and ebb tide, noticed by Lieut. Greely, I think is explained by the fact, that, to the south of Robeson Channel, the sound is kept open more or less by a strong current, and the water so exposed loses more of its latent heat in the winter than that to the north where it is protected by the ice-cap; and as the difference was only about .2 of 1°, it may be there is a difference of density. As to Mr. Cremin's theory that the flattening of the earth at the poles brings the outer crust nearer to the internal fires of the earth, I can only say that I know it to be a fact that the surface indications within the arctic circle do not bear him out in his theory. As it is a well-known fact that the earth north of the arctic circle is perpetually frozen to a great depth, and as the earth probably cooled from the surface, it is fair to presume that it at least cooled as fast at the poles as at the equator; and I think that a residence of a year or two will convince any reasonable man that the crust is tolerably thick up there, if extreme cold has any thing to do with it.

P. H. RAY.

Washington, Sept. 13.

Discrediting American science.

On p. 48 of the current volume of *Science* you take occasion to say, —

"Work of value upon the subject of micro-organisms is not done in this country, nor will it be until some such encouragement is offered to investigators as is the case in France and Germany. This kind of research requires the rare combination of many forms of training, added to a critical, analytical, and judicial mind. These we can have; but until the facilities for work are offered, until the necessity for personal sacrifice and self-denial is done away with, we can hope for no better work in the future than has been done in the past: in other words, what is first needed in order to place our own investigations upon an equality with those of the two countries mentioned above, is a thoroughly equipped, fully endowed laboratory, with a strong corps of well-trained and salaried officials."

Now, while you doubtless had in mind, when penning this paragraph, the great desirability of more systematic investigation in this country of those plagues of mankind which annually cut short so many valuable lives, I cannot allow this sweeping and unjust assertion to pass unnoticed, and to stand as a disparagement to American science and a reproach to American investigators. Whether you realize it or not, it is nevertheless a fact, that the patient student of micro-organisms in this country has been laboring under the enormous disadvantage that his work, however valuable it may be, is discredited at home, and unnoticed abroad, while the most absurd generalizations of the European worker are received with approval there, and enthusiasm here.

Sternberg has worked for years on intermittent fever, tuberculosis, septicaemia, yellow-fever, germicides and allied subjects; and, beyond his own writings and the reviews of his books, what is there in American literature to show that such a man has existed? About the time that Pasteur announced the discovery of his now celebrated 'new disease' produced by inoculating rabbits with the saliva of a child dead of hydrophobia, Sternberg demonstrated the virulence of normal human saliva when rabbits were inoculated with it.[1] He also demonstrated beyond question that this was due to a micrococcus which might be cultivated to the eighth culture without losing its virulence, and even showed that an immunity might be granted by protective inoculation.[1] Both had been working at the same time with the same organism, and had reached substantially the same result. Pasteur's work was published as something remarkable the world over; while Sternberg's — well, we must admit it received some credit abroad, even if it fell flat at home. Again: Sternberg's tests of germicides are, perhaps, the most extensive and satisfactory investigations in this line that have ever been made. He was certainly one of the first who attempted to obtain exact results by allowing a disinfectant of a given strength to act on a particular disease-germ for a given length of time, and then tested his results by cultivation and inoculation experiments.[2] And surely his experiments and results in photographing micro-organisms cannot be set down as entirely valueless.[3]

A short time ago the rather absurd speculations of Tyndall, in regard to the nature of the immunity from contagious diseases which is conferred by a previous attack, attracted wide-spread attention both in Europe and America. Tyndall's views were based upon the theories of Pasteur; and these, in turn, rested upon a very narrow basis of experimentation with fowl-cholera, which, at the time they were put forth, were far-fetched, and now are antiquated. Pasteur is a chemist, and Tyndall a physicist; and neither has any adequate conception of the fact that there are processes going on in the animal body which both chemistry and physics are incompetent to explain. Pasteur's chemical explanation of the mystery of immunity — that it was the exhaustion from the body of something necessary for the nutrition of the virulent germ; something that, once exhausted, was not again replaced — had a great fascination for the great English physicist, and he received it with childlike trust. What objection could there be, indeed, from his stand-point, to the view that a living body may be compared in every respect with the test-tube and the flask with which he is in the habit of experimenting in his laboratory? And when a Frenchman and an Englishman unite in pressing so plausible a theory, we surely could hardly expect from past experience that the American scientific editor would pay much attention to the vulgar home worker, no matter how striking his experiments, or how conclusive his demonstrations. I trust, however, you will pardon me for calling your attention to the fact that more than two years ago I demonstrated that immunity was only relative, and never absolute; that the most susceptible individual possessed a certain degree of immunity which can be accurately measured; and that all degrees of immunity may be overcome by a sufficient increase in the dose of virus. The immunity of the animal body, then, in no sense resembles the exhausted cultivation-liquid in the flasks of Pasteur and Tyndall, which no increase in the amount of virulent material added can ever induce to support the development of new generations of the microbe; and the honor of demonstrating this radical difference is due to American investigations.

I went farther than this, however, and showed that this theory of our European friends was absolutely untenable; because broth made with distilled water from the flesh of an animal that had been granted a very complete immunity was just as favorable a medium for the growth of the virus as that made from

[1] Bulletin of National board of health, April 30, 1881.

[1] Bacteria. By Dr. ANTOINE MAGNIN and GEORGE M. STERNBERG, M.D., F.R.M.S. New York, *William Wood & Co.*, 1884. pp. 355-376.

[2] *Ibid.*, pp. 209-235. National board of health bulletin, July 23, 1881.

[3] Photo-micrographs, etc. By GEORGE M. STERNBERG, M.D. New York, *William Wood & Co.*, 1884.

susceptible animals; and that such virus lost nothing in virulence by being grown in such a medium.[1] I will not trouble you with the theory of immunity which I developed from these experiments. Like other American work, it may have no value; but it may interest you to know that an able German writer in the last number of Virchow's *Archiv* has propounded a theory, which, in its most important points, is identical with my own.

In 1880 Mr. Chauveau published some observations and experiments which indicated that even a very virulent virus of a fatal disease might be made to produce a mild attack, if the dose administered was sufficiently small. About the same time I demonstrated by more numerous and direct experiments that this was true; but I went beyond this, and have the incontestible priority of demonstrating, —

1. That a certain number of the most virulent and fatal germs may be introduced into the most susceptible body without producing the least appreciable effect;[2]

2. That by increasing this number slightly, but still using a relatively small dose, germs which ordinarily multiplied throughout the whole body, and produced a constitutional disease, may be compelled to multiply locally, and cause only an insignificant local lesion without constitutional symptoms;[3]

3. That this local multiplication confers an immunity upon the whole body;[4]

4. That the immunity produced by a single inoculation with this diluted virus equals that produced by two inoculations with Pasteur's attenuated virus.[5]

Again: when Pasteur announced his discov ·y of the method of attenuating the virus of fowl-cholera, he coupled it with the theory that this attenuation was due to the action of atmospheric oxygen; and, although the evidence in favor of this theory was neither direct nor abundant, what there was of it came direct from Paris, and this was sufficient to secure it universal attention and unqualified indorsement at the hands of scientific editors. A few experiments led me to conclude, however, that this theory was incorrect, that the attenuation could be secured in the absence of oxygen as well as in its presence, and that it was really due to loss of vitality, the result of keeping the germs for a considerable time under unfavorable conditions of life.[6]

It is true that these conclusions did not receive the least notice, favorable or unfavorable, either at home or abroad; but, as they have since been established beyond question by the elaborate researches of Chauveau and others, I am inclined to think that the fault was neither with me nor my experiments, but that it is confined to the fact of the work being done by an American, and on American soil.

I need no more than call your attention here to the fact that I demonstrated which one of the several germs that had been described as peculiar to swine-plague was the actual cause of the disease, and that this was more than a year before the same experiments were duplicated by Pasteur and his assistants, who, nevertheless, succeeded in bearing off the honors that belonged to the American discoverer. This subject was placed before your readers in sufficient detail in a recent number of *Science*.[7]

In regard to the peculiarities of the germ of fowl-cholera, and the exact effect of disinfectants and various conditions of existence upon it, you will find in my reports the record of nearly one hundred and fifty experiments which it has seemed to me might have a little, though possibly a very slight, value from the light which they throw upon the germ-theory in general, and especially upon that group of diseases caused by organisms which do not form spores.[1]

The admirers of Koch are ever on the alert for an opportunity to enlarge upon the perfection of his apparatus and the security of his processes. They forget, however, that the most satisfactory work which he ever did, that which raised him from an obscure physician to be an acknowledged scientific authority, was accomplished with an apparatus so primitive and imperfect, that, were any one to use it to-day, it would only create ridicule and contempt. I refer to his cultivations of the bacillus anthracis in unsterilized liquid on ordinary microscopic slides, placed over wet sand in a soup-plate to prevent evaporation, covered with a plate of glass, and warmed over an oil-lamp. His disciples can, perhaps, afford to criticise imperfect apparatus now; but it may not be out of place to remind them, that, if their master's first work had been rejected on this account, he would probably still be an unknown physician in an obscure German hamlet.

After all, what is there in Koch's method of cultivation on the surface of solid media that makes it preferable, or even equal, for general purposes, to cultivation in liquids? Is any scientific man at this day so ignorant as to believe that the intermittent heating of blood-serum for half a dozen times to 137° F. (58° C.) is sufficient to safely sterilize it?[2] Is it not an incontestible fact that cultivations on solid substances cannot be made, examined, and reproduced, without exposing a large surface to contact with unsterilized air and the countless germs which it contains?

It is not my desire, however, to detract in any way from the well-earned reputation of Dr. Koch and Mr. Pasteur (there is no danger that they will ever receive too much honor); but, when American science is sneered at and rejected because of alleged imperfections, one can scarcely avoid calling attention to the fact that Europeans also are fallible, and their methods not beyond criticism.

You do not seem to be aware, Mr. Editor, of the fact that appropriations for the investigation of the contagious diseases of animals have been made on a liberal scale for the past six years, and that a considerable part of this money has been used in the study of micro-organisms. The results of these investigations have been so satisfactory to the people at large and to congress, that a permanent bureau was established at the last session, a part of the duties of which is to continue this line of research. I have had a laboratory and an experiment-station under my direction in Washington for more than a year. And while I am willing to admit fallibility and imperfections, if one can judge from scientific articles and from the reports of those who have visited the laboratories of Koch and Pasteur, I see no reason why we should fear a comparison of our laboratory, apparatus, and methods, with those in use on the other side of the water.

It is true that the enormous development of our animal industries brings up a multitude of inquiries foreign to the subject of micro-organisms, which

[1] Department of agriculture, Annual report, 1881 and 1882, pp. 283–300.
[2] Department of agriculture, Annual report, 1883, p. 48.
[3] *Ibid.*, 1881 and 1882, pp. 285–288; also 1883, pp. 44–49.
[4] *Ibid.*, 1881 and 1882, p. 288.
[5] *Ibid.*, 1881 and 1882, p. 288.
[6] *Ibid.*, 1881 and 1882, pp. 283, 284.
[7] *Science*, iii. p. 155.

[1] Department of agriculture, Annual report, 1880. *Ibid.*, 1881 and 1882, pp. 272–306. *Ibid.*, 1883, pp. 44–52.
[2] Les organismes vivantes de l'atmosphère (P. MIQUEL, Paris, 1883), footnote, pp. 154, 155. Department of agriculture, Annual report, 1881 and 1882, p. 264.

divides the time of the director, and distracts his attention; but we are endeavoring to overcome these difficulties by a division of labor; and when the new bureau is fairly organized, and running smoothly, we hope, if not to satisfy all, at least to keep adding to our knowledge of animal contagia until we are able to combat them successfully.

A few weeks ago, in a review of the last report of the department of agriculture (*Science*, iii. pp. 689, 690), you took occasion, while speaking very kindly of the work that had been done, to intimate that the proposed investigations for the discovery and supply of vaccine to be used in preventing contagious diseases were uncalled for and useless; the argument being that the profession could be relied upon to prepare and apply such vaccines if they were of sufficient value.

In reaching this conclusion, you evidently left out of consideration the most important elements of the problem. In the first place, we have but a mere handful of veterinarians in the whole country, and these mostly located in cities where they are of little use in treating the diseases of meat-producing animals: in other words, the stock-raisers of the country are practically beyond private veterinary assistance, and will remain so for years to come. In the second place, there are not more than two or three veterinarians in the country who have had the training, or, indeed, who have any conception of the processes, necessary for the study and cultivation of the group of organisms to which the disease-germs belong. You admit more than this in the editorial to which I referred in the first part of this communication. In the third place, there is nowhere in this world a single man who can tell the exact conditions under which the germs of the diseases that are most dangerous in this country must be cultivated so that they will be safe as vaccines, and at the same time capable of conferring a certain immunity. This must be worked out by long and costly experiments; and surely no individual is likely to be found who will attempt so difficult and dangerous a service at his own expense. In the fourth place, you will observe that even those medicines of which the processes of manufacture are tolerably well known (such as quinia and nitrite of amyl, for instance) are produced by chemists, — specialists, — and not by the medical profession. How much more necessary would it be, then, for specialists to control such delicate manipulations and complicated apparatus as are required in the reproduction of uncontaminated germs, especially when these are to be held at a given point in the scale of virulence. But how can you ask our people to depend upon such specialists in one number, and within a month or two assure them that there is no one in the country who is doing work of value in this direction? If you turn your eyes to Germany, you will see Koch, as a government official, using national appropriations to study the organisms which produce the diseases of men and animals. Turn to France, and you see Pasteur, also by the help of the government, endeavoring to discover methods for the production of vaccines that may be used to prevent animal diseases. Do you see the unassisted veterinary profession in either country accomplishing any thing in this direction, though they vastly excel ours both in numbers and education? Why, then, should not the officials of our government do the same kind of work, and strive to attain the same ends? And, if supplying vaccines to our farmers should prove the most economical and satisfactory means of fighting certain contagious diseases, why should not the agricultural department furnish such vaccines?

Finally, if you are right in your supposition that "there must be some misconception lurking in the minds of the department officials, if they really suppose that the veterinary profession is necessarily incompetent to deal with a problem because, forsooth, the known methods of solving it happen to be delicate and expensive," I would like to ask how it happens that the animal plagues of this country are increasing their ravages from year to year without an effort, on the part of the veterinary profession, to hold them in check? If 'an ordinary citizen' supposes that our future is likely to be different from our past in this respect, he certainly shows a surprising ignorance of the methods that have been found necessary in every country where any success has been achieved.

Is it not our duty to accept great national problems as they actually exist, rather than in the shape they are pictured by the distorted imagination of the editorial philosopher, who comes in contact with germ-diseases in books and periodicals, but never sees them on the farm and the ranch, where their ravages amount to millions and tens of millions of dollars annually?

In closing, permit me to express my personal disappointment at the course which the editor of *Science* has decided to adopt in regard to this branch of our home work. It was expected that this periodical would be a true representative of American science, defending its conquests, and encouraging its workers to renewed exertions. With certain departments it has not failed to do this; but with others, as I trust I have shown in this communication, its only effect has been to discourage and discredit when honest and successful work was being accomplished; and in saying this, I know I am not alone in my opinion, for a number of well-known scientific men have recently expressed to me the same idea.

If, Mr. Editor, this communication is open to the charge of egotism and garrulousness, I hope it will not be forgotten that the American investigator who is overburdened with modesty stands but a poor chance in the struggle for existence with the conditions of environment so decidedly against him.

D. E. SALMON.

[We have but to repeat, that "work of value upon the subject of micro-organisms is not done in this country." If all work upon micro-organisms that any observer chooses to publish — the result of unskilled labor — is of value, then we have doubtless cast an unwarranted slur upon American investigators. If, on the other hand, only that work is of value in this field which is the result of untiring industry, long training, and judicial criticism, then our remark was just. To be of value, such work must be *complete* in all its details; and the relationship between a bacterium and a pathological process must be established beyond a reasonable doubt, provided the methods are correct, and there has been no error of observation. It remains to be seen whether American work of permanent value in this branch of research will not receive the same hearty recognition from our coworkers abroad as it has in all other branches where our excellence has deserved acknowledgment. In conclusion, we wish to state that we do not care for controversy, nor did we intend to excite it. It was our belief, that, on the whole, we have not yet thoroughly mastered all the requirements necessary for this most delicate branch of investigation, and that a reminder of that fact would do no harm. We sympathize with unrecognized merit, but would console it with the reflection that *Aucun chemin de fleurs ne conduit à la gloire.* — ED.]