vesicles from which lymph is taken, that we must look for maintaining stocks of active lymph.

The public vaccination in this country, as it exists at present, is chiefly in the hands of the guardians of the poor, who contract with their medical officers for its performance; and although undoubtedly, in many districts, it is carried out very efficiently, still the system is far from satisfactory, and, being subject to no supervision whatever, has been found to vary very much as to efficiency, as reference to the Government reports amply testifies.

There is the greatest want of uniformity of opinion amongst public vaccinators themselves as to what constitutes efficient vaccination, as well as on various other important practical details. Another serious objection to the system is the fact of vaccination being identified more or less with the administration of parochial relief, and the necessary arrangements being in the hands of the guardians, who are not unfrequently found to sacrifice efficiency to false notions of economy. In many cases, all the evils of subdivision exist to the fullest extent. The records of what is done are for the most part too incomplete and irregular to render them of the slightest value.

A certain but a much smaller portion of the public vaccination is more or less under the control and supervision of the National Vaccine Establishment, their stations being in some instances under the management of vaccinators salaried out of the parliamentary grant; in others, they are connected with medical charities; and in a few instances they have adopted a parochial medical district. Now, I have already shewn that it is the stations of the National Vaccine Establishment which furnish almost the entire supply of lymph for the public service; and I may add, that it is at these stations alone that vaccination in all its details is invariably and uniformly carried out in the best known manner. A careful record is kept, showing the source of the lymph, and the result, in every individual operation. The system, in fact, so far as it goes, is probably as satisfactory as could be desired; and all that in my belief is required to place vaccination on a proper basis in this country is to enlarge the but too limited powers of the National Vaccine Establishment by increasing the parliamentary grant, and giving to that body the complete control and responsibility of the whole public vaccination of the country. This would involve its disconnection with parochial relief, an uniform and suitable division of districts, the appointment and adequate remuneration of thoroughly competent vaccinators, and the necessary amendments in the various Acts of Parliament relating to the subject.

It has sometimes been urged, that a plan of this kind might be found to interfere with the rights and privileges of a certain number of the medical profession; but I can conceive no foundation for this apprehension. Almost the whole of the vaccination which would be included in such a plan would be that of the poorest classes of the community, the class which almost exclusively furnishes the bulk of the unprotected, and to whom the ravages of small-pox are almost entirely confined. The more intelligent and well-to-do class are able to take care of themselves; and their medical attendants, having a thoroughly efficient system of public vaccination to fall back upon for an unfailing source of lymph, would be much aided in their efforts to maintain vaccination of the highest character amongst the children of the wealthier section of the community, who, at present, are to a certain extent deprived of the full benefits of vaccination, as a natural result of the imperfections in the national system of public vaccination.

Original Communications.

CONSIDERATIONS RESPECTING CHOLERAIC COLLAPSE.

By C. HANDFIELD JONES, M.B., F.R.S., Physician to St. Mary's Hospital.

THE condition of choleraic collapse must be admitted, I think, to be essentially independent of any evacuations from the blood. Setting aside cholera "sicca" (so-called), it certainly seems to occur when there is no possibility of any material loss of fluid; and, on the other hand, is not observed in certain instances of profuse intestinal discharge. In the cold stage of malarious fever, and more rarely in typhus, we meet occasionally with very perfect examples of choleraic collapse, understanding thereby blueness of the surface and icy coldness, shrinking of the body, failure of the heart's action and pulse.

Different views may be taken as to the essential cause of the above-mentioned symptoms. We may hold with Dr. Johnson that they all depend on spasm of the small pulmonary arteries causing arrest of the blood-flow through the lungs; or with Dr. Parkes, that the obstruction giving rise to them is the result of deposition of fibrine in the capillaries and pre-capillary vessels; or with Briquet and Mignot, that they are the result of the hyposthenising toxic influence acting principally on the heart and cerebro-spinal nervous system. Lastly, it may be held, that the algid phenomena are produced in the same way, I do not say by the same agent, as they probably are in pernicious ague; viz., by a morbid stimulation of the vaso-motor nerves, especially of the limbs and surface; or, according to a modification of this view, that they depend on severe irritation of the intestinal nerves or ganglia.

In Dr. Johnson's theory, there is much that is very truthlike, and perhaps it expresses truth, but not the whole. The chief objections to it seem to me those which arise from a consideration of the events of fibrinous deposition in the heart, which have been so ably investigated by Dr. Richardson. Here, supposing the obstruction to be on the right side, we have great and peculiar dyspnoea, often tumultuous action of the heart, wandering of the mind from failure of supply of blood to the brain, livid pallor and coolness of the surface, and more or less tendency to rupture of the air-cells and emphysema. Dyspnoea there may be in cholera; but it is not so constant, nor so severe, as in fibrinous deposition. The inspirations taken are shallow and frequent; whereas in the latter affection they are rapid and deep. The respiratory distress, when present, is further augmented, as Briquet and Mignot state, by a tearing pain, or oppressive weight at the epigastrium, or by a sensation of pressure or constriction at the base of the thorax. This is not present in the state with which I am contrasting the choleraic phenomena. According to Dr. Parkes, the respiratory murmur is altered in every case where the distinctive features of cholera are present. The inspiratory sound is lessened, becomes rougher and more bronchial, and the expiration lengthened and exaggerated. In fibrinous deposition, the breathing is full and free, as long as there is no obstruction on the left side. The muscles in the one condition are affected with cramps; in the other with restless prostration. The brain in the former, though evi-

dently scantily supplied with blood, is able to function effectively; in the latter it fails, and the mind wanders. If the blood were prevented from reaching the lungs by spasmodic closure of the pulmonary arterioles, there would almost surely be for some time violent struggling action of the right heart. I well remember a case of malaroid fever, in which the first paroxysm set in with pulselessness and a sensation of imminent death. On auscultation, I found the heart acting violently, and was greatly puzzled to understand how, with such forcible cardiac action, there could be an absence of radial pulse. In a few hours, the pulse returned, and the hot stage set in. No doubt can exist that, in this instance, the arteries of the limbs were closed by spasm, and that the heart was struggling to carry on the circulation in spite of this obstacle. In a case recently under my care, where fibrinous deposition in the heart was diagnosed during life and proved to exist after death, the action of the organ was greatly excited. In cholera, on the contrary, though the action of the heart may be sometimes rapid and tumultuous, yet for the most part enfeeblement and failure are the prominent phenomena. The surface of the body is notably bluer and more icy cold in cholera than in fibrinous deposition in the right cavities. Pulmonary collapse is not a feature of the latter condition any more than interlobular emphysema is of cholera.

The foregoing seem to me to constitute substantial differences between the two pathological states, which should not exist if the theory in question were correct. It is, however, to be remarked, as a notable point of resemblance, that in fatal cases of both the lungs are found anæmic, and that inflammatory affections of the lungs previously existing have been considerably diminished after the supervention of both. This, however, might ensue whatever was the cause of the arrest of the blood-flow.

A weighty objection to Dr. Parkes's theory seems to me the rapid though temporary improvement obtained by injections into the veins. These could not remove capillary emboli.

Against Briquet and Mignot's theory, it must be considered that hyposthenisation of the heart and cerebro-spinal nervous system is what the miasms of typhus and typhoid fever effect; but their symptoms are very unlike those of the algide stage of cholera.

Whatever opinions may be held as to the degree of real affinity existing between cholera and the pernicious malarious fevers, it cannot be questioned that the causes, supposing them different, produce almost identical results in the algide period, the more peripheral parts being icy cold and devoid of circulation in each, while the internal are the seat of active hyperæmia and congestion, and have their temperature increased.* In both disorders there may be profuse evacuations or none. Now, it not unfrequently happens, that a morbid state may be produced by several distinct causes, yet be to all appearance identical from the operation of each. Thus, erysipelas certainly may arise from contagion; but there can be no question it often is set up quite independently. In our treatment of it, however, we have regard solely to the existing condition and not at all to the cause. So I hold it to be with respect to cholera. Its cause may be more or less different from that of pernicious fever; but the morbid processes may be almost or quite identical; and where the results are so similar, we can hardly fail to de-

rive instruction with respect to one from a careful examination of the other.

In endeavouring to frame to ourselves a *rationale* of the algide condition in malarious fever, we may observe: 1. That there is almost a certainty that the poison acts especially on the nervous system, both on its cerebro-spinal and sympathetic departments. 2. This action seems to be at first more exciting and subsequently paralysing; the former mode being more apparent in the peripheral parts, the latter in the abdominal viscera. The rigors may be viewed as indications of excitement of the spinal cord, and it is worth remarking that they do not occur in palsied limbs. The vaso-motor nerves may be so excited that the radial pulse is obliterated during the entire cold stage. 3. The enfeeblement of the heart's action may be well accounted for by a similar state of the plexuses supplying its nutrient arteries. 4. The peculiar coldness of the surface is to be accounted for by the contracted arteries ceasing to transmit a supply of aerated blood, and probably also by a special influence of the excited nerves on the nutritive vital actions of the part. I am the more inclined to adopt this latter view, as the rise of temperature, which has often been noticed after death, seems necessarily to imply the cessation of some force which depressed the calorification during life. As the phenomenon occurs some time after death, it is of course independent of any movement of the blood. 5. The blueness depends on the same cause; viz., the want of an efficient *vis a tergo*, in consequence of which the capillaries and small veins become filled with stagnant corpuscles of a dull red colour. This condition may be seen, not unfrequently, perfectly developed in the frog's web, as is well described in the following quotation from Mr. Wharton Jones's paper, p. 21: "In a third case, the two larger webs of a frog's foot were very much congested, the blood stagnant in a great many capillaries, and flowing slowly or oscillating in others. The flow in the veins sluggish. The arteries all more or less constricted. Sometimes one artery would be seen to dilate a little; and, in proportion as the flow of blood in it thus became free, the circulation in the capillaries to which it led was re-established. The application to the web of solution of salt was followed by uniform dilatation of the arteries, a brisk flow of blood, and dissipation of the congestion." Section of an artery has nearly the same effect, as constriction, congestion, and stagnation, take place in the venous radicles and capillaries connected with the lower end. The effect of cold in producing the above described condition is notorious, and was well exemplified in the following observation. An ice-bag had been applied for some length of time over a pulsating abdominal tumour, and had produced dusky venous redness of all the area of integument where it lay, while the surrounding surface was quite pale. A warm finger was laid on the discoloured part, and immediately the dusky tint gave place to the natural pallor. No one can doubt that, in this instance, the arteries were constricted by cold and relaxed by warmth, just as one may see them to be under the microscope in transparent parts. Other more obscure causes act occasionally in the same way as cold. Thus, it is stated in the *Echo Méd. Suisse*, that a girl, aged 17, without known cause, felt violent pain in the hands; the four fingers of the right and two of the left became slate-coloured, cold, insensible—in short, exhibiting all the symptoms of incipient gangrene. Movement was almost entirely abolished in these fingers. The induced current was applied, giving rise at first to increased pain, but soon arresting the sufferings of the patients. After ten or twelve sittings, at about the end of a week, the sen-

* Dr. Macpherson states, that late experiments in Paris show that the temperature rises to 103° in the rectum. (P. 58.) Similar observations have recently been made in London. (Vide *Lancet*, August 18th, 1866.) The temperature of the vagina in one instance was 102°·4.

sibility, the normal temperature, and colour as well as motion were restored. The epidermis came off to the extent of the first appearance of the gangrene. It seems impossible that the Faradisation could have availed anything in this case, if the vessels had been obstructed by emboli; whereas it is intelligible that it might have been very efficient in removing a state of spasm. In my work on *Functional Nervous Disorders*, I have referred to some other instances of the same kind, which are, I think, sufficient to show that algidity may occur as a local phenomenon, and may, therefore, be independent of any intra-thoracic derangement.

It appears to me to be a question of great interest why the loss of *vis a tergo* so often conditions stagnation of blood in the capillaries and venules. That it does not do so constantly is certain. I have repeatedly seen in the frog's web whole tracts of capillaries, empty apparently, and devoid of corpuscles when the arteries communicating with them were constricted. The capillaries themselves remained perfectly patent. It is said that frost-bitten parts become of a waxy white; while less intense cold produces, we know, a dull red. This might suggest that in the former case the arteries were more completely constricted than in the latter. I doubt, however, very much whether this is a sufficient explanation; and I am rather inclined to believe that the attractive force of the tissue through which the vessels pass has much to do with the result. In inflammatory stasis it certainly has.

Considering the intimate relation of malarious fevers to the neuroses, of which no practical man can doubt, the unquestionable control also of the nervous system, especially its vaso-motor department, over calorification, the evidence afforded by the microscope of the effect of constriction of the small arteries, and the nature of the remedies which prove beneficial, it seems to me that there is strong ground for believing that the *rationale* above offered of the causation of the algide condition in malarious fever is in the main correct.

The lesions revealed by *post mortem* examination are great engorgement of the large veins and of the right cavities of the heart, oedema of the lungs, or extravasation of blood into their tissue, formation of pigment in the blood, and deposition of it in various parts, a pale and flabby state of the cardiac muscle, which is stated to be much softened in the algide choleric forms.

We may next consider whether there exists any sufficient reason for refusing to class the algide condition of cholera with that above noticed. The phenomena during life scarcely differ, except in the character of the evacuations, and in their resisting, in the case of cholera, the administration of quinine. Neither of these seem to me absolutely distinctive. The evacuations in both cases are copious outpourings of serous fluid; and the rice-water appearance is only the result of the admixture of fibrinous flocculi or epithelium, and is perfectly imitated in some instances of hypercatharsis from drugs taken in a poisonous dose, as croton-oil and colchicum. If the contagious quality of the fresh evacuations was demonstrated, it would be a strong point of difference. The utility of quinine in established cholera does not appear to me surprising. Bark has often failed in pernicious fever; and Livingstone found that quinine did not avail in the endemic fevers of the Zambesi. The chief peculiarities, which dissection discloses in fatal cases of cholera, are said to be the bloodlessness of the lungs, their collapse, and the quantity of blood which is retained in the pulmonary arteries, and pours out when they are divided. Now, these features do not appear to be constant by any

means. Annesley says, p. 111: "The lungs were always completely engorged with blood of a pitchy or black appearance." This evidently has reference to cases dying in the algide stage, as well as subsequently; and cannot, I think, possibly be meant to describe only the condition of the larger vessels. Leudet, in the majority of the cases fatal in the algide stage, found that no other morbid change existed than engorgement of the lower and posterior parts of the lungs with dark blood. In some instances, this was so complete as to cause portions of the pulmonary tissue to sink in water. In certain cases, the pulmonary tissue throughout was full of dark blood. Reinhardt and Leubuscher state that the lower lobes (that is, I should say, fully one-half of the lungs) were in general full of dark blood. Virchow states similarly, "The lower lobes were for the most part much congested." These quotations are taken from the Report of the College of Physicians. Parkes writes: "The quantity of blood in the substance of the lungs was considerable in three cases, in one there was extravasation of blood in the lower lobe of one lung; in six cases, there was a considerable quantity in the vessels of the lower lobes, but none in the upper; in ten cases, the quantity was small; in eleven cases, there was no blood in the minute texture of the lungs." According to this statement, the amount of pulmonary congestion was notable in ten out of thirty-one cases. Again he says, p. 20: "In other cases, there is more blood in the minute structure, a corresponding dark colour of the lung, and a variable amount of frothy serum." In some recent reports of autopsies of cholera patients dying in the algide stage, it is mentioned that a woman under Dr. Moxon's care died after eighteen hours' illness; the lungs were considerably engorged with dark blood, and all the heart-cavities contained white gelatinous clots (*Lancet*, July 28, 1866); also, that a case died in collapse at the Middlesex Hospital, where, at the autopsy, both lungs* were found much congested (*Lancet*, Aug. 4).

These statements seem to me to prove that absence of blood from the lungs cannot be considered an essential condition in fatal cholera. It is surely not so constant an occurrence as blueness and coldness of the cutaneous and adjacent surfaces. Collapse of the lungs cannot be essential to fatal cholera, as it has been found altogether or nearly absent in autopsies. Out of thirty-nine cases examined by Parkes, this condition seems to have been marked in seventeen; in eight it was slight; and in fourteen it was more or less prevented by old adhesions. In a recent fatal case at St. Mary's, I am informed, the lungs were not collapsed more than in death from other diseases. Moreover, the same authority tells us that, "almost till the last moment, the patient can breathe deeply, if told to do so." It is probable, therefore, that collapse of the lungs is rather a *post mortem* phenomenon than an *ante mortem* condition; and that it is simply the result of the unopposed action of the elastic fibres of the tissue. Perhaps the anæmia of the lungs may be due in some measure to the contraction thus taking place, which squeezes the blood out of the capillaries and smaller vessels into the larger.

Retention of blood in the pulmonary arteries and right side of the heart is a striking feature in many instances of cholera, perhaps in the great majority; and, taken together with the more or less bloodlessness of the lungs, it appears to afford a good deal of

* It ought, I think, in fairness to be stated that, in some instances of complete obstruction of the pulmonary artery, the lungs are found much congested. (*Vide* Meissner's Report—Schmidt's *Ber.*, vol. cxvii, p. 235.) This goes to assimilate the choleric condition with that of right side fibrinous obstruction.

appui to the view that the blood is prevented from passing onward by spasm of the smaller pulmonary vessels. The theory referred to represents this spasm as the essential condition of the arrest of the circulation; it must, therefore, be of constant occurrence; and the tokens of its existence ought to be constant also. But Dr. Parkes states that "in other cases, however, the lungs are also excessively shrunk, and contain no blood or serum; yet, on cutting through their roots, no blood escapes, and all the cavities of the heart are nearly empty. There is, therefore, no evidence of obstruction." In Briquet and Mignot's experience, the signs of pulmonary artery obstruction (*viz.*, accumulation of blood on the nearer side of the barrier, and its absence on the distal) do not seem to have been at all constant. They write: "Sometimes the cavities of the heart were engorged, especially the right. The left cavities, however, had no tendency to emptiness." Dr. Andrew states, in his account of a case which died at St. Bartholomew's, that both pulmonary arteries and pulmonary veins contained a considerable quantity of blood, and so did the right and left cavities of the heart. Of another case, he states that both sides of the heart contained clots of dark blood and decolorised clots; while the lungs, which were emphysematous, contained a fair amount of blood. (*Med. Times and Gaz.*, July 21, 1866.) In the record of a case like the two preceding, dying in collapse at the German Hospital, it is stated that the pulmonary vein was quite filled with thick dark blood; the lungs were dark red. (*Med. Times and Gaz.*, Aug. 4, 1866.) If spasm of the pulmonary artery existed sufficient to obstruct seriously the current of the blood, there would surely be visible distension of the jugular veins during life, at least in cases where no considerable evacuations had taken place. No such appearance, however, is mentioned in the descriptions extant.

From the above evidence, I feel much inclined to admit the probability of choleraic collapse being produced very much in the same way as that of algide fever is. I think it not unlikely that the pulmonary arteries may participate in the constriction which affects the systemic, the vaso-motor nerves of each being affected alike; but I believe the chief seat of obstruction and arrest is in the aortic, and not in the pulmonary ramifications; and that the affection of the latter is non-essential. As to the argument, that, if the arrest were in the systemic vessels, the left side of the heart and the arteries would be gorged, it does not seem to me of weight. We see and know that, as a consequence of arterial constriction, the capillaries and veins become engorged; and if the blood accumulates, as we know it does, in the larger and smaller veins, it is no wonder that it is absent from the left cavities and their afferent channels.

Although we are not able to consider the physical changes produced in cholera-blood as the chief cause of the arrest of the circulation, yet we cannot but regard them and those taking place in the tissues as exponents of the action of an injurious influence, which, by gravely interfering with the several nutritive processes, aids greatly in deranging those conditions on which the free transit of the blood through the capillaries depends. The small intestines are the chief focus of these lesions, where we find not only vast effusions of serosity taking place into the cavity of the canal from a vascular surface denuded of its epithelium, but also infiltrations of its coats with exuded fluid, and not unfrequently more or less extensive hæmorrhagic extravasations. Recent catarrhal irritation, with increased epithelial exfoliation, was found constantly in the urinary pas-

sages by Virchow. The uterus was commonly hyperæmic, and often presented small extravasations of blood on its inner surface or in its tissue. Similar extravasations were found in various parts—the pericardium, the endocardium, the lungs, the pleura, the spleen. Their occurrence can only be referred, I think, to an altered condition of the walls of the capillaries, as there certainly must be a considerable decrease, and not increase, in the amount of intravascular pressure, except, perhaps, in the case of the vessels of the small intestines. The kidneys presented indications of the commencement of morbid changes, which were more developed after reaction took place. The pneumonia, and engorgement or œdema, which was so frequently observed during this stage, must be considered to have had its origin in the loss of capillary tone or retentive power, of which there were sufficient traces during the algide stage. The softening or alteration of the muscular tissue of the heart, which was observed by Briquet and Mignot in four-fifths of the cases fatal in the algide period, is a strong evidence of the injurious influence of the poison on the tissues, and must be taken into account in estimating the immediate causes of the fatal event. It is also to be noted, as showing the tendency to cardiac paralysis, that Parkes found, in some instances, the ventricles flaccid and inexcitable soon (about one hour) after death, though they subsequently contracted firmly. The paralysis in these instances was probably of the inhibitory kind. Owing to its natural weaker conformation, it is conceivable that the right ventricle might be paralysed before the left, as the above mentioned writer suggests. The dark, tar-like, wasted condition of the blood, is not referable, I believe, to the abstraction of water and salts only; for it is noted as having these qualities in Dr. Stokes's case of algide typhoid fever, where there were no unusual evacuations. Its tendency to deposit fibrine, as well as its other peculiarities, seem to me more probably the result of the direct action of the poison, than of its imperfect aëration. In a case mentioned in the *Medical Times and Gazette*, July 28th, it is stated that a fibrinous mass, of the size of a cherry, was found in the right lung.

The general tenor of the above facts is to show that the operation of the poison is by no means limited; that it affects the blood, the arteries, the capillaries both pulmonic and systemic, the mucous and serous membranes, and glands. From this I should conclude that the morbid process is not confined to any one region, but affects many, probably with varying degrees of intensity in different instances.

Although the above exposition seems to me to be the nearest the truth, it ought, I think, to be considered also whether in many cases, perhaps more especially in some epidemics, the symptoms may not be dependent, in a greater or less degree, on irritation reflected from the morbidly affected intestine. In a case of poisoning by croton-oil, the patient having at the same time typhoid fever, the symptoms, as Dr. Macpherson remarks, approach wonderfully close to those of cholera;* and it is especially observable that the algide phenomena were apparent three-quarters of an hour before purging commenced. The depression of the circulation which occurs in acute peritonitis, like that resulting from severe irritation of the mucous surface, is no doubt dependent on morbid impressions conveyed by the intestinal nerves to the ganglia and cord, and thence reflected on the heart and arteries. In a fatal case,

* Dr. Greenhow has recently recorded a case in which they were absolutely identical even to the rice-water evacuations. (*Wide Medical Times and Gazette*, August 11th, 1866.)

I found the left ventricle quite empty and firmly contracted, just as it has been found in cases of cholera. The right was occupied by a largish fibrinous clot extending up into the pulmonary artery. It did not appear of *ante mortem* origin. The lungs were congested. The absence of abdominal pain is not an argument against this view; as we know, from observation of epilepsy, that an unfelt irritation may give rise to very severe symptoms. Cases of this nature would be more amenable to treatment than others; inasmuch as the intestinal irritation might be removed by appropriate remedies, as calomel and opium, castor-oil, etc. It is to be feared, however, that, in many cases, the heart, kidneys, and other organs, are too severely stricken in their vital powers to carry on their necessary functions, even when the irritation of the intestinal nervous structures is removed.

In conclusion, I may say that I regard cholera as by no means a single and uniform morbid process, but one which consists of several varying elements. These may be enumerated as cardiac paralysis, constriction of arteries, alterations in the blood and capillaries, intestinal irritation. They may be variously developed in different instances, some may be more marked in one case, others in another, and remedial means should be varied accordingly.

REMARKS ON SYPHILISATION.

By GEORGE GASKOIN, Esq., Surgeon, Chevalier of the Order of Christ, Portugal; Surgeon to the Artists' Benevolent Fund; formerly House-Surgeon and House-Pupil, St. George's Hospital.

[Continued from page 41.]

IN the continuation of his second lecture, Mr. Lee says that the immunity obtained in animals through repeated inoculations was attributed by M. Auzias to saturation of the system with the syphilitic virus; and then he goes on to say that "the early theory formed of the process was embraced by the word given to it; but that theory has perished, though the term has been maintained. Syphilisation is a misnomer. For, let us allow that we can make an indefinite number of ulcers on the person of a syphilitic by repeated inoculations of the pus of an indurated chancre, there is here no analogy to the process by which animal poisons are carried into the constitution—to vaccination, for example." Here we have it implied that Auzias Turenne has fled from the principles with which the public were first deluded and amused; he has experienced a defeat, we must suppose (for has he not shifted his ground?); a loathsome mode of treatment is maintained, though the plea for its introduction is found to be groundless and untenable; "the theory embraced by the word given to it" is utterly destroyed; while, in obedience to necessity, the venue now is changed, and a source of professional notoriety is supported on some other ground of conclusion equally false and subvertible.

But why, oh! Henry Lee, do you persist in laying your readers on a wrong scent by suggesting in others the existence of untenable and varying opinions, for which there is only the very faintest foundation in truth? Why range yourself with the oppressors of a great discoverer, as if it were not plain to every eye that his tardily and meanly acknowledged merits are a reproach to the age in which we exist, even more than the false glitter of reputations which flourish by his side. Was it not truly said by the wicked lawyers of James's reign, Give us a line of writing from a man's hand; it shall

be enough for us to hang him straightway? and you have got now this unfaithful record of a dream, you rake up this word "saturation", which, in conjunction with syphilis, expresses so revolting an idea. The intention and legitimate aim of the discoverer to regulate and destroy the action of the virus by means of the same virus, and thus to rescue the patient's health from destruction, is kept out of sight altogether; and we have this "saturation" presented to our notice, which is asserted to have been M. Auzias-Turenne's original conception in proposing a new treatment of syphilis; and, "therefore", say you, "this process was naturally termed syphilisation—this theory has perished, while the term has been retained". But did this theory ever exist?"

See we not rather here revived the prejudices of the previous century, when to give a man the small-pox, or matter from out of the body of a cow, was called flying in the face of Providence, and marring God's likeness in his creatures, just as now we are accused of piling syphilis on syphilis? What is your ground of warranty for this assertion? No greater than what we shall see to follow. In the earliest physiological communication made by M. Auzias-Turenne to any learned society, November 1850, in the brief summary of his crude ideas on syphilitic vaccination, or syphilisation, as he then commenced to call it, one phrase, or short section of an introductory character, runs simply thus. No. 3. "The noun-substantive syphilisation (the corresponding verb being syphilise) may be used to denote a sort of saturation of the living organs, or better a condition of immunity obtained by successive chances"; and by and bye he says, in so many words, that the virus offers itself to us as the best means of contending with the difficulties of the disease.

Could anything be more modestly expressed by a youthful aspirant? But, just as any stick may serve to beat a dog, we see here how unfashionable talent may cry out in vain for protection against the imputation of absurdity; nay, even when one of the cleverest men and one of the best physiologists of the time is in question. If he be not buoyed up by a clique, he must go to the wall; and abroad, out of his own country, where his personal qualities and antecedents are unknown, he must be content to rank with a Hahnemann and a Paracelsus, or with smaller fry. You threaten the public with an accumulated syphilis; and ask, by implication at least, if Auzias-Turenne and the rest are to be allowed to fill up human creatures like beer-barrels, or swell them out like sponges, with syphilitic poison.

What can give a more sad idea of the times in which we live than to see favoured and diligent men like Ricord and Henry Lee, representative men of the day, putting themselves so entirely in the wrong with regard to the degree of merit to be found in their contemporaries? What a picture is thus afforded of the condition of science amongst us, when all sorts of forced interpretations, imperfect apprehensions, and explanations ill understood, weary out the thread of life and exhaust all philosophic patience! Such experience might seem to make a return to the middle ages desirable when ghostly charity attempered a material persecution, altogether not more terribly felt perhaps than this crushing prejudice from the regions of fashion, while the very organs of public liberty are made to swell the chorus of dispraise.

In what way, we ask, is syphilisation a misnomer? With a thorough-paced dualist, it is liable to such an interpretation, because he looks on the whole thing as a sham. Yet with no reason on his part. For these wise men of Lyons have lately decided