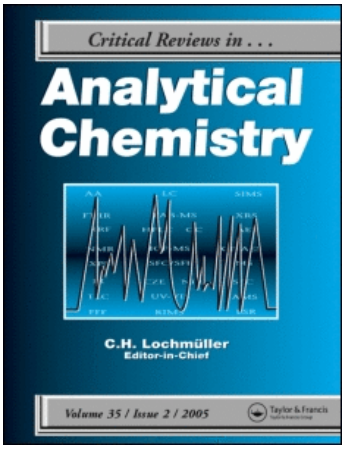

Critical Reviews in Analytical Chemistry
Publication details, including instructions for authors and subscription information:
http://www.informaworld.com/smpp/title~content=t713400837

Writing, Reviewing, and Editing in Analytical Chemistry

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To cite this Article Chalmers, Robert A. and Zuman, Petr(1970) 'Writing, Reviewing, and Editing in Analytical Chemistry', Critical Reviews in Analytical Chemistry, 1: 2, 217 — 232
To link to this Article: DOI: 10.1080/10408347008542735
URL: http://dx.doi.org/10.1080/10408347008542735

WRITING, REVIEWING, AND EDITING IN ANALYTICAL CHEMISTRY

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INTRODUCTION

Publication of the results of scientific research is important to science as a whole as well as to the research workers themselves, but for rather different reasons. For the scientist, publication means recognition of the value of his work, establishment of a reputation, and, sometimes, the means of obtaining promotion or appointment to a better paid job. For science, publication means (or rather *should* mean, since poor work sometimes escapes detection by even the most vigilant referees and editors) that the work is original, based on sound thought or experimentation and, above all, useful. This last criterion, that of usefulness, is not always heeded, the excuse being that what appears trivial or without application now may become important at some time in the future; and this argument is supported by one or two examples. However, it is not too difficult to distinguish between what is of potential use and what is demonstrably useless. In analytical chemistry, for example, scarcely a month goes by without the appearance of a paper describing a new reagent for the spectrophotometric determination of copper, cobalt, or palladium, complete with a detailed account of the experimentation involved in proving that the compound produced has the formula that could have been predicted from an elementary knowledge of chemistry, and in establishing that the sensitivity and selectivity of the reaction are so poor that the reagent has no advantages whatever over most of the hundreds of other reagents that have been similarly described in the literature. It is, of course, useful to know that the reagent *is* without value, but all that is needed is a simple statement to that effect, and that could occupy a few lines of print instead of as many pages. In view of the so-called "literature explosion" (an expression which raises some startling images in the mind) there is a good case for the exercise of greater selectivity in the choice of papers for publication. It is the object of this review to give an account of current practice in the publication of papers on analytical chemistry, to delineate the duties of the author, the referee, and the editor, and, in the course of doing so, to suggest ways in which that practice might be modified in order to give the best service to the author and the reader. It should be stated at the outset that the writer of this review functions in all four of the capacities just mentioned and will frequently be expressing personal opinions (and prejudices) based on fairly wide experience.

THE PUBLISHING PROCESS

Journals are often classified (usually for purposes of polemics) as "learned society" and "commercial," with the implication that the former are of higher scientific quality, although there is no real foundation for such a belief. The quality of journals should be judged from their contents, not from the name of the publisher. The main differences between journals dealing with the same field of work lie in the mechanics of handling contributions. Some send all papers to two referees for assessment of accuracy and quality; others use only one referee or even none at all. Some have publication committees or editorial boards which decide, on the basis of the referees' reports, which papers shall be accepted, and the editor's powers are confined to correcting the grammar of the manuscript and to seeing that it is presented in the normal style of the journal. Others give the editor complete freedom of action, with power to overrule the referees' opinions. Whatever the system used (except that in which no referees are used at all), all contributions will be submitted to scrutiny by acknowledged or reputed experts on the topic of the paper. At this stage things can begin to go wrong. The referee may do his job properly (see below) and write a conscientious and objective report designed to tell the editor (and/or any committee) whether the paper is scientifically sound, has any errors of fact or logic, is useful or trivial and worth publication or not, and may indicate ways in which it could be improved. That is what the editor hopes the referee will do. Some referees go even further and repeat part of the work in their own laboratory to test the author's claims. Unfortunately, however, some referees appear not to take the trouble even to read the paper

but simply recommend its acceptance or rejection, without giving reasons or criticism. Now, recommendation of acceptance outright is legitimate if the paper has been thoroughly examined and found worthy of publication and free from fault, but it often happens that during the editing of a paper that has not been criticized by a referee it soon becomes obvious that the paper is far from being as perfect as the referee implied, and that, in fact, the referee has not done his work properly. In that case the editor can consult a referee who is more reliable (and cross the first one off his list of potential referees); or if the faults are glaringly obvious, he can take up the matter with the author direct, a course of action which will produce rather quicker results.

Under no circumstances is it justifiable to recommend rejection of a paper without giving reasons. The editor has to write to the author to explain why the manuscript has been rejected so that the author can see for himself where he has erred. As the editor is unlikely to be expert in all topics covered by his journal, he will need informed guidance to help him in tactful persuasion of the author. If the referee does give reasons for proposing rejection, the author should be given the right to reply unless the work is so patently in error or trivial that consultation of the referee was purely a formality. (Typical examples from experience are a paper based on application of a previously published method simply to confirm an already known constant, and one in which a plot of a conductometric titration gave a single break-point where the authors thought there ought to be three, so they redrew the graph by plotting the resistance against the volume of titrant and drew tangents at appropriate points on the resulting hyperbola.) An author faced with a recommendation of rejection, supported by a reasoned argument, has a choice of action. He can argue and attempt to convince the referee, with or without doing further work; he can accept the fact that he may have been wrong and was saved from making a fool of himself in print; or he can withdraw the paper and submit it to another journal in the hope of meeting a more amenable referee. If he chooses the last course of action, he would be well advised to retype the paper in the style of the new journal to make it less obvious that he is trying again, and he should take due heed of the referee's criticisms where he feels these are valid. One of the important functions of the refereeing system is that it gives the author the chance to forestall public criticism, by modifying his paper in private.

By far the most common turn of events is for the referee to point out to the author any weaknesses or points overlooked in the draft, or to suggest what further experimentation or interpretation of results might be needed, and, generally, to criticize constructively with a view to helping the author to make the most of his work and to present it in the best possible way, thus helping the reader as well. In that case a duolog between author and referee will ensue, via the editor as intermediary, until agreement is reached.

Once it has been agreed that the paper should be accepted, the manuscript is edited to conform to the house-style of the journal in matters of spelling, punctuation, units, abbreviations, references, etc. (though the conscientious author will already have anticipated this and have written his paper correctly), and the grammar will be corrected where necessary. This last is certainly one of the most important functions of the editor, and may even be second only to that of maintaining the scientific standard of the journal, which is so overwhelmingly important that it scarcely seems to need discussion. Grammar is important because most journals have an international readership, and many readers must translate as they read; if the original language is incorrectly or ambiguously used, the chances of the translation's being wrong are increased. For that reason the editor must insist on accuracy of grammar whether the author likes it or not; otherwise the editor is failing in his duty to his readers, without whom there would be no journal to edit. It is, unfortunately, the experience of most editors that the charges of illiteracy leveled against scientists are only too firmly based on fact. Something over 90% of papers published have required correction at some point or other, some of them needing almost complete rewriting to make them acceptable. While this is understandable and indeed to be

expected from authors writing in a foreign tongue, it is quite inexcusable from authors writing in their native language. Words are just as much precision instruments as balances are and, if incorrectly used, will give inaccurate results. The use of language can be learned in the same way as the use of tools is learned, and while the degree of elegance that can be achieved will vary with the ability of the user, the final product should at least be workmanlike and free from gross faults. If an author is incurably ham-fisted in the use of language, he can always make a draft of what he wants to say and ask a more skilled colleague to polish it. Very often skillful editing will make a sentence clearer by shortening it, usually by changing the word order to avoid circumlocution (I once wrote "The errors in the results were so small that they could safely be neglected," which reduced to "The errors were negligible"). A good editor can cover his salary by the saving he can make in printing costs by shortening without removing any essential information. It is, of course, most important that the content and meaning should not be changed during editing and abbreviation.

When the editing is completed the manuscript is marked up to show the printer the style and size of type to use and is sent to the press. If editing has needed to be so extensive that the text is difficult to read, the manuscript may have to be retyped by the editorial staff or the author so as to make things easier for the compositor and to cut down cost (printers charge more for setting "dirty" copy). Retyping will also help to reduce the number of errors introduced during typesetting and, hence, the cost of correction later. Any new errors arising from the retyping should be corrected by the editor before the paper goes to press. Any diagrams will be sent to the blockmaker at this stage, with or without redrawing and relettering. Some journals redraw all material in order to have a uniform style, but each such extra task means delay in publication and increase in costs. In due course the author and editor (among others) will receive proofs of the article, and these should be corrected where errors are found. The corrected proofs are collated by a sub-editor and divided into pages which are put together with those of other papers to make one issue of the journal. A single issue will usually be composed of multiples of 16-page units, each known as a signature, this being the number that is conveniently printed on both sides of a single sheet of paper which can then be folded and cut to give a set of pages which can be bound with other signatures. The type is then rearranged so that a number of separate copies of each paper can be run off to provide the author with reprints (also known as separates, offprints, etc.) of his paper for distribution to colleagues.

The costs and economics of printing and publishing journals are a matter of widespread curiosity and ignorance. Publishers of commercial journals are naturally rather loath to disclose circulation figures, costs, and profits. The costing of society journals is rather more readily accessible, either from the accounts of the society or from articles in the literature. In any event, the prime costs have the same origins, namely, editorial salaries and ancillary expenses, typesetting charges (which remain the same whether a single copy is printed or ten thousand), cost of paper and covers, printing charges for machine work, distribution costs (packaging and postage), and a contribution to general overhead charges such as office rent, salaries of ancillary staff, and directors' fees. The subscription rate in cost per page or per thousand words is determined largely by the circulation. The greater the number printed and sold, the lower is the unit cost. For journals which accept them, advertisements can be an important source of revenue and help to reduce the cost to the purchaser, and here again circulation plays an important part. Advertisers will use only journals which have high circulations; high circulation decreases costs and, hence, subscription rates; high subscription rates cause low circulation; low circulation prevents defrayment of cost by advertising revenue. The prime example of the power of circulation and advertising is *Analytical Chemistry*. One of the claims made on behalf of society journals is that the members of the society receive journals at very low cost because they are subsidized by non-members who buy the journals at the full economic rate. Some publishers of commercial journals offer

very favorable personal subscription rates to individuals buying the journal for their own use, a subsidy again coming from institutional and company purchasers.

At each stage of publication, different people play their parts. The role of the publisher and printer is outwith the control of the author or editor and, for present purposes, can be treated as purely mechanical. The roles of the author, the referees, and the editor will now be considered in turn.

THE AUTHOR

The author's first job is to do the necessary scientific work in a proper manner, paying due attention to all those aspects of experimental design and investigation that experience has shown to be necessary. Unfortunately, experience has a habit of requiring to be gained afresh by each individual, and even when advice and instruction can be handed on, a good instructor may still produce a bad pupil. There is a considerable need for establishment of a uniform pattern of development of analytical methods, and a start has been made with spectrophotometric methods.[1] Similar papers on gravimetric and titrimetric analysis, atomic-absorption spectroscopy, and so on are in active preparation. If a proper investigation has *not* been made, the referee is likely to ask why and to recommend that additional work be done. During the research itself, if the scientist is honest with himself, he will soon decide whether the results are important enough to warrant publication and, if so, in what form. As already pointed out, if a new procedure has no advantages whatever over the old ones, there is no justification for publishing an account of it at great length. All that is needed is to say that it has been tried and found wanting, so no one else need bother with it further. Any data such as stability constants or molar absorptivities can simply be quoted for future incorporation in tabular compilations. All too often "research" seems to consist of taking a reagent off the shelf, finding that it reacts with several metal ions, and then systematically working through these, writing one paper for each element examined. In the worst cases, the resulting papers have exactly the same format, with only the names and numbers changed. An alternative line is to take a particular metal–reagent system and to substitute various groups into the reagent molecule (often in positions where they could safely be expected to have no effect on the system) and to write a separate paper for each reagent. Another favorite trick is to develop a method with a reagent used gravimetrically (Part I), then spectrophotometrically (Part II) with solvent extraction, then by atomic-absorption measurement of the metal in the extract (Part III), and so on. My own pet retirement project is to continue (with due acknowledgment to the suppliers of copious free samples of reagents) the work of Carpené[2] who found that platinum underwent no change if stood in red wine for 192 hours. Nowadays that would have been Part I on the time factor alone, with all the permutations of types of wine still to play with. The results will need the founding of a new journal, to be called *Journal of Useless New Knowledge* (the *Chemical Abstracts* abbreviation will be *J.U.N.K.*). It is most important that the work done should be of direct use and demonstrated to be of direct use by application to samples of materials likely to be met with in common analytical practice or in specialist applications, and the results obtained should be compared with those obtained by other methods or with the known composition of a synthetic sample. In other words, there must be adequate justification for expenditure of space on publication of the procedure, and personal vanity, self-aggrandizement, or promotion-seeking do not constitute valid reasons for publication. If the author has not sufficient insight to assess his work objectively, he must not complain if the deficiency is supplied by the referees and editors to whom he submits his work, only to find it rejected.

Having completed the work and decided it is worth writing up, the author must choose an appropriate journal. There is not much point in submitting a paper on zone-refining of chelate complexes to a journal dealing exclusively with food analysis, unless the author is a keen stamp collector. (I once received an article on the law of contract between mas-

ter and servant, with a request to consider it for publication at my usual rates; I refrained from sending a bill for considering it not to constitute a contribution to analytical chemistry.) Once the journal has been chosen, the author should carefully study the style of writing and layout used and follow it in preparation of his paper. Doing so will save a great deal of editorial time (and temper) if the paper is accepted and will expedite publication. Points to which attention should be paid are usually listed in a set of notes for authors, or in a handbook (notably those of the British and American Chemical Societies), and include order of presentation of material, kinds of heading and sub-heading, units, abbreviations, use of diagrams and tables, and style for writing references. The last, by the way, is a fairly sure giveaway for a paper submitted to another journal after rejection elsewhere, since most journals have different styles for references; the moral is, be sensible, do some homework, and retype.

As recently pointed out by Ziman,[3] the traditional format and style of scientific articles constrain the author to present an unnatural picture of the way in which he arrived at the final results, since the unproductive exploratory steps are usually omitted, and the research is made to appear to have progressed smoothly and logically every step of the way. It has been known for referees to insist on a paper's being presented in a fashion that bears no relationship whatever to the way in which the work was originally conceived and done. The author should be allowed freedom to express himself in a way that reflects the real development of the work and not be compelled to describe it as someone else thinks it might have been done. On the other hand, the author must not be allowed to indulge in irrelevancies. Any part of the manuscript that does not serve a truly useful function is likely to be excised by the editor (as indeed it should be). What is wanted is an accurate concise account that will enable the reader to repeat the work and to obtain essentially the same results. In analytical chemistry there are certain fundamental items of information which are necessary for this end to be achieved, and these have been catalogued by Wilson in his papers on "performance characteristics."[4,5] i.e., those parameters which permit judgment of the quality of an analytical procedure.

What the reader wants to know is the kind of situation to which the procedure can usefully be applied, what sort of equipment is needed, whether or not the operator need be skilled, precisely how the work is to be done, what snags and difficulties there may be, how much time is needed and how many samples can conveniently be handled together, which factors are highly critical and which are not, how sensitive, selective, precise and accurate the method is, and (perhaps most important of all) what the underlying theory is, so that if things go wrong or he wishes to apply the system to a new situation, he has some chance of modifying the procedure successfully.

Some of this information is sometimes essential, sometimes not. For example, it is quite usual to specify that a particular brand of reagent or particular instrument was used, but unless there is some overriding reason (by virtue of purity or performance, for example) for using the article specified, this information is unnecessary, since a simple statement of reagent grade or instrument performance will do as well. Otherwise, mention of brands is perhaps justified only when a crucial experiment is described, or a claim of a particularly high degree of accuracy has to be judged in relation to the known performance of the equipment. Authors must also be careful about using trade names, especially those registered as trade-marks, as generic names; editors do not like receiving letters from lawyers about infringement of rights, although their private opinion may be that companies should be pleased to think that their products are so well known that the names have become household words.

In describing the procedure itself, it is necessary to state or imply what tolerance is permissible in the conditions laid down. Many authors give instructions to weigh some multiple of the formula weight of a reagent, quoting all the decimals of the atomic weights, and obviously never having considered the purity of the reagent. A quick glance through a list of specifications for analytical-reagent grade chemicals will show how few can be relied upon to be

within even a few tenths of a per cent of nominal purity, and how many require standardization of their solutions. The same authors fail to see the lack of logic in going on to state (as they usually do) "standardize the resulting solution." Again, it would be helpful to know whether a pipet is necessary (or if a graduated cylinder will be adequate) for addition of a liquid. When pH control is necessary, the permissible range of pH should be investigated and quoted and an adequate buffer system suggested. If the pH range is so narrow that a pH-meter must be used to check the pH adjustment, the method is not as simple as one in which a buffer can be used. The buffer itself should not contain ions which will give side-reactions with the species being determined and should be a sensible one (many authors specify hydrochloric acid–potassium chloride buffers for low pH values, forgetting that the original purpose of the potassium chloride was to maintain a constant ionic strength in the system, and that all the buffer capacity resides in the hydrochloric acid itself; a really nonsensical proposal that sometimes occurs is the use of hydrochloric acid–acetic acid mixtures at pH values of about 1).

The reason for each step in the procedure should either be briefly explained or be obvious from the general chemistry of the system, so that the user of the procedure knows which steps may need modification for particular sample matrices, and which steps may be omitted in certain circumstances. If there is no reason for an operation, the operation is unnecessary. It is most important that the composition of likely samples should be taken into account in planning any study of interfering elements, and studies in conjunction with the auxiliary reagents required in the procedure, especially in view of the effect of formation of inert mixed complexes (such as the copper–citrate–chromium[6] and aluminum–citrate–uranyl[7] complexes) which may effectively mask the whole of a metal ion being determined. There is not much point in investigating the effect of ions which are unlikely to have any chemical interaction with the reagent other than a salt effect on an equilibrium, and it would save a lot of experimental time if ions such as those of the alkali metals and alkaline earths were investigated collectively in a single test instead of individually. The concentration levels used for tests of interfering elements should also be realistic. All too often a method is proposed that will tolerate other ions in amounts only a few times greater than that of the element being determined; the practical consequence is that the method can be used for direct determination of the element only if it is present in the sample in major amounts (say 10% or more) and a preliminary separation is necessary for determination of minor or trace quantities. The concentration ratios used should cover the full range likely to be met with in any application envisaged, plus a safety margin of at least 100%, and various combinations of interfering elements in varying concentration ratios should be tested to see if there are any coupled effects. For papers dealing with methods and their uses it is essential that *some* applications should be not only proposed but also actually tested, and papers not conforming to this rule should not be accepted. The reason for this requirement is that whereas it is comparatively easy to get accurate results for pure solutions or synthetic samples composed of pure solutions of individual species (and really such tests are mere confirmation of a calibration curve or of the stoichiometry of the system), it is quite another matter to carry a real sample through a decomposition procedure and obtain equally accurate results. One reason, of course, is that every additional step carries its own attendant possibilities for introduction of error. A more cogent one is that the chemistry of the opening-out process may convert the determinand into a state in which it will not react in the same way as in the procedure. Many of the platinum metals will react in ways that depend on the previous history of the solution; the early literature of the heteropoly acids abounds with the fruits of lack of knowledge of the existence of two forms of heteropoly acid with different light-absorption characteristics, and many an analysis must have been inaccurate because part of the determinand has volatilized during the decomposition process.

Details of the experimental technique may be of paramount importance. The researcher has gradually developed the technique to its

highest pitch and may not have noticed the introduction of small but vital variations in operations; the newcomer to the method may not be able to obtain such good results because of some deficiency in technique caused by incomplete description.

If the method is an instrumental one, the performance required of the instrument should be specified so that the reader can decide whether his own is capable of doing the job. Any special modifications should be described in full, with details of any circuitry (values of resistors, condensers, etc.) or metal or glass work (with dimensions and hints on construction). When recorders are used, their accuracy, precision, and dead-band characteristics[8] should be quoted and made use of in assessing the reliability of the results obtained.

The results of the research should be quoted in the manuscript in sufficient amount for the referee to judge the validity of the claims made but need not all appear in the printed paper. The reason for this is quite simple. Any reader who would disbelieve a simple statement about the results would be just as likely to disbelieve a table of results or a graph, so these would serve no purpose. The referee, on the other hand, requires the fullest information for checking the quality of the work, and since the labor required to concoct a convincing but fictitious set of results is probably as great as that needed actually to do the work, the number of results is a fair guarantee of authenticity. This last statement may be regarded as implying that some scientists are dishonest; a recent article on the behavior of certain referees,[9] together with the folklore on the subject of research results, shows that the charge may not be altogether without foundation. There are cases known where claims have been made that an author has taken someone else's ideas and results and passed them off as his own; it is extremely difficult, if not impossible, to prove such claims, but the very suspicion casts doubt on the probity of the author. In general, of course, the genuineness of results is beyond question, but unnecessarily detailed results are often quoted in papers because of the fear of disbelief if statements are not supported by the magic properties of numbers. The provision of a full set of results for the referee is, however, essential. He can then conduct statistical tests if the author has omitted to do so, can assess whether wild claims have been made in respect of accuracy and precision (many authors still quote as many significant figures as there are decimal places in the logarithmic mantissae used to calculate the results, and at least one has claimed precision to six significant figures because the recorder company's engineer had assured him that measurement to ± 1 μV meant an error of 1 part in 10^6 irrespective of the voltage measured—in fact it was a few mV). Statistical tests should be applied by the author whenever possible, and calculations should be checked by independent methods. (One author produced an illustrative set of raw data in support of a series of least-squares calculations of slopes and intercepts; a statistical analysis showed that the slope had a standard deviation equal to about 25% of its value, and graphical display of the information revealed two errors of subtraction in the values of the independent variable, resulting in two pronounced discontinuities. Another author produced a calibration curve consisting of a number of points through—or, rather, between—which a curve had been drawn; inquiry as to whether a straight line would not have been as good elicited the further information that the points were the means of results that were in rather poor agreement.)

Statistics and their real meaning are still apparently understood by too few authors, though analytical chemists are probably much more aware of statistics than are many others. One bacteriologist writing on the use of a platinum loop as a means of taking a reproducible sample volume quoted statistics for the results; they revealed that some drops would be twice as big as others, although this fact was not noted in the report.

Results may be reported in the form of tables, graphs, or statements in the body of the text. Diagrams cost more and take longer to be produced than do tables which, in turn, cost more than text. If an author is interested in having his paper appear as soon as possible, he will avoid the use of diagrams but, if he must have them in, will provide good quality drawings that can be used without redrawing or retouching by the publisher's art depart-

ment. Straight-line graphs should never be used since the equation of the straight line can be quoted in the text. Again, a reader who will not believe the equation will not believe the graph either. Diagrams showing Job plots, mole-ratio and slope-ratio plots, potentiometric, spectrophotometric, and conductometric titration curves are also unnecessary, except for use by the referee, unless they show some unusual feature. (One author gave graphs for all but one of the Job-plot investigations made, and a table of results for the last one; a plot of the results showed the existence of a second break-point on the curve, and this was not mentioned in the text.) Tabular material can often conveniently be summarized and included in the body of the text, and space can be saved by setting two-column tables across the page instead of down it. Authors should remember that wasted space still has to be paid for.

The final section of a paper is usually the literature references. These are fully as important as any other part and should be checked and double-checked for accuracy against the original literature, if that is accessible, rather than against abstracts. Carelessness in copying out a reference is quite inexcusable and can lead to considerable difficulty in tracing the correct source if a totally irrelevant one is quoted. It is essential that the author should follow the journal style for putting references into the body of the text and for writing them out at the end. If the journal style requires that references be numbered serially in order of appearance in the text, and an author produces a review paper containing scores of references arranged in alphabetical order, there is a considerable chance of error occurring in the renumbering, especially if the paper is revised and additional material introduced or the old material is rearranged. It is not unknown for papers to be submitted with references appearing in the text but not in the bibliography, or vice versa, or even with references missing from both. One author had only two references in his paper, the second being to his own work and quoted incorrectly; another gave a reference which proved to be non-existent and, on inquiry, changed it to "in the press," a statement which seemed most unlikely to be true since the reference was not to his own work; editorial diligence revealed that the paper referred to had appeared in 1922. Misspelling of names and omission of accents from names is not only careless but also discourteous, and so is the reference to multiple authorship as R. Roe et al. in the bibliography, though this is not only permissible but also desirable in the text. Authors living in countries where the family name is customarily given first, followed by the given name, should be careful not to assume that other countries have the same habit. On one occasion a reference to Christina and Robert was rather endearing but not very helpful in finding the paper concerned.

Once the paper has been properly written, if the author's judgment of its worth has been sound, there should be no difficulty in getting it accepted for publication. If there *is* criticism of it, either of form or content, the author should remember that the referee and editor are acting as informed arbiters of presentation and quality and are making suggestions in the author's own best interests. It is much better for an author to have his faults pointed out and corrected in private than to suffer the ignominy of having this done in public after they have been exposed in print to general examination. If the criticisms are justified, the author should accept them and learn from them. If the difference between author and referee is one of opinion on a matter that cannot be resolved by scientific test, then the editor must decide in favor of the author, though he may suggest a form of wording that will minimize the risk to the author's reputation should the referee's opinion be the one generally held.

THE REFEREE

Although the term reviewing is used in the title of this paper, and American rather than British usage is required for style, the term refereeing has been used exclusively in the text because it carries with it the connotation of fairness and impartiality. In an ideal world referees really would be impartial but, unfortunately, as stated by Wright,[9] there are some who are not. Be that as it may, the referee is chosen, whether by an editor or a committee,

as an expert who can give a valid opinion on the merit and accuracy of the work submitted. The referee's task, therefore, is to read the paper carefully, making due allowance for limitations in the author's command of the language used, and to set it in context against his knowledge of that sphere of research so that he can decide whether it contributes something new and worthwhile to knowledge and is based on sound reasoning and experimentation. If he is satisfied that the work fulfills all these criteria, he will recommend publication, possibly with minor corrections designed to improve the presentation. If the work is basically sound and useful but suffers from gaps in reasoning or from incomplete investigation, he will point these out and suggest what needs to be done to make the work completely acceptable. If the work is trivial, or has already been done and published by someone else, or is obviously grossly incorrect, he will recommend rejection. There is one exception to this rule, however, and that is that when two workers have independently examined the same system practically simultaneously but have submitted papers to different journals; both may and should be published.

At this stage the submitted manuscript is, in principle, a confidential document, and the referee is not entitled to abuse his position by adopting delaying tactics to hold up publication of the paper while he rapidly rushes through a work of his own to anticipate the material he is adjudicating. Strictly speaking, he should make no use of the information given until it has appeared in print, or at any rate been accepted for publication. If he does do this, and too quickly, it may be possible for the author to identify him even though the referee is supposed to remain anonymous. The question of anonymity of referees is a vexed one. On the one side it is argued that it gives the referee power to express himself freely and avoids the possibility of development of scientific feuds; on the other that the referee should write nothing that he is not prepared to back by a signature. On balance it seems best to retain the system of anonymous refereeing with the proviso that by consent of the referee his name may be revealed to the author if so doing is likely to resolve a deadlock. In my own experience this has arisen only once, when the author was quoting the referee's own work in opposition to the referee, and withdrew all objections when apprised of the referee's identity. The guarantee of anonymity does permit the younger worker to feel free to criticize when necessary without fear of petty retaliation or victimization. At the same time it often gives the editor the extra task of editing the referee's and author's remarks about each other before transmitting them and would involve extra labor and expense if detective work[9] by the author were to be circumvented by retyping all reports.

Referees vary considerably in quality. Some are prepared to do the job properly and spend a good deal of time on reaching a just opinion of the paper; it may take several days to check a complicated argument and verify statements made. Others may read only the name of the author and decide on that basis whether the work is likely to be sound or not. Certainly the refereee should be no respecter of persons and should base his opinion solely on the work described. If work is bad or wrong, it should be labeled as such, no matter how distinguished its author. But there is really no need for the referee to be libelous in his comments, though he may be rude if it relieves his feelings (the editor will usually soften the blow to the author). A pithy remark may well be justified; an author who has split up a single piece of work into several papers cannot really complain if a referee comments that the meat has been published elsewhere and that all that remains is the gristle.

The amount of work involved in conscientious refereeing raises the question of whether referees should be paid for their work. The view has been expressed that while it is all right for referees to perform their task without payment when the paper is sent by a society journal, they should be paid for it by a commercial journal. This seems a curious argument, because society journals are just as "commercial" as any others, in the sense that they have to pay their way. The propounders of this view never make any suggestions as to rates of remuneration or means of ensuring that the journal gets its money's worth. As already said, some referees appear not even

to read the material sent them, and they would certainly not be entitled to receive as much as someone who had spent several days in appraisal. The editor would have to referee the referees in order to decide their worth. In practice, the editor is likely to draw on a large number of referees in the course of a year and not to overburden many of them. The referee who receives only one or two papers in 12 months is scarcely overworked. If an editor makes regular use of a particular referee and asks him to deal with several papers in a year, then there is a good case for recognizing the referee's services by sending him books to review or giving him a free subscription to the journal, and this is indeed often done by commercial journals. Again, a commercial journal will usually have an advisory board, a panel of well known experts in its specialty, and these will receive a complimentary subscription, in return for which they may fairly be asked to referee a reasonable number of papers. In principle, the referee for the commercial journal is in exactly the same situation as the referee for the society journal, in that it is in his own interests to help to maintain the standard of papers appearing in that journal if he proposes to publish in it himself, and his refereeing is, in effect, a quid pro quo for the service someone else will do him when his turn comes to submit a paper. However, so many feel that commercial journals can well afford to pay for the referee's services that these journals might be well-advised if they did so. The referee's task is onerous, but if approached in the right spirit of impartial scientific inquiry, it can be rewarding (in one sense at any rate).

THE EDITOR

The editor's main function is to act as arbiter of scientific quality and to see that proper standards of scientific integrity and worth are maintained. It is, therefore, essential that his own integrity should be beyond question, and it must be generally recognized that though he may make errors of judgment they are not motivated by personal feelings such as malice or animosity.

His duties are manifold. When a manuscript is received he must see that it is acknowledged, given a reference number for identification and recording purposes, and find a referee. Finding the referee may be a simple matter of choice from personal knowledge of the field concerned or from past experience, but in the case of an esoteric or completely new topic it may involve a literature search to find someone not too far away who can do the job competently. The stress on distance arises because sending a manuscript to another country not only increases the time the paper spends in transit and the cost of sending it and getting it back (return postage by the cheapest convenient route is paid by the journal, and usually means airmail parcel post for overseas mail), but because it is easier to reach a slow referee by telephone if he lives in the same country as the editor. There is also less risk of loss of mail in transit. Experience shows that it is riskier to send mail to some countries than to others, and that transatlantic traffic may be faster than internal mail. When the referee has reported, after a reminder if necessary, the report must be considered in conjunction with the paper itself. If rejection is recommended, it must be verified that the recommendation is justified, and a second (and third if necessary) referee found if it is not. The editor is finally responsible for the contents of the journal and should not hesitate to overrule a referee if for some reason the referee's judgment is at fault or is biased. That is why it is essential, when publication policy is not in the hands of a committee of experts but the sole responsibility of the editor, that the editor should be sufficiently well versed in the whole field of analytical chemistry to be able to make reliable value judgments in relation to other work. If proposals are made for improvement of the paper, or errors are pointed out, the editor must transmit these to the author, edited if necessary, and act as intermediary in ensuing correspondence. If the author fails to reply after a reasonable interval, the editor will then inquire as to whether the report has been received and will usually send a copy. Once agreement has been reached and the paper accepted as of good enough standard (and again the editor must overrule the referee if a paper

is recommended for acceptance but fails to meet the general standard associated with the journal, though he must then adduce cogent reasons for doing so) the process of copy-editing begins. Either the editor or one of his assistants will check through the paper for conformity to house-rules for style, and will correct any errors of spelling or grammar. At this stage there is a check also on the work of the referee, because an editor who is a working chemist will automatically referee the paper again as he reads through it and may notice points that have escaped the attention of the referee (especially if the latter has made a report that is substantial enough to appear complete without really being so). The manuscript will be marked with directions for the printer as to type style, etc., with headings and subheadings indicated in order of weight. Throughout this work the editor should try to place himself in the position of the average reader, one who is chemically educated and able to think logically, but is not necessarily thoroughly conversant with the topic of the paper. The editor will then have two golden rules. The first is that if he cannot understand what the author means, then neither will the majority of the readers, and clarification must be sought. The second is that a word or phrase should be removed if it is not doing a specific job of work in conveying some information. Authors might note that similar remarks apply to reagents and experimental details when procedures are being developed and described. The author may be rather upset to find that an editor has considerably recast a paper, but he should reflect that the editor was originally appointed on grounds of linguistic as well as chemical ability; and whereas the author might write only a score of papers during his working life, the editor will have dealt with as many hundreds or even more. The difference is akin to that between the week-end golfer and the circuit professional. The editor will always be glad to explain to the inquiring author precisely why certain changes were made, although it is not normally part of his duties to give lessons in grammar.

The editor will also often check the references, especially if his eye is caught by something he knows to be wrong, such as a missing accent from a well known name, or an obvious error in the tally between journal name, year, and volume number. It has been known for a reference to be given to an issue that had not appeared and had not even gone to press at the time the paper was written. It will not endear the author to the editor if it proves necessary to retype an extensive list of references in the correct style for the journal.

The editor will also decide which illustrations are absolutely necessary for proper understanding of the work, and which can be deleted and described in the text, and will see that the captions are adequately worded. Drawings will be inspected and arrangements made for any retouching or relettering needed to make them of acceptable standard.

The work of assistant editors should be checked by the editor to make sure that all has been done properly. It is very easy to miss a point of grammar or a minor spelling error. The author is then notified that his paper has been accepted, and the edited manuscript is sent to the printers. Once the paper has been set and proofs pulled, the editor reads the proofs and makes a final check to pick up any errors he made in editing.

The editor's main concern is to maintain the standard of his journal and, if possible, to raise it. At the same time he has a responsibility towards authors to see that their work is fairly judged and presented in a way that will do them the most credit. Authors who feel that their work has been ruined by the editorial activities should reflect that if the paper had been left in its original form, it might have been so poorly expressed that the human error of judging the content by the form could lead to the value of the work being totally obscured by bad presentation. The editor is anxious to render the best service he can to the readers of the journal and to the authors, and in protecting the interests of the one group he serves those of the other. His own work is judged by the readers on the basis of the number of errors he allows to be committed and on the quality of the papers he accepts.

In one respect the editor is in a dilemma: the two factors that predominate in attracting papers to his journal are the prestige associated

with the journal's standards and the speed of publication. To achieve rapid publication he must minimize the delays caused by traffic between the author and referee, and while he can ensure that his part of it is dealt with expeditiously, he is at the mercy of the other two parties, and a dilatory referee or author can cause considerable delay. Even with a paper that encounters no hold-ups there will be a minimum time lapse before publication. If a three-day average transit time is assumed, then the referee will not receive the paper until a week after its dispatch by the author. If the referee deals with it promptly and there are no queries about it, it would reach an assistant editor about two weeks later (assuming a week for refereeing), and go to the printer not earlier than a month after initial sending. The printer probably takes between six and eight weeks to set it in print (depending on whether blocks are needed) and proofs will arrive after a total time lapse of some three months. A single issue of a monthly journal has to be made up from its component papers one month before it is due to arrive on the subscriber's desk, so if the proofs are returned in time to be included in the next issue, the paper *could* just manage to appear four months after its submission, but will more likely come out five months after submission. The time can only be shortened by using different printing methods, but even if setting time were reduced to a week, it is very doubtful whether publication could be achieved in less than three months. If the editor tries to take short cuts, the standard of the journal will suffer.

SOME GOLDEN RULES

In view of the many criticisms made above, it seems desirable to offer some help and advice to potential authors, in the hope of avoiding disappointment and of improving the standard of the literature. At present, the rejection rate for those journals which operate a refereeing system probably runs at 20–30% of all papers submitted and represents a considerable waste of effort. Some journals (which for obvious reasons will not be named) seem to accept any paper sent to them; and human nature being what it is, there is a tendency to judge their quality by that of the poorest papers appearing in them. By the same token, additional prestige attaches to publication in one of the journals noted for the strictness of its refereeing and editing. A paper is more likely to be accepted if it describes work that is useful and of good scientific standard, and is sufficiently well written for the referee to be able to understand it readily without need for analysis of the language. It is also likely to be published rather more quickly if extensive editing is not needed.

Advice to Authors

Do decide the real value of your work and write at a length appropriate to it. Write a full paper only for an important contribution. For a minor piece of work use a "short communication" and for a mere piece of gap-filling in published data write only a paragraph or two, giving references for any standard techniques of investigation or calculation. Choose an appropriate journal, *read* both its notes for authors and several papers in it, and write in the general style of the journal. If you are writing a series of papers on one topic, send them all to the same journal—it is more convenient for the reader. If you have had a paper rejected, do consider the referee's and editor's comments before submitting it elsewhere—they were designed to help you. If you do submit the paper to another journal, do recast it in the new style first, and do not (as has happened) submit Parts x and nx simultaneously—it arouses the suspicion that Part x has been systematically rejected by rather a lot of journals. Pay particular attention to references, using the appropriate style (note that the *Chemical Abstracts* abbreviations recommended by IUPAC appeared in the 1961 volume, attached to the Author Index, and supplements have appeared at regular intervals since) and check the typescript against the original literature as far as possible. Be careful to copy names correctly (you would object if your own were misspelled) with due regard to accents, but be even more careful about names transliterated from Russian or Chinese—the abstracting services have differing ideas.

If you are writing in a language that is not your mother tongue, do not depend on a dictionary for translation in one direction only. Use another dictionary to translate back again, to see whether a mistake has occurred because of the large number of undefined synonyms usually given. The traditional example is the Swahili translation of the English phrase "out of sight, out of mind" as "invisible idiot," but genuinely observed examples include the description, in an Italian journal, of a mathematical derivation as "the entertainment is simple and elegant" (*treatment* mistranslated) and a mistranslation (in a paper submitted) of "coupling," which severely tempted the editor to paraphrase the passage to read, "Copulations were attempted under the red light in a refrigerated dark-room; they were successful." The same error will be found in the index of one volume of a middle-European journal.

Do check the grammar and, in doing so, consider whether all the words used are strictly necessary. For example, "The determination of vanadium was carried out by titration with potassium permanganate with the end-point being determined by potentiometric means" (and I did not altogether invent that—it is a pastiche of phrases actually used by authors) is rather more elegantly expressed as, "Vanadium was titrated potentiometrically with potassium permanganate." Note that titration conventionally implies a quantitative result and, hence, determination. If a sentence appears to be getting out of hand and requiring several conjunctive phrases, try turning it round the other way, with a different subject; the connecting phrases will usually drop out. Beware of the hanging gerund, and especially of using "using." All too often, analysis of a sentence containing this word shows that it has been incorrectly used. Many titles of papers tell us that such-and-such was detected using so-and-so. It is the scientist who does the using. The correct form is, for example, "A was titrated with B, C being used as indicator." Try to avoid slang or jargon—German may contain a verb "pipettieren" but in English "pipette" (sorry, "pipet") is always a noun, and no one has yet been observed to "buret" a solution (and even if he had, it would not be clear whether the liquid was being put into the buret or taken out of it—cf. bottling). Likewise, "excess" is never an adjective, and "analyze" and "determine" are not the synonyms they are so often assumed to be. The word "only" is a pitfall for the unwary, as exemplified by the following sentences:

I am only the editor of X.
I am the editor of X only.
I am the only editor of X.
Only I am the editor of X.
I am the editor only of X.

How many of them mean the same thing? Negative statements injudiciously mixed with positive ones in the same sentence can make an author appear to say the opposite of what he intended; put all the negative statements together at the end of the sentence so that the negation cannot be mistakenly attached to a positive one. Infinitives *may* be split, but not as much as "to in some small way partially ameliorate."

Be careful with punctuation, especially in the use of the comma. For example, "separation of antimony from other elements with a large decontamination factor" changes its meaning if a comma is inserted after "elements." Do not protract a sentence unduly; break it up into shorter ones. Do not use too many parenthetical statements; this article contains many examples (more or less deliberately included as a warning) of the resultant dilution effect and the need for the reader to concentrate.

Finally, read as much well-written material as possible. There are several books available on style in technical and scientific writing; but unless they are being used in a systematic course of instruction they probably tend to damp the reader's enthusiasm by a surfeit of earnestness. It is rather more fun and probably as useful to read some of A. P. Herbert's books and articles (the latter appeared in *Punch*). One particularly useful book is Eric Partridge's *Usage and Abusage* (Penguin Books) which first appeared in the United States in 1942 and has the merit of covering both British and American usage.

Advice to Referees

Do read the paper sent to you, and think about it conscientiously. If you have criticisms to make, back them up with chapter and verse if they are criticisms of fact, or with cogent argument if they are of logic. Remember that the author may be working under severe difficulties, such as shortage of materials and equipment, especially in certain countries, and modify your opinions accordingly. At the same time, remember that bad science is to be branded as such whatever its source, but discuss it in such a way that the author can learn from your experience (and from his own in having his work criticized). Do not hold up a paper through forgetfulness, pressure of work, or malice. You are in a privileged position in that you have been allowed to see the work before it has become public knowledge, and you have been chosen because of your good scientific reputation. Do not abuse the privilege or risk your reputation. A journal usually allows two weeks for refereeing. If you are unable to deal with a paper within that time plus an allowance of say seven days (the time needed to return the paper and for the editor to get it to another referee), then return the paper *immediately* with a note of explanation, and there will then be the minimum delay incurred. Of course, if you do not wish to undertake refereeing work at all, one way to insure not being called on is to behave badly and "lose" papers and/or give useless reports, but it would be more efficient and ethically sound simply to write to the editor to tell him so.

If you find you are unable to resolve a difference between you and the author, give the author the benefit of the doubt—it is his reputation that is at stake, not yours, and he is entitled to cut his scientific throat if he is determined to do so, though the editor may decide that the suicide should take place in a different journal. In general, temper justice with mercy, and give reasons for everything you suggest. Remember that you will have papers refereed by others, so do unto others as you would have done unto you.

Advice to Editors

Remember that the standard of scientific writing and work lies to a large extent in your hands. Decide on a minimum standard of scientific worth for publication in your journal, and stick to it. Do not be afraid to reject bad work, no matter how eminent its author. Do not hesitate to rewrite papers, in their entirety if necessary, to establish and maintain a tradition of accurate writing. Do not expect an author to rewrite a paper in better English (or other language)—do it for him but see that he inspects the amended version along with the original, in the hope that he will learn from it. If a referee says a paper is good when it is bad, or vice versa, overrule the referee and remove him from your list. Lose your temper as seldom as possible. Remember you are not really God.

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