

community. A high standard has here been established and it will be maintained. What more need I say than that we welcome you to our ranks, pledge you our aid and urge you to improve every opportunity offered you for gaining knowledge of the profession to which you have devoted your lives. May this day be an auspicious one to you all, full of encouragement to those who return after a season of rest to the prosecution of their studies, and presaging success to those who, with honest purpose and entire devotion claim entrance to the ranks. On behalf of the faculty I greet you once again and bid you cordial welcome to this place.

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It is no disparagement to say that there are few chemists whose research work, at any given time, is of vivid interest to all classes of their chemical colleagues. To address an assembly of this kind on the experimental results of another man, to which results one has nothing of one's own to contribute, is to lay oneself open to a cross-fire—one part of the audience will ask why the speaker did not select a subject of which he had an adequate knowledge, whereas the other part will enquire why he did not deal with something that was *really* interesting. I have protected myself against both lines of attack by choosing a very large topic. I am confident that, intrinsically, it is interesting to each of us, because we all read our own papers and occasionally the publications of our friends, especially if we believe them to be erroneous, or think that they are going to interfere with our particular results!

During the past three years I have

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had the sole active charge of the *American Chemical Journal*, and I propose to take its history as the basis of my remarks. I select it simply because of my familiarity with it; the subject could be equally well illustrated by our own *Journal* and, so far as its age permits, by the *Journal of Physical Chemistry*. The first number of the *American Chemical Journal* is dated April, 1879. Volume 1 (1879–80) contains 460 pages. Volumes 10 and 20 (1898) comprise 472 and 890 pages, respectively. After that year two volumes were issued annually, the last one, number 37 (January to June, 1907), includes about 650 pages. To put it in another way, at the end of ten years the quantity of published matter per annum was the same as at the end of the first year; at the end of the twentieth year it had doubled, and eight years later it was three times greater than during the tenth year. An inspection of the earlier volumes suggests many reflections concerning the almost complete change which has taken place in the names of contributors during the past twenty-eight years. I shall not indulge in these beyond saying that death accounts for only a few of them. I feel sure, however, that you would not wish me to pass in silence over the fact that of the earlier contributors, practically only three, Professors Arthur Michael, H. N. Morse and W. A. Noyes, continue to contribute, at the present time, as successfully and copiously as ever to the extension of scientific knowledge.

Returning now to the consideration of the enormous increase of published matter, especially during the past fifteen years, the question arises, To what is it due? Undoubtedly the amount of scientific research carried out in this country is greater, both relatively and absolutely, and a comparison of the papers published in American chemical journals with those appearing

in other countries will show, beyond question, that on the average the home product is fully equal in quality to the foreign output. Although we may be satisfied on this point, yet it is certain that very much more might be done if an extra effort were made by the many whose training has taught them something of the methods of research, and this extra effort would not fail of its speedy reward.

A second most potent factor in increasing the quantity of work published in this country is undoubtedly due to the fact that, until comparatively few years ago, much if not most of the results of the best work done here was published abroad. At present not more than two or three people of any importance in the chemical world habitually send their results to Europe, yet some who should know better often communicate preliminary papers, or short articles containing the cream of their results to foreign journals. When, as sometimes happens, this is done essentially to secure duplicate publication, it is, of course, inexcusable, and even when the motives are unexceptionable the practise is one that should be indulged in sparingly, because it simply encourages the highly conceited and very prevalent German habit of ignoring or belittling American chemistry. Further, we must remember that if *we*, without detriment, can *now* publish our results at home, it is because of the patriotism and self-sacrifice of men like Professors Edgar F. Smith and W. A. Noyes, who, in the darkest days of our society, deliberately published some of their most important papers in its *Journal*, and thus compelled serious foreign recognition of it. It is the results of their devotion which we are now enjoying.

Leaving this matter of publication, I wish to direct your attention to another phase of the subject. Many persons have the *name* of research on their lips, but the

thing itself is far from their minds. This is true of the college president who "encourages research" by giving an unfortunate teacher a couple of free hours per week, loading him down with numerous courses of instruction and with faculty work, and then wondering why published results are not forthcoming! The greatest sinners in this respect are probably our technical brethren. All of us are familiar with the man who gives an address and, after telling us of the vast importance of his industry, proceeds to formulate a series of questions regarding the behavior of leather, coke, tar, paper, etc., or the materials from which they are made, winding up with the suggestion that *somebody* in search of a problem should attack the subject. The speaker then complacently wends his way home, evidently feeling that he has done his part towards the advancement of research. Of course, what he has done is to demonstrate, in the clearest manner possible, his own complete innocence of the slightest conception of what scientific research really is and under what conditions it is carried out. It is safe to say that everybody who is capable of independent investigation is overcrowded with problems of his own; what he requires is time and help to investigate them. Those who lack the problems also lack the ability to attack them successfully without detailed and continuous help. In this country probably not more than a score of men are paid directly for doing research in chemistry; the remainder carry out their investigations at very great personal sacrifices of time, energy and money and, consequently, their absolute right to select their subject with perfect freedom is beyond question. If I meet my technological friend on his way to the golf links and tell him of the large load of wood which I want split, pointing out that it will afford him admirable exercise besides being *useful*

to *me*, I fear that he will suggest that I split it myself or pay somebody else to do so, he, for his part, preferring to spend his well-earned hour of liberty in his own way without reference to my desires or convenience. Frankly, technical men must follow the practise of the best firms and bear the cost of their own research.

As my editorial work is at an end, I shall conclude this address with a few remarks concerning the difficulties which are encountered in such a position. Some little time ago a friend who edits and partly owns a scientific (non-chemical) journal, urged upon me the view that, essentially, an editor's duties should be confined to proof reading. That manuscripts should be printed exactly as sent in and that an author should be allowed the utmost liberty to make any exhibition of himself that he might choose. The prospect to an over-worked assistant editor was very tempting, but, of course, the answer is that most journals profess to be published for the readers rather than for the authors. It is strange that a man will spend months or years working hard at a problem and will then neglect the few minutes required to verify the spelling of proper names (and occasionally of others), to place punctuation marks, or give his references in accord with the system adopted by the journal to which he is contributing. Some authors appear to be incapable of checking up their empirical formulæ or their analytical results, others show a preference for quinquivalent carbon atoms, whereas some appear to bend their energies to the reproduction of the graphic formulæ of such complicated compounds as methane or carbon dioxide, which latter they may write as one word. All this, like blots and smudges, which of course the printer will try to set up, is due to carelessness. On the whole, the standard of English is fairly good, of necessity one meets with

"shall" and "will" misplaced, with "gotten" and "proven," with "glas" and "gass," while some of those who have spent a few months in Germany naturally forget their native language for the remainder of their lives and spell hydriodic acid and quinone according to the Teutonic fashion. The facts that, in English, the names of alcohols end in *ol*, those of amines in *ine* and that amino acids and amides are *not* identical appear to be beyond the limits of knowledge of some contributors. In general the meaning of an author is fairly clear, but occasionally some very startling statements are made, as, for example, one by a chemist of considerable prominence, who said, of a liquid, that it "heaped up about 150°." Another man, almost equally well known, describes a number of solids, all of which "melt under decomposition," whatever that may mean. These remarkable materials should certainly find place in the National Museum!

The really important point about such errors is this: Scientific workers in general and, I am sorry to say, American scientific workers in particular, have a bad reputation for the form in which their results are presented. In the vast majority of cases this reputation is not deserved, nevertheless the many suffer for the sins of the few. Authors should remember that the simple act of publication constitutes, in itself, an invitation to the world to give due credit and honor for the work which is described in their papers. A chemist is, presumably, not likely to under-rate the value of his own work; if *he* does not consider it worthy of clear and accurate description, he has no right to expect that busy people will take the time and trouble to acquaint themselves with his results, no matter how important they may be.

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