gineers and power users. The steam consumption of a modern steam-turbine of moderate size compares very favorably with that of the better class of large reciprocating engines, but what is of greater importance is the evident superior steam economy under variable loads. The steam consumption per horse-power-hour varies little from one third to full load; at overloads the economy, as shown by numerous tests, may be even better.

This feature predestines the steam-turbine to the special field of electric lighting and power generation, where it must inevitably become a formidable rival of the larger-sized slow-speed reciprocating steam-engine.

It is a significant fact that immediately following upon the installation of the large 8,000-horse-power compound steam-engines at the central station of the Manhattan Elevated Railway, New York, we find three 5,000-horse-power steam-turbines under construction for the Rapid Transit Company, of New York.

The high rotative speed of the steam-turbine is a prominent factor in favor of its adoption in connection with electrical generators, since the cost of the generator end of the equipment ought eventually to be very materially reduced; but for many lines of work the high rotative speed of the present types of steam-turbine is prohibitive, nor can it be adapted successfully to belt driving, except by the use of gearing. However, it is fair to presume that the present limitations of the steam-turbine are not insuperable, and that the attention which is now being given to its development will evolve a more universal type of motor adapted to general power purposes with large and small units alike.

The economies already obtained with both the steam-turbine and the gas-engine have brought each into a prominence which is at least suggestive of the important developments that are taking place in methods of obtaining and using power.

JOHN JOSEPH FLATHER.

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*THE PERPLEXITIES OF A SYSTEMATIST.**

A FORMER Chairman of this Section gave utterance in his retiring address to the following frank expression of sentiment: 'So welcome to the old-fashioned systematist, though his day be short, and may he treat established genera gently!'

If this cheerful prognostication is to be realized, the perplexities of the systematist are of short duration at best, or worst, and it were better for us, in view of our impending doom, to come before you to-day with the historic 'Morituri te salutamus,' and then kindly but firmly retire to the oblivion so imminently before us.

But on second thought we find ourselves not at all in the mood to fulfil the expectations of the genial oracle referred to, and, indeed, very much alive and willing to continue in the struggle for existence, although an even worse fate than death is offered as an alternative when the same prophet predicts that 'the future systematic work will look less like a dictionary and more like a table of logarithms.' Of course there is no gainsaying the fact that those who prefer logarithms will have them, but I will also predict that the number who will choose the lesser evil of the dictionary will remain for an indefinite length of time very much in the majority, even if this choice dooms them to the outer darkness where the 'old-fashioned systematists' are to be relegated by the logarithm proposers.

However this may be, certain it is that there will always be need for the men who perform the hard and often thankless task

* Address of the chairman of the Section of Zoology and vice-president of the American Association for the Advancement of Science. Read at the Washington meeting, January 27, 1902.

of the systematist, and those of us who are still pushing forward in spite of the almost overwhelming perplexities of the work, to say nothing of the frankly expressed contempt of the men in whose service we toil, are by no means called upon to sing our 'Nunc dimitis.' It has occurred to me that it would be profitable for us to consider on this occasion the position in which we stand, make confession of our sins, which are many, state as clearly as possible the embarrassments which at times nearly overcome us, and attempt at least to point out some of the means by which we can better our position and our work.

As to our position before the general public, it must be confessed that the general public cares for us not at all. Of all departments of biological science, none offers so little that is attractive to the average man as that which has to do with classification and the host of outlandish names that the systematist delights, in popular opinion, to inflict upon the literature of his subject. The average college student agrees with the general public, and will be prone to elect anything rather than systematic zoology or botany. There is absolutely nothing that seems to him more hopelessly dull, forbidding and profitless than all matters pertaining to classification and nomenclature. But it is in the house of our friends that we are wounded most cruelly. Even the best of our fellow zoologists and botanists wish us nothing better than a speedy and painless, at any rate speedy, death, and the worst of them would be glad to hasten the day.

It is not my purpose to discuss at present the attitude of the general public, nor even that of the college student, important as it is to all of us, but some attention ought surely to be paid to the prevalent opinion of our colleagues.

Let us inquire then, briefly, into the reasons for the unfortunate attitude of these who ought to be our best friends. In my opinion the most fundamental cause for their discontent is to be found in their irritation in finding nothing fixed or definitely settled in our classifications, or even in specific or generic names.

It certainly does not conduce to the tranquility of mind of the morphologist who desires to discuss the variation of a certain structure in a given group of animals to find that his friend the systematist is utterly unable to delimit the group for him, or that no two authorities can agree as to the number of species, much less as to their names! Wishing to get upon some solid ground for his discussion, the morphologist asks in desperation: 'What is a species, anyhow?' And the systematist, if he is honest, is forced to admit that he doesn't know. Again, the morphologist, with a commendable desire to learn something of the classification in a general way, laboriously masters some scheme which seems to have met with general acceptance, only to find that the next authority that he consults scorns it utterly. Still again, wishing to discuss the geographical distribution or ecology of some limited group, he finds that no two systematists agree as to the number of species included or the names by which they should be called.

Now, all this is exasperating to the last degree, and we must deal gently with our friends who exclaim in desperation: 'Is there anything definitely settled in regard to any group of animals whatever?' or 'Have the systematists any real basis for their decisions, or are they anything better than the merest personal whims?' Can we wonder that they resort at times to absolute brutality, and propose logarithms?

Having thus admitted the unfortunate position in which we stand before our fellow zoologists, let us now turn our attention to the highly edifying endeavor to honestly confess our sins. I suppose that

every zoologist who does systematic work starts out with the idea that there is nothing else quite so desirable and altogether ecstatic as the discovery and naming of new species; and this feeling results, it must be confessed, in numerous synonyms and great confusion. That this is an almost inevitable phase in the career of the ambitious systematist must be frankly acknowledged, and must be endured with as much philosophy as possible, the prospect being cheered by the reflection that the phase is exceedingly evanescent, and is of inconsiderable duration as compared with the whole career of the systematist. I know that I shall be backed by every worker of experience when I assert that any systematist who has gotten beyond the callow period would very much prefer to be able to place a given form in a previously described species than to be forced to describe it as new.

Besides, those of us who are sufficiently unregenerate can take great comfort in the thought that no one more eagerly embraces the chance to describe a new species than the morphologist who thinks he has discovered a novelty, and he it is who most often dodges the necessity of careful research along bibliographical lines, and at the same time artlessly evades all proper responsibility for his crimes by the formula: 'If this interesting form proves to be new, I propose for it the following name.'

The naïve innocence of some of our embryo naturalists is sometimes quite refreshing. For instance, a year or so ago a young and enthusiastic student in a western state wrote me that he thought he had a new species of a group in which I am interested, and asked me to kindly send him the literature on that group. Not finding me able to see my way clear to accommodate him, he proceeded to describe the supposed new species, and gave it a name. The result proved that the name was preoccupied and that the species was only a somewhat common color variety of a well-known form.

We have all of us made ridiculous mistakes, however, and no systematist of any experience could afford to throw the first stone were the biblical condition enforced. We should be cautious, however, and not leave too many cracks in our harness to be discovered by our friends the enemy. There are certain things that we ought to stop doing, and stop at once. One of the worst sins of the systematist is inadequate description of species. The scientific world has a right to demand good clear descriptions, and is not slow to express its contempt for any remissness in this direction. As an example of this particular sin I would cite an instance given by an entomological friend, which I quote verbatim:

"The variety harrisii of *Cicindela sexguttata* is described thus: It differs from typical *sexguttata* in the color, which is olivaceous green, and *in living at a considerable elevation.*"* It is not often that the variety maker is so refreshingly frank as this.

Another illustration is furnished by one of our energetic and intrepid young ornithologists, who evidently believes that each geographical locality ought to yield a trinomial for each bird inhabitant. He says:

"The differences characterizing this new form are not such as may be graphically described, but they are, nevertheless, quite apparent on comparison of specimens."

It appears from the context that this subspecies is based on a single specimen, but, coming from a different region, like the 'living at a somewhat higher altitude' of the insect referred to above, seems to be in reality, if not professedly, a zoological character. It seems to your speaker

* The italics are mine.

that a difference that is so elusive that it cannot be graphically described is not a proper basis for even a new subspecies.

The question here arises: Is there any legitimate limit to the refinement of description and niceties of distinctions between species or subspecies? There are many that hold that any difference whatever is sufficient basis for a specific description so long as there is no intergradation with other forms. Now it is evident that differences may be so small that intergradations are practically, although not theoretically, impossible. The keen eye of the expert systematist becomes almost microscopic in its function and sees differences that appear perfectly evident to the observer, but that are really intangible to the general zoologist, to say nothing of the scientific public at large. Should each of these microscopic differences be dignified with a separate name? If so, can we wonder that the non-systematic brother becomes thoroughly disgusted with our discussions of the zoological 'filioque' and consigns us all to quick extinction or a lurid future of logarithms?

It is to be hoped that the future will disclose some method of preserving scientific exactness, and at the same time obliterating the excessive pedantry that at present seems to be the main objective with certain systematists. And there is good biological ground for this hope in the law enunciated by our lamented Cope as the 'law of the unspecialized.' This, he says, 'describes the fact that the highly developed or specialized types of one geologic period have not been the parents of the types of succeeding periods, but that the descent has been derived from the less specialized of preceding ages.' There is no doubt that the extremists have their time and their uses, but they are not likely to be followed in their extreme positions by their successors of coming generations. It may be confidently predicted that the future will disclose a safe mean between the lax methods of many of the older zoologists and the indefensible hair-splitting of the extremists among the so-called advanced systematists of to-day.

In the estimation of the general scientific public the most grievous of our sins is the making of synonyms, and there is no question that we have much to answer for in that direction. There are few, however, that are in a position to realize the difficulties, amounting almost to impossibilities, that confront even the most conscientious worker. He has in hand a form that he cannot place in any known species, although he would be saved a deal of trouble if he could. He must call this troublesome animal something. He cannot call it by an old name and so, perforce, he must find a new one for it. It belongs to an old and well-established genus to which hundreds of species have, in the course of more than a century, been referred. Every descriptive term that can possibly be made to apply to such an animal has long ago been used. Though the worker may live in some great library center, such as Boston or Washington, it is impossible for him to have access to all of the literature pertaining to even a limited group. Though he spend months in looking through dealers' lists and catalogues, he is bound to miss a number of papers any one of which may contain matter vital to his purpose. Having exhausted every available source of information, he at last ventures to decide on a name which seems to him to be apt, and not preoccupied. The more experienced he is as a systematist the less confidence he has that his name will stand, nor is he greatly surprised to be reminded by some loving friend that that name was used twenty years ago in a paper published in

Russian and issued by a local scientific society in Kamchatka.

To illustrate the hopelessness of consulting all of the literature on even the most limited subject I will venture on a bit of personal experience.

For the past ten years I have earnestly endeavored to consult all of the papers regarding a very small group of animals in which I am particularly interested. In addition to buying everything that was mentioned in numerous lists and catalogues from the best European book dealers, the libraries of Harvard, the National Museum, the Congressional Library, the private library of Dr. Agassiz at Newport, the library of the Naples Zoological Station and other famous libraries in Europe were faithfully consulted and a card catalogue of every reference to a species included in the group under consideration was made. After which it seemed that I could at last work with some confidence that nearly all of the possible synonyms were where I could get at them when wanted. A few weeks ago the mail brought me a paper published in Geneva, in which occurred no less than one hundred titles of papers relating to the group of animals in which I had been working, not one of which I had been able to find.

Now if it is so difficult, nay impossible, for one who has access to a number of the best libraries to feel confident of avoiding the creation of synonyms, how can we expect the young worker with access to only a few books to avoid the same catastrophe? Of course it is easy to say that he has no business to attempt systematic work, and perhaps we should be justified in such a remark. But, after all, our position would be sadly like that of the historic mother who forbade her daughter to go near the water until she had learned to swim.

There is a distinct danger in attempting to restrict systematic work to those exceptional persons who have access to first-class libraries. Thoroughly equipped systematists will be needed in the world for a long time to come, in spite of frankly expressed views to the contrary, and the ranks of those passing away must be filled by competent men. Such men must be supplied mostly from our colleges and universities, and it is futile to expect the few institutions having adequate libraries to turn out a sufficient number of men to do this work.

As a matter of fact, the very universities that are in the best position to do such work are the ones that offer the least encouragement to the would-be systematist. In my opinion, our best-equipped universities are falling far short of their proper function in not paying more serious attention to this part of biological science. Some time ago I received a letter from a zoologist holding a high position in one of our largest museums, in which he complained that, while they were able to find plenty of young men who could work out the histology of a definite organ, or the embryology of a species, or undertake experimental work, there was only one university that he knew of, and that a western one, that gave students the training that was necessary to make them competent to work up a collection. For years there have been waiting for suitable men the vast accumulations of material in our great museums, and it is impossible to find men able to work up some of the most important groups.

Such, then, is the situation. There is the most urgent need for competent systematists, and our universities, the natural source of supply, are doing next to nothing in the way of training men for this important work.

But the objection may here be raised that the systematist is a specialist of a kind that cannot be trained for his work in the ordinary university course.

Of course it is impracticable to turn out full-fledged systematists, but it is practicable to give men the kind of education that will enable them to take up systematic work to advantage after their college days have been completed. The mental or intellectual equipment needed by the systematist includes three prime requisites: (1) accurate observational power, (2) a well-trained and reliable power of discrimination, and (3) the power to describe accurately and in good English. Now, be it observed that these three accomplishments are the very ones that are the most valuable intellectual gifts in almost any walk in life, and hence it follows that that sort of education which turns out good timber for systematists is the very one that serves the best and most useful pedagogical purposes; and the plea which I here make for more attention being paid by our colleges to preparing men for systematic work, is at the same time a plea for the best and strongest preparation for almost any walk in life.

It will, of course, be conceded that the first of the requisites cited above, namely, accurate observational power, is the primary aim of work in all material science; and it will also be conceded that the education of the power of discrimination or judgment is also included in any thorough scientific work; but I do not believe that any other branch of biological science does so much toward evoking fine descriptive power as does systematic work, either in botany or in zoology. After an experience of some seventeen years as a teacher of science, it is my deliberate judgment that good descriptive ability is much more rare than the ability either to observe or to discriminate, which is really a part of observation. It would be laughable, were it not pathetic, to see the utter helplessness of even the better class of university students when they are told to describe even the simplest object. Time after time I have found that a class of twenty or more sophomores did not contain a single one who could really describe any definite object with even approximate success. But it is a never-failing delight to see the power that they can acquire in this direction after a year of faithful work along systematic lines.

Teaching of the sort that I have indicated need not be confined to the largest and best-endowed colleges. Fairly large collections in certain definite groups are a necessary prerequisite, but such collections can be secured at less expense than the laboratory equipment that includes a good compound microscope for each student, and in many cases the teacher can, with the help of students, make suitable collections in such groups as birds and insects.

The whole scheme of systematic arrangement lends itself admirably to the gradual evolution of descriptive power. Commencing with the larger groups, the student is drilled in discriminating the broader characters, such as differentiate classes and orders, for instance; then closer work is required in studying the families. Lastly, some few families are taken up and the work becomes focused on the fine discriminations required in describing genera and even species.

In the University of Iowa, for instance, the student works for one third of a year on the classes and orders of the lower invertebrates. Then he studies the groups of mammals, down to and including the families, for an equal length of time, the remainder of the collegiate year being devoted to the study of birds, more than half of this latter period being given to a careful study of the Passeres. The work is focused more particularly on birds because the university museum is particularly well equipped in birds, they are

pleasing objects of study for most students, and they are particularly available for illustration in such objects as coloration, geographical distribution and, strange as it may seem, ecological problems.

You will pardon me, I hope, for thus intruding the work of my own department upon your attention. But it serves to illustrate my meaning in claiming for systematic work the highest grade of pedagogical value. It does teach the student to observe carefully, discriminate with something of that judicial nicety so rare and so helpful in any life, and lastly—and it seems to me that this is the crowning achievement in education—to describe accurately not only from a scientific but also from a literary standpoint. Lucidity and accuracy of language accomplishes marvels in the way of inciting to lucidity and accuracy of thought, and, so it seems to me, actually precedes them in time.

All this may seem a digression from the main theses of my address, but it will be remembered that we are trying to find a remedy for the scarcity of men competent to occupy the field of systematic work, and the first thing needful is a realization on the part of our colleges and universities that they have too long neglected the educational value of training along systematic lines. Were they led to recognize this at its just value, it would be provided for on at least an equal footing with morphology in the curricula of all reputable colleges, and this would result in the graduation, yearly, of a number of young men and women who have the preliminary training that will enable them to take up systematic work in earnest.

Of course this real systematic work can only exceptionally be done in colleges. Not even as post-graduate work can it be attempted, save under circumstances seldom realized. But the men, if worthy, will find the place to work in centers where great museums and libraries will be at their command. In this connection the thought forces itself to the fore that the great and greatly discussed Carnegie Institution can do a most important work in seeing to it that such young men, equipped particularly for systematic work, can receive enough of a stipend to feed and clothe them while necessarily away from home and doing important systematic work in overhauling and bringing order out of the chaos that prevails in most if not all great museums, where a wealth of material has been allowed to accumulate for decades awaiting the time when the right man can come to the aid of overworked curators and intelligently and efficiently disentangle the all but hopeless masses of material, and, with keen insight and trained powers of description, successfully trace the obscure web of relationships and of descent. Thus the curators will be left free to do better and more worthy work along the lines of their chosen studies, relieved of at least a part of the all but intolerable burden under which they are staggering, and in spite of which so much excellent work has been done.

While no one more heartily condemns scientific provincialism than does your speaker, still we can rightly indulge the hope that the time will come, and that soon, when it will be unnecessary to send to Europe for men competent to report on collections made by our government expeditions, and when collections will be entrusted to American zoologists, *not* because they are American, but because they are best able to do the work in a satisfactory manner.

It is probable that nine out of ten systematists, if asked what, in their opinion, was the most thankless and wearying part of their work would unhesitatingly answer, 'The bibliographic work.' In nothing are our energies so wastefully and often need-

lessly expended. Now that the Congressional Library is at last in working order, it seems to me that it ought to be possible to undertake a work in this direction that would be not only an unspeakable boon to all who are engaged in systematic investigation, but also to the scientific public at large; for nothing that I can think of would go so far towards reducing the pernicious activity of the maker of synonyms to a minimum as a methodical and exhaustive publication of bibliographies in connection with which synonyms* would be promptly 'spotted' and reported at once to the scientific world.

Our Congressional Library is worthy of a nation's pride. Having had occasion to work there myself, I can say that nowhere can better service or more helpful courtesy be found than is accorded one who desires to do serious work within its walls. One must use it before he can form any just idea of the wonderful change that has been brought about since the present building was completed. Here is undoubtedly the best place in America to do bibliographic work, and here could be undertaken a public service that would be second to none in helpfulness to the naturalist, the systematic publication of bibliographies, perhaps following the general lines of the Concilium Bibliographicum, which has already rendered invaluable service, so far as current publications are concerned.

The Concilium Bibliographicum, however, can furnish but little help regarding publications of other than comparatively recent date, and this is the most pressing need of the systematist. This task, colossal as it is, could be accomplished if attacked systematically by a sufficiently large force of competent workers. It would not be necessary to complete the work in any group before the results could be available for general use. By a periodical mailing of cards some relief could very shortly be extended to all those who are known to be interested in any group, and as the history of our science covers less than a century and a half, a vigorous prosecution of the work would enable us to have authentic and reasonably complete bibliographies brought up to date within a very few years.

Such work need not, indeed should not, be confined to bibliographies of publications, but should include bibliographies of specific names. Every reference to a species should be given a separate card. These could be arranged both alphabetically and chronologically, and when such a bibliography is completed up to date a synonym can be detected with unerring accuracy. I speak from some little experience when I say that such an arrangement of cards is the greatest possible assistance and time-saver, as I have myself made a card bibliography of a single order of animals with which I am working. It includes some six thousand cards, and involves a card catalogue of authors, with their publications, of families, of genera and of species.

Of course such a plan as has been indicated could only be carried out by a corps of specialists, each having immediate charge of the work pertaining to some limited group, and the whole should be under the supervision of some public scientific organization such as the Smithsonian Institution, or possibly the Washington Academy of Science; such bodies being particularly available on account of their being situated in Washington, where most of the actual work would be done.

But what answer shall we give to our friends who plaintively implore us to 'deal gently with established genera'? It is in connection with this question that we are

* The word synonym is here used in its more general sense, including both autonyms and synonyms in a strict sense.

confronted with some of the most perplexing of our difficulties. How far are we justified in overturning that which is firmly established by usage in order to introduce schemes of classification that seem to us better and more rational?

Hoping that your patience has not been exhausted by the references already made to personal experiences, I beg your indulgence while I refer once more, for illustration, to my own work, which is a monographic treatment of an order of cœlenterates. In attempting to discuss the genera of a single family, the Sertularidæ, it was found that there were included in it about twelve apparently well-established genera. These had been carefully defined and the classification seemed a logical and good one. When, however, the great amount of sertularian material accumulated during the past twenty years by the *Albatross* and other government agencies, together with the results of recent work by our cousins across the water, came to be worked over, the fact became more and more apparent that not a single one of these established genera could hold, unless some entirely unnatural and arbitrary characters were used, such as would be employed in the construction of artificial keys. Not a single one of these genera, as defined, was exempt from almost ideal intergradation with one or more other genera. Here the investigator is confronted with a dilemma with several horns, if the bull be allowable, either one of which was fraught with most uncomfortable consequences. The following courses were open:

1. To adopt an entirely artificial system, for convenience only.

2. To throw all of the old genera into one, for the sake of scientific consistency.

3. To make a new grouping, involving a new lot of genera.

4. To use the old and well-established genera, pointing out the intergradations and frankly admitting their scientific insufficiency.

Considering these in order, we find that the first proposition, that is, to adopt an entirely artificial system for convenience only, would be eminently unscientific, a backward step that should not have serious consideration.

To throw all the old genera into one would be the course to which the strict dictates of the scientific conscience would impel the investigator. If one could set aside every consideration save the letter of the law, and be willing to be pilloried by his colleagues, this would be the proper course to pursue. As a matter of fact, however, such a course would involve the renaming of about nine tenths of the hundreds of species involved, and throwing all the knowledge so laboriously attained by our predecessors and contemporaries into pi, resulting in every worker in that group, or every one that wanted to mention a species, being forced to find out what the thing would be called under the new system, no matter how familiar he might be with the group. Should any one have the hardihood to precipitate such a disaster, he would not only be pilloried and execrated, but, I doubt not, would fail to secure a single follower, and all of his work would die with him and his name be anathema.

The third course, that is to make a new grouping under new generic names when necessary, and old ones when possible, would be an excellent solution were it not for the fact that months of the hardest study, with ample literature and material hitherto unsurpassed in abundance has resulted in the sad conclusion that no grouping can be devised that will not be open to the original difficulty, that of intergrading forms in all directions. Nothing would be gained, and much confusion

would result from this course, which, like the others, cannot wisely be adopted.

There remains then but one suggestion. That is to use the old and established genera, which will work in perhaps ninety-five per cent. of the cases, and frankly call attention to the intergradations so that no one will be misled.

In this way we can heed the pleading of our friends to 'deal gently with established genera,' and not bring disastrous confusion into the already overworked synonymy.

Of course this solution is far from ideal, and will doubtless meet with no little criticism, but it is an honest one, and it is hoped will meet with the gratitude of those who plead with us to 'deal gently with established genera.'

It is to be feared that we have been too lenient with those who have been heedless in the matter of overturning existing classifications before they are certain that they have something better to offer. The old proverb, 'Be sure you are off with the old love before you are on with the new' is one all too apt to be forgotten by the enthusiasts who are unable to distinguish the difference between becoming great and becoming notorious. A little wholesome conservatism is by no means to be despised. A system of classification is not necessarily better because it is new, and we need to redeem ourselves from the charge, all too well founded, that we are capricious in tinkering with matters that need the most careful pondering, and an application of Davy Crockett's motto, 'Be sure you're right, and then go ahead.'

Of course all real progress must be encouraged, and it will never do to allow considerations regarding public, or even scientific, opinion to deter us after we are sure we *are* right. Conservatism by no means means stagnation, but it does mean deliberation.

But I have already trespassed too long upon your time without even touching on several questions of vital importance, such as the 'A. O. U. Code,' the best medium of publication, an authoritative tribunal for the settlement of such questions of nomenclature as could rightly be submitted to such a body, and other matters that I had hoped to discuss.

In conclusion, let me urge the necessity of hearty cooperation and a good understanding between systematists and other workers in the field of biological research. None of us can afford a contemptuous attitude toward any other who is honestly striving to extend the limits of knowledge, even though his faults are many. In early days out West there hung in a popular dance hall the suggestive notice: 'Don't shoot the orchestra. He's doing the best he knows how'! The same plea in thought, if not in language, we would enter in behalf of the systematist.

C. C. Nutting.

## SCIENTIFIC BOOKS.

*Geschichte der Chemie und der auf chemischer Grundlage beruhenden Betriebe in Böhmen dis zur Mitte des 19 Jahrhunderts.* Von Adalb. Wrany. Prag. 1902. 8vo. Pp. vii + 397.

Dr. Wrany's volume deals with the progress of chemical science and its allied industries in the kingdom of Bohemia from the earliest times to a comparatively recent period, in an exhaustive manner. The first section considers the development of alchemy, it being a part of the history of civilization; it records that the first Archbishop of Prague, Arnest von Pardubic, who became chancellor of the newly founded University of Prague, attended universities in Italy to study chemistry and alchemy; he died in 1364, being a century later than Roger Bacon, Albertus Magnus, and the noted physician Arnold de Villanova, but preceding Paracelsus by an equal number of years. The first Bohemian writer on alchemy was Johann von Tetzen, whose verses