REPORT ON CHOLERA.

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It might fairly be expected that a considerable amount of practical instruction would be supplied by a pathological report founded on the combined experience of the members of the Provincial Medical and Surgical Association, and yet it is a painful duty to record the fact that, with regard to the nature, history, treatment, and prevention of the disease which has formed the subject of this inquiry, very little new information has been gleaned. It may appear ungracious thus coldly to acknowledge the courtesy and zeal of the many able and industrious practitioners who have so promptly answered the queries submitted by the Council, but the result is thus plainly stated, because truth is the only basis on which investigations of any kind can be established, and the test by which all conclusions must be judged.

The design of the following pages is to shew as concisely as possible *what deductions* may legitimately be derived from the united testimony of those associates who have kindly contributed their aid.

FIRST—AS TO THE NATURE OF CHOLERA.

It seems to be almost unanimously believed that the disease depends upon a *bonâ fide* choleraic virus, which being admitted into the system produces its own specific consequences, unnecessary here to be enumerated. The mode of admission is undecided. It may probably be received indirectly through the medium of pulmonary or cutaneous absorption; or directly, by the act of swallowing, into the alimentary canal; or it may be introduced by each of these processes under different circumstances. Another point almost unanimously believed is the facility of the communication of the disease, either from infected persons or infected localities. This conclusion is founded on *many facts*, which prove that the poison may be transferred—1st. Through the medium of the atmosphere. 2ndly. Through the medium of clothing; and, 3rdly, through the medium of the evacuations.

The *precise character* of the poison does not appear to be in the slightest degree understood. Various suppositions have been entertained concerning it, but none are satisfactory. It has been suggested that *sulphuretted hydrogen* is the production upon which cholera is mainly dependent, but that this idea is not consistent with sound reasoning is proved by the fact that sulphuretted hydrogen and the other gases which are evolved during the decomposition of animal and vegetable substances, may be generated for *years* in a given locality, without a single case of genuine cholera being observed in those who are exposed to the constant inhalation of the vitiated air; so that no gas of which

our senses are cognizant is adequate, *per se*, to originate the malady.

It is possible that very slight circumstances may so modify the combination of atmospheric elements as to produce the malignant poison, but we do not yet know what these modifications are, or how they are determined. We are acquainted with many poisons whose ultimate elements are present in the air, or accessible to contact with it, and which, when peculiarly combined, become highly destructive of animal life. It is therefore fair to conclude that in the subtle laboratory of nature many other combinations may be called into temporary existence, and produce most fatal ravages. We cannot tell *how* the combinations are effected any more than we can tell *why* oxygen and hydrogen may be preserved for any length of time in a glass vessel without combining, and the combination be instantly determined by a piece of spongy platinum, or by an electric spark.

A very important question connected with the choleraic virus may here be mentioned, viz., how far it is connected, either in its formation, operation, or propagation with cess-pools, burial grounds, defective ventilation, and want of cleanliness. It must be remembered that although these circumstances may have some specific alliance with the poison, there is another mode in which they may act most powerfully, viz., by predisposing the system to become specially susceptible to *all* morbid agencies. It is well known that the *poorer* part of our population is chiefly liable to these unfavourable influences, and that with proximity to cess-pools, and permanent atmospheric impurity, are *generally* associated defective nourishment, personal uncleanness, and intemperate habits. With such an array of debilitating causes in operation it is not surprising that cholera, like other diseases, should rage most fatally and extensively among those on whom these causes are ceaselessly acting; and it will be seen, when we consider the history of the epidemic in 1849, that from the data on which this report is founded, *no other essential link has hitherto been established*. Nor is there any evidence by which it can be proved whether the deleterious principle be of *animal, vegetable, or meteorologic* origin, since, like hydrophobic and other elements, it has never yet been isolated so as to become palpable to our senses.

Nor is anything proved as to the peculiar condition of the atmosphere at the time that cholera prevailed, electrical and other changes having been noticed in some localities, for which *unsuccessful* search was made in others where the malady was equally destructive. A diminution of the disease, coincident with thunderstorms and reduction of atmospheric temperature, was partially observed. A peculiar blighting of vegetable crops in the vicinity of the disease has also been occasionally witnessed, but no inference can be drawn from either phenomenon of so limited an application. The operation of the poison does not seem to have been uniform. In some cases the characteristic flow from the gastro-intestinal membrane was entirely absent, and the patients sank immediately, as if the nervous system

were suddenly paralyzed. In a few instances, on the other hand, there appeared to be a distinct period of incubation, extending over several days, calculated from the time of supposed contact with infection to the development of the symptoms.

It may be stated with regard to the degree of the *strength* of the poison, that as a rule the virulence was more marked in the first cases which occurred in a given district, especially when they could not be traced to human communication, but seemed to arise primarily in an atmosphere not known to be contaminated.

The only additional point that requires notice as to the *nature* of malignant cholera is this,—that chlorine in a diluted state did not invariably neutralize the poison, it being shewn that three patients were attacked in the hospital for consumption whose daily custom it was to inhale that gas.

THE HISTORY of the cholera (in 1849) supplied by the members of the Provincial Association, represents the disease as prevailing, more or less, during the months of June, July, August, September, and October. It does not appear to have been governed by any laws of which we are cognizant. Its ravages were witnessed in many places which we should, *à priori*, have expected would have been exempt from its incursion; and many places, where it might have been expected to be fatal, either entirely or partially escaped; and other places, concerning which no *à priori* opinion could be formed, suffered, or did not suffer, without the existence of any *apparent* causes either for immunity or attack.

It generally happened that the same districts which suffered in 1832 were also attacked in 1849. But there were some exceptions in favour of 1849, owing, it was supposed, to sanitary improvements effected in the interval. Yet it is remarkable, that in some instances the very parts of a town so improved were severely attacked in 1849; and that the parts which remained unimproved since 1832 *escaped*. Again, it is clear, beyond controversy, that the disease “broke out” in an extraordinary manner in some of the highest and most salubrious situations, where drainage was perfect, ventilation perfect, and the inhabitants had previously been in a state of unusual health; and where no communication with infected persons or houses could be traced. The effect has been compared to that which might be produced by the “rushing of a poisonous blast,” striking down every hapless victim that lingered in its mysterious pathway.

The *course* pursued by the disease was sometimes as singular as its origination, and could not be accounted for either by atmospheric phenomena, contact with the sick, debility of constitution, or irregularity of diet.

The *mode* of attack was likewise very variable. In some cases the seizure was sudden, and proved fatal without preliminary warning; while in other cases there were earlier symptoms, between which and the occurrence of the marked features of malignity many hours and even days intervened.

The *disappearance* of the disease was also as strange

as its commencement and progression. Solitary cases were observed—isolated deaths were recorded, among the inhabitants of thickly-populated districts; and when the work of destruction was completed upon a few devoted sufferers, the destroying angel seemed to take his flight.

But while there is a sufficient number of *exceptional* instances to envelope the onset, progress, and disappearance of the disease in insuperable difficulties, there is also a sufficient number of *general* facts from which one or two important conclusions may be derived.

The first is this—that the vast majority of attacks occurred in persons whose systems were in a condition of *unquestionable debility*; hence the districts occupied by the most debilitated inhabitants would necessarily become the special seats of the disease; and this was so, as verified by abundant testimony. It is in such districts that nuisances of every kind predominate, and are constantly producing the enervation both of body and of mind. If these sources of injury were *frequently* identified with the ravages of *cholera*, and it could be demonstrated that the disease held its exclusive sway where they existed, or at intervals more brief than were observed under more auspicious circumstances; or indeed that the genuine symptoms were *ever* manifested in these ill-favoured spots, excepting at the very self-same time that those symptoms prevailed in *other* places, after a similar immunity for *years*, there would be some legitimate reason for believing that the decomposition of animal and vegetable substances had a specific connection with *the* epidemic. It is hoped that a quotation from the last edition of Dr. Christison’s admirable work on poisons will not be regarded as inappropriate:—“The great Nackery of Montfaucon, which has existed close to the walls of Paris for several centuries, is an enclosure of many acres, where the contents of the necessaries of the city are collected in enormous pits; and where horses, dogs, and cats are flayed, to the amount of forty or fifty thousand annually. The fat is melted for blow-pipe lamps; the bones are in a great measure burnt upon the premises for fuel; the intestines are made into coarse gut for machinery; the flesh, blood, and garbage are heaped to putrefy for manure; and in summer a bed of compost is spread to breed maggots for feeding poultry. There is no drain. Description cannot convey an idea of the stench: yet the workmen and their families are stout, healthy, and long-lived.”

It must be specified that the debility which was, in so marked a degree, a *predisposing* cause of cholera, was not limited, as to its origin, to impurity of atmosphere and its accompaniments. It was found that persons *depressed* from *any* cause were peculiarly susceptible of the disease. Thus fear, fatigue, night-watching, and a variety of enfeebling influences, each produced, in those who were otherwise favourably circumstanced, such a lowering of the constitutional stamina as rendered them easy victims of the “pestilential air.”

It does not appear that any practical deduction can be drawn as to the absence or presence of *premonitory*

symptoms in malignant cases. Slight diarrhoea and sickness frequently existed during several days before the supervention of rice-water drainings and collapse: and in a great number of cases the severer indications commenced without any previous intestinal disturbance.

It is difficult to say whether, in some instances, the preliminary diarrhoea was a part of the subsequent disease, or whether it only acted as a debilitating agent, rendering the patients more susceptible of the poison, to the influence of which they were exposed, both during and after the presence of the diarrhoea. There are some cases which seem to admit of this interpretation, for in some infected houses apparently simple diarrhoea existed for many days in persons, who afterwards sank under the malignant disease; giving the impression that had they been removed from the sphere of infection, when the relaxation began, they might have escaped the more formidable invasion, as others appear to have done under similar circumstances; their systems not yielding to the specific virus till weakened by the preceding looseness. On the other hand, it must be stated that a *few* of our learned brotherhood thought that a *moderate* diarrhoea was a *preservative* against the more serious malady, and consequently prescribed purgatives in preference to astringents. But there is an overwhelming majority in opposition to this very *lax* idea.

THE SECOND important deduction from the general history of cholera is its undoubted *communicability* from one human being to another, both through the medium of the *atmosphere*, the *clothes*, and the *evacuations*.

Attention has been especially directed to the extensive propagation of the disease through the medium of *ingesta* contaminated by choleraic discharges. The most remarkable examples are furnished by cases in which the discharges were introduced into fluids used as beverages, particularly *water* and *milk*. The names of Dr. Snow and Dr. Norris must be mentioned in connection with these very important and interesting observations. Several pamphlets have been published by Dr. Snow, and will repay the study of all who wish to gain information on the subject. It is impossible to enumerate in a brief report the *details* by which the communicability of cholera is *proved*; suffice it distinctly to assert, that the aggregate of evidence supplied by the members of the Association decidedly preponderates in favour of contagion, infection, or any other term which expresses the *transmission* of the disease from person to person, and from place to place. Negative examples prove little or nothing when compared with an overwhelming weight of *positive* facts. It would be absurd to deny the salivating power of mercury, because *some* persons can resist its influence; or to contend that opium is not a narcotic, because in some idiosyncracies its only effects are those of wild excitement.

The next point to be noticed in the history of cholera is its connection with other diseases, viz., diarrhoea, dysentery, and typhus.

There is no doubt that during much of the time at

which cholera was prevalent an extraordinary amount of bilious diarrhoea was prevalent also, both in places where the former malady did and did not commit its ravages; but there is no proof that the diarrhoea was specific, inasmuch as in some localities it was not in a single instance attended or followed by rice-water evacuations, vomitings, cramps, and collapse. And in those districts where *cholera* was prevailing it would be difficult to shew that the bilious diarrhoea *became choleraic*, apart from the possibility of exposure to the malignant virus. It must, therefore, be admitted that the identity of autumnal diarrhoea with the graver epidemic is not established. The fact of autumnal diarrhoea occurring *regularly between 1832 and 1849*, without "running into" true cholera, certainly seems to indicate a wide distinction between the nature of the two diseases; although it is not improbable that when cholera is prevalent there may be an atmospheric condition calculated to increase the general susceptibility to intestinal derangements.

The evidence with regard to dysentery shews the existence of that disease in many places both before, during, and after the appearance of cholera. It also existed in some places where very few cases of cholera occurred; also in some places that were exempt from cholera; and in one or two instances it abdicated the throne which it had held in former years, and cholera "reigned in its stead."

It is suggested that in a large and healthfully-situated workhouse (in Exeter) malignant dysentery was communicated by a case of cholera, and was very fatal, collapse supervening rapidly. As only *one* case of cholera is specified as the centre of emanation, and as a case of dysentery had preceded the case of cholera in that establishment, the proof must be considered as, at least, uncertain.

In a few places typhus was also coincident with cholera; but there are no data from which any definite inference can be drawn. It might be anticipated that the districts and class of persons chiefly attacked by cholera would, under ordinary circumstances, be most liable to suffer from low fever; the debilitating causes which produced so remarkable a predisposition to the former disease being exactly similar to those which favour the propagation and mortality of the latter. And such was the case.

It was noticed in some neighbourhoods that the *type* of *all* diseases was peculiarly *lowered* for a few months before the cholera appeared; and that after the epidemic ceased the typhoid type was gradually removed, and acute diseases resumed their sthenic character.

It is singular that the majority of cholera cases commenced in the night; the reduction of atmospheric heat has been proposed as the cause. It may be that the resisting power of the system is at its weakest point during the first hours of sleep, and that at that point the operation of a poison would be most easily developed.

The mortality of cholera in 1849 may be thus simply expressed:—*Half* the patients who were attacked by the disease in its *indisputable* form *died*. This is as near an approximation to the truth as appears to be

either feasible or necessary. In 1832 the proportion of deaths was considerably less, although the number of persons attacked was in *most* places greater. The last epidemic seems to have been more malignant in its character, but more limited in its diffusion. The question—Why?—must remain unanswered in our present state of ignorance.

The *post-mortem* examinations were very few. The only important information derived from them, in addition to what was previously well known, is the fact that the state of the intestines was such as must have rendered *any* mode of *treatment* inefficient. They are described as being “either filled with a gruelly fluid, or else coated with a tenacious pasty layer of mucus.” Under such circumstances it would be scarcely possible for any medicines to come in contact with the mucous membrane, so as to operate locally, or by absorption. They would probably glide rapidly through the alimentary canal, without producing the least effect.

Signs of organic life shortly after apparent death were in a few instances observed.

Having thus glanced at the nature and history of cholera we come to the *third*, and it is to be feared the most vexatious and unsatisfactory division of the subject, viz.,

THE TREATMENT.

It is almost needless to assert, for every one is already acquainted with the truth, that nearly everything both in and out of the Pharmacopœia was tried, and in some cases nearly all the preparations that could be shaken in a bottle or beaten in a mortar were *combined* for the purpose of counter poisoning, if possible, the unhappy victims. The peculiar condition of the intestinal membrane, already adverted to, must certainly have been the cause of immunity from the effect of poisonous doses when administered, as it was also the cause of no benefit arising from the introduction of sanatory medicines.

According to the evidence submitted for consideration *every* method was successful, and *every* method failed. Venesection and brandy; croton oil and gallic acid; opium and ammonia; sulphate of magnesia and acetate of lead; capsicum and ice; calomel and strychnia; scammony and starch; antimony and quinine; blisters and cider; nitrate of silver and gruel; venous transfusion and cupping; gallons of salines, and total abstinence from fluids; carbon and oxygen; hot fomentations and cold sheets; in short, *everything* and nothing has been *invaluable*.

What deduction must be drawn from such a chaos, when the average mortality under every system was nearly similar! What is to be stated as established, when we find a most intelligent practitioner losing fourteen out of sixteen cases, in spite of every exertion that science and industry could make, and another gentleman assuring us that out of *two hundred* cases of *premonitory* diarrhoea he only lost a single patient? The only conclusion that can be justified by reason, must be this:—That there are *many* cases, especially at the commencement of choleraic visitations, in which

the poison is so virulent that *nothing* can resist its withering influence, and the prostration so complete that *nothing* can restore the stricken energies. That there are other cases in which the poison being of a less malignant character, and it may be the constitution of the patients less yielding, the disease, if treated at the first onset by *any judicious* method, may be effectually checked. And that there is a third class of cases in which early treatment does not prevent the progress of the malady; it may be said to run its course even to the access of apparent death, and yet from that stage the patient *may* gradually recover, and return as it were from the very threshold of the grave.

We have thus *three* distinct divisions before us, in one or other of which the majority of cases may be arranged. It is clear that the fatal cases must belong either to the first or third division—*i.e.*, death must occur at an early period before medicines have had time to combat with the enemy, or at a later period when the disease has steadily progressed in defiance of persevering treatment. It should be remembered, that in about *half* the cases in 1849 death *did* occur notwithstanding all our efforts; and it will probably be admitted, with regard to the *first* class of cases, that no means with which we are acquainted *could* have saved the victims from destruction. In the third class some patients are to be found who did recover from a state of extreme collapse—patients in whom *every* symptom of cholera was manifested, and who only just escaped in time to live.

It is with reference to these persons that the greatest difficulty arises; for if no treatment could produce the least impression on the disease till the whole train of consequences, with the exception of actual death, had been fully developed, and then, at the precise moment when the last breath had apparently been drawn, a favourable change began under the most opposite systems of medication, how can it be fairly concluded that the change was owing to any circumstance, except the single fact that the virus was *exhausted*, and the patient had strength to *struggle* back to life? There is no satisfactory evidence that in the worst description of cases one method of treatment was decidedly better than another; nor is there satisfactory evidence that the recoveries which were witnessed under *those* circumstances were attributable to *any* treatment whatever.

We now come to the *second* or medium class of cases mentioned, viz., those which were amenable to remedies at a comparatively early stage of the disease—those cases in which actual collapse did not supervene. *Why* did it not supervene? If a physician were called upon to attend a hundred patients who had swallowed the same specific poison, and he applied, experimentally, a different medicine to each, and fifty sank into collapse, and the other fifty did *not* do so, what would be his inference? Surely that they who *did not* had either taken a *smaller* dose, or a *weaker* preparation, or had *stronger constitutions*. Our inference with regard to cholera must be analogous.

We concluded that in the restorations from collapse *no* medicine was efficacious, and we must now conclude

that in cases which were free from collapse, *all* medicines were efficacious. And why? Because the dose of poison was smaller, the preparation weaker, or the constitution stronger. Upon this supposition, and upon this alone, the conflicting testimony as to successful treatment can be explained. Calomel, in small doses, might restore the arrested secretions. Purgatives might carry off deleterious elements. Opium and astringents might prevent a further exudation. Antimony might relieve the mucous surfaces by acting on the cutaneous capillaries. Blisters, cataplasms, and fomentations, might produce a similar effect. The abstraction of blood might relieve the inflammatory tendency which was in many cases noticed. And, in others, stimulants might help the system to maintain its energy until the primary mischief was removed. So that, whatever plan be pursued in a *certain class* of cases, intelligible relief may be obtained, because *each* plan is capable of producing some effect; and it appears to be of little consequence in which way the benefit is conferred.

It should be stated that the administration of calomel and opium, in small doses and at short intervals, appears to have been the *favourite* practice; but there is abundant evidence to prove its failure, and also to prove that in cases as malignant as any which ended favourably under that plan, recovery also ensued without a grain of either being used during any stage of the disease. Two authentic cases are reported in which the only treatment was *cider, ad libitum*. One of these patients was quite pulseless when the cider was prescribed—*both recovered!* The saline treatment does not seem to have been generally tried, although it was found to be successful in the hands of several careful observers.

It seems to be needless to expatiate further upon this part of our subject, as no additional information is deducible from our premises. The treatment of *bilious* diarrhoea, not proved to be choleraic, does not require comment. The ordinary remedies arrested the ordinary malady as in former years.

THE PREVENTION of cholera is the last point that remains to be noticed.

It is asked whether any circumstances are known which have *uniformly* exempted persons or places from the inroads of the diseases? Certainly *not*. The *less* susceptibility of some parties has been mentioned—namely, butchers, and persons who are exposed to the fumes of lead and arsenic. *Doctors* might, perhaps, be added to the list. But, unfortunately, the observations are not sufficiently numerous to prove any “*uniform exemption*.” The same may be said as to cider drinkers, cider districts, and fruit eaters. Some of these persons and districts suffered severely, others escaped. It may be suggested as a rule, that butchers live remarkably well, and are strong hale men;—that people who work in the fumes of arsenic and other poisons generally obtain high wages, and therefore *may* live well, and that they who are *able* to drink *much* cider and habitually to eat *much* fruit, with impunity, must possess an alimentary system whose natural strength would be *calculated* to resist choleraic influences.

Geological exemption is also “not proven.” The *most* uniform exempting cause is, undoubtedly, a vigorous state of body and mind, maintained by *generous diet, fresh air, temperance, and cleanliness*.

There is an important precaution that should be carefully adopted by those who are in attendance upon cholera patients, namely, to avoid contact with the evacuations, and also to wash the hands and mouth if such contact has occurred. There is little doubt that the disease may be communicated by taking food without this precaution, and also by taking food that has been itself exposed to contact with the poisonous media. It is also very important that nurses should be relieved from continuous employment, and that fresh watchers should be substituted *before* the others are fatigued, it being obvious that many suffer when the system is depressed by want of sleep and rest, who might, by prudent alternations, entirely escape injury. The early removal of the healthy from infected houses, and also the early removal of the dead, are unequivocally recommended.

Dr. Williams begs to state, at the end of this report, that he has *endeavoured* to do his work faithfully in representing the opinions of others. No member of the medical profession can be ignorant of the fact that very different conclusions may often be formed from the same premises; and the writer of the preceding pages sincerely hopes, that if any of his brethren should differ from him as to the strict accuracy of his deductions, they will, at any rate, believe that he has performed a somewhat difficult undertaking *as well as he was able*.

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Worcester, 1850.

ON THE PROTECTIVE POWER OF VACCINATION.

By THOMAS HUNT, Esq., LONDON.

Read before the Annual Meeting of the South-Eastern Branch.

A CORRESPONDENCE on this subject having recently occupied the pages of the *Journal*, I take the opportunity afforded by this meeting to inquire of the members present how far their experience would lead to the conclusion that septennial revaccination is requisite to ensure protection from the small-pox for life. And, by way of provoking discussion, I beg to lay before the meeting certain considerations which appear to me to militate strongly against the necessity of revaccination; premising, that in cases where it is doubtful whether the patient has ever really had the genuine vaccine disease, there cannot be any objection to test the validity of a former vaccination by an introduction of pure, fresh, fluid vaccine lymph into the arm. But I maintain that, as a general rule, a patient cannot have the disease called cow-pox more than once; and that, therefore, septennial vaccination, or even revaccination is not only needless but actually impossible. It is not probable that the real source of difficulty, as well as of difference of opinion, is to be found in the extensive circulation of a deteriorated lymph, which produces in many patients, though not in all, a spurious vesicle? In what other way are we to account for certain facts apparently contradictory, but by supposing the existence of a lymph, imperfect in character, which has the power of communicating to one individual the genuine vaccine disease, and to another a spurious form of vaccinia

which may be followed by a modified variola? That this or any other vaccine influence should become protective for seven, fourteen, or twenty-one years, and then leave the subject exposed to the contagion of small-pox until he is revaccinated, is a modern doctrine, opposed to all the analogies of pathology; and, as far as I know, wholly unsupported by facts. It is on this point that I appeal on this occasion to the experience of my brethren. Not only the immortal Jenner himself, but Willan and Bateman, men, whose respective names will be scarcely less known to posterity than his own, were singularly enough agreed in opinion as to the nature of the cow-pox, the appearance and progress of its vesicle, and its permanent protective power. And the facts which are to overturn the opinions of such men ought to be numerous and weighty indeed. And it must be remembered that the Jennerian side of the question, from its very nature, cannot be indebted to positive facts for its support. It consists of a negative proposition, and it derives its whole support from a negative source. Dr. Jenner challenged the world to show him a case of small-pox after vaccination. He minutely described the true vaccine vesicle, and cautioned his brethren against mistaking it for a spurious form of the disease. His sagacity enabled him to perceive by analogy what has since been proved abundantly by experiment, that the cow-pox *is* the small-pox in the cow—that the small-pox can be transferred from the human being to the cow, and from the cow to the human being without losing its specific properties, without suffering deterioration, without any change but being freed from its virulence and danger; and that therefore it ought to protect for life: and it did so. At the end of the sixteenth year from the promulgation of Jenner's discovery a very few cases of spurious small-pox after vaccination had attracted the notice of the profession, and none of these were true small-pox. To this hour there are few medical men, *if any*, who have ever *seen* the two diseases both well marked, genuine in appearance, and regular in course in one and the same person; and, unquestionably it is just as easy to meet with a practitioner who has seen the small-pox *after* the small-pox, as to find one who has seen the small-pox after the cow-pox. Moreover, there was a time when the success of vaccination was absolutely triumphant. I allude to the period from 1810 to 1820. Any medical man who at that time had been bold enough to question, before such a meeting as this, the permanent protective power of vaccination would have shared the fate which Jenner suffered when he first propounded his discovery to the Medical Society of London. In a large medical practice in Hertfordshire, embracing the majority of the population of thirty square miles, only one case of small-pox occurred in seven years—from 1812 to 1819, and that *not* after vaccination. At that time medical men only vaccinated. The operation was never performed gratuitously, or cheaply. No zealous clergyman nor benevolent ladies, much less nurses nor midwives, had then presumed to introduce an animal poison into the human system, and to sit in judgment over its results. The diagnosis