What then is the total result? It is this—that every important line in the spots, every important line in the storms, has been picked up by this method, and in fact *the map of basic lines along this region is practically a map of the lines widened in spots and present in storms, and nothing else.* Now it may be said that result is interesting, and perhaps important, but that it deals only with a very limited part of the inquiry. That is perfectly true.

The spectrum of iron, and the spectra of other substances have however been attacked in other regions. It is unnecessary to go into many details, but the general result is the same; in other regions we have as in the old region an almost perfect coincidence between the lines most widened in spots, and the lines regarded as basic by previous observers.

So much then for the result in the case of iron, to which, although we have not absolutely limited our attention, we have to a very large extent confined it. This result may be expressed in rather a different way, and it will then be easy to see the extraordinary parallelism which goes on between two perfectly distinct sets of facts, first, the statement of the spectroscope that such and such a line is seen in the spectra of two or more substances, and then the statement of the telescope with the attached spectroscope that such and such a line is seen widened in spots or brightened in flames. Here we have the numbers for the two regions which I have already discussed the region from F to *b*, and from *b* towards D.

Iron

	F-*b*	*b*-548
Total number of lines	96 ...	67
Number in spots and flames	38 ...	41
Basic lines	15 ...	17
,, seen in ,, ,, ...	14 ...	15
,, not seen ,, ,, ...	1 ...	2

The total number of iron lines in the first case is 96. Of those 96 lines only 38, or less than half, are found in the spots and flames. When we go into the lower regions of the solar atmosphere, we leave in fact more than half of the iron lines on one side. Of these 96 lines 15 are found by other observers as well as myself to be common to two or more substances. Now comes the question, what is the behaviour of these common lines with reference to spots and storms? The table shows that among the lines seen in spots and storms fourteen of these basic lines are seen. It must be remembered that our records only give us day by day the results of the 12 most widened lines, and not of all the lines widened. In the next region the number of iron lines is somewhat less—67; 41 of these, or more than half, are picked out by spots and storms; Seventeen are basic; of the 17 basic ones 15 are seen in the spots and storms, and only two are lines that are not seen.

Now, we will turn to another substance, nickel, and there we see very much the same kind of thing at work. In nickel for the region F to *b* we have 20 lines recorded by Thalén.

Nickel.

	F-*b*
Total number of lines	20
Number in spots and flames	3
Basic	5
,, Seen	3
,, Not seen	2

Of these 20, 17 are dropped, abolished, when we come to observe the bright lines and the widened lines of nickel in the spots and storms—the 20 comes down to 3. Among the 20 lines 5 are found to be common to two substances. Of those 3 are seen in the flames, that is to say, every line of nickel seen in a spot or flame is common to two substances, and only 2 are visible in the 20 lines not affected by spots and storms. This is all the work of this nature which we need now consider, but it is not all the work that has been done. Neither my assistants nor myself, I am sure, have spared our attempts, nor ourselves, for the matter of that, in trying to get at the bottom of this matter, and the facts which have been here brought forward are typical of a much larger number of facts which have been observed. In the case of every part of the spectrum, in the case of every substance, the verdict is the same. We have the fact, that two things are going on exactly parallel to each other—first that some lines are common to two substances; next that the lines common to two substances are seen almost exclusively alone, both in the sun's spots and in the sun's flames. So that in addition to the fact that the hottest regions of the sun seem to simplify the spectra of the substances enormously, we have this result that the simpler the spectrum becomes, the more complex becomes the origin of the lines; by which I mean that in the ordinary solar spectrum there are a great many lines due to iron, and to nothing else; but the moment we come to the simpler spectrum yielded to us in the spots or the flames, then we have no more right to say that those lines belong to iron than that they belong to titanium, cerium, nickel, and other substances with which those lines are generally observed to be basic.

This, then, is a help towards the demonstration of the view which was first announced in the year 1874, that the line-spectra of bodies (we are dealing almost exclusively with line-spectra) are not produced by the vibration of similar molecules, but they probably represent to us the vibrations of a great number of simplifications brought about by the temperature employed to produce the incandescence of the vapours.

Can we go further than this? Here we must confess both our imperfect instrumental and mental means. We cannot talk of *absolute* coincidence because the next application of greater instrumental appliances may show a want of coincidence. On the other hand there may be reasons about which we know at present absolutely nothing which should make absolute coincidence impossible under the circumstances stated. The lines of the finer constituents of matter may be liable to the same process of shifting as that at work in compound bodies when the associated molecules are changed; but however this may be the fact remains, whatever the explanation may be, that the lines of the elementary bodies mass themselves in those parts of the spectrum occupied by the prominent lines in solar spots and storms.

J. NORMAN LOCKYER

(*To be continued.*)

STATE MEDICINE[1]

FIRST: a few words on what may be called the general theory of our subject-matter. The term "State Medicine" corresponds to the supposition that, in certain cases, the Body-Politic will concern itself with the health-interests of the people—will act, or command, or deliberate, or inquire, with a view to the cure or the prevention of disease. Before any such supposition can be effectively realised, the science of medicine—that is to say, the exact knowledge of means by which disease may be prevented or cured—must have reached a certain stage of development; and unless the science be supposed common to all persons in the State, the existence of State medicine supposes a special class of persons whom the unskilled general public can identify as presumably possessing the required knowledge. Thus, given the class of experts to supply the required exact knowledge, the Body-Politic undertakes that, within the limits of its own constitutional analogies, it will make the knowledge useful to the community.

I have intimated that in State Medicine (just as in private medicine) the medical function may be exercised either in curing or in preventing disease; but practically these two departments of State Medicine are not of equal magnitude, nor are dealt with in quite the same spirit.

As regards curative medicine, modern Governments have in general found it needless to interfere in much detail in favour of persons who require medical treatment.

Larger and far more various than the action taken by the State with reference to the cure of disease is that which it takes in regard of prevention; and it is particularly of preventive medicine that I propose to speak. In its legal aspect it is represented by a considerable mass of statutes (nearly all of them enacted within the last thirty-five years), and by an army of administrative authorities and officers appointed to give effect to those enactments. I need not describe in detail the laws and administrative machinery to which I refer, but I may remind you of the largeness and variety of the rascope, even by quoting only the terms in which I was able, t elve years ago, to speak of the public health law of England: "It would, I think, be difficult to over-estimate, in one most important point of view, the progress which, during the last few years, has been made in sanitary legislation. The principles now affirmed in our statute-book are such as, if carried into full effect, would soon reduce to quite an insignificant amount our present very large proportions

[1] An Address delivered at the opening of the Section of Public Medicine, in the International Medical Association, by John Simon, C.B., F.R.S., D.C.L., LL.D.

of preventible disease. It is the almost completely expressed intention of our law that all such states of property and all such modes of personal action or inaction as may be of danger to the public health should be brought within scope of summary procedure and prevention. Large powers have been given to local authorities, and obligation expressly imposed on them, as regards their respective districts, to suppress all kinds of nuisance, and to provide all such works and establishments as the public health primarily requires; while auxiliary powers have been given, for more or less optional exercise, in matters deemed of less than primary importance to health; as for baths and wash-houses, common lodging-houses, labourers' lodging-houses, recreation-grounds, disinfection-places, hospitals, dead-houses, burial-grounds, &c. And in the interests of health the State has not only, as above, limited the freedom of persons and property in certain common respects, it has also intervened in many special relations. It has interfered between parent and child, not only in imposing limitation on industrial uses of children, but also to the extent of requiring that children shall not be left unvaccinated. It has interfered between employer and employed, to the extent of insisting, in the interest of the latter, that certain sanitary claims shall be fulfilled in all places of industrial occupation. It has interfered between vendor and purchaser; has put restrictions on the sale and purchase of poisons; has prohibited in certain cases certain commercial supplies of water; and has made it a public offence to sell adulterated food or drink or medicine, or to offer for sale any meat unfit for human food. Its care for the treatment of disease has not been unconditionally limited to treating at the public expense such sickness as may accompany destitution; it has provided that, in any sort of epidemic emergency, organised medical assistance, not peculiarly for paupers, may be required of local authorities; and, in the same spirit, it requires that vaccination at the public cost shall be given gratuitously to every claimant. The above survey might easily be extended by referring to statutes which are only of partial, or indirect, or subordinate interest to human health; but, such as it is, it shows beyond question that the Legislature regards the health of the people as an interest not less national than personal, and has intended to guard it with all practicable securities against trespasses, casualties, neglects, and frauds."[1] At the time when that description was written I unfortunately had to confess that the intentions of the Legislature were not carried into effect; for that the then existing laws (especially in respect of the local authorities which should give effect to them) were in a state of almost chaotic confusion and unworkability; but since that time an entirely new constitution of local authorities has been made, some thousands of additional officers have been appointed, and the general fabric of the law has been consolidated, and its powers in some respects extended and made more stringent, with a view to the better prevention of disease, so far as legal powers and facilities can attain that object.

Such being the very large contribution which the Body-Politic makes to the purposes of State Medicine in this country, let us next see how we of the medical profession stand in respect of the scientific contribution which we distinctively owe to the same great object.

In preventive, just as in curative, medicine, it occasionally happens that consequences more or less valuable result from some mere chance-hit of discovery; but except so far as this may sometimes (and but very rarely) happen, disease can only be prevented by those who have knowledge of its *causes*—knowledge which does not deserve to be called knowledge, unless in proportion as it is *conclusive and exact;* and thoroughly to investigate the causes and their mode of operation is the quite indispensable first step towards any scientific study of prevention. Essentially we know how to prevent, by having first learnt exactly how to cause. Therefore it is that preventive medicine has had almost no development until within these later times. The germinal thought of it may be traced in even the first days of our profession. The spirit in regard of which Hippocrates has been aptly called the Father of Medicine—the scientific spirit of observation and experiment, as distinguished from the spirit of priestcraft, was one which his medical writings equally showed in their preventive as in their curative relations; and when he, some twenty-three centuries ago, expounded to his contemporaries that pathology is a branch of the science of nature—that causes of disease are to be found in physical accidents of air and earth and water, and in quantities and qualities of food, and in personal habits of life, he (not without risk of being denounced for impiety) virtually proclaimed for all time the first principle of preventive medicine, and indicated to his followers a new line of departure for those who would most largely benefit mankind. His followers, however, have had their work to do. True knowledge of morbific causes could only come by very slow degrees, and as part of the development with which the physical and biological sciences have, little by little, with the labour of ages, been building themselves up; and so no wonder that, despite the lapse of time, even the most advanced of nations are hitherto but beginning to take true measure of the help which preventive medicine can render them.

Now what is the nature of that *study of causes* through which we may gradually arrive at counter-causing or prevention?

Addressing a skilled audience, I shall utter what to them is the merest commonplace when I say that, in the physical and biological sciences we acknowledge no other study of causes than that which consists in *experiment*. And the study of morbific causes is no exception to that rule: it is solely by means of experiment that we can hope so to learn the causes of disease as to become possessed of resources for preventing disease.

The experiments which give us our teaching with regard to the causes of disease are of two sorts: on the one hand we have the carefully pre-arranged and comparatively few experiments which are done by us in our pathological laboratories, and for the most part on other animals than man; on the other hand, we have the experiments which accident does for us, and, above all, the incalculably large amount of crude experiment which is popularly done by man on man under our present ordinary conditions of social life, and which gives us its results for our interpretation.

When I say that experiments of those two sorts are the sources from which we learn to know the causes of disease, I of course do not mean that the mental process by which an experiment becomes instructive to us is the same in regard of the two sorts of experiment. On the contrary, the ætiological problem (so long as it is a problem) is approached in the two cases from two opposite points of view; and the dynamical continuity of relation, which we call cause and effect, is traced, in the one case, from the one pole, and in the other case, from the other pole of the relation. In the one case, starting with knowledge of our own deliberately-prepared *cause*, our question is, What will be its effect? In the other case, starting from a certain *effect* presented to us, our question is, What has been its cause? But in the second case, just as in the first, when the question is answered, when the problem is solved, when the relation of cause-and-effect has been made clear, we recognise that the conjuring-power which has brought us our new knowledge is the power of a *performed experiment*.

Let me illustrate my argument by showing you the two processes at work in identical provinces of subject-matter.—What are the classical experiments to which we habitually refer when we think of guarding against the dangers of Asiatic cholera? On the one side there are the well-known *scientific* infection-experiments of Prof. Thiersch, and others following him, performed on a certain number of mice. On the other hand, there are the equally well-known *popular* experiments which, during our two cholera epidemics of 1848-9 and 1853-4, were performed on half a million of human beings, dwelling in the southern districts of London, by certain commercial companies which supplied those districts with water. Both the professor and the companies gave us valuable experimental teaching as to the manner in which cholera is spread. I need not state at length the facts of those experiments, probably known to all here, but may rather justify my parallel by referring to an ætiological question which will presently be discussed in our section.

It concerns the *causation of tubercle*—the most fatal by far of all the diseases to which the population of this country is subject. On that subject, for the last sixteen years, we have had a new era of knowledge. It was the great merit of a Frenchman, M. Villemin, that he, in 1865, first made us fully aware that tubercle is an infectious disease. He did this by certain *laboratory experiments* performed on other animals than man. He found that general and fatal tubercular infection of the animal was produced when he inoculated it subcutaneously with a little crude tubercular matter from the human subject. That first laboratory investigation of the subject has been followed most extensively by others; and the further experiments, while

[1] "Eleventh Report of the Medical Officer of the Privy Council," 1869, pp. 20, 21.

entirely confirming M. Villemin's discovery, have shown that subcutaneous inoculation is not the only mode by which the tubercular infection can be propagated. Dr. Tappeiner and others have shown that the same effect is produced on the animal if tubercular matter (such as the sputa of phthisical patients) be diffused in spray in the air which the animal breathes; and Prof. Gerlach of Hanover showed twelve years ago with regard to the bovine variety of tubercular disease (the perlsucht of the Germans), that its infection can be freely introduced through the stomach if bits of tubercular organs be given in the food, or if the healthy animal be fed with milk from the animal which has tubercle. That the communicability of tubercle from animal to animal is also being tested to an immense extent by *popular experiments* on the human subject, is what a moment's reflection will tell; and from that wide field of experiment I select one instance for illustration. I have every reason to believe that Prof. Gerlach's experiments on the communicability of tubercle by means of milk are very extensively parodied by commercial experiments on the human subject. I learn, on what I believe to be the highest authority in this country, that tubercle (in different degrees) is a malady which abounds among our cows; and that so long as the cow continues to give milk, no particular scruple seems to prevent a distribution of that milk for popular use. To the persons who consume that milk an important question as to the causability of tubercle is put in an experimental form. Whether they will become infected with tubercle is a question which the individual consumers do not stand forward to answer for themselves, like the animals of the laboratory experiments: but Dr. Creighton's lately-published book, entitled, "Bovine Tuberculosis in Man," and a paper in which I am glad to say he brings under notice of our section the very remarkable series of facts on which he grounds that startling title, seem to suggest a first instalment of answer in accordance with Prof. Gerlach's experimental finding.

The two sorts of experiment—the scientific and the popular—differ, as I have noted, in this particular: that the popular experiment is almost always done on man; the scientific almost always on some other animal. It is true that many memorable cases are on record, where members of our profession have deliberately given up their own persons to be experimented on by themselves or others for the better settlement of some question as to a process of disease; have deliberately tried, for instance, whether, in this way or in that, they could infect themselves with the poison of plague or of cholera; and as regards one such case which is in my mind, I think it not unlikely that the illustrious life of John Hunter was shortened by the experiments which he did on himself with the ignoble poison of syphilis. There have been cases, too, where criminals have been allowed to purchase exemption from capital or other punishment at the cost of allowing some painful or dangerous experiment to be performed on themselves. And cases are not absolutely unknown where unconsenting human beings have been subjected to that sort of experiment. But waiving such exceptions, the rule is, as I have said, that scientific experiments relating to causes of disease are performed on some animal which common opinion estimates as of lower importance than man. Now, as between man and brute, I would not wish to draw any distinction which persons outside this room might find invidious; but, assuming for the moment that man and brute are of exactly equal value, I would submit that, when the life of either man or brute is to be made merely instrumental to the establishment of a scientific truth, the use of the life should be economical. Let me, in that point of view, invite you to compare, or rather to contrast with one another, those two sorts of experiment from which we have to get our knowledge of the causes of disease. The commercial experiments which illustrated the dangerousness of sewage-polluted water-supplies cost many thousands of human lives; the scientific experiments which with infinitely more exactitude justified a presumption of dangerousness, cost the lives of a few dozen mice. So, again, with experiments as to the causation of tubercle:—judging from the information which I quoted to you, I should suppose that the human beings whose milk-supply on any given day includes milk from tubercular cows might be counted, in this country, in tens of thousands; but the scientific experiments which justify us in declaring such milk-supply to be highly dangerous to those who receive it were conclusive when they amounted to half-a-dozen. So far, then, as regards the mere getting of experimental knowledge, we must not, with a view to economy of life, be referred to popular, rather than scientific, experiment. And in the same point of view, it perhaps also deserves consideration that the popular experiments, though done on so large a scale, very often have in them sources of ambiguity which lessen their usefulness for teaching.

Let me now briefly refer to the fact that, during the last quarter of a century, all practical medicine (curative as well as preventive) has been undergoing a process of transfiguration under the influence of laboratory experiments on living things. The progress which has been made from conditions of vagueness to conditions of exactitude has, in many respects, been greater in these twenty-five years than in the twenty-five centuries which preceded them; and with this increase of insight, due almost entirely to scientific experiment, the practical resources of our art, for present and future good to the world, have had, or will have, commensurate increase. Especially in those parts of pathology which make the foundation of preventive medicine, scientific experiment in these years has been opening larger and larger vistas of hope; and more and more clearly, as year succeeds year, we see that the time in which we are is fuller of practical promise than any of the ages which have preceded it. Of course, I cannot illustrate this at length, but some little attempt at illustration I would fain make.

First, let us glance at our map. When we generalise very broadly the various causes of death (so far as hitherto intelligible to us) we see them as under two great heads, respectively autopathic and exopathic. On the one hand, there is the original and inherited condition under which to every man born there is normally assigned eventual old age and death, so that, sooner or later, he "runs down" like the wound-up watch with its ended chain; and, as morbidities under this type, there are those various original peculiarities of constitution which make certain individual tenures of life shorter than the average, and kill by way of premature old age of the entire body, or (more generally) by quasi-senile failure of particular organs. On the other hand, as a second great mass of death-causing influence, we see the various interferences which come from outside; acts of mechanical violence, for instance, and all the many varieties of external morbific influence which can prevent the individual life from completing its normal course.

As regards cases of the first class—cases where the original conditions of life and development are such as to involve premature death (which in any such case will commonly show itself as a fault in particular lines of hereditary succession)—the problem for preventive medicine to solve is, by what cross-breeding or other treatment we may convert a short-lived and morbid into a long-lived and healthy stock; and this, at least as regards the human race, has, I regret to say, hardly yet become a practical question. But, as regards cases of the second class, evidently the various extrinsic interferences which shorten life have to be avoided or resisted, each according to its kind; and here it is that the scientific experimenters of late years have been giving us almost daily increments of knowledge.

Two early instances, vastly important in themselves, though of a comparatively crude kind, I have already mentioned; and I now wish to glance at some illustrations of the immense scope and the marvellous exactitude of the newer work.

The invaluable studies of M. Pasteur, beginning in the facts of fermentation and putrefaction, and thence extending to the facts of infectious disease in the animal body, where M. Chauveau's demonstration of the particulate nature of certain contagia came to assist them—they, I say, partly in themselves, and partly in respect of kindred labours which they have excited others to undertake, have introduced us to a new world of strange knowledge. We have learnt, as regards those diseases of the animal body which are due to various kinds of external cause, that probably all the most largely fatal of them (impossible yet to say how many) represent but one single kind of cause, and respectively depend on invasions of the animal body by some rapidly self-multiplying form of alien life. This doctrine, which scientific experiment initiated, has, for the last dozen years, been extending and confirming itself by further experiment. As soon as the doctrine began to seem probable, science saw that, should it prove true, it must have the most important corollaries. If the cause of an infecting human disease is a self-multiplying germ from the outside world, the habits of that living enemy of ours can be studied in its outside relations. It becomes an object of common natural history, it has biological affinities and analogies. We can cultivate it in test-tubes in our laboratories, as the gardener would cultivate a rose or an apple, and we can see

what agrees and what disagrees with its life. And then, as the next and immeasurably most important stage, where nothing but experiment on the living body will help us, we can try whether perhaps any of our modifications of its life have made it of weaker power in relation to the living bodies which it invades, or whether, through our more intimate knowledge of its vital affinities, we can artificially give to bodies which it would invade, a partial or complete protection against it. Such, at first blush, were the obvious possibilities of research which the new doctrine of infectious disease suggested to the mind of the pathologist; and never since the profession of medicine has existed, had a field of such promise been before it. The promise has not been belied. A host of diseases has been worked at in such lines as I just now indicated, and with many of them important progress has been made.

It would be impossible for me even to name a twentieth part of the investigations which have been more or less successful. As regards some which have most struck me, I pass with but a word Dr. Klein's investigation of the pneumo-enteritis of swine; Prof. Cohn's and Dr. Koch's and Dr. Buchner's respective contributions to the natural history of the anthrax bacillus; Dr. Bollinger's recognition of the microphytic origin of an important cancroid disease of horned cattle, with Dr. Johne's illustration of the inoculability of this disease; the research by Drs. Klebs and Tommasi-Crudeli into the intimate cause of marsh-malaria; and, not least, the demonstration (as it appears to be) which Dr. Grawitz has recently published, that some of the commonest and most innocent of our domestic microphytes can be changed by artificial culture into agents of deadly infectiveness. I pass these and others, in order that I may more particularly speak of some which have already shown themselves practically useful; for in respect of some of them the time has already come when abstract scientific knowledge is passing into preventive and curative knowledge.

First, and not in a spirit of national partiality, I will mention the application which M. Pasteur's doctrine has received at the hands of Mr. Lister, with regard to the antiseptic treatment of wounds: an application which, enforced and illustrated at every turn by Mr. Lister's own eminent skill as an experimentalist, has been confirmation as well as application of the parent doctrine; and the beneficent uses of which, in giving comparative safety to the most formidable surgical operations, and in immensely facilitating recovery from the most dangerous forms of local injury, are recognised—I think I may say, by the grateful common consent of our profession in all countries, to be among the highest triumphs of preventive medicine.

Secondly, out of the experimental studies of anthrax—chiefly out of those of Dr. Sanderson and Mr. Duguid in this country, and those of Dr. Buchner in Germany and M. Toussaint in France, has grown a knowledge of various ways in which the contagion of that dreadful disease can be so mitigated that an animal inoculated with it, instead of incurring almost certain death, shall have no serious illness; and the further knowledge has been gained that the animal submitted to that artificial procedure is thereby more or less secured against subsequent liability to the disease. In other words, with regard to that disease, an infliction which sometimes spreads to man from his domestic animals, and one which in some parts of Europe is of serious consequence to agricultural interests, as well as to animal life, the later experimenters—of whom I may particularly name M. Toussaint and our countryman, Prof. Greenfield, seem to be giving to the animal kingdom, and to the farmers, the same sort of boon as that which Jenner gave to mankind when he taught men the use of vaccination. Quite recently, our great leader, M. Pasteur, seems to have made, by new experiments, still further progress in the mitigation of anthrax.

Thirdly, a similar discovery has been made by M. Pasteur, with reference to the contagium of a very fatal poultry disease, known by the name of fowl's cholera; he has learnt to mitigate that contagium to a degree, in which, if fowls be inoculated with it, they will suffer no serious ailment; and he has found that fowls so inoculated (or, as he, in honour of Jenner, would say, "fowls so vaccinated") are proof against future attacks of the disease.

Fourthly, Prof. Semmer of Dorpat, through experiments done under his direction by Dr. Krajewski, has made a similar discovery in regard of the infection of septicæmia; has found, namely, that by treatment like that with which M. Toussaint mitigates the contagium of splenic fever, he can bring the most virulent septic contagium into a state in which it shall be mild enough to serve for harmless inoculations; which inoculations, when performed, shall be protective against future infections.

Finally, in a different direction of experimental work, let me name the recent most admirable research which Dr. Schüller of Greifswald has made, nominally in respect of certain surgical affections of joints, but in reality extending to the inmost pathology and therapeutics of all tubercular and scrofulous affections. A knowledge of the fatal infectiveness of crude tubercular matter had been given (as I before said) by Villemin and those who followed him; and Prof. Klebs, four years ago, declared the infective quality to be due to the presence of a microphyte (micrococcus), which he had succeeded in separating from the rest of the matter by successive acts of cultivation in fluids of inorganic origin. Dr. Schüller solidly settles, and widely extends, that teaching. According to his apparently quite unquestionable observations and experiments, the micrococcus which characterises tubercle characterises also certain affections popularly called "scrofulous"—namely, "scrofulous" synovial membrane, "scrofulous" lymph-glands, and lupus: so that these diseases may be defined as essentially tubercular, and that inoculation with matter from any of them, or with a cultivation-fluid in which the micrococcus from any of them has been cultivated, will infect with general tuberculosis. The rapid multiplication of the tubercle-micrococcus in the blood and tissues of any inoculated animal can be verified both by microscopical observation, and by inoculative experiment; and an extremely interesting part of the research, in explanation of certain of our human joint-diseases, is the demonstration that if in the inoculated animal a joint is experimentally injured, that joint at once becomes a place of preferential resort to the micrococcus which is multiplying in the blood, and becomes consequently a special or exclusive seat of characteristic tubercular changes. Even thus far the practical interest of Dr. Schüller's book is such as it would not be easy to overstate, but still greater interest attaches to the last chapter of the book, in which, confidently resting on the pathological facts which I have quoted, he makes proposals for the treatment of tubercle on the basis of its microphytic origin, and shows the successful result of such treatment as he has hitherto tried, from that basis, on animals artificially infected by him.

I venture to say that in the records of human industry it would be impossible to point to work of more promise to the world than these various contributions to the knowledge of disease, and of its cure and prevention; and they are contributions which from the nature of the case have come, and could only have come, from the performance of experiments on living animals.

At this most productive epoch in the growth of medical science, our English studies have been interrupted. An Act of Parliament, passed five years ago under the title of the Cruelty to Animals Act, has made it difficult or impossible for scientific observers any longer to follow in this country any such courses of experiment as those which of late years, at the cost of relatively insignificant quantities of brute suffering, have tended to create an infinity of new resources of relief for the sufferings both of brute and man. The Act does not in express terms interdict all performance of such courses of experiment: it nominally allows them to be done under a variety of limited licences which may be granted by a Principal Secretary of State; but the limitations under which these licences are granted, and the trouble, delay, and friction which necessarily to some extent, and, in fact, often to an intolerable extent, attend the obtaining of any one of them, are practically little better than prohibition.

The Act apparently contemplates, as the chief subjects of its operation, an imaginary class of unqualified persons, who, with no legitimate relation to scientific research, would, under pretence of such research, torture, and (it is supposed) take pleasure in torturing, live animals; and against this devilish class of persons the Act is very indulgently framed: for, instead of expressly refusing licence to unqualified persons, and perhaps hinting to such of them as would do wilful cruelty under pretence of study that the lash and the treadmill are for such scoundrels—instead of this, I say, the Act virtually confounds together that imaginary class of unqualified and cruel persons, and, on the other hand, our professional class of *bonâ fide* scientific investigators, on whom the progress of medicine depends, and whose names are sufficient security for their conduct. What is counted good for the one class is also counted good for the other. The law will trust no licensed experimenter farther than

it can provide for his being minutely watched and regulated by the Secretary of State: and in respect of the details of experimental procedure, the supervision of that high political officer is substituted for the discretion and conscience of the scientific investigator.

Consider for a moment what this means in regard of the members of our profession whom it affects. Contrast with it the almost unbounded trust with which the world, from time immemorial, has regarded the character of our profession. Consider the relation of inmost confidence in which members of our profession in every corner of the kingdom are admitted to share in the sanctities and tendernesses of domestic life. Consider our immense daily responsibilities of human life and death. Consider that there is not a member of our profession to whom the law does not allow discretion that, in certain difficulties of childbirth, he shall judge whether he will kill the child to save the mother. And in contrast with all this, is it to be seriously maintained that society cannot trust us with dogs and cats? that our foremost workers—(for it is essentially they who are affected)—cannot be trusted to behave honestly towards their brute fellow-creatures, unless they be regulated and inspected under a special law in much the same prevenient spirit as if they were prostitutes under the Contagious Diseases Act?

I have reason to believe that, if that Act continues on the statute-book, one of two results will follow. Experiments, indispensably necessary for the growth of medical science in relation to the cure and prevention of disease, will cease, or almost cease, to be done in this country; or, as the alternative to this, persons who desire to advance the science of their profession, will be tempted to clandestinely ignore the law and to run their chance, if the worst comes to the worst, of having to try conclusions with the common informer.

Let me illustrate this by two personal references: I have already mentioned Prof. Lister as an experimenter, whose name is now classical wherever science has reached, and whose work has been of signal advantage to mankind. Last autumn Mr. Lister wished to do some experiments in extension of the particular branch of knowledge with which his name is identified, and at a point which he considered of extreme importance in surgical pathology. He found he must either abandon his investigation or must conduct it in a foreign country, and in his zeal for science he chose the latter course. His experiments (which had to be on large animals) were done at the Veterinary College of Toulouse; and in stating this fact in a letter, from which I quote, Mr. Lister added that "even with reference to small animals, the working of the Act is so vexatious as to be practically prohibitory of experiments by a private practitioner like myself, unless he chooses to incur the risk of transgressing the law." A second name which I have mentioned is that of Prof. Greenfield, who has so highly distinguished himself in developing, by means of experiments, the preventive medicine of splenic fever. Dr. Greenfield, in order to perform his inoculation-experiments, had of course to become a licence-holder under the Act; and his experience of the hindrances which attach to that position is expressed to me in the following terms: "It is my deliberate conviction, as a result of my experience, that these hindrances and obstacles are so numerous and so great as to constitute a most serious bar to the investigation of disease, and even of such remedial measures as would by common consent be for the direct benefit of the animals experimented upon. When to this is added all the annoyance and opprobrium which are the lot of investigators, it is to be wondered at that any one should submit to be licensed." Dr. Greenfield's experimental operations consisted only in inoculating the virus of animal diseases, and he says: "I have not been engaged in other investigations for the simple reason that, with the present restrictions and the difficulty in obtaining a licence, I regard it as almost hopeless to attempt any useful work of the kind in this country."

As I feel sure that the Act must at no distant time be reconsidered by the Legislature, and as I also very strongly feel that, quite apart from any question of legal enactments, there is the question of moral right or wrong to be considered in the matter, I would beg you to allow me to make my own public confession of faith (from which I dare say yours will not much differ) in that extremely important matter of controversy.

The question being whether medical science can rightly use living animals as subjects for experiments which may be painful, and even, in exceptional cases, very painful to them, the answer may be sought in either of two directions: 1. What says the voice of the experimenter's own conscience? and 2. What says the standard of common contemporary conduct in analogous cases?

As regards the first, if I may take the liberty of expressing my own feeling, I would say this. I do not in any degree regard it as matter of indifference that, in certain cases, by my own hand or by that of some one acting for me, I must inflict death or pain on any living thing. I, on the contrary, think of it with true compunction; but I think of it as good or bad according to the end which it subserves. Where I see my way to acquire, at that painful cost, the kind of exact knowledge which, either in itself or in contribution to our common stock, will promote the cure or prevention of disease in the race to which the animal belongs, or in the animal kingdom generally, or (above all) in the race of man, I no more flinch from what then seems to me a professional duty, though a painful one, than I would, in the days before chloroform, have shrunk from the cries of a child whom I had to cut for stone. If, in a case of the latter sort, the surgeon nerves himself to his work by the conviction of an indispensable usefulness in what he has to do, so does the pathologist in his, and surely in a much larger sense. The agitated parent of the child might sometimes be tempted to say: "Forbear giving this cruel pain; let the poor little sufferer die"; but the surgeon's reply would have comforted her. And so with the physiological experimenter: except that he, instead of looking at one individual life to be saved, is looking at a race or at many races, and reflects how, in respect of some grievous physical misery, the whole of them, in all their multitudinous successions, may be redeemed through the suffering of the few. This is my personal view of the abstract right or wrong in the question. I state it because, in matters of right and wrong, no man ought to shelter himself behind authority. I believe I may add that if it, or something very like it, had not for centuries been the general view of the medical profession, our professional knowledge would probably be standing in this present age about where it stood in the days of the Plantagenets. Of Harvey and Hunter and Bele, we well know that such was the view on which they acted in rendering their immortal services to mankind; and I am not aware that any man, whose opinion really counts in matters of medical science, would express any material dissent from it.

The second standard to which I referred was that of the common conduct of men in *analogous* cases. I pointedly say "analogous," rather than "similar," because common life does not in fact give cases which, properly speaking, are "similar" to ours. But what, I ask, is the common *principle* of behaviour of civilised man towards the so-called lower animals? He in every respect subordinates their lives to his own. If he thinks he can get an advantage to himself by killing or painfully mutilating an animal, he does so with apparently no hesitation. See, for one instance, the sexual mutilations which are inflicted on all but a small minority of most kinds of domestic animals, and, as regards some kinds, on many of the females. When I appeared as a witness six years ago before the Royal Commission which was considering the question of our experiments, I particularly endeavoured to draw their attention to this view of the case; and in one of my answers (No. 1491) I entered on it more fully than would be suitable to the present occasion.

Thus, either way, whether I look to what I may call the general conscience of the medical profession, or look to the principles by which men in general govern their conduct towards the brute creation, nowhere do I see fair ground on which exception from outside can be taken to a limited, a strictly economical, use of animal life for purposes of scientific experiment.

No doubt there can be found, outside the medical profession, excellent persons, and plenty of them, whose first inclination would be to dissent from that position of ours; and some such persons have (as I think, hastily) given public utterance to such first impressions, and done their best to promote legislation against us. Among names which I see identified with opinions different from ours, are some for which I have deep respect. Particularly of one such man, whom I have the honour to know, I think it may be truly said that his own whole life has been one of practical beneficence, and I would not willingly incur the censure of any such man. But even to him I would fearlessly say, that I think he has not done justice to the case of our profession. To him, and such as him, I would confidently appeal to reconsider their first impressions. On him, and such as him, I would urge that the practice of scientific experimentation on

living animals is but an infinitesimally small application of the licence which common life claims for itself in regard of animals; and I would challenge such men to examine, with strict impartiality, what are their own responsibilities, direct and indirect, in regard of the infliction of pain on living animals.

I protest against any man's applying to this extremely important question a purely arbitrary standard of right or wrong. Those who pronounce judgment on their neighbours must be prepared to state the principle on which they judge. "Compound for sins you are inclined to, by damning those you have no mind to," is the Pharisee's easy-going formula. Where would life be if that were generally accepted? Suppose a *genus* of action; let men draw an arbitrary line across it—a line prescribed by no better rule than that which governed the lady's dislike to Dr. Fell; let them affix a nickname of praise to all on one side of the line, and a nickname of dispraise to all on the other: truly we should thus have the readie t of royal roads to unlimited mutual persecution.

And I protest against a standard of right and wrong being fixed for us on grounds which are merely sentimental. In certain circles of society, at the present time, æsthetics count for all in all; and an emotion against what they are pleased to call "vivisection" answers their purpose of the moment as well as any other little emotion. With such sections of society, our profession cannot seriously argue. Our own verb of life is ἐργάζεσθαι, not αἰσθάνεσθαι. We have to think of usefulness to man. And to us, according to our standard of right and wrong, perhaps those lackadaisical æsthetics may seem but a feeble form of sensuality.

Of the mere screamers and agitation-mongers who, happy in their hysterics or their hire, go about day by day calumniating our profession and trying to stir up against it the prejudices and passions of the ignorant, I have only to express my contempt.

I regret to have had to speak at so much length of the heavy cloud which at present hangs over the study of scientific medicine in England, and which, in my opinion, is likely to be of specially disastrous effect on the progress of preventive medicine. As a very old public servant in that cause, I should indeed grieve to see it brought to a stand-still for want of the scientific nurture which, in truth, is its very basis of life; and, speaking publicly of the danger on this occasion, I have hoped that the occasion may give importance to what I say.

And now, gentlemen, from contemplating that cloud, which happily is but local, and which perhaps may be but temporary, I gladly turn to skies which have no cloud. If there exist in the social organism any function whatsoever for which development and eventual triumph may be foretold, surely it is that of State Medicine. Of the two great factors concerned in it—the two strong powers which within our own time have converged to make it the reality which it is—the growth of science on the one hand, and the growing stress of common humanity on the other, neither one is likely to fail. Of our science it is needless to say that it will grow. To the science of nature indeed is allotted that one incomparable human day which knows no sunset. In the pure light of its ever-present daybreak, individual workers will pass away, generations will change, but the studies of Nature, and, above all, the gathering of such knowledge as can lessen man's physical difficulties and sufferings, will surely grow from age to age, and, as on Proserpina's sacred tree, one golden fruit will follow another: "simili frondescet virga metallo." And no less also in the other direction, the auguries are wholly for our cause. Popular education is gradually making its way, and it will grow to be a force on our side. Masses of mankind that now have to be humbly pleaded for by others, will then be strong to speak for themselves. Physical interests, now but little understood, will then be within grasp of all men's apprehension. Not only will health be recognised at its true value, and its elementary requirements be regarded, but also the frauds and villanies which are now committed against it will have become intelligible to the common mind; and the workman of the future will strike against being cheated in health as he would now strike against being cheated in wages. As such times come to the world, the science and the profession which care for man as man will get to be better appreciated than now. And in proportion as an educated people grows to become Body-Politic, State Medicine will be seen to represent the true ideal of Government-action which sets its standard of success in the "greatest happiness of the greatest number."

OUR ASTRONOMICAL COLUMN

THE GREAT COMET OF 1881.—The observations of this body in both hemispheres from its discovery on May 22 by Mr. Tebbutt at Windsor, N.S.W., to the end of last month, are closely represented by a parabolic orbit. The intensity of light is now rapidly going off, and if any decided deviation from the parabola is established it can only be through the later observations in these latitudes. It is therefore important for the theory of the comet that the larger instruments in our observatories should be brought to bear upon the accurate determinations of position, and that this should be continued as long as practicable. The following ephemeris for Greenwich midnight is calculated from elements, which are likely to give the comet's places pretty closely:—

	Right Ascension. h. m. s.	Declination. ° ′	Log. Distance from Earth.	Log. Distance from Sun.
August 20	14 31 0	+77 19·6	0·1206	0·1501
22	38 10	77 3·0		
24	45 22	76 47·1	0·1376	0·1672
26	52 36	76 31·9		
28	14 59 52	76 17·2	0·1532	0·1837
30	15 7 10	76 3·2		
Sept. 1	14 31	75 49·2	0·1676	0·1995
3	21 54	75 35·7		
5	29 18	75 22·4	0·1810	0·2148
7	36 46	75 9·3		
9	44 16	74 56·3	0·1934	0·2295
11	51 51	74 43·4		
13	15 59 29	74 30·8	0·2050	0·2436
15	16 7 11	74 18·1		
17	14 57	74 5·4	0·2160	0·2572
19	22 47	73 52·6		
21	30 42	73 39·7	0·2264	0·2704
23	16 38 41	+73 26·7		

The intensity of light on September 23 will be only one-third of that on August 20.

Dr. B. A. Gould has published in pamphlet-form an account of the Cordoba observations of this comet, with particular reference to his observations of June 11, to which we referred last week. We give his conclusions respecting the object seen that evening in his own words:—"La latitud considerable presta poca probabilidad á la hipótesis de que esta estrella haya sido un planeta interior. El movimiento relativo en declinacion, y la falta de cualquier objeto visible de la misma clase en la vecindad del cometa el dia siguiente, no parecen admitir la suposicion que el cometa se hubiera dividido como el de Biela. El brillo que se necesitaba, para que fuese visible la estrella en aquel momento y aquella posicion, indica una magnitud no inferior á la tercera.

"Esta observacion tambien tiene que esperar su solucion en lo futuro, y tal vez solo despues de muchos años."

SCHÄBERLE'S COMET.—According to M. Bigourdan's elements, the position of this comet at Berlin midnight on August 23 will be in R.A. 11h. 42·5m., Decl. +40° 34′, and at the same hour on August 25 in R.A. 12h. 16·0m., Decl. +34° 14′, and the intensity of light will be at a maximum between these dates. It may be observable in the other hemisphere for some weeks after perihelion passage.

THE COMPANION OF SIRIUS.—Prof. Colbert of the Dearborn Observatory, Chicago, has calculated the following orbit of the companion to Sirius:—Apastron passage, 1867·0, position of node, 42°·4; node to periastron in the direction of the star's (retrograde) motion, 133°; inclination, 57°·1; eccentricity, 0·58; semi-axis major, 8″·41; period, 49·6 years. These elements give, for 1881·2: angle of position, 45°·6; distance, 9″·9; and for 1882·2, position 43°·1; distance, 9″·5. For 1890·2 the position is 322°·2; distance, 2″·2; and the distance is near its minimum.

SCIENTIFIC SERIALS

The Journal of Anatomy and Physiology, vol. xv. Part IV. July, 1881, contains: On the ovary in incipient cystic disease, by Dr. V. D. Harris and A. Doran (Plate 23).—The anatomy of the Koala (*Phascolarctos cinereus*), by Dr. A. H. Young.—On the lymphatics of the pancreas, by Drs. George and F. Elizabeth Hoggan (Plate 24).—A case of primary cancer of the femur, by R. Maguire (Plate 25).—A case of chronic lobar pneumonia, by