

SCIENCE

FRIDAY, DECEMBER 2, 1887.

A VERY SIGNIFICANT DISCUSSION on the subject of manual training took place at the late annual meeting of the school superintendents of New York State, held at Rochester. A year or two ago such a discussion would not have been possible. In the first place, the superintendents themselves would not have been able to discuss the subject intelligently at that time, nor would it have been regarded as at all a pressing matter. The events of the last twelve months have, however, conspired to bring about the result which made possible the discussion to which we refer. The continued agitation of the subject by those best qualified to discuss it, the increase of the intelligent literature on manual training, and the magnificent display of the results of this training which was made at the meeting of the National Educational Association at Chicago last July, have all had their effect. They have brought light to many minds where darkness was before, and produced a conviction even among the most determined scoffers at the movement. The discussion at Rochester was introduced by Superintendent Cole of Albany, in which city a very gratifying progress has been made toward the introduction of manual training, and whose school board has a most intelligent idea of the whole subject. The superintendents of Newburg, Dunkirk, Ogdensburg, Binghamton, Owego, and Elmira seem to have been to a greater or less extent in favor of manual training. The event of the discussion, however, must have been the remarks of State Superintendent Draper, for it was reserved for him to advocate manual training in the public schools, not because it is disciplinary, but because of its eventual utility. The attitude of the State superintendent only shows to what remarkable extremes the complete misunderstanding of this subject may be carried. We have frequently heard manual training opposed because of its utility, and because it was claimed that it has no disciplinary value; but Mr. Draper is the first person who has discussed the subject in public who has sufficiently misunderstood the whole subject to advocate it on that ground. He is reported as saying that he had no sympathy with the argument advanced, that industrial training should be carried on for its intellectual force. He claimed that the present school system of the State contained all the intellectual force that was needed. We fancy that the mere statement of these two propositions is sufficient comment upon them. It is hardly necessary to undertake to controvert them seriously. It would be interesting to know, however, whether Mr. Draper proposes to carry his theory into practice, and to eliminate from the school course all subjects which have a disciplinary value, and to replace them with those which have a practical utility. If so, the coming generation in New York may not know how to read, write, cipher, draw, and parse, but it certainly will be able to manage a steam-engine, lay transatlantic cables, and drive horse-cars.

THE DANGER TO COMMERCE from derelict vessels on the high seas cannot be too often pointed out, as it is not generally realized how long they are liable to keep afloat and pursue their aimless course, — a constant menace to navigation, and the cause, no doubt, of the loss of many a fine vessel by collision. This is well illustrated by the following instances, taken from the records of the Hydrographic Office, and it should be remembered that no such record can be complete. Long intervals often elapse without any report being made, and the track during this time, assumed as a straight line on the chart, must generally fall short of the actual

distance travelled. The ship 'Ada Iredale' (voyage from Androsan, Scotland, to San Francisco) was burned in the South Pacific through the spontaneous combustion of the coal with which she was laden. She was abandoned Oct. 15, 1876, latitude $13^{\circ} 30'$ south, longitude $107^{\circ} 45'$ west, about 1,900 miles east from the Marquesas Islands. The crew of twenty-three men reached the Marquesas group in twenty-five days, with the loss of one man and one of their three boats. The still burning wreck of the vessel drifted slowly to the westward in the south equatorial current, to Tahiti, Society Islands, 2,423 miles distant, and was towed into port by the French cruiser 'Seignelay,' June 9, 1877. She continued to burn till May, 1878, when she was repaired, and as a handsome iron bark, named 'Annie Johnston,' has done good service in the trade with China. The drift was 2,423 miles, and the time nearly eight months. The ship 'Oriflamme' was abandoned, on fire, in June, 1881, latitude $18^{\circ} 12'$ south, longitude $92^{\circ} 42'$ west. On Oct. 24 the steamship 'Iron Gate' (voyage from Adelaide, Australia, to Portland, Ore.) passed in latitude $13^{\circ} 27'$ south, longitude $125^{\circ} 19'$ west, an iron ship, apparently burned, with no masts standing, and sent a lifeboat alongside, but could see no signs of life. On Feb. 12, 1882, the hull of an iron ship laden with coal and iron drifted ashore on the island of Raroia, one of the Paumotu or Low Archipelago (latitude $15^{\circ} 55'$ south, longitude $142^{\circ} 12'$ west). She was visited by some natives, who brought away a small bell upon which was engraved "'Oriflamme,' 1865." She was completely burned out, and in a short time sank in deep water. The drift was 2,840 miles, and the time about eight months. The abandoned schooner 'Twenty-one Friends' was first reported March 24, 1885, about 160 miles off the capes of Chesapeake Bay, latitude $36^{\circ} 45'$ north, longitude $72^{\circ} 40'$ west. The Gulf Stream carried her in a direction about east-north-east, to latitude $51^{\circ} 30'$ north, longitude $27^{\circ} 40'$ west (2,130 miles in four months and a half). Thence she drifted in an easterly and south-easterly direction towards the northern coast of Spain, and was last reported Dec. 4 of the same year in latitude 45° north, longitude 8° west (about 130 miles north-north-east from Cape Finisterre). She was reported, in all, twenty-two times, which in itself shows how especially dangerous such a derelict is on the North Atlantic. The drift was 3,525 miles, and the time eight months and ten days.

A CONSPIRACY OF SILENCE.

THERE is an interesting discussion going on in England at present between Professor Huxley, Professor Bonney, and the Duke of Argyll. The question at issue is whether the influence of a great name has become so great in science as to interfere with free discussion in questions of a purely scientific nature. It seems that some seven or eight years ago Mr. Murray offered an explanation of the origin and structure of coral reefs which controverted some of the opinions expressed by Darwin. It is maintained by one side that this theory of Murray's has not been given free publication and discussion, and that, while it is intrinsically more probable than the older theory of Darwin, it is still held in obscurity by a conspiracy of silence on the part of the leading men of Great Britain. To make clear the present state of the controversy, we publish below the articles published in *Nature* by Professor Bonney and the Duke of Argyll.

[COMMUNICATION FROM PROFESSOR BONNEY.]

THE Duke of Argyll is eminent as a statesman, and has won distinction as a man of science. The mental qualities, however, which lead to success in these capacities are widely different; nay, in the opinion of some, are almost oppugnant. To the man of

science, truth is as a 'a pearl of great price,' to buy which he is ready to part with every thing previously obtained: to the statesman, success is the one thing needful, for the sake of which hardly any sacrifice appears too great. This is not said wholly as a reproach: it "takes all sorts to make a world." The ardor of the follower of the ideal, which may degenerate into recklessness, is wholesomely checked and beneficially qualified by the calmness of one who has to deal practically with mankind, and has learned by experience that evolution rather than catastrophic change is the law of life, and is in accordance with the analogy of nature. Still the two types of mind are commonly diverse, and the Duke of Argyll has recently afforded a remarkable instance of the extreme difficulty of combining in one person these apparently opposite characters.

This instance is afforded by an article which appeared in the *Nineteenth Century* for September last, and is commented on by Professor Huxley in the number for the present month. The duke's article bears the somewhat imposing title of 'The Great Lesson.' Professor Huxley's reply forms a part of an article entitled 'Science and the Bishops.' As the charge which the duke has in effect brought against men of science is a very grave one, and as some of the readers of *Nature* may not be constant readers of the chief monthly magazines, a brief notice of both accusation and reply may not be without interest.

The moral of 'The Great Lesson' is practically, 'Beware of idolatry.' The scientific world, in the duke's opinion, has been for some time bowing down to the idol of Darwin and the theory of evolution, which is the fundamental dogma of that cult. Like a prophet of old, he raises a warning voice, and points out that the feet of the golden image are in part composed of clay. In the North has been hewn the stone which shall shatter those fragile supports and lay the idol prone in the dust. To abandon metaphor, this is the state of the case. Among the results of Mr. Darwin's labors during the voyage of the 'Beagle' in the years 1831-36, when he accumulated that vast store of observations which served as a foundation for 'The Origin of Species by Means of Natural Selection,' was a theory of the formation of coral reefs and atolls, set forth in a volume entitled 'On the Structure and Distribution of Coral Reefs' (published in 1842 and republished in 1874). Of this theory the duke gives an outline in 'The Great Lesson,' executing this portion of his task so fully in the spirit of a just judge, and with so little of the craft of an advocate, as to leave nothing to be desired for lucidity of statement and cogency of reasoning. In fact, in the judge's summing-up, the case for the defence appears stronger than that for the prosecution; so much so, indeed, as to suggest that the difference is due to their inherent merits rather than to the mode of statement. However, be that as it may, the duke thus pronounces judgment, and in so doing passes a censure, stinging if deserved, on the men of science of this generation.

These are his words (*Nineteenth Century*, p. 305):—

"Mr. Murray's new explanation of the structure and origin of coral reefs and islands was communicated to the Royal Society of Edinburgh in 1880, and supported with such a weight of fact and such a close texture of reasoning, that no serious reply has ever been attempted. At the same time, the reluctance to admit such an error in the great idol of the scientific world, the necessity of suddenly disbelieving all that had been believed and repeated in every form for upwards of forty years, of cancelling what had been taught to the young of more than a whole generation, has led to a slow and sulky acquiescence, rather than to that joy which every true votary of science ought to feel in the discovery of a new truth, and—not less—in the exposure of a long-accepted error."

Again:—

"The overthrow of Darwin's speculation is only beginning to be known. It has been whispered for some time. The cherished dogma has been dropping very slowly out of sight. Can it be possible that Darwin was wrong? Must we indeed give up all that we have been accepting and teaching for more than a generation? Reluctantly, almost sulkily, and with a grudging silence so far as public discussion is concerned, the ugly possibility has been contemplated as too disagreeable to be much talked about; the evidence old and new has been weighed again and again, and the obviously inclining balance has been looked at askance many times.

But, despite all averted looks, I apprehend it has settled to its place forever, and Darwin's theory of the coral islands must be relegated to the category of the many hypotheses which have indeed helped science for a time, by promoting and provoking further research, but which in themselves have now finally kicked the beam."

This, then, is 'The Great Lesson':—

"It is that Darwin's theory is a dream. It is not only unsound but is in many respects the reverse of the truth. With all his conscientiousness, with all his caution, with all his powers of observation, Darwin in these matters fell into errors as profound as the abysses of the Pacific."

This is plain speaking. In words which admit of no ambiguity the duke declares that Darwin was wrong; that Mr. Murray set him right; and that the latter, instead of receiving a welcome, was met with a virtual conspiracy of silence on the part of scientific men. Of these three assertions,—which are to a considerable extent independent one of another,—the first and second are obviously very much matters of opinion, because, if the third statement be true, it is clear that no verdict has been delivered by experts, but that, like an Irish jury, they have professed themselves unable to agree, because the facts were so strong that even they could not bring in a verdict of acquittal. The third assertion, however, is much more a matter of fact, not difficult to substantiate, and at any rate, if incorrect, easy to disprove.

In regard, then, to the first and second, it may suffice to follow Professor Huxley's example, and be content with expressing a doubt as to the accuracy of the duke's assertions. In the face of statements so definite as those quoted above, this may seem presumptuous. They read almost like the sentence of an ecclesiastical court, which it is heresy to question. *Caledonia locuta est, causa finita est*, seems to be their tone; and if one whisper a doubt, one expects the familiar conclusion, *Anathema sit!* But men of science, as all the world knows, are sceptics. Have they yet awakened and rubbed their eyes, and said of Darwin's theory, "Lo! it was a dream"? What says Professor Huxley? He asserts that Darwin's confidence in the accuracy of his own theory was not seriously shaken, as the duke alleges, and quotes as conclusive evidence a letter from Professor Judd, who gives the results of a conversation which he had with Darwin no long time before the death of the latter. Professor Huxley also intimates that to himself, though tolerably familiar with coral reefs, the new theory is at first sight so far from fascinating, that, until he can devote a considerable time to a re-examination of the whole subject, he must be content to remain "in a condition of suspended judgment," and that Professor Dana, "an authority of the first rank on such subjects," has pronounced against the new hypothesis in explicit terms. Undoubtedly, Mr. Murray has obtained distinguished converts, but with such differences of opinion among those best qualified to judge, it is certainly going further than is warranted by facts to insinuate, if not to assert, that he has convinced the scientific public. Very probably more than a minority of them are in my own position, which perhaps I may be pardoned for stating. They, like myself, have never had the opportunity of forming an independent judgment upon the matter, but they see some very serious difficulties—difficulties which are of a general rather than of a special nature—in the new explanation. At present these difficulties do not appear to them to have been overcome; so that, while admitting that Mr. Murray's hypothesis may sometimes apply, and that Darwin either may have expressed himself a little too sweepingly, or may have been understood so to do, the theory of the latter is capable of a more general application, and presents less serious general difficulties, than does that of Mr. Murray.

We come, then, to the third charge, which is the most serious one, because it affects the morality of scientific men; and many of them, like myself, are old-fashioned enough to resent being called a knave more than being called a fool. Has Mr. Murray been met by 'a conspiracy of silence'? The duke, in asserting this, must have been strangely oblivious of, or, among the cares of a statesman, have failed to keep himself *au courant* with, the literature of geology. Professor Huxley denies the assertion, and adduces in his support an answer to an inquiry which he had addressed to Professor Judd. The facts according to these authorities are briefly as follows: Mr. Murray's views were duly published, as the

duke himself states; they were favorably regarded by the authorities at the 'Challenger' office; they were expounded, one might almost say advocated, on more than one occasion by Dr. A. Geikie. His text-book in the year 1882 not only took the leading place, as it still does, but also was then the only complete text-book on a large scale for this country. On p. 468 is a full statement of Mr. Murray's views. They have also been referred to at more or less length in many treatises and journals, both English and foreign. As Professor Judd remarks, "if this be a 'conspiracy of silence,' where, alas! can the geological speculator seek for fame?"

Thus the main charge is disproved. One special item in it, however, as peculiarly offensive, yet calls for a brief notice. The duke states, "Mr. John Murray was strongly advised against the publication of his views in derogation of Darwin's long-accepted theory of the coral islands, and was actually induced to delay for two years." Now, if these words do not amount to an imputation of bad faith on the part of Mr. Murray's adviser, and are not by insinuation extended to others, I do not know what they mean, or why they have been penned. But, as Professor Huxley observes, "whether such advice were wise or foolish, just or immoral, depends entirely on the motive of the person who gave it." The remark is perfectly just. Who, I would ask, who is old enough to look back on a quarter of a century of work, has not occasionally said, "Wait a bit," to some younger friend, who has come in the first incandescence of a brilliant hypothesis? I have so sinned. Sometimes I have been wrong and my young friend right, but not always. Still, I know myself fallible. As the late master of Trinity said, "We are all fallible mortals, even the youngest amongst us." Yet I am not ashamed. I will not put on sackcloth and ashes, and I mean to sin again. Perhaps it is because I am naturally unimaginative; perhaps I am come to the season of autumn leaves; but I have always looked askance at a brilliant hypothesis, and now distrust it more than ever. I have lived long enough to see many a one go up *whoosh!* like a sky-rocket, all stars and sparks, and come down exploded, all stick and stink.

So the 'great lesson' has been read, and the scientific world, I fear, has not repented or rent its clothes. But it has heard, and not without indignation. The Duke of Argyll has made grave charges against the honor and good faith of men of science, and they ought to be grateful to Professor Huxley for his prompt repulse of the attack and his stern rebuke of the assailant. As it seems to me, reply is only possible on one point; namely, the special charge mentioned above. Hence the Duke of Argyll is bound to establish or to withdraw the accusation.

Men of science are justly sensitive on this question. Doubtless they are no more exempt from human frailty than any other class of men: we all fail sometimes—nay, too often—to live up to our ideal standard; still, such shortcomings are not common, and any thing like a 'conspiracy of silence' or any kind of scientific 'boycotting' is a thing so improbable as to be almost incredible. Each man must testify according to his own experience: so in conclusion, though it may be deemed impertinent, I will express my own. I have lived now for not a few years among the rank and file of scientific men on more intimate terms than can have been possible for the Duke of Argyll, owing to his exalted station and his high occupations of state, and I am bound to declare, that, in a fairly wide experience, I have never found men as a class less self-seeking or more earnest in their desire for truth, more steadfast as friends, or more generous as antagonists.

T. G. BONNEY.

[COMMUNICATION FROM THE DUKE OF ARGYLL.]

THE article which I contributed to the September number of the *Nineteenth Century*, on the coral islands of the Pacific, has done what I intended it to do. It has called wide attention to the influence of mere authority in establishing erroneous theories and in retarding the progress of scientific truth. The vehement assault made upon it in the current number of the same review by Professor Huxley, and the article by Professor Bonney in this journal, are to me gratifying evidences of success. But both of these writers are entirely wrong in the interpretation they put on a few expressions in my paper. They interpret these expressions as conveying imputations on the probity and honor of scientific men in the habitual and wilful suppression or discouragement of what they know

to be truth. But there is nothing to justify this interpretation. I have made no such accusation, and, if any one else were to make it, I should join the two indignant professors in repudiating it. Scientific men are not only as good as other men in this way, but generally a great deal better. Professor Huxley has been irritated by some 'anonymous sermon,' which I have not seen, and for which I am not responsible. He admits that it is in this anonymous production that the 'slanders' against scientific men have taken the peculiarly offensive form; but he maintains that this unknown writer has been 'inspired' by my article on coral islands. On the strength of this assumption,—which may be true, for aught I know,—he goes on through some seven pages to dissect certain parts of my paper, and to read into it a great deal that is due to his own excitement, and to nothing else.

I have no difficulty in expressing clearly, and without any circumlocution, exactly what I do mean, and what I have intended to say. Professor Bonney interprets it very fairly, in abstract, when he says that the moral of my paper is, 'Beware of idolatry.' Some theory, hypothesis, or doctrine is propounded by a great man. It becomes established, partly perhaps by certain inherent elements of strength, or, at all events, of attractiveness. But soon it stands unassailable and unassailed upon the vast foundations of general acceptance and admitted authority. It becomes what Professor Huxley on a celebrated occasion, and with at least a momentary insight, called 'a creed.' The effect of such a position is tremendous. Some men who see cause to doubt are daunted. They keep silence. Others are prevented from even thinking on the subject. A few who do think, and who do doubt, and who do venture to express their doubts, are discouraged and discountenanced. A great many others take refuge in a suspended judgment, even after the production of evidence, which, in the absence of a 'creed' and of authority, would have been deemed conclusive. In all this there may be, and in general there is, nothing worse than timidity on the part of those who are the laggards, or the opponents, in some great advance. It is more difficult for some men than for others to face a prevalent opinion or an accepted doctrine. It is all very well to say, as Professor Bonney says, that "to the man of science truth is a pearl of great price, to buy which he is ready to part with every thing previously obtained." But scientific men are human. They are, I admit, immensely superior to the politicians, especially just now. But they have their failings; and every one who knows the history of science must be able to call to mind not one instance only, but many instances, in which the progress of knowledge has been delayed for long periods of time by the powerful and repressive influences of authority, exerted in one or other of many ways.

My contention is, that Darwin's theory on the origin of the coral islands is a case in point. I believed in it, or accepted it, for many years, as others did. Professor Bonney admits that I have described it not only fairly, but as forcibly as if I were still its advocate. This is exactly what I tried to do. I now hold that it has been disproved, and has been replaced by another theory quite as grand, and more in harmony with other natural laws which are of universal operation, but have been only lately recognized. I affirm, further, that this new theory or explanation has been received with the timidity, the discouragement, the discountenance, and the obstruction which are characteristic in such cases. That Dr. Geikie has supported it, is most creditable to him. But his voice is not enough to disprove the truth of my contention. That Professor Huxley and Professor Bonney should be unable to make up their minds upon such evidence as has been before us now for several years, is, in my opinion, a strong confirmation of the law which is operating upon them. There are some discoveries in science—some explanations of curious phenomena—which are self-luminous. They shine with their own light. The moment they are suggested, with a few cardinal and certain facts to illustrate them, they are their own proof. Every thing that turns up speaks in support of them. My conviction is that such is the character of Mr. Murray's theory of the coral-island formations in the Pacific.

Professor Huxley challenges me to re-affirm with better proof the fact I allege,—that Mr. Murray has met with discouragement. I respond at once to that challenge. I have seen the letter from Sir Wyville Thomson in which that naturalist urged and almost insisted that Mr. Murray should withdraw the reading of his papers

on the subject from the Royal Society of Edinburgh. This was intaker or somebody informed me." Then the culprit is brought before him, and the fact revealed that he indirectly aided a criminal.

February, 1877. No special reason was assigned, but the terms of the letter indicate clearly that Sir Wyville dreaded some injury to the scientific reputation of the body of naturalists of whom he was the chief, and for whom, as connected with the 'Challenger' expedition, he was in some degree responsible. He had not himself at that time, I believe, fully accepted the new doctrine. But that would have been no sufficient reason for discouraging free discussion, if it were indeed as free as it ought to be. In my article I understated the delay which was thus occasioned. Three years, not two, elapsed before Mr. Murray was at perfect liberty to advocate his views in the proper place, before a scientific body.

But the challenge of Professor Huxley has brought to my knowledge a new bit of circumstantial evidence to the same effect, which is highly significant. Among the investigators of the Pacific corals, no man has done better work than Dr. Guppy, surgeon of H.M.S. 'Lark.' Since my article was written, his volumes on the Solomon Group of islands have been published. The geological volume is an admirable memoir. It is the record of observations as patient, detailed, and conscientious as have ever been made on the great geological problem which is at issue. After his return home, he was advised by Mr. Murray to offer a paper on his researches to the Geological Society of London. He did so in the spring of 1885. But his paper was refused, much to Dr. Guppy's disappointment. It was not orthodox. His facts effectually removed some difficulties in the way of Mr. Murray's theory, — facts which in more than a corresponding degree were adverse to the theory of Darwin. As a consequence the Royal Society of Edinburgh has had the honor of receiving and publishing Dr. Guppy's most interesting memoir. As a Scotchman, I am proud of this contrast. I make no accusation of wilful unfairness against the authorities of the Geological Society of London, of which my critic Professor Bonney was, I believe, at that time the president. They did not consciously discourage truth. On the contrary, they probably smelt heresy. But if their minds had been free from this prepossession, — if they had been alive to the breadth and sweep of the questions at issue, and open to receive with welcome the crucial evidence bearing upon them which is contained in Dr. Guppy's paper, — the rejection of it would have been impossible.

As regards Darwin's own state of mind upon the subject, I can only say that my information was as good as that in the possession of Professor Huxley. I am not struck by the perfect candor of his reference to Darwin's letter to Professor Semper in October, 1879. If he had quoted the very next sentence to that which he does quote, a very different impression would have been left on the reader's mind. But I attach no importance to this point. I prefer to believe that Darwin's mind was open to conviction, and to hope that others will follow his example.

ARGYLL.

THE AMERICAN PUBLIC HEALTH ASSOCIATION.¹

DR. CARL HORSCH of Dover, N.H., read a paper entitled 'The Necessity of Burial-Permits and Inspection of Bodies of Deceased Persons.' He based this necessity on the following grounds. (1) It is the best safeguard against the possibility of premature burial, and also that the apparently dead may not be placed in cold rooms or on ice, and frozen to death. (2) Cases of concealed contagious and infectious diseases will be detected, and an epidemic may be averted. (3) Murder and suicide may be detected; and if cremation, the surest method for the destruction of disease-germs, is generally established, there will be also less danger that the body of a murdered person will be cremated, and the crime concealed. (4) Life-insurance frauds may be prevented. (5) Where the fear exists of being buried alive, the family physician can overcome that fear by that examination, and his assurance that the loved one is dead. (6) In order to sign a certificate for a burial-permit legally, that inspection gives the most important evidence. If a physician gives his signature to such a certificate without seeing the body, he may be brought in the following unpleasant position: he is called into court, the certificate is laid before him, the questions asked, "Did you sign that certificate?" Answer, "Yes." "Did you know that the person was dead?" The only answer could be, "The under-

Dr. Rohé, secretary of the committee on disinfectants, presented the report of that committee. The following conclusions were drawn from their work:—

1. The temperature required to destroy the vitality of pathogenic organisms varies with the different organisms.
2. In the absence of spores the limits of variation are about 10° C. (18° F.)
3. A temperature of 56° C. (132.8° F.) is fatal to the bacillus of anthrax, the bacillus of typhoid-fever, the bacillus of glanders, the spirillum of Asiatic cholera, the erysipelas coccus, the virus of vaccinia, of rinderpest, of sheep-pox, and probably of several other infectious diseases.
4. A temperature of 62° C. (143.6° F.) is fatal to all of the pathogenic and non-pathogenic organisms tested, in the absence of spores (with the single exception of *sarcina lutea*, which in one experiment grew after exposure to this temperature).
5. A temperature of 100° C. (212° F.), maintained for five minutes, destroys the spores of all pathogenic organisms which have been tested.
6. It is probable that some of the bacilli which are destroyed by a temperature of 60° C. form endogenous spores, which are also destroyed at this temperature.

Dr. John S. Billings, U.S.A., read a paper on some forms of tables of vital statistics, with special reference to the needs of the health-officer.

A resolution was adopted to appoint a committee, with Dr. Sternberg as its chairman, to study experimentally the methods and effects of protective inoculation against infectious diseases.

Dr. Horsch presented a paper entitled 'Inspection of Animals required for Food,' in which he recommended the inspection of animals by competent persons before they are slaughtered, and a thorough examination of their viscera afterwards.

Dr. Azel Ames, jun., of Chicago presented a paper on the meat-food supply of the nation, and its future. In it he gave statistics of the resources of the country with reference to its animal food, and showed, that, as the population increased and the grazing country diminished, these resources were proportionately declining. He criticised adversely the policy of the general government in dealing with the public lands. Legislation was asked of Congress for the suppression of pleuro-pneumonia, and for the taking of a thorough census of the cattle of the country and their products. Dr. Ames denounced the tax imposed by the oleomargarine act as being unjust to the poor, and wrong in principle, and demanded its repeal.

A paper by Dr. J. H. Rauch, secretary of the State Board of Health of Illinois, dealing with the subject of cholera and quarantine, excited great interest. Dr. Rauch described the defects of the quarantine at the port of New York, and said that in the West its results were looked upon with distrust. He asked that the entire quarantine system of the United States should be placed under national control. In the discussion which followed, Dr. A. N. Bell criticised most severely the arrangements of the New York quarantine, but expressed the opinion that the measures which had been applied by the health-officer in the management of the passengers of the steamers 'Alesia' and 'Britannia' had been successful.

The paper of Dr. Dickinson was discussed by a number of the members of the society. Dr. Eliza M. Mosher remarked that the point of greatest interest in connection with the subject was whether the loose corset injured the health of the wearer, and, if so, what could be offered as a substitute. Most girls, according to her experience, wore them sufficiently tight to limit respiratory movements. It was difficult to measure the injury done, since the chest was already crippled, and its expansion was below its possibilities. In addition to the thinning of the abdominal wall described by Dr. Dickinson, there was atrophy of the entire surface covered by the corset, with lack of development of muscular tissues due to restricted movement. This was apparent by the often-repeated remark of ladies that they could not sit up straight without their corsets. It was often observed how useless were the arms of most young ladies for any manual labor, even though their lower extremities were capable of long-continued muscular movement. A well-developed nipple was almost an unknown thing with a woman or girl who had worn

¹ Continued from *Science* of Nov. 25.