I trust, serve not only to exalt your views of the value and dignity of our profession, but to divest your minds of the overpowering dread that you can ever become—especially to woman, under the extremely interesting circumstances of gestation and parturition—the minister of evil; that you can ever convey, in any possible manner, a horrible virus so destructive in its effects and so mysterious in its operations as that attributed to puerperal fever.

Thus, with fair words he waved, as it were, into space what he did not wish to believe; but his lecture was, at any rate, couched in unobjectionable and inoffensive language. Not so was the chapter in Dr. Meigs's volume which treated of *Contagion in Childbed Fever*. There are in it expressions which, as Wendell Holmes said, "might well put a stop to all scientific discussions were they to form the current coin in our exchange of opinions." But Holmes declined to take offence or attempt any retort. "No man," he says, "makes a quarrel with me over the counterpane that covers a mother with her newborn infant at her breast. There is no epithet in the vocabulary of slight and sarcasm that can reach my personal sensibilities in such a controversy." And so he contented himself, in an introduction to the reprinted essay, with a detailed examination of the various points raised by Dr. Meigs and with some remarks on the comparative worthlessness of negative evidence,† to which Dr. Meigs attaches undue importance. Dr. Meigs's position may be illustrated by a single quotation. Speaking of such outbreaks of puerperal fever as those described by Wendell Holmes, he says: "I prefer to attribute them to accident, or Providence, of which I can form a conception, rather than to a contagion of which I cannot form any clear idea, at least as to this particular malady." A practitioner who meets with epidemic cases is for him simply "unlucky." "We do not deny," says Holmes, "that the God of battles decides the fate of nations; but we like to have the biggest squadrons on our side, and we are particular that our soldiers should not only say their prayers, but keep their powder dry."

DEALING WITH OPPOSITION.

"The subject of my paper," he writes in 1855, "has the same profound interest for me at the present moment as it had when I was first collecting the terrible evidence out of which, as it seems to me, the commonest exercise of reason could not help shaping the truth it involved. It is not merely on account of the bearing of the question—if there is a question—on all that is most sacred in human life and happi-

*An excellent and impartial sketch of the life and work of both these professors will be found in Whitridge Williams's sketch of the *History of Obstetrics in the United States up to 1860*, pp. 38 to 46. (Published originally in Professor Dohrn's *Geschichte der Geburtshülfe der Neuzeit, zugleich als dritter*. Band des Versuchs einer Geschichte der Geburtshülfe, von Eduard von Siebold, Tübingen, 1903. Erste Abtheilung, pp. 193-264.)

†That is, exposure without subsequent disease. On this point Holmes quotes the following passage from the chapter on continued fever in Watson's *Lectures on the Practice of Physic*. "A man might say, 'I was in the battle of Waterloo and saw many men around me fall down and die, and it was said that they were struck down by musket-balls; but I know better than that, for I was there all the time, and so were many of my friends, and we were never hit by any musket-balls. Musket-balls, therefore, could not have been the cause of the deaths we witnessed.'" He also refers to the statement of John Hunter that he knew a case in which of twenty-one persons bitten by a rabid dog, only one died of hydrophobia.

ness, that the subject cannot lose its interest. It is because it seems evident that a fair statement of the facts must produce its proper influence on a very large proportion of well-constituted and unprejudiced minds. Individuals may, here and there, resist the practical bearing of the evidence on their own feelings or interests; some may fail to see its meaning, as some persons may be found who cannot tell red from green; but I cannot doubt that most readers will be satisfied and convinced, to loathing, long before they have finished the dark obituary calendar laid before them. I do not know," he continues, "that I shall ever again have so good an opportunity of being useful as was granted me by the raising of the question which produced this essay. For I have abundant evidence that it has made many practitioners more cautious . . . and I have no doubt it will do so still, if it has a chance of being read, though it should call out a hundred counterblasts. . . ." "I do not expect," he says, in another place, "ever to return to this subject. . . . I trust that I have made the issue perfectly distinct and intelligible; . . . this is no subject to be smoothed over by nicely-adjusted phrases of half assent and half censure divided between the parties. The balance must be struck boldly and the result declared plainly. If I have been hasty, presumptuous, ill-informed, illogical; if my array of facts means nothing; if there is no reason for any caution in the view of these facts; let me be told so on such authority that I must believe it, and I will be silent henceforth, recognizing that my mind is in a state of disorganization. If the doctrine I have maintained is a mournful truth; if to disbelieve it, and to practise on this disbelief, and to teach others so to disbelieve and practise, is to carry a desolation, and to charter others to carry it, into confiding families, let it be proclaimed as plainly what is to be thought of the teachings of those who sneer at the alleged dangers and scout the very idea of precaution. Let it be remembered that *persons* are nothing in this matter; better that twenty pamphleteers should be silenced, or as many professors unseated, than that one mother's life should be taken. There is no quarrel here between men, but there is deadly incompatibility and exterminating warfare between doctrines. . . . Let the men who mould opinions look to it; if there is any voluntary blindness, any interested oversight, any culpable negligence even, in such a matter, and the facts reach the public ear, the pestilence carrier of the lying-in chamber must look to God for pardon for man will never forgive him."

And thus, with burning words, forged at that white heat which alone befitted the subject, or could give adequate expression to the writer's intense convictions, he brings his introduction to a close. When one reads these words now—I hope I may say it without irreverence—it is impossible to avoid comparing them with the eloquent but terrible warnings of the preacher-prophets of the Hebrew scriptures. It was not the fault of these old stalwarts that the message they had to deliver was unillumined by a Gospel that had not yet been revealed, any more than it was the fault of Wendell Holmes and of the distinguished Hungarian who, a few years later, and as the result of independent observation, arrived at the same truth, and encountered an opposition even more relentless, that they were unable to point to the more excellent way of prevention with which we, thanks to later revelations, have since become familiar. Pasteur and Lister had not as yet instituted those researches of which the result has been to place in our hands, if we would but use it, an effectual means not only of checking the spread of puerperal fever but of stamping it out. All honour to the men who, without these researches to guide them, and merely from a study of clinical facts, recognized the main factors in the propagation of puerperal fever, and advocated for its prevention measures which differ in degree rather than in kind from those enjoined by the most scientific and advanced teachers of the present day.

Holmes and His Work.

It is interesting to remember, in connexion with recent discussions as to the time of life at which a man turns out his best work, that this essay was published when Holmes was 33. It was not until four years later that he was appointed to the Parkman Professorship of Anatomy and Physiology in the medical school of Harvard University, which he described as being not so much a chair as a whole settee, and the anatomical portion of which he retained for thirty-six years. He had as yet no literary fame except such as was derived from his contributions to various college magazines, a prize essay on Intermittent Fever in New England (1837), and two lectures on "Homoeopathy and its Kindred Delusions," delivered before the Boston Society for the Diffusion of Useful Knowledge in 1842. But if any one is inclined to quote this early essay of Wendell Holmes as a proof that a man produces his best work before he is 40, it must be remembered, on the other side, that the earliest chapters of the Breakfast Table series, which first made him famous and upon which his literary reputation really rests, were not published until the year 1857, when their author had reached the comparatively mature age of 48.* Nevertheless, the fact that a physician comparatively so young and so obscure, and with as yet no official position† to ensure him a hearing, should have dared to publish such an essay as that on puerperal fever, shows him to have been endowed with singular courage and remarkable independence of judgement.

The question was once put to him in a Canadian journal, by an enthusiastic admirer bearing the honoured name of William Osler, whether he had derived the greater satisfaction from having been the author of that exquisite little poem, *The Chambered Nautilus*, or from having published the *Essay on Puerperal Fever*. The journal reached Wendell Holmes, who thereupon wrote to Professor Osler a letter, the original of which he has, with characteristic kindness, placed in my hands to show to you on this occasion.

The letter bears date January 21st, 1889, and is as follows:

I have rarely been more pleased than by your allusion to an old paper of mine. There was a time, certainly, in which I would have said that the best page of my record was that in which I had fought my battle for the poor poisoned women. I am reminded of that essay from time to time, but it was published in a periodical which died after one year's life, and therefore escaped the wider notice it would have found if printed in the *American Journal of the Medical Sciences*. A lecturer at one of the great London hospitals referred to it the other day, and coupled it with some fine phrases about myself which made me blush either with modesty or vanity, I forget which.‡

I think I will not answer the question you put me. I think oftenest of *The Chambered Nautilus*, which is a favourite poem of mine, though I wrote it myself. The essay only comes up at long intervals. The poem repeats itself in my memory, and is very often spoken of by my correspondents in terms of more than ordinary praise. I had a savage pleasure, I confess, in handling those two professors—learned men both of them, skilful experts, but babies, as it seemed to me, in their capacity of reasoning and arguing. But in writing the poem I was filled with a better feeling—the highest state of mental exaltation and the most crystalline clairvoyance, as it seemed to me, that had ever been granted to me—I mean that lucid vision of one's thought and all forms of expression which will be at once precise and musical, which is the poet's special gift however large or small in amount or value. There is more selfish pleasure to be had out of the poem, perhaps a nobler satisfaction from the life-saving labour.§

In this letter he purposely walks round the question. What was his real feeling, however, towards the essay is shown quite unmistakably in a passage in *The Professor at the Breakfast Table* and from a later letter. The passage in the *Professor* is as follows: "By the permission of Providence, I held up to the professional public the damnable facts connected with the conveyance of poison from one young mother's chamber to another's—for doing which humble office I desire to be thankful that I have lived, though nothing else good should ever come of my life. . . ."

The later letter of which I have spoken has a special interest and pathos, for it was written in 1893, the year before he died, when he was an old man of 83, and could speak of the essay as having been written fifty years ago. Will you bear with me while I read it? It is addressed to his old friend and mine, the late Dr. J. R. Chadwick,‖ of Boston, the founder and for many years the indefatigable secretary of the American Gynaecological Society:

296, Beacon Street,
May 8th, 1893.

Dear Dr. Chadwick,

You tell me that the President of the Gynaecological Society, Dr. Parvin, proposes to make mention at the meeting of the Society next Tuesday of my early efforts to put a stop to the conveyance of the poison which produces puerperal fever from one lying-in woman to another by the practitioner to whose care she is entrusted.

It is just fifty years since my essay on the contagiousness of puerperal fever was published in the *New England Journal of Medicine and Surgery*. It had been previously read at a meeting of the Boston Society for

* It is true he had written a few stray chapters in a similar vein twenty-five years previously, but these attracted no attention, and may practically be left out of account.

† He did not become Parkman Professor at Harvard until 1847.

‡ There appears to be good reason for supposing that this allusion has reference to a passage at the close of an address on the preventability of puerperal fever, which I delivered at St. Thomas's Hospital in October, 1888.

§ *Johns Hopkins Hospital Bulletin*, October, 1904.

‖ Dr. Chadwick's death took place whilst this address was being written. A sympathetic notice of his life and of the great work he accomplished for the Boston Medical Library was contributed by Professor Osler to the *Lancet* for October 14th, 1905.

Medical Improvement, at whose request it was given to the press. The periodical in which it appeared, though well conducted and promising well, died in its first year, having been seen by few readers and supported by few subscribers. My warning and counsel had their effect, however, in our own community, as I have often been assured by competent authorities. The essay attracted respectful notice abroad, as the names of Copland and Ramsbotham and the fifth annual report of the Registrar-General of England sufficiently show. Still, it was not read by some who might have profited by it. If Dr. (afterwards Sir James) Simpson had read the first rule I laid down* he would not have left the record that after freely handling the diseased parts from an autopsy of a victim of puerperal fever his next four childbed patients were affected with that disease.

I thought I had proved my point and set the question of the private pestilence, as I called it, at rest "for good and all." I thought I had laid down rules which promised to ensure the safety of the lying-in woman from disease and death carried to her unconsciously by her professional attendant.

Still, I was attacked in my stronghold by the two leading professors of obstetrics in this country.

I defended my position with new facts and arguments and not without rhetorical fervour at which after cooling down for half a century I might smile if I did not remember how intensely and with what good reason my feelings were kindled into the heated atmosphere of superlatives.

I have been long out of the way of discussing this class of subjects. I do not know what others have done since my efforts; I do know that others had cried out with all their might against the terrible evil before I did and I gave them full credit for it.

But I think I shrieked my warning louder and longer than any of them and I am pleased to remember that I took my ground on the existing evidence before the little army of microbes was marched up to support my position.

Always yours cordially,

OLIVER WENDELL HOLMES.

THE STATE OF THINGS TO-DAY.

I have endeavoured in this address to remind my medical brethren of the great service rendered by Oliver Wendell Holmes to science and to humanity by the publication of his remarkable essay. In doing so it may seem to some that I have been needlessly disturbing the ashes of a painful and forgotten controversy, and labouring at a subject that has long ago ceased to have any living interest. But has the subject of the propagation of puerperal fever ceased to have a living interest? Let us look for a moment at the facts. During the fifty-seven years (1847-1903) for which the statistics for England and Wales are available, there were registered no fewer than 93 243 mothers as having died from puerperal septicaemia, and the enormous sacrifice that these figures represent has been going on steadily all the time, and shows no signs of undergoing diminution. If it has changed at all it is in the direction of increase.†

When, in my opening address as President of the Obstetrical Society of London in 1897, I called attention to this state of things, my lamented friend, the late Dr. Milne Murray of Edinburgh read my words, as he afterwards told me, with much surprise, and with the feeling that however true they might be as regarded England and Wales, they certainly could not be true of Scotland. He made inquiries, however, and found, to use his own words, that he, like many others, had been living in a fool's paradise, and that matters were not one whit better in Scotland than they were in England. And let it be remembered that the returns of the Registrar-General take no account of the vast array of non-fatal cases with their train of suffering and often of permanent ill-health, or of the many fatal cases that, for various reasons, have been attributed on the death certificate to some other than the true cause; but even taking the certified cases only, we have to face the unpleasant fact that the lives of more than 2 000 women are annually lost in England and Wales from a cause which is almost, if not entirely, preventable, and that puerperal fever continues to prevail, as though Pasteur and Lister had never lived. And if, notwithstanding all this, any one be inclined to urge that at least tragedies such as those which Holmes recorded are things of the past, let him refer to the second of Dr. Williams's Milroy Lectures for 1904, or to Mr. Foulerton's paper in the *Practitioner* for March, 1905, and he will be undeceived.

Some of these tragedies occur, no doubt, in the practice of ignorant and untrained midwives. Dr. Williams shows this to be strikingly the case in Glamorganshire, where most of the midwifery is in the hands of these women; but as I showed in the presidential address already alluded to, we cannot to use Dr. Milne Murray's striking figure, make the midwife the only scapegoat, and send her into the wilderness bearing the whole burden. There is, I am afraid, no doubt that much of the general mortality from puerperal fever and even a certain proportion of the tragedies* occur in ordinary private practice.

After speaking of the lying-in institutions of this country, and of their condition being so vastly improved that their mortality from puerperal fever has "decreased almost to a vanishing point," Dr. Williams goes on to say that, such being the case, we are "forced to the conclusion that this undiminished mortality takes place outside these institutions—that is, in general practice—and that the conditions under which women are confined outside" have, for some reason or other, "not shared to an equal degree the improved methods and care adopted" within their walls.

CONCLUSIONS.

This is not the occasion to discuss at any length the causes to which the persistence of puerperal fever in private practice is to be attributed. I have said my own say on the subject elsewhere, and will content myself to-day with calling attention to the extremely suggestive words uttered by Dr. Milne Murray in his Presidential address to the Obstetrical Society of Edinburgh in 1900. "Why," he asks, "are the results of private practice becoming worse and worse in spite of all that has been done for our science and art during the closing century? . . . I feel sure," he says, "that an explanation of much of the increase of maternal mortality from 1847 onwards will be found in, first, *the misuse of anaesthesia*, and second, *in the ridiculous parody which, in many practitioners' hands, stands for the use of antiseptics* . . . Before the days of anaesthesia interference was limited and obstetric operations were at a minimum, because interference of all kinds increased the conscious suffering of the patient. . . . When anaesthesia became possible and interference became more frequent because it involved no additional suffering, operations were undertaken when really unnecessary, on the demand of the patient or for the convenience of the practitioner. And so complications arose and the dangers of labour increased. . . . Then came the antiseptic era. Here now was the panacea for all the dangers of childbed. All that was necessary was to dip the instruments for a few minutes in a carbolic lotion, and the hands in one of half the strength for half the time, and all danger was at an end. . . . Normal labour," he continues, "is a natural process which is best left to itself, and the less the patient is disturbed with the paraphernalia of obstetrics, before or after, the better. . . . Until men realize this and recognize the fact that the simplest obstetric operation

* See Appendix.

† Writing in April of the present year (1905) Dr. Boxall, one of our leading medical statisticians, states that the death-rate from puerperal septic diseases has, if anything, shown a tendency to increase in each division of the kingdom (England, Scotland, and Ireland).

* In an introductory address, delivered at St. Thomas's Hospital in 1888, I stated that I knew of a country town not very far from London, where, within the preceding twelve months, puerperal fever had singled out the patients of one of the local practitioners exactly as it did in the instances I had been narrating. For six months every patient that he attended (with the exception of two in whom labour was over before his arrival, and whom, therefore, it was unnecessary for him to touch) had died of puerperal fever (*Puerperal Fever a Preventable Disease*, London, 1888, p. 32). The circumstances were at that time of too recent occurrence to permit of my giving the details, but I know of no reason why they should any longer be withheld. The facts, then, are as follows: The wife of a labourer in a Sussex village was delivered with instruments some time in 1886 and died of blood-poisoning. Shortly afterwards a young woman, the wife of a master miller, was attended in her second confinement by the same doctor, and died in ten days. In December, 1887, another labourer's wife in the village was confined, and died of blood-poisoning. The same doctor had attended her. On January 21st, 1888, at 9.30 A.M., he delivered a coachman's wife. Instruments were used, and the assistant was summoned to render additional help. The patient died of blood-poisoning at 9 P.M. on January 29th. The nurse who had been in attendance along with the doctor on all these cases was now warned to attend no more cases for six weeks. The doctor continued his practice as usual, and, so far as is known, adopted no means to rid himself of infection. On or about February 9th he attended a young farmer's wife, aged 23, the daughter of a clergyman, in her third confinement; she died of blood poisoning on the tenth day. People now began to be afraid of employing the doctor, but, in the month of May he was called to attend a labourer's wife of middle age, who had last been confined nine years previously. Some difficulty occurred, and after endeavouring for some time, and with the help of two assistants, to deliver with the forceps he decided that the child's life must be sacrificed. Whilst the best means of procedure was being discussed a living child was born spontaneously. The mother, however, died of blood poisoning at the end of ten days. These last four were all the cases attended by the doctor in question between December, 1887, and June, 1888, with the exception of two, in each of which he arrived too late, the nurse doing all that was necessary. One of these cases occurred in December, 1887, the other at the end of January, 1888. Both of them escaped—a circumstance which was quoted on the doctor's behalf as a proof that he could not have been the source of infection in the fatal cases. Ultimately, however, he was prevailed upon to go away for a time, and shortly afterwards he left the neighbourhood. During the period covered by the above series of cases no case is known to have occurred in the practice of any other doctor (or of any midwife) in the district.

demands not one whit less of care as to antiseptic precautions than is required of one before opening the abdomen, we shall get no further forward. When the practical obstetrician realizes his responsibility, and that no small share of this terrible maternal mortality of a certainty lies at his door, he has made the first step towards true progress. When he realizes that labour is a natural process which in the great majority of cases it is criminal to disturb; when he realizes that every interference increases the inherent danger a hundredfold; and when under this consciousness he brings with him to the lying-in room all that is possible of those principles of antiseptic surgery which have been at the bottom of the triumphs of modern gynaecology, we shall not have long to wait for the lightening of the dark cloud which hangs over us now."

And with these weighty words of my deceased friend I must conclude. I trust that, the mortality from puerperal fever in Great Britain being what it is, you will not think it has been either ill-timed or out of place to recall that noteworthy chapter in the history of the subject of which it has to-day been my great privilege to speak. No sermon can be considered complete without at least a few words of application, and so I feel sure I shall be forgiven for attempting to apply the lesson of Holmes's essay to the circumstances of to-day, and to show that there is needed now, as there was needed then, a strong voice to rouse us from our lethargy, and to plead with desperate earnestness for the lives that are still being quite unnecessarily sacrificed.

"If," says Dr James Jamieson, "anything in the field of practical medicine can be taken as proved, it is that puerperal fever is a preventable disease, and the means of prevention at our disposal are both more reliable and more easily accessible than in the case of almost any other of the infectious diseases."* If that be true, and I for one firmly believe it is, surely we ought not to be satisfied until puerperal fever has been banished from amongst us.

NOTE.—The references, necessarily numerous, are omitted here owing to exigencies of space. They will, however, duly accompany the address when it is printed in pamphlet form.

APPENDIX.

Conclusions, including rules for preventing the spread of puerperal fever by infection, published at the end of Holmes's Essay (1843):

1. A physician holding himself in readiness to attend cases of midwifery should never take any active part in the *post-mortem* examination of cases of puerperal fever.
2. If a physician is present at such autopsies, he should use thorough ablution, change every article of dress, and allow twenty-four hours or more to elapse before attending to any case of midwifery. It may be well to extend the same caution to cases of simple peritonitis.
3. Similar precautions should be taken after the autopsy or surgical treatment of cases of erysipelas, if the physician is obliged to unite such offices with his obstetrical duties, which is in the highest degree inexpedient.
4. On the occurrence of a single case of puerperal fever in his practice, the physician is bound to consider the next female he attends in labour, unless some weeks at least have elapsed, as in danger of being infected by him, and it is his duty to take every precaution to diminish her risk of disease and death.
5. If within a short period 2 cases of puerperal fever happen close to each other, in the practice of the same physician, the disease not existing or prevailing in the neighbourhood, he would do wisely to relinquish his obstetrical practice for at least one month, and endeavour to free himself by every available means from any noxious influence he may carry about with him.
6. The occurrence of three or more closely connected cases, in the practice of one individual, no others existing in the neighbourhood, and no other sufficient cause being alleged for the coincidence, is prima facie evidence that he is the vehicle of contagion.
7. It is the duty of the physician to take every precaution that the disease shall not be introduced by nurses or other assistants, by making proper inquiries concerning them and giving timely warning of every suspected source of danger.
8. Whatever indulgence may have been granted to those who have heretofore been the ignorant cause of so much misery, the time has come when the existence of a *private pestilence* in the sphere of a single physician should be looked upon not as a misfortune but as a crime, and in the knowledge of such occurrences the duties of the practitioner to his profession should give way to his paramount duties to society.

* Childbirth Mortality in the Australian Colonies, *Australian Medical Journal*, October 15th, 1887.

Clinical Remarks

ON

SOLITARY NON-PARASITIC CYSTS OF THE LIVER.

By J. BLAND-SUTTON, F.R.C.S.,
Surgeon to the Middlesex Hospital, and to the Chelsea Hospital for Women.

ALL cysts in the pathologic limitation of the term arise in pre-existing epithelium-lined spaces and ducts: even ducts and canals of microscopic proportions are often the source of cysts of such dimensions that they cause so much inconvenience and distress as to necessitate surgical treatment. This may be illustrated by the liver, an organ thoroughly permeated by minute passages—the bile canals.

Two forms of cysts are found in the liver arising from its canals and ducts, namely, multiple cysts and the solitary cyst. Each will be separately considered.

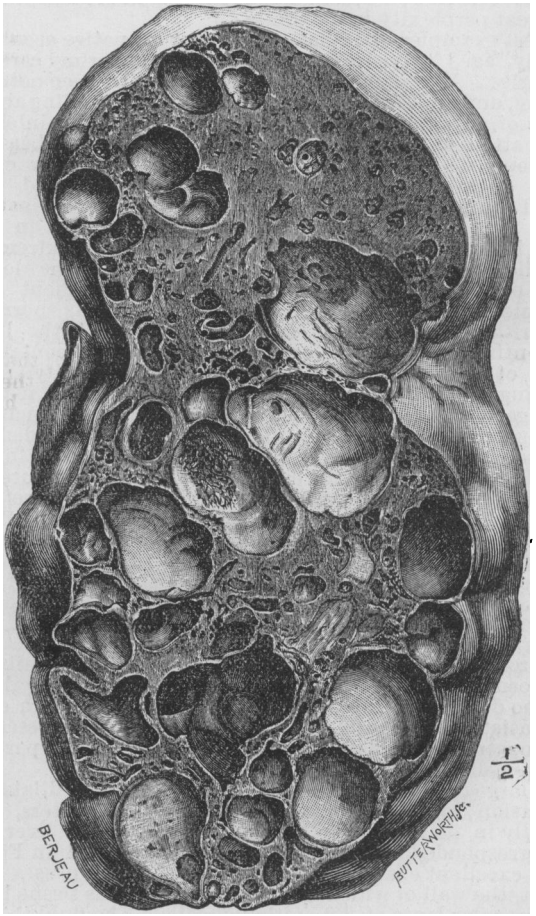


Fig. 1.—A liver shown in section. The spaces on the cut surface are dilated bile canals. From a woman 46 years of age. (Museum of the Royal College of Surgeons, London.)

1. MULTIPLE CYSTS.

This variety has long been recognized by pathologists under the term general cystic disease of the liver. In typical examples the liver is converted into a huge honeycomb like mass (Fig. 1). The cavities vary greatly in size—some are as small as grape-seed, others may exceed a ripe cherry in size. The cysts may project on the surface of the liver, but though this organ may be enormously enlarged, and weigh 35 lb., yet its shape is preserved. The smaller cysts are lined with epithelium. This is best seen in specimens with the cystic change in an early stage when the dilated canals look like sharp definite punctures in the liver substance. As the cysts increase in size and number the hepatic tissue is encroached upon, and appears as narrow bridges between large tracts of