The second paper, by A. H. Taylor, on 'Resonance in Aerial Systems,' was a discussion by the author of some recent advances in wireless telegraphy; it was illustrated and showed the fourfold tuning necessary for the transmission of large amounts of energy. The sender, the two aerials and the receiver were successively attuned, enough energy being transmitted several meters to light three small incandescent lamps.

VICTOR LENHER,
Secretary.

THE ELISHA MITCHELL SCIENTIFIC SOCIETY OF THE UNIVERSITY OF NORTH CAROLINA.

THE 154th meeting was held in the Physics Lecture Room, Tuesday evening, April 12. The following papers were presented:

PROFESSOR C. L. RAPER: 'The World's Production and Consumption of Coal.'

PROFESSOR H. V. WILSON: 'Grafting in Vertebrate Embryos.'

PROFESSOR R. H. WHITEHEAD: 'Protozoa in Smallpox.'

A. S. WHEELER,
Recording Secretary.

DISCUSSION AND CORRESPONDENCE.

SCIENCE, NATURE AND CRITICISM.

THERE appeared in a recent number of SCIENCE a somewhat unscientifically savage attack upon William J. Long and his books by Mr. William Morton Wheeler. The attack, which was ostensibly made on scientific grounds, was speedily followed by another and more personal one, written by Mr. Frank M. Chapman, and by a third by Mr. W. F. Ganong, who, on the principle that 'any stick will do to beat a dog with,' sent in an old criticism that was first published and answered in a St. John newspaper. The first object of the present article is to examine these attacks, and see what truth underlies them.

Very obviously there are two questions here, a question of animals and a question of animus. With the latter we have nothing whatever to do, except to deplore it. The original *Atlantic* attack upon the books in question, which was quoted and commended, can hardly be regarded as scientific, so far as this means a calm, dispassionate sifting of facts and evidence; and the writers, in following their leader, have been, perhaps, too much influenced by the great Frenchman's maxim that 'among wolves one must howl a little.' But, laying all that aside, the readers of SCIENCE have undoubtedly asked, how much truth is there in these animal stories, which have not only been called in question but have been denounced as falsehoods and inventions?

I take the most incredible of all, the case of the woodcock that set his broken leg in a clay cast, which was ridiculed by Mr. Wheeler in SCIENCE of February 26. Now, forgetting all the ridicule and misrepresentation and facetiousness of the article, what evidence have we for believing the story as recorded? For myself, having seen the incident, it has passed beyond the realm of opinion or belief into the realm of fact. Nevertheless, I pass over this, and also over the strong supplementary testimony of my friend, who might be considered as partial, to submit other evidence of which there can be no reasonable doubt as to its truth or disinterestedness.

Soon after the surgery article first appeared in *The Outlook*, the editors of that magazine received a letter from a lady in Galion, Ohio, who told of finding a woodcock that had set its broken leg in a clay cast in a way precisely similar to that recorded in the article on 'Animal Surgery.' When the attack of Mr. Wheeler, in SCIENCE, was called to my attention, I wrote to the lady, asking her to send me any supplementary details of her observation and the names of any other reputable people who might know of the circumstances. Here is the result—and I have submitted all documents and letters to the editor of SCIENCE that there may be no question as to their genuineness:

My dear Dr. Long:

The circumstances in regard to the woodcock are just as my father writes (see following letter), but I send a few facts in addition to those he has given. A short time before my father shot the bird we had read that the woodcock can put its own leg into a clay cast, but this hardly seemed credible. I was cleaning the game and had cut off the legs of the woodcock before I noticed that one leg had upon it a lump of dried mud. Immediately what I had read flashed through my

mind, and I saved the leg. * * * It was exhibited by my father to several physicians in town and all admitted it to be a broken leg perfectly mended by the bird itself. Dr. C. L. Coyle and Dr. H. R. Kelley (both deceased, but both well known and reputable physicians) exhibited this curiosity, one at a meeting of Galion physicians, the other at a meeting of the Crawford County Medical Society. No one denied or doubted in any way that it was what we thought it to be. * * * (Signed) RENA REESE.

GALION, OHIO, April 4, 1904.

DR. WM. J. LONG,

Dear Sir: I have been in business in this place for the past thirty-two years. During this time I have always spent a few days yearly in hunting the different game birds in their various seasons. One day, a number of years ago, when hunting woodcock, I shot one which had evidently broken its leg above the knee joint. There was a bandage around it, composed of a hard clay-like substance, interwoven with grass or a woody fiber of some kind. The bone seemed to have been set properly and had knit perfectly. The natural swelling was nearly all gone; the bandage was loose and in my opinion would soon have dropped off.

I gave the leg, with the bandage on it, to one of our leading physicians and surgeons. He upon examining it expressed himself in a very emphatic way by saying that it was a better job than nine tenths of the surgeons could do. Dr. Coyle kept the leg at his office and later exhibited it at a convention of the physicians and surgeons of this country. After his death it was again exhibited at a meeting of the physicians of this city. Much interest was manifested in this curiosity, the like of which had never been seen by any one here, though some had read of such a thing.

(Signed) S. M. REESE.

* * * I carefully examined the specimen referred to, and can verify the statements of Miss Reese in every particular.

(Signed) F. L. BROWN, M.D.

* * * A number of years ago there was exhibited in my place of business (apothecaries) the leg of a woodcock, which had been broken, and which plainly showed, by the clay and fiber entwined about it, that it had been dressed by the bird itself. * * *

(SIGNED) L. K. REISINGER.

Here is another case from a different state. I quote from the sworn statement of Mr. David E. Smith, of Bridgeport, who until a short time ago, when the sale of game became unlawful in this state, was engaged in the business of buying and selling game birds for the market, in connection with his regular business as a gun- and locksmith.

* * * Almost eighteen years ago Mr. Thomas Finn, a member of the police force of Bridgeport and who was accustomed to hunting game birds in season, brought to me the leg of a woodcock which he had shot. About midway between the foot and knee there was a clay cast in which some small feathers of the bird and some grass had been interwoven, apparently to make it more adhesive. This cast around the leg was a little over one half inch in length and about as large as an ordinary lead pencil. This leg of the woodcock was on exhibition at my store for a long period of time.

About eight years ago Mr. George W. Hayes, a well-known sportsman of this city, brought me the leg of a woodcock he had shot, and it presented an appearance substantially the same as the one above described. I opened the clay cast * * * and found that the leg had been broken. I exhibited this leg, with the part of the cast that I had not detached, to several persons in this city.

Since then I have seen another woodcock's leg that had been cut off by another sportsman presenting the same conditions; and four years ago, in a purchase of birds for re-sale, I found that one of the woodcock had a clay cast on one of its legs similar to the other three that had come under my observation. I exhibited this leg with its clay cast in my show window for two years, and a great many persons in Bridgeport saw it.

The cast was so constructed and so attached to the leg as to preclude any theory of accidental attachment; for in each case it was uniformly attached around the leg and fashioned in a way to indicate that it was attached for a purpose, and was in each instance made more effective by the interweaving of dry grasses or small bird's feathers. * * *

(Signed) DAVID E. SMITH.

BRIDGEPORT, CONN., 23 April, 1904.

We, the undersigned, have seen in the possession of David E. Smith, on various occasions, a woodcock's leg with a clay cast surrounding the leg, presenting the appearance described in the foregoing affidavit. * * *

(Signed) WILLIAM B. TUTTLE,
JOSEPH H. SMITH,
WM. K. WOLLAN.

Personally appeared before me David E. Smith, William B. Tuttle, Joseph H. Smith and William K. Wollan, each of whom is personally known to me, and made oath to the truth of the foregoing statements.

(Signed) Stiles Judson, Jr.,
Notary Public.

Here is certainly warrant for believing not only that the woodcock sets his own broken leg, but also that the habit is more common and widespread than I supposed possible when I published my own observations. I have other letters and evidence from three different states bearing on the same question, and to the same effect; but these are probably enough. It may safely be left to the readers of Science to determine whether or not my story of the woodcock in 'A Little Brother to the Bear' is carried out, even to the smallest detail, by this disinterested evidence.

The second attack, by Mr. Chapman, is an extraordinary one for a man to make in the name of science. Starting with the assumption that, in the woodcock article and in all my books, I am falsifying and misrepresenting, he endeavors to account for it on the ground of personal characteristics. With calm and scientific judiciousness he omits the biographical dictionary and the testimony of all who know me, and fastens upon a newspaper clipping. That is generally regarded as rather poor scientific evidence; but even so, Mr. Chapman finds it 'illuminating,' and so let us examine it such as it is.

The *Transcript* article professes to be written by a friend of mine, an intimate acquaintance, who was a classmate at Andover Seminary, and who recounts certain occurrences in the class-room as an eye-witness. As a matter of fact, I do not know the man, and never saw him to my knowledge. He was never in the class-room with me, nor in the seminary during my three years' residence. The striking incident which he relates of me happened to another fellow, on the subject of Greek exegesis. He evidently got hold of it by some rumor, applied it to me, and touched it up with a vivid bit of personal recollection to brighten the effect.

A single bit of his testimony may be considered as typical of all the rest. He represents that I fitted myself for Harvard 'by solitary study,' and missed the supreme importance of freshman year; and, therefore, I have been ever since 'easily tempted to overrate my personal knowledge.' The facts are, that I graduated from the classical course in a good high school, which still regularly fits for college; that I took the full four years' course, classical and scientific, at the Bridgewater Normal School, which required an enormous amount of class work; then followed the Harvard degree, and Andover Theological Seminary, and three years in foreign universities, for all of which I have parchments to show that the work was regular and well done. I have undoubtedly seen more 'solitary study' and midnight oil than is good for a man; but, so far as there is any saving grace in class work and professors and in rubbing elbows with better men, I have had rather more than my share of the covenanted as well as of the uncovenanted mercies of our educational system.

All the rest of the statements are of the same kind. They are, almost without exception, errors, or misrepresentations, or pure inventions.

So these 'illuminating paragraphs,' upon which Mr. Chapman lays such emphasis, are illuminating chiefly in showing the enormous presumption with which a man will rush into print and join in a controversy of which he knows nothing. Incidentally, they may shed a little light upon Mr. Chapman's scientific way of collecting evidence.

As for the observations upon which he throws discredit, if he will read the books he will see instantly that he has misrepresented half the cases which he cites so carelessly. As for the others, the crows that played a game with a china ring, the porcupine that rolled down hill, the ducks that drowned mussels in fresh water—for these, and for every other observation which he discredits, I have more written evidence and more oral testimony from reliable observers than for the woodcock, which has just been considered. If scientists and comparative-psychologists are honestly looking for new facts in the animal world, I have enough to fill several reg-

ular editions of SCIENCE, every one of which is supported not only by my own personal observation, but by the testimony of other honest men whose word can be taken without hesitation.

The question naturally arises, and has indeed been asked with some irritation, why, with all these facts at hand, a man does not write as a scientist and produce his evidence. The answer is threefold: (1) I am accustomed to be believed when I speak. Knowing my subject, and with the evidence of my own eyes before me, it has hardly seemed necessary, for the sake of a few critics who will not believe, to refer to supplementary evidence, of which I have a plenty; to 'cross my throat,' boy fashion, as an evidence of sincerity, and to state after every observation: Mr. So-and-so saw the same thing in Such-a-place; if you don't believe it, ask him. (2) I have gone into the outdoor world as a nature lover, not as a scientist; for recreation, not for work; and my aim, as that of other nature writers, is chiefly to influence other people to go out of doors themselves, and by telling the whole truth, so far as I can see it, to open their eyes to the facts of animal life which the scientist, as well as the vacationist, has overlooked, under the supposition that birds and animals are governed solely by instinct and reflex impulses. And (3) while the scientist deals with laws and generalizations and works largely with species, I have dealt always with individuals, and have tried to understand every animal from moose to woodmouse that I have met in the wilderness.

That birds and animals (and even the insects, especially the solitary wasps and spiders) differ greatly among themselves in individual characteristics and habits, is now beyond a question. Sooner or later science will collect these individual differences and go to work upon new laws and generalizations; but at the present moment when one goes into animal individuality he crosses the borderland of science into a realm where our present laws and classifications apply only in the most general way. Every animal he studies closely is different from every other animal, for nature seems to abhor repetition as she abhors a vacuum. As among men, the differences, which lie deep are much harder to detect than the resemblances, which are mostly on the surface. All the men of a city street are alike from a third-story window, which is nearer than we generally get to wild animals. There are even women who declare that the generalization holds true at a closer inspection —but that is another matter. Two men in dress suits will pass the same general social muster at a dance or reception, and may be indistinguishable across a small room; but it will take some intimate acquaintance to discover that they are as far apart as Beelzebub and Gabriel. And any one who has ever learned to know intimately a litter of pups or a litter of fox cubs will recognize instantly that the same differences in character and disposition which prevail among men prevail also, though in less degree, among the beasts of the field, and are the last things to be discovered.

Though the field is an immense one, and practically unknown so far as wild animals are concerned, there are as yet only a few pioneers scattered over it. The facts are plenty enough, but the observers who have the patience and sympathy for the work are very few, and it will be years before they make any impression upon our general ignorance about birds and animals. It must be said also, of the nature students as distinct from the scientists, that they go into the field for pure love of it, rather than from any desire to make a book, or a theory, or to be enrolled among the discoverers of science. The element of personal taste also is a factor against them; they hate to kill and destroy, to stuff and label and put into a museum.

The ornithologists, for instance—and I have known many of them intimately—have been busy for years making collections of nests and eggs and bird skins; they have determined the range and distribution of species fairly accurately, and have gathered much interesting information as to food and breeding places of our native birds. These are the acknowledged 'scientists' of the bird world; and we have watched their work with interest, though at times with regret at the enormous and un-

necessary amount of killing which generally accompanies their investigation. They deal with species and general habits, and their work, so far, covers little more than the surface of bird life. Meanwhile the individual bird, with his own thoughts and feelings, his own life to live and his own problems to solve, has remained almost unknown till a few nature lovers and students entered the field and, leaving behind the gun and the egg-case and the 'identification of species' as the one thing to seek after, have hidden and watched and followed and loved the bird, and have understood exactly in proportion as they have loved him. The derisive cry of 'interested observer' raised against them by certain professed scientists has no reasonable foundation. No man watches and no man records in any field except he be interested. His observations are valuable exactly in proportion as love impels him to find out about things. Scientist and nature student are both seeking truth, and finding the particular manifestations of truth that they seek after. The difference is something like this, that the ornithologist loves specimens and the identification of species and other superficial things, while the nature student loves birds and the life that is akin to our own. The latter may prove, in the end, to be more scientific than the former.

At present the nature student is simply trying, without prejudice, to understand and record life as he sees it, and asks no scientific consideration beyond that suggested by common honesty and courtesy. When his record is written, his facts may be collected, and the comparative-psychologist, who now knows almost nothing of the life of the wild bird or animal, will then be able to finish the work which the ornithologist only began. Not till then shall we have anything like an edequate picture of bird life; and till then it may be well for critics to remember that truth is a large proposition and, like honesty, is not subject to monopoly.

Since the above article was written, another attack in the same spirit has appeared in Science, by Mr. William Harper Davis, a psychologist, who adds the name of Columbia University to support his claims. My first care, after reading the long article carefully, is to cut out from it all the personal abuse, the gratuitous insults to myself and to certain literary men, the repeated sneers at an honored body of some millions of young people, the satire, the ridicule, the sophomoric egoisms and several other things which have no bearing on the subject in hand, and which ought not to have been permitted to appear in a magazine under the great name of Science. What remains of the article consists, as do the other criticisms, of a few paragraphs of dogmatic assertions, denials and accusations, without a shred of evidence to support them.

Two things, however, may be profitably considered by the readers of Science who have seen this new attack, which is extremely characteristic of all the others:

1. Mr. Davis assures us that his article has no personal or unworthy bias. 'No personal feeling of any sort whatever prompts or accompanies this letter,' he assures us. Now here are a few, out of many such, words and epithets which he applies to certain gentle books and their author, and which, since no personal feeling is involved, are supposed by this scientific critic to be purely scientific and impersonal descriptions: '*Sham, crass, crude, aimless, pitiful, preposterous, ludicrous, false, meretricious, unintelligible, distortions, prejudices, farce, abominations, menace, prostitution, hocus-pocus, ignorance, arrogance, erotic effusion, general incapacity, vicious notions, crass misrepresentations, hopeless confusion, inordinate gullibility, a facile fabricator, an influence for evil, chief of a tribe, hopeless romancer, incapable of reform, type of his species, intellectual anarchist, wild ass, a sad case*'—all these for me. And I pass over as irrelevant, '*nuisance, blatancies, bigotries* and *cocksureness*' as applied to popular education.

Such is the language of this 'impersonal' criticism by a scientist. One can not help wondering what would happen to the unfortunate man who should really stir Mr. Davis out of his scientific calm and cause him to write personalities. Certainly even the present language and style are somewhat different

from that to which we are accustomed in the scientific books and treatises of our acquaintance. In an ordinary person this would be called Billingsgate, and the feeling that produced it might be termed anger, irritation, jealousy, malice, envy, spite, or some other purely personal and unscientific stimuli. Since, however, he has no personal feelings in the matter, it might be well for him, being, as he tells us, a 'technical student of psychology,' to examine himself seriously in order to ascertain what extraordinary mental state it is which, without feelings, causes all the symptoms of intense nervous irritation, and which, in a normal scientist, causes him to write in such very unscientific language. We suspect he has mistaken his symptoms, and that he must revise either his language or his psychology.

2. The one specific case which Mr. Davis mentions and ridicules, and which has been derided also by Mr. Burroughs and one or two other critics of his kind, as showing nothing but my own 'gullibility,' is the case of the orioles' nest. In the case of the woodcock I have already given the kind of evidence which supplements my own personal observations, and which I can produce abundantly to verify every one of my published records of animal habits. The orioles' nest is a somewhat different matter, in that it is not the direct result of my own personal observation. I referred to the nest in a magazine article simply to illustrate, from another's observation, the unexpected recurrence of a rare phenomenon, such as an oriole's fastening two twigs together with a piece of twine, which I had once seen done. Since, however, some readers may have an honest question as to why I should accept such an unusual observation, I submit certain facts which, for obvious reasons, it hardly seemed necessary to publish at the time I referred to the nest in the *North American Review*.

I first noticed the nest hanging in a room where a man lay dying. It was a sad story —but that is no part of the evidence. The dying man was being cared for with infinite patience by a kindly workingman, who was no relation whatever. It was the latter who owned the nest, who had watched it building, and who told me about it, one day, noticing my unconscious interest. After the funeral it was given to me, unexpectedly, in gratitude for certain little kindnesses which I had been able to show to the family and to the dying man, who also knew all about the nest. Every circumstance in the case was such as to preclude any thought of deception, even had there been the slightest ground for such a thing.

The nest itself is, without a question, the work of orioles, and the only possible doubt can be in the matter of the framework. The sticks are not such as a man would choose, and the tree in which it hung is the very last that a man would select for hanging such a framework. It is a huge buttonwood, and no man or boy living could climb out on the slender branch to where the nest was hung. Only a ladder would be possible, and in the whole neighborhood of the nest there was not a ladder found long enough to reach it. When it was proposed to cut it down, in the autumn, an extra long ladder had to be brought from some masons who were repairing a chimney; and this had to be stood almost straight on end before it barely touched the branch. Two men were required to hold the long ladder in place while a third went up with difficulty to cut down the nest. If a man had made the framework for the birds' use, he would certainly, unless crazy, have hung it in a different tree and in a more accessible place. All these external facts, which I have verified myself, point to the whole marvelous structure as the work of birds alone.

At least four persons, two men and two women—all of them honest and trustworthy people—saw the nest at different stages of its construction, and when questioned, separately and unexpectedly, gave substantially the same testimony. I submit the sworn statement of the man beside whose house the nest was built, who watched the work of construction from beginning to end, and who cut down the nest after the birds had raised their young in it and flown away:

I certify that I watched, from beginning to end, the construction of the nest now hanging in Dr. Long's study, and described by him in the *North*

American Review. The nest and framework are wholly the work of the orioles themselves. They tied the three sticks together, with string, in the form of a triangle; they swung this triangle by means of cords below the limb of a buttonwood tree and fastened it there, and then built the nest on their own framework. Beyond the bits of thread and string which they collected about the house, they received no help from any human agency. (Signed) F. G. Leslie.

Stamford, Conn., 25 April, 1904.

Subscribed and sworn to before me this 25th day of April, 1904. E. L. Scofield,
Notary Public.

It is hardly necessary to add that we have here a simple question which can not be ridiculed by the cry of 'hunter's yarn' or 'practical joke,' or befogged by the call for expert testimony. It is not a question of instinct or intelligence or comparative psychology, or anything else to call for experts or trained observation. The question is, whether or not certain birds tied three sticks together and hung them unaided from the limb of a tree. That in itself is a sufficiently wonderful fact; and again I leave it to the readers of Science to say whether or not I was justified in accepting it as reasonable evidence.

Let us hear the conclusion of the whole matter. Here are certain books which, by almost universal consent, are doing good in the world. They arouse not only a love for animals, but an intelligent interest and, if the testimony of hundreds of educators is to be believed, a keen interest to study and understand the animal life about them. They are not stories, but studies, and incentives to study; and if unwittingly they contain any error, the error is bound to be swept away by the very interest in nature study which the books themselves arouse. And here, on the other hand, are a few critics, who, in the name and with the authority of science, condemn the books and warn an innocent public against being deceived by falsehood and inventions. Now what is the scientific explanation of this phenomenon? By scientific I mean simply that which will take into account the facts and, so far as possible, all the facts. The alleged facts brought forward in the criticisms which appeared in Science are seen to be dogmatic denials mixed with considerable error and misrepresentation. Here are certain other facts to be duly considered:

1. The books in question record hundreds of observations, the great majority of which are known to be true. The rest are unusual, and some, indeed, seem incredible. On the other hand, it may be said for the latter that we know very little of the lives of the animals described, and the most striking things recorded are no more incredible than scores of well-authenticated instances of the intelligence of dogs and cats and horses. The only question, therefore, is, can we safely attribute to the wild animal the same individuality that we see in our domestic one? In other words, are the wolf and fox less intelligent than the dog, the black duck less keen than the barnyard fowl, the wild turkey and the grouse of less wit than the chickens, the deer and mountain sheep less resourceful than domestic cattle?

2. The observations recorded in the books in question have been made by an experienced observer who has put himself with much care and patience in the position to see what he describes. It is possible that he has made honest mistakes in his observations; but, on the other hand, those facts which have been most denied, like the woodcock, have been verified by other observers.

3. The author studies the living animal in his native haunts and in every case writes from first-hand knowledge, after long experience and with unusual opportunities for observing the wild creatures. The critics, with far less experience or knowledge of the animals in question, and with different interests, deny the observations on general principles, or on the ground that they have not seen them.

4. The attacks which have been made thus far are mostly ill-tempered and intemperate, as far as possible from the scientific spirit which they invoke. Though written in the name of science, they show none of her careful, painstaking methods; though their professed object is truth, they do not verify their own statements nor prove their accusations. The attacks are generally made by men who have

themselves written less successful books or articles on the same general subject.

5. The critics present denials, dogmatic assertions, negative testimony. Not one particle of positive evidence has yet been presented against the books which are so vigorously condemned. Meanwhile the fact remains that, though six or seven volumes and a score of articles have already been published, only two slight errors have thus far been pointed out, and they were promptly and gratefully acknowledged.

Other facts and considerations will undoubtedly suggest themselves, but perhaps it were well to consider these first in forming one's judgment as to the books and their critics.

WM. J. LONG.

STAMFORD, CONN., May 7, 1904.

[We hope that this discussion will not be carried further.—EDITOR.]

THE METRIC SYSTEM.

TO THE EDITOR OF SCIENCE: The suggestion of Professor W. Le Conte Stevens that a compromise be made between the metric and the British system of weights and measures, making a foot the fourth part of a meter and an inch two per cent. smaller than the British inch, might be a good one if the English-speaking race were to disappear from the earth, and all its tools and its technical literature be destroyed, but as long as that race continues to use its existing tools and books, so long must the inch persist with its present value. His article is useful, however, in showing the impossibility of the general adoption of the metric system in its present form by the people of this country. He well says: "What may be the form taken by legislation in England and the United States, the people can not be compelled to adopt nomenclature that is thrust upon them as a substitute for that to which they have always been accustomed." WM. KENT.

ICHTHYOLOGY IN THE 'ENCYCLOPÆDIA AMERICANA.'

TO THE EDITOR OF SCIENCE: Referring to Dr. Gill's note on the 'Ichthyology of the Encyclopædia Americana,' I may say that he is quite right in supposing that the proofs of the figures which illustrate my article on fishes were not submitted to the author. Many of these seem to be wrongly named as noted by Dr. Gill. DAVID S. JORDAN.

SPECIAL ARTICLES.

THE MULTI-NIPPLED SHEEP OF BEINN BHREAGH.*

ON two former occasions† I have had the honor of presenting communications to the academy concerning the multi-nippled breed of sheep on my farm at Beinn Bhreagh, near the town of Baddeck, Nova Scotia.

It will be remembered that in 1889, upon the purchase of some property at Beinn Bhreagh I found myself in possession of a flock of sheep; and that in the spring of 1890, one half of the lambs born upon the place turned out to be twins.

This large percentage of twins led me to examine the mothers of all the lambs with the object of discovering, if possible, some peculiarity that would enable us to distinguish twin-bearing ewes from others.

Upon examining the milk-bags of the sheep a peculiarity was observed that was thought might be significant. Normally, sheep have only two nipples upon the milk-bag, but in the case of several of the sheep examined, supernumerary nipples were discovered which were embryonic in character and not in a functional condition. Some had three nipples in all, and some four. Of the normally nippled ewes 24 per cent. had twin lambs; but of the abnormally nippled 43 per cent. had twins. The total number of ewes, however, was so small (only 51) as to deprive the percentages of much significance. Still the figures were suggestive of a possible correlation between fertility and the presence of supernumerary nipples, and it seemed worth while to make an extended series of experiments to ascertain (1) whether, by selective breeding, the extra nipples could be developed so as to become functional, and (2) whether ewes possessing four functional nipples instead of two would

* A paper read before the National Academy of Sciences in Washington, D. C., April 21, 1904.

† See SCIENCE, Vol. IX., May 5, 1899, pp. 637.