well as above the one-sidedness of autodidactic studies—both of which are excellent as far as they go and not to be despised by any means in themselves, but neither of which can cope with a carefully devised and conscientiously carried out scheme of training the mind for the profession of a technical chemist such as I have had the honor of laying before you. Nobody is more conscious than I that at no existing institution has the beau ideal of such a scheme as yet been attained, but at Zurich we believe that we are, on the whole, on the right way, and that we shall do well not to exchange our plan for a totally different one, but rather try to improve it on the lines on which we are now working.

THE NEED OF STANDARD METHODS FOR THE ANALYSIS OF IRON AND STEEL, WITH SOME PROPOSED STANDARD METHODS.[1]

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MUCH might be said upon the desirability, nay even necessity of different chemists being able to get concordant or agreeing results, when working in the same field, or upon the same sample. If two chemists happen to be engaged in an investigation, or research in the same field, it is obvious that the value of any conclusions, which they may reach, is very small, if the analyses obtained by each, do not agree within reasonable limits of error. Or again if chemical analyses are to be used as the basis of commercial transactions, and the chemist in the interest of the buyer, does not get the same results as the chemist in the interest of the seller, it is clear that the transaction can only be brought to a conclusion by arbitration, or mutual concession. Of course what is desired in every case is the truth; but if two chemists working in the same field, or on the same sample, do not agree, where is the truth? That there is a difficulty of this kind in the chemical analysis of iron and steel at the present time, we are confident few who are well informed on the state of affairs will be bold enough to deny. And it is not difficult to see why there should be discrepancy between chem-

[1] Read before the World's Congress of Chemists, August 21, 1893.

ists. The amounts of the various substances affecting the quality of these metals, is so small in many cases, the limits of the specifications upon which enormous quantities of iron and steel are bought and sold. are so narrow, that there is necessity for the very best analytical work. Furthermore, the time within which results, in order to be useful, must be obtained, is so short, and the quantity of work required of many laboratories is so great, that even if we assume that all chemists are equally able, there is still reasonable ground for expecting discrepancies. The demands made by the iron and steel industry upon analytical chemists, at the present time, are very great. But on the other hand, only those who are constantly using the results of chemical analyses of iron and steel, know how disheartening it is to be continually met with discordant results obtained by different chemists, and still worse how this discrepancy tends to throw doubt on the value of all applied chemistry. The manager of a large and successful iron works, who has himself during the past twenty years had no small share in helping on the development of the iron and steel industry in this country by applying chemistry to it, recently said: "What is needed to-day in the chemical analysis of iron and steel, is such an agreement between the different chemists working on the same sample that those who have to use the results of the analyses may have a reasonable ground for the belief that the work is near enough to the truth to be trusted." The time has certainly come when those who are engaged in the analysis of iron and steel must provide some means of eliminating this discrepancy and consequent doubt, or analytical chemistry must be content to take a position which I am sure none of us are willing to see it occupy.

This is no new state of affairs. The difficulty has been recognized before this, and steps have already been taken to overcome it. The formation of the "Committee on International Standards for the Analysis of Iron and Steel," which you are all familiar with, is a step in this direction. The plan of that committee it will be remembered, was to meet this difficulty of discrepancy in the work of different chemists, by furnishing each one, who desired it, a sample of a standard iron or steel, in which the amount of the various constituents, was known, and thus furnish opportunity for him to check up his work. If any chemist felt in doubt about his work, or failed to get the same results as others, he could make an analysis of the standard sample, and if he obtained the known amounts of the various constituents he would be at liberty to conclude that his work was correct. If not, of course there is an error somewhere, which must be found and remedied.

At first thought, this plan seems to be entirely satisfactory, and it would almost appear as though nothing more was needed. If the work of that committee, it would seem, could be pushed forward to rapid completion, the difficulties due to discrepancy, which are now so annoying and vexatious, could be successfully managed, since it is obvious if we locate the difficulty, it must disappear.

Since this matter is so important, however, it may be wise not to reach a conclusion too soon. Is it really true that if we have in our hands a sample of steel in which the phosphorus, for example, is accurately known, and we make a determination of the phosphorus in this standard sample and get the known results, we are entitled to claim that our phosphorus work on other steels is accurate? Or again if we have a sample of steel or a series of samples, in which the carbon is known, and we check ourselves up by a comparison with these samples, are we entitled to claim that our other carbon work is correct? Or once more, if we are working on sulphur in iron and steel, and by our method and manipulation, we get exactly the same results on the standard steel or iron that it is known to contain, are we entitled to claim that our work on other samples is as close to truth as the standard sample.

At first sight it would seem as though these conclusions must follow. If we know how much phosphorus, carbon or sulphur a sample of metal contains, and with our appliances and methods and manipulation we get the known content of any of these constituents in the standard sample, it would almost seem that no other conclusion could follow, but that our work done with the same appliances, methods, and manipulation on other samples must be reliable and accurate.

We are inclined to think, however, that such a conclusion can hardly be successfully maintained. It is obvious, we think, that the condition in which the constituent, that we are working on, exists in the standard sample, has a most important bearing here. For example, if all the phosphorus in the standard sample, exists as phosphate, instead of phosphide, and some chemist who has been accustomed to determine phosphorus by dissolving his metal in hydrochloric acid, starts in to check himself up by comparison with the standard sample, it is clear that he may get exactly the same results as the standard is known to contain, and yet his work on other samples, in which the phosphorus exists, either wholly or in part in an unoxidized form, be worthless. Or again if the carbon in the standard sample is in such condition that the whole of it is shown by the color test, it is obvious that a chemist who checks himself up against the standard sample, would not therefore, necessarily, get correct results on other samples, unless they perchance all likewise had their carbon in the same condition in which it exists in the standard sample. Or still once more, if the sulphur in the standard sample exists in such form that it is all released as hydrogen sulphide by the method employed, and this is the characteristic feature of the method, it is obvious that however closely to the known content of the standard sample a chemist might get when working on this sample, his work by the same method on other samples, in which the sulphur is not all released as hydrogen sulphide, as is commonly the case in pig irons, would not be reliable and accurate.

These examples seem to us to indicate that standard samples in which the content of the various constituents is known never so accurately will not enable us to overcome the difficulty we are considering, independently of the method used. Obviously this is true, unless perchance the standard sample contains its various constituents in all the forms in which it is possible for them to exist. It, of course, may be urged that a series of standards, in which, taken as a whole, the various constituents exist in all possible forms would obviate the difficulty. But this means a multiplication of standards which might be very serious, and it means also the check by each chemist who is using the standards, of his method, manipulation, etc., against all of them, before he can be satisfied that his results are reliable. Perhaps we shall find that the difficulty can be obviated easier in some other way.

There is another phase of this case. Let us suppose that we have a standard sample in which the amount of carbon, phosphorus, sulphur, etc., is accurately known; that some chemist desires to check himself up; and that the method he uses is such that the difficulty discussed above in regard to the condition in which the various constituents exist in the metal is eliminated, does it now follow that, if the chemist in question gets the same results that the standard is known to contain, he is at liberty to conclude that his work on other samples is reliable? We are inclined to think that even under these circumstances the desired conclusion does not follow, for it may happen that the compensation of errors in the method and manipulation used, is such that on the standard sample, the correct result is obtained, while on a sample containing a different amount of the constituent which is being determined, this result would not follow. This point is too well recognized to need argument or illustration, and in the work of the Committee on International Standards an attempt has been made to meet this difficulty, by having a series of standards, differing from each other in the amounts of the various constituents. How large the number of standards would have to be, to completely overcome this difficulty, it is not easy to say, and for the purpose which we have in mind it is not necessary to go further into this phase of the discussion

It must not be supposed that we are trying to belittle the work of the Committee on International Standards. One of us is a member of that Committee, and in our judgment, let it be said modestly, the work already done, and the results already obtained by that Committee, have more than justified its formation. But the point we have in mind is that although standard samples in which the amounts of the various constituents are known are valuable and perhaps essential, as an element in overcoming the discrepancy in chemical work, which is now so annoying, the possession and use of these standard samples alone, independently of the method and possibly of other conditions, cannot be relied on to accomplish this desirable end.

A few words at this point upon the causes of discrepancy between the work of different chemists, may not be amiss. If we are right, the causes of discrepancy may all be comprehended under the following heads:

(1) Lack of uniformity in the sample.
(2) Impurities in the chemicals, or defects in the apparatus used.
(3) The chemist.
(4) The method.

The first of these causes of discrepancy is too obvious to need comment. If the samples worked on by the different chemists are not alike in the constituent in question, there is an obvious reason why the results should not be alike.

Also impurities in the chemicals or defects in the apparatus, are so clearly a sufficient cause for discrepancy between the work of different chemists, that no illustrations are needed.

Under the heading "the chemist," several points are involved. First, both the skill and the care with which the work is done are of prime importance in avoiding discrepancy. Not all men, and indeed not all who have studied analytical chemistry, seem to be fitted by nature to handle liquids and delicate apparatus with the skill essential to success. Possibly it is true that chemists, like poets, are born, not made. Second, many chemists although possessing manipulative skill in a high degree, do not seem to be willing to take the care and pains necessary to secure good work. If a method requires exact quantities of a reagent, or that a certain part of the work shall be done at a definite temperature, the chemist who pours in the reagent by guess or does not use a thermometer ought certainly not to be surprised if his results do not agree with those of another chemist, who puts upon his analysis, the additional labor required, in giving proper attention to these points. Third, it is clear that a part of the knowledge required in making any analysis, is furnished by the method. This is the function of the method, *i.e.*, to tell what is to be done, to describe the successive steps of the process, to state the amounts and kinds of reagents which must be used, and point out the precautions necessary to avoid error or secure the desired end. But over and above this, every successful chemist must have a large amount of acquired knowledge, which the method does not give him. No single method can be a treatise on chemistry. Fourth, every working chemist knows the value of experience in enabling him to avoid error and secure good results. Everyone who is constantly making analyses knows that there are a very large number of little details, any one of which might injuriously affect the result, and which it is impossible to mention in connection with each method, which the chemist of experience looks after almost without thought, and which the chemist lacking in experience is very apt to overlook. Among these may be mentioned proving that the beakers or other apparatus are clean before starting, knowing that the distilled water is pure, seeing that the top of the reagent bottle is not contaminated with dust or other deposit before pouring out any of it, care that the reagents used have not deteriorated or changed from standing, and dozens of other little points, all small, but all essential. Fifth, there is another characteristic of the chemist which may have an exceedingly important influence on the results, *viz.*, a clear understanding or failure to do so, of the method step by step, and also the close application of the mind, or the reverse, to the analysis, while it is going on. The probability of a chemist getting reliable results with a method are very much diminished if he does not understand the changes which take place, when the various reagents are added, and the effect of each part of the manipulation on the final result. Without this knowledge there is great liability of something being done inadvertently which will vitiate the result. No description of a method can hope to be so minute as to obviate the necessity for this mental conception. Also, there is the greatest possible need of the close application of the mind to the work in hand, while it is going on. Many routine chemists seem to do their work with the mind in any other place than on their work. It is almost inevitable that error should creep in under these conditions. A trace of precipitate is lost, the washing is not quite clean, or perchance the analysis is contaminated by impurities from a contiguous beaker in which another operation is going on. Such chemists may say truthfully, and usually do say, that they did everything exactly as the method required, and yet, as we can all see, their work is worthless.

That the method may be a cause of discrepancy between chemists few will be willing to deny. Some methods are inferior to others from indefiniteness of description, and some because the reactions involved are not quantitative to the same degree of accuracy, or perchance the insolubility of the final form is not as great in one method as in another. Whatever the cause, all methods are not equally accurate and without doubt some of the discrepancies of chemical work are due to difference of method.

How much of the present discrepancy in the analysis of iron and steel is due to each one of these causes, it is perhaps difficult to say. Some are inclined to think the lack of uniformity in samples is a very fertile source of differences in results, and there is much information pointing in this direction. Others are inclined to think that the various causes of discrepancy, which we have treated above, under the heading, "the chemist," are the principal source of the difficulty. Poorly trained, overworked and inferior chemists, they say cannot well do anything but inferior work, and when the results of such chemists. are brought face to face with a much higher grade of work done by a much better class of chemists, it is no wonder that there are discrepancies. Still others blame the chemicals and the method. Which is the most potent cause, however, is not the real question. What we want to know is, first, what can be done to reduce the discrepancies to a minimum, and after this has been accomplished, to provide some means by which the discrepancies can be reconciled, and the error of one or both of the results be eliminated. It is manifestly absurd to expect that the time will ever come when there will be no discrepancies between the results obtained by different chemists. To do this would require uniform samples, absolutely pure chemicals, chemists incapable of error, and methods which give absolutely accurate results. The most that we can hope to accomplish is to provide some means by which, when discrepancies do occur, it can be decided which, if either, of the two results is the more correct, and consequently the more entitled to be trusted and used.

Let us now look at the causes of discrepancy, mentioned above, and see whether there is any way out of the difficulty when the discrepancies are due to any of these causes.

If two chemists do not get the same results, due to the fact that the samples worked on do not actually contain the same amounts of the various constituents sought, there is a very simple way of managing the difficulty, *viz.*, exchange of samples. This is common practice to-day, and is usually the first thing done, when disagreement of results is announced. If we may trust our experience, this is all that is necessary to be done in very many cases. Also, if the discrepancy is due to impurities in the chemicals, or to defects in the apparatus, it is usually not difficult to locate the trouble. An examination and test of the chemicals, an exchange of the chemicals, or the change of, or the setting up of a new piece of apparatus, by one or both will usually show which is in fault. This likewise has been done in a number of cases, with more or less valuable results. For the discrepancies which are due to the various causes mentioned above under the heading, "the chemist," there is likewise a remedy not difficult to apply, *viz.*, to have the two chemists make an analysis in presence of each other. This is not uncommon practice in assaying. That lack of skill and care, lack of knowledge and experience, lack of attention, etc., would be eliminated when two chemists who disagree are required to work in the presence of each other, is too obvious to need further comment.

There is another way of handling the discrepancies due to two at least of the causes mentioned above, that is those arising from impure chemicals and defective apparatus, and those grouped under the heading, "the chemist," *viz.*, the plan proposed by the Committee on International Standards. If the discrepancy is due to an actual difference in the sample, we see no way of locating the difficulty, except to exchange samples. But if discrepancy arises between chemists from impure chemicals or bad apparatus, or from the chemist, himself, it would seem that an analysis by each of a standard sample in which the amount of the substance sought was known, would show whether the work of either or both of the two disagreeing chemists was faulty from either of these causes.

So far therefore, as discrepancies are due to three of the four causes given, there seems to be no serious difficulty in managing them. A sincere disposition on the part of the disagreeing chemists to get at the truth, will soon find the trouble. If it is due to method, however, the difficulty is not so easily overcome. Two chemists of equal ability perhaps, and certainly both equally honest, obtain results on the same sample which do not agree, and the disagreement is due to difference of method, who is going to say which is right, and which results should be used? Both have used good methods, which have been approved, and both are equally confident in, and tenacious of their own results, and both have an equal right to their opinion and belief.

This state of affairs seems to us to be a genuine difficulty, which at present there is no means of overcoming. We have tried to show that the proposed international standards for the analysis of iron and steel, although undoubtedly a step in the right direction, and a most valuable aid, still do not quite meet the difficulty. We therefore have the honor to suggest as a remedy, the establishment of standard methods for the analysis of iron and steel. If methods can be found, which in the hands of different chemists, will give the same results, and which are sufficiently rapid and accurate to be available and useful, and then an agreement be obtained to regard these methods as standard, and to use them as such until they are modified by the same authority that established them, we are confident that a long stride will be taken in the direction of overcoming the difficulties which are now so vexatious and annoying, in the analysis of iron and steel. Can such methods be found, and can this agreement be obtained? We are of the opinion that for most, if not all, of the constituents usually determined in iron and steel, satisfactory methods are now available. It is for this body to say whether they will do anything to help forward the needed agreement.

Before attempting to present for your consideration any proposed standard methods, it may not be amiss to discuss two or three points a little further. First, let us suppose that there is no known method that will satisfactorily fill all the requirements of a standard method. It is satisfactory in most respects, but not in all. What shall be done in such a case. We are inclined to think that this will not introduce any insurmountable difficulty. If the only known method, for example, that is sufficiently rapid to be available as a standard, is not quite as accurate as another, which is too slow for use as a standard, it will of course be requisite for all to agree to accept the results as given by the less accurate method, until a better can be obtained. We are very strongly of the opinion that as far as the applications of chemistry to commercial transactions are concerned, this will not be a serious matter. If there were standard methods for the analysis of iron and steel, so recognized by the profession, it would not be long before all contracts involving the chemistry of iron and steel, would contain a clause specifying that the analyses should be made in accordance with the standard methods. Of course, those making the contracts would know of the inaccuracy in the standard method, and would be able to allow for it, and we are confident that the value of agreement, and the avoidance of dispute owing to discrepancies, would be much preferred in the commercial world, to such a straining after accuracy as makes a method unusable.

Second, it may be urged that the establishment of standard methods would interfere with the progress of chemistry. The method being fixed, and the results being accepted without question, there would be no stimulus to obtain better results, and consequently no progress. In opposition to this view we would say that while progress may be stimulated in a measure by the antagonism of two chemists who do not agree, we are inclined to think that practically such antagonism often results in a squabble, rather than real progress and also that most of the progress in chemistry thus far has arisen in another way. Usually those who develop and publish new methods or modifications of old ones, do so, not so much to vanquish an antagonist, as to help themselves forward in their own work, or to obtain from their fellows, by publicity, the recognition which they feel is due. Both these stimulants to progress are fully as strong with standard methods in existence as without. Indeed, it may fairly be queried whether there would not be a stronger stimulus to upset or modify a standard method than would be the case if there were none. Certainly the reputation which would come to one who successfully changes or replaces a standard method, would be greater than that which would fall to one who simply offers a new method or modification, which must stand on its own merits among a lot of others independent of any organized action.

Third, what is the sphere of a standard method? Can a standard method be used for all purposes, or do the qualifications which make it available for one class of work render it unfit for use in another? This evidently is a matter of some importance. If the requirements of one kind of work are such that no method can be found that will fill these requirements and at the same time meet the requirements of another class of work, it is obvious that one class of work or the other will have to get along without the proposed standard methods. Let us examine into this a little. The chemical work done on iron and steel, may perhaps be fairly divided into three classes. (1) Quite a large amount of chemical work is done in connection with steel works, for the guidance of the manager or superintendent in making the product. The special requirement of this kind of work is speed, since it is impossible to hold the metal in the furnace for any long period of time. (2) By far the largest portion of the chemical work done on iron and steel at the present time is that in which commercial transactions are more or less directly involved, including under this head all analyses connected with the applicability of raw materials to make a certain kind of product, and all analyses having any relation to the sale of the output, and its acceptance or rejection by the consumer. The special requirement of these analyses is speed and accuracy. (3) A considerable amount of chemical work is done each year which may fairly be classed as investigation or research, such as studies made for the purpose of increasing the output of any plant, or an inquiry by those interested in the possibility of making any desired product, or an investigation by a consumer as to what kind of metal will give best results in any given surface. The special requirement of this work is accuracy. If now no method can be found which will give the accuracy required in an investigation, along with the speed essential in the operation of a steel works, it is obvious that the same standard method cannot be used in both cases. Fortunately some of the best and most accurate methods known are likewise sufficiently rapid, so that in most cases the standard will probably be applicable to all classes of work; but unfortunately at the present time, this is not true of all. It will undoubtedly be the delightful work of those chemists who have time and opportunity, to develop in the near future methods for all the various constituents of iron and steel, which will be applicable to all purposes; but at the present moment we are compelled to ask for what class of work shall we attempt to prepare standard methods?

Undoubtedly there is a reasonable opportunity here for a difference of opinion. Perchance some will think the method required for an investigation is none too good to be adopted as a standard. Others may think, perhaps, the methods in common use in steel works are sufficiently good. Our own idea is that the criterion by which a method should be judged, as to its fitness as a standard at the present time, is its applicability for use in commercial transactions. If it is sufficiently rapid for such use, for as all know, in commercial transactions time is money, and sufficiently accurate so that both parties to a transaction are willing to abide by its decisions, when their pockets are involved, it is a sufficiently good method to be regarded as a standard, at least until something better can be proposed. This criterion may seem to some to be ignoble, and to derogate from the dignity of chemistry as an exact science, but it must be remembered that in the chemical analysis of iron and steel, questions of pure science are involved in only a very limited degree. We are using chemistry as a means to an end, and however delightful it may be to pursue the truth, for the truth's own sake, this is not the problem which we have before us. Furthermore, we are confident that those who think the class of chemical work done on iron and steel where commercial transactions are involved is inferior and of low grade, will if they make a little careful investigation, find that this kind of work is very much better than they had supposed. The reasons why this should be so are not hard to find. (1) The practice which chemists who are engaged in commercial work get is very great, and every working chemist knows how much more reliable and uniform his results are after he has had considerable experience and practice with a method, than when it is new to him. (2) By far the largest portion of the analyses made where commercial transactions are involved are made with the thought in mind that the results will be questioned by another chemist on the opposite side of the transaction. In fact, a very large number of the results obtained by the chemist of the buyer, are met with results obtained by the chemist of the seller, and if the results do not agree, a third entirely impartial chemist is often called in to decide between the two. The value of this criticism of results on the accuracy of chemical work must be evident to all.

Fourth, what are the requirements of a standard method?

First, obviously it must be sufficiently accurate. But what is sufficiently accurate? Accuracy is a question of limits. No method gives results which are absolutely accurate. Even many of the atomic weights, upon which the very best possible chemical work has been done, are still in dispute. Furthermore, it hardly answers the question to say that sufficiently accurate is the highest accuracy that any known method will give, since other qualifications, some of which unfortunately are antagonistic to the highest possible accuracy, are likewise requirements of a standard method. It is evident that the line must be drawn somewhere, and more as a means of developing the opinion of the profession on the matter, than as desiring to dictate to them, we would suggest that a method which in the hands of different good chemists of experience will give three or four results on the same sample, agreeing within the following limits for the six constituents usually determined in iron and steel, is sufficiently accurate to be regarded as a standard method, *viz.*:

	Per cent.
Carbon, within	0.01
Phosphorus, within	0.005
Sulphur, within	0.005
Silicon, within	0.02
Manganese, within	0.01
Copper, within	0.005

We are confident that commercial transactions will readily adapt themselves to these limits and that if the discrepancies between different chemists do not exceed these limits no questions will ever be raised.

Second, a standard method must be sufficiently rapid. But here likewise the same question arises, What is sufficiently rapid? And here again it can fairly be said, this is a question of limits. Of course the desire, both on the part of the chemist and on the part of those who are to use the results of the analysis, is to have the chemical work done in the shortest possible time. For the chemist rapid methods save both time and effort; and in commercial transactions it is often of the greatest possible moment to have the results of analysis quickly. Furthermore, it is not enough to say that if business men want to use the results of chemical analysis in their business, they must simply allow time enough to have the analysis made. Undoubtedly this is true as an abstract statement, but it is also true that if three or four days or a week must elapse before the results of analysis are available, thousands of analyses will never be made. The analytical chemistry of to-day must adapt itself to the requirements of to-day, or it will have no work to do. And this brings us again to the question, What is a sufficiently rapid method? We are inclined to think that methods are now available for most, if not all the substances usually determined in iron and steel, which will enable results to be obtained the same day, if the sample is taken in hand in the morning. With some of the methods half a day suffices. We are also strongly of the opinion that these limits will be fairly satisfactory to those who are most required to use the results of analyses. In order to have something definite, and not beat the air in our discussion, we would suggest that no method be regarded as standard which will not give a result in one working day.

Closely connected with this point, is a third requirement of a standard method, *viz.*, it should be simple, or in other words, not be so laborious that one chemist can only turn out a very small amount of work in a day. Even though a result can be obtained in one day by a method. if it requires such close application and so much work on the part of the chemist that when night comes he has only the one analysis, obviously the cost of that analysis will be much higher than if a dozen or twenty results could be obtained in the same time. This is evidently a matter which must receive consideration. If the cost of an analysis is high, much fewer analyses of that kind will be made, than if the cost is more reasonable. It is perhaps not too much to say that this point has in the past seriously retarded the growth of analytical chemistry. But it is obviously difficult to say, where to draw the line in this matter of the output or capacity of one chemist, as affected by his method, since the magnitude of the transaction in which the results of the analysis are used clearly has an influence. It is perhaps sufficient to direct attention to it as having an important bearing on the possibilities of a method being regarded as standard.

A fourth requirement of a standard method is, it must be so well understood, and the control of the conditions affecting the result must be such that the results are uniform, that is, that three or four or half a dozen analyses made on the same sample by the same chemist, must agree so far as agreement is affected by the method. It is common experience that some methods are not as reliable in this respect as others. Sometimes good results are obtained with a method, and sometimes anomalous ones appear. This may be due to the fact that the method is not well worked out, that is, all the conditions affecting the result have not been sufficiently studied. It may be due again to the fact, that the control of the conditions is difficult to obtain. The reaction in a subsequent part of the operation, may be affected by some reagent, previously used, and which it is either difficult to remove completely, or to be sure that it is all removed. Furthermore, some methods have a still more radical defect, *viz.*, the reactions on which they are founded are apparently quantitative for certain conditions, in which the substance sought exists in the material being analyzed, but this is not true for all the conditions of the material. We will not take your time to cite examples of these various peculiarites of methods. It is obvious, however, that a standard method should not be defective in any of these respects.

Closely connected with this is a fifth and final requirement of a standard method, *viz.*, different chemists must be able to get concordant results, so far as the results are a function of the method. If the standard method is a reliable one, as has just been explained, the ability of different chemists to get concordant results with it, so far as these results depend on method, is evidently a question of description. Many methods suffer from indefiniteness of description. Too much is left to the judgment and experience of the chemist, and it seems reasonable to suppose that some of the differences between good chemists using the same method may be explained in this way. Obviously the description of a method is affected sometimes by the peculiarities which are to be described. We all know that some characteristics of a method are exceedingly difficult of description. For example, suppose a precipitate is perceptibly soluble in the wash water. It is clear that over washing is as much to be avoided as under washing. But how to define just the point at which the washing should stop, in many cases, is not easy. Again many methods are insufficiently described. The amounts of the different reagents to be used are not definitely stated, and precautions necessary to be taken are not clearly pointed out. Since, as has already been stated, no method can be a complete treatise on chemistry, it is reasonable to suppose that those who are describing any method are naturally in doubt how much to give and how much to withhold. It is clearly not easy to draw the line, and if standard methods for the analysis of iron and steel are ever adopted, it is probable that this will be one of the difficult points to cover.

But this paper is, we fear, occupying far too much time and space; and yet a few words, showing what has already been done in developing the proposed standard methods, are perhaps essential.

About a year ago in connection with our regular work for the corporation with which we are employed, it became necessary to prepare specifications for steel for various uses in which the limits on some of the constituents usually determined in this metal were pretty close, and, judging from our past experience, we foresaw that when we came to receive the material on these specifications, and to examine each shipment as is our custom, there would be difficulty due to discrepancy between ourselves and the chemists at the various works where the material was made. The various causes of discrepancy were gone over and the same conclusion reached that is expressed above. *viz.*, that the discrepancies arising from all causes except the method, could be successfully managed. How to meet this difficulty however, was a problem. The work of the Committee on International Standards was not yet complete, and no standard methods had yet been agreed upon. After considerable thought we decided to put the methods by which the various constituents should be determined in print, in minute detail, and then make these printed methods a part of the contract on which the material was bought. In pursuance of this plan, three methods have already been printed, *viz.*, "Phosphorus in Steel," "Carbon in Iron and Steel," and "Sulphur in Steel." It will be observed that the ground is as yet only partially covered. It should also be stated that very little experience has been had with any of the methods as yet. If this congress had come a year later, it would have been possible to say something about a part, at least, of the methods from actual experience with them in practical work. We are not rash enough to expect or even hope that the methods which we have tried to put into definite shape to enable us to meet an emergency which has arisen in our own work, will be accepted by the profession as standard methods for this important branch of analytical chemistry. Nor do we claim any special originality in connection with the methods. We have freely appropriated any work we could find anywhere that was applicable, have made a good many analyses to check up individual points, and have in a limited way, tested the methods in the form in which they are presented. It is our intention to cover in the way described above that part of the analysis of iron and steel which comes within the scope of our work. Such as they are, these methods are offered as a starting point from which to develop standard methods, with the sincere hope that this body will take some action, looking toward the accomplishment of this greatly to be desired end.