

are accurate enough for the purpose for which they were compiled, that is, to show the relatively small area covered. There is, however, another side to the picture, the hopeful one. In South America the arc of Peru is being remeasured and extended by the French government. As the work is being carried out with the advice of the most distinguished mathematicians of France, the results will be, in their importance, out of all proportion to the extent and area involved.

Mexico has made a brave beginning and is working towards a connection with an extension of the ninety-eighth meridian measurement, of which the United States has completed about three quarters of the amplitude lying in her own domains. Work on the Pacific coast arc has been resumed and it has nearly been completed from San Diego to the Columbia River.

Two years ago the Russians and Swedes jointly completed an arc in Spitzbergen between latitudes 76° and 81° . The European arcs are being extended eastward by Russia, and one must look forward to the ultimate connection between the Russian triangulation at Astrakhan or Orsk and the Indian triangulation, however improbable it may seem if looked at from a political view point.

In Africa the work of extending the South African arcs northward from the Cape towards Alexandria is well under way, and no doubt need be entertained that the British and Germans will carry it through.

A general review of this part of the field of geodesy shows that while some great geodetic measurements have been completed or are approaching completion, new ones are being undertaken under the fostering care of different governments.

Reasoning from the experience of the past, we may conclude that the solution of one problem in geodesy will disclose the

existence of another, and from the trend of the investigations of the present that other than purely mathematical and astronomical sciences will be advanced by the search for their solution.

That the progress of the branches of science to which this section of our association devotes itself was greatly affected by the problems of geodesy was pointed out by Humboldt in language which may fittingly conclude these remarks:

Except the investigations concerning the parallax of the fixed stars, which led to the discovery of aberration and nutation, the history of science presents no problem in which the object obtained—the knowledge of the mean compression of the earth and the certainty that its figure is not a regular one—is so far surpassed in importance by the incidental gain which, in the course of long and arduous pursuit, has accrued in the general cultivation and advancement of mathematical and astronomical knowledge.

O. H. TITTMANN.

U. S. COAST AND GEODETIC SURVEY.

*FUTURE DEVELOPMENTS IN PHYSICAL CHEMISTRY.**

It has been the custom of the retiring officers to discuss the development of some portion of that field of chemistry in which they were most interested. Since the president of the American Chemical Society will speak on physical chemistry to-morrow night, it has seemed to me that I might break with tradition and discuss the future of physical chemistry rather than its present or its past.

We have reached a critical stage in the development of the electrolytic dissociation theory. The work of Kahlenberg has shown that there are a number of facts which we did not anticipate and which we can not explain satisfactorily at the present time. The recent experiments of Noyes show that the dilution law does not hold for any strong electrolyte and that the

* Address of the vice-president and chairman of Section C—Chemistry, Philadelphia, 1904.

same empirical equation describes the behavior of binary and of ternary electrolytes. This last fact appears to be fatal to all explanations based on the assumption that electrostatic effects are the disturbing factors. While the mutual attraction or repulsion of two ions or of three ions may easily change the dissociation formula for a binary or a ternary electrolyte, it is very improbable that the changes will be such as to make two radically different formulas identical. Of course, the hypothesis of hydrated ions gives us some leeway but the outlook is not what it was five years ago. It is too soon yet to say whether we are merely to remodel the electrolytic dissociation theory or whether we are to replace it by something else. My own opinion is that reform is what is needed and not revolution. It is evident, however, that we have gone ahead too fast and that we must test more thoroughly the premises on which our conclusions are based. We know of one error. The proportionality between molecular weight and osmotic pressure holds only for the cases in which the heat of dilution is zero. This is stated clearly in van't Hoff's original deduction of the van't Hoff-Raoult formula $n/N = \log p/p_1$, but has been pretty generally overlooked. Since the heat of dilution is rarely zero in any actual case, our deductions as to the molecular weights of solutes are always somewhat in error. In the case of the metals of the alkalis and the alkaline earths dissolved in mercury, the lowering of the vapor-pressure due to the heat of dilution is practically equal to that due to the molecular weight, and we therefore have the surprising result that the apparent molecular weight is only about one half the atomic weight. One of the first things we have to do is to eliminate this source of error in all cases.

Another distressing feature in the quantitative physical chemistry of to-day is that

the field which it covers is daily growing less. A tenth-normal solution is now considered a concentrated one, and some people are so extreme as to maintain that we can not expect agreement between theory and experiment for anything except infinitely dilute solutions. To my mind a theory which holds only for infinite dilution is necessarily wrong. Here again one probable source of error is easy to find. The van't Hoff-Raoult formula is deduced on the explicit assumption that there is no specific attraction between solvent and solute. If this assumption is wrong, it is reasonable to suppose that the error thus introduced would become less as the concentration approaches zero. Under these circumstances the van't Hoff-Raoult formula might represent the facts at infinite dilution without being a true formulation. This is the case with another well-known and important formula. The Helmholtz and the Nernst equations for the electromotive force of concentration cells are identical for infinitely dilute solutions and for these only. The Nernst equation ignores the concentration of the undissociated salt, while the Helmholtz formulation does not. The two equations become identical at the moment when the concentration of the undissociated salt and the disturbing factor due to it become zero, that is, at infinite dilution. Since the Helmholtz formula applies to all concentrations, the Nernst formula is necessarily only approximately accurate. This has been recognized explicitly by Planck, though the point is often overlooked. It is quite conceivable that the shortcomings of the van't Hoff-Raoult formula may be due in part to theoretical inaccuracies and that we have laid too much stress on 'variations from the gas laws.'

If we introduce the conception of a specific affinity between solvent and solute in certain cases, notably those in which the

heat of dilution is marked, we combine all of what has stood the test with what is good in Kahlenberg's conceptions and I believe that we are nearly ready to take a long step forward. One point must be kept in mind, however. Raoult's experiments preceded his formula. Before we can hope to work out a satisfactory theory of concentrated solutions, we must have accurate measurements on concentrated solutions and at present we have practically none. We need experiments at constant temperature on the compositions of co-existent liquid and vapor phases for binary systems with one volatile component and with two volatile components. These measurements are not easy to make and that is one reason why they have not been made. We have measured boiling-points and freezing-points because they are easy to measure; but for a theory of concentrated solutions the value of such measurements is very small. This is because we are then measuring the combined effect of the change of the pressure with concentration and with temperature, whereas we ought to study the two separately. Further, if we are to express our results in volume concentrations we must give the volume concentrations of both components. It would be absurd to pass from dilute to syrupy solutions of sugar, for instance, and to treat the concentration of the water as constant. Personally, I believe that the theory of concentrated solutions is relatively simple and that the difficulties have been chiefly of our own making. My own experience with ternary mixtures confirms me in this view. In developing a theory of concentrated solutions we must also keep in mind the actual properties of the components, a thing which we have not done in the past. Thus the dissociation equation for liquid chloral hydrate can not be the same as that for liquid chloral alcoholate because chloral is miscible in all pro-

portions with alcohol and forms two liquid layers with water. This is a perfectly obvious fact, yet no reference to it is to be found in any text-book on physical chemistry.

In the last ten years the work of Roozeboom and others has brought the phase rule to the front as a basis of classification and as an instrument of research. The importance of the phase rule is going to increase very rapidly in the next decade. The study of alloys has really only just begun. Our knowledge of the carbon steels is still very incomplete and unsatisfactory. In fact, we know the constitution only of a very limited number of binary alloys. Nothing systematic is yet known about the chemical properties of alloys or about the conditions for electrolytic precipitation. The variation of the engineering properties, such as tensile strength, torsional resistance, ductility, etc., with varying concentration and varying heat treatment is a subject which can only be worked out satisfactorily with the phase rule as a guide. On the basis of what has been done it appears quite safe to predict that we do not yet know one half the possibilities of our structural metals.

Quite recently the constitution of Portland cement has been established and we owe this result to an application of the phase rule. It will not be long now before we get much clearer ideas on the causes of the strength of cements and of the plasticity of the clays. The time will soon come in our engineering schools when the subject known as 'Materials of Engineering' will have to be taught by the chemist rather than by the engineer.

The applications of the phase rule to petrography will be numerous and will come soon. It is evident that no rational classification of minerals can be possible until the constitution of the minerals has been determined. The situation in regard

to petrography is much the same to-day as it was in regard to alloys a few years ago and we may reasonably expect as satisfactory results from rocks as from metals. More and more people are experimenting with fused salts and the new geophysical laboratory at Washington is planning to study igneous rocks in the same thorough way that van't Hoff studied the Stassfurt deposits. The problem is a difficult one experimentally, but it can and will be solved.

The classification of electrochemistry under the phase rule is a problem of the immediate future. Some work has been done already, but it is confined to the discussion of the electromotive forces of certain reversible cells. What I mean is something vastly wider than this, the application of the phase rule to all electrolytic and electrothermal processes. Since electrochemistry is essentially chemistry, a classification which is of fundamental importance in chemistry must be equally necessary in electrochemistry.

The extension of the phase rule to organic chemistry is an achievement about which we like to dream, but the realization of it seems far off. To treat a large portion of organic chemistry as a system made up of carbon, hydrogen and oxygen will some day be possible; but at present we are balked by so-called 'passive resistances to change.' Theoretically methyl ether, $(\text{CH}_3)_2\text{O}$, and ethyl alcohol, $\text{C}_2\text{H}_5\text{OH}$, are two modifications of the substance $\text{C}_2\text{H}_6\text{O}$ and they should be mutually convertible. Practically they are not. Only one of the three dibrombenzenes can theoretically be the stable form. Actually, we can not convert any one of them directly into either of the other two.

In spite of all this there is really quite a mass of material waiting to be worked up. Reversible equilibrium between hydrogen and oxygen can be realized at all tempera-

tures. Reversible equilibrium between carbon, carbon monoxide and carbon dioxide is possible above 200° , while reversible equilibrium between carbon, methane, acetylene, ethane and hydrogen can be observed above 1200° without catalytic agents. Carbon monoxide and water react at 430° in presence of copper. Methane can be made from carbon monoxide and hydrogen at 250° in presence of nickel, while methyl alcohol can be changed to carbon monoxide and hydrogen by zinc dust. The decomposition of alcohols into aldehydes, or ketones, and hydrogen is reversible. Aldehydes can be changed into carbon monoxide and paraffines, though the reverse reaction has not been accomplished satisfactorily. Methylal and acetal are formed by a reversible reaction, while the ester formation has been studied for years. Formic acid decomposes into carbon monoxide and water when heated by itself, and into carbon dioxide and hydrogen when heated in presence of rhodium. Starting from carbon monoxide and caustic soda we can make sodium formate, carbonate and oxalate.

As yet only a few of these reactions have been studied with care and we do not know how many of them are reversible or what are the temperature limits. We do not even know whether colloidal metals act more effectively than the pulverulent metals, although it is very probable that they do. While we can not yet tell how far we may be able to go, it is clear that the attempt to apply the phase rule to organic chemistry opens up a most interesting field of research both as regards organic chemistry and as regards the theory of catalytic agents.

The usefulness of the phase rule in studying basic and double salts is being realized more and more by our friends the inorganic chemists. The recent work on the changing solubility of the hydroxides of

many of the metals calls attention to a possibility of error which must not be overlooked. In all cases of hydrolysis there is always a possibility that equilibrium may not be reached in weeks or months. The only safe way is to reach the equilibrium from both sides. In this way and only in this way do we get any clue to the magnitude of the error involved and it is only after we have done this that we are justified in assuming that a reaction is irreversible.

The application of the phase rule to the fractional crystallization of rare earths would certainly lead to marked improvements. There are few people who could separate potassium and sodium chlorides by fractional crystallization, getting out all of each salt entirely pure. Even fewer would be able to separate potassium sulphate and copper sulphate. In spite of this we start in cheerfully on the fractional crystallization of an unknown number of elements having unknown properties. The result of all this is that we reach a point where further separation is impossible and yet we do not know why. This state of things is really the fault of the physical chemist and not of the inorganic chemist. It is not to be expected that the inorganic chemist can start in off-hand and apply the phase rule to the study of basic and double salts or of rare earths. Before this can be done the physical chemist must work out the methods and must be prepared to give explicit working directions, possibly in the form of recipes.

It must also be clear to you that a study of the conditions of existence of compounds, atomic and molecular, is a prerequisite to any theory of valency,

In the past, reactions in organic chemistry have been studied by physical chemists chiefly as examples of reaction velocity. There are two other fields which will receive more attention in the near future, namely, yields and irreversible reactions.

The question of yields is in a very bad way. In Lassar-Cohn's admirable book on laboratory methods in organic chemistry there is an enormous amount of valuable material; but there is really very little in the way of theory. Although we know that a reversible reaction will run to an end if the concentration of one of the reacting substances be kept practically zero, surprisingly little use has been made of this principle. We know that certain reactions take place better in dilute solutions or at low temperatures or in certain solvents, but in most cases we can not tell why. In the pyridine method for introducing acetyl or benzoyl groups the pyridine is said to be effective because it is a weak base; but it is much more probable that it acts as a catalytic agent. We do not know how far the dehydrating action of certain reagents is simply a question of vapor-pressure or how far there is a specific effect due to the particular reagent. The action of sulphuric acid in the formation of ether is something more than a dehydrating effect, and the same is true of the effect of zinc chloride in the synthesis of ethyl chloride.

A single instance will be sufficient to show the state of confusion that exists. Anschütz's method of preparing certain esters was to saturate the solution with hydrochloric acid gas and to allow the solution to stand overnight. Fischer improved on this by adding less acid and by raising the temperature. He boiled for two hours and found that the hydrochloric acid concentration could be reduced to three per cent. without affecting the yield. There the matter is left and we are led to look upon a three per cent. concentration as having special merits, whereas this is undoubtedly merely a result of boiling for the arbitrary period of two hours. If Fischer had boiled for one hour only he would have had to use a stronger acid to

have reached equilibrium in the allotted time. If he had boiled three hours, the lowest permissible concentration of hydrochloric acid would undoubtedly have been less than three per cent. Anschütz, on the other hand, worked at ordinary temperature and his solutions consequently needed more acid and more time to approximate to equilibrium. All of this is really first principles and it is only one case out of many. If any one will try to classify and explain the results given in Lassar-Cohn's book, he will find himself provided with enough interesting research to last him the rest of his natural life.

The second field for research to which I have alluded is that of irreversible reactions. In inorganic chemistry there are as yet no well-authenticated cases where a reaction starts and then stops short of equilibrium. The results of Pélabon on hydrogen and selenium and of Hélier on hydrogen and oxygen have been disputed by Bodenstein and must for the present be considered as wrong. In organic chemistry we appear to have many such reactions, typical instances being the formation of nitro-benzene and the decomposition of aldehyde into methane and carbon monoxide. While it is possible that these and other reactions run to an end in infinite time, we have not infinite time at our disposal, and it may, therefore, prove profitable to find out whether and how the apparent end-point varies with varying initial conditions. This work is desirable now and will become necessary if we should ever revise our opinions as to the theoretical possibility of an irreversible equilibrium. By definition we can not determine the existence of an irreversible equilibrium by approaching the end-point from the two sides. It seems to me probable, however, that we can draw conclusions from the reaction velocity. If we are dealing with a case of a theoretically reversible reaction running practically

to an end, I can see no reason why the concentration of the decomposition products should have any effect on the reaction velocity, so long as we confine ourselves to gaseous systems. If, however, we are dealing with a theoretically irreversible reaction which does not run to an end, the reaction velocity would vary with the concentration of the decomposition products.

It should be noticed that it will not do to reason from the behavior of a system in presence of a catalytic agent to that of a system without a catalytic agent, since the catalytic agent may displace the equilibrium. Thus ethyl alcohol is decomposed by heated copper into aldehyde and hydrogen, while heated alumina changes it chiefly into ethylene and water. It was the study of organic solutes in organic solvents which led Raoult to the formulation of his law. It seems probable that a study of organic reactions may lead to an entirely new class of equilibria. If this happens it will throw much light on the preceding problem because it is very difficult to explain some of the peculiarities in regard to yields in organic chemistry so long as we are obliged to postulate reversible reactions only.

The theorem of Le Chatelier has been applied chiefly to heat and work effects, but this is by no means the extent of its usefulness. Wherever we get a reversible displacement of equilibrium by light, it must be possible to make use of this theorem. The change of color of the silver photochlorides is in accordance with the theorem; but there seems to be no reverse change in the dark. The simplest case with which to begin would appear to be the formation of ozone. There seems to be a contradiction here. Ozone is known to absorb ultra-violet light and yet it is believed to be formed by the action of ultra-violet light. Whether we are dealing with the same sets of rays in the two cases is a point that has not been settled. In fact, we do

not know definitely whether ozone is formed by the action of ultra-violet light in the absence of electrical waves, though this is a matter easily settled by experiment. We know that ozone gives out light on decomposing, but we do not know anything about the spectrum of this light. It is quite probable also that we must formulate the theorem of Le Chatelier more exactly than we have hitherto done before we can apply it successfully to the phenomena of light. An instance based on electrical phenomena will show what I mean. If a voltaic cell be short-circuited the chemical change will be such as to decrease the electromotive force of the cell. If we do not keep the cell at constant temperature the Joule heat will cause the temperature to rise and this may either raise or lower the electromotive force of the cell. We are then really considering two phenomena, the electrical and the heat effects. One may mask the other completely.

In one case, at any rate, we know that we can apply the theorem of Le Chatelier to light phenomena. Suppose we have a gas enclosed in a transparent adiabatic vessel and concentrate upon it light of a wave-length that is absorbed by the gas. The temperature of the gas will rise and equilibrium will be reached when the gas has changed so that it no longer absorbs light of that particular wave-length or when the gas emits light of the same wave-length and intensity as that which is acting upon it. This emission by a gas at some temperature of the light which it absorbs at the same temperature is Kirchhoff's law, which thus appears as a special case of what the chemists call the theorem of Le Chatelier. To be frank, I do not now see how we are to apply this theorem to the phenomenon of phosphorescence, and yet we are dealing with an absorption and an emission of light. I venture to suggest that it is to the application of the theorem

of Le Chatelier that we must look for a rational treatment of phosphorescence, fluorescence, chemiluminescence, etc., rather than to a theory of vibrating molecules. It will be time enough to discuss the application to radiations when we have solved the simpler problem of the theory of cold light.

A discussion of equilibrium relations would not be complete without some reference to the future of thermodynamics in chemistry. There are two radically distinct ways of considering the relation of thermodynamics to chemistry. One is to look upon thermodynamics as a mathematical shorthand. The aim of thermodynamics is then to present a consistent and formal treatment of the known energy relations. In this case thermodynamics deals with the past and not with the future; with the classification of knowledge and not with the discovery of new laws. This is the point of view of most mathematical chemists and it is because of this that we do not turn to the mathematical chemist for new ideas. There is another way of considering thermodynamics, namely, as an instrument of research. It is not too much to say that the mathematical chemist can work out in a few hours or days results which would take his less fortunate colleague months or even years to obtain. At present the race is to the tortoise and not to the hare; but I can not believe that this will always be so. Other things being equal, the man who can handle his thermodynamics will beat the man who can not; but in order to have that take place thermodynamics must be considered as an instrument of research and not as a branch of metaphysics. We must confess that the mathematical chemistry of the past decade has not done what it should have done and that there is no immediate prospect of any improvement. In the meantime we do not despair. There are great possibilities in

the application of mathematics to chemistry and some day they will be developed.

So far we have considered problems involving equilibrium only. When we begin to study the conditions which make a reaction possible and which govern its rate, we are brought face to face with our need for a satisfactory theory of catalytic agents. We know experimentally the catalytic action of many substances on many reactions, but we have not even the first suggestion of an adequate theory. This is a subject of more vital importance than may appear at first sight. I wish to call your attention to two very important matters which depend directly upon catalytic agents. The first is the chemistry of plants. We can make in the laboratory many of the substances which the plant makes. Some of them, such as alizarine and indigo, we can make more cheaply than the plant can, and of a higher degree of purity. As yet we can not make any of them in the way the plant does, and this gap in our knowledge will have to be filled by the physical chemist, as the problem apparently does not appeal to the organic chemist. The plant does not use reverse coolers or sealed tubes; it does not boil with sulphuric acid or fuse with caustic potash; it has not metallic sodium and chlorine gas as reagents. The reagents on which the plant can draw are air, water and a few mineral salts. As catalytic agents it has heat, light, difference of electrical potential, enzymes—and itself, namely, living protoplasm. From the work of Bredig and others we know that colloidal metals, the so-called inorganic ferments, can be substituted for enzymes in some cases. As we do not yet know our limitations, it is quite possible that we can substitute inorganic catalytic agents for the enzymes in all cases. If that proves to be true we can then duplicate everything except the plant itself, and we shall be ready to determine how closely we can duplicate

the reactions of the plant. The experiments of Sabatier and Senderens in France are distinctly encouraging, even though they do not carry us very far. By means of nickel powder it is possible to reduce acetaldehyde to alcohol with hydrogen at 30°. This is the best result that has been obtained and it indicates the possibilities. When we get a satisfactory theory of catalytic agents we shall undoubtedly be able to duplicate many of the plant syntheses and our failures will be interesting as bringing us nearer to the most difficult problem of all—that of life. Pending the development of a satisfactory theory of catalytic agents, there is much to be done in the way of experimenting. In view of the fact that mixtures of two catalytic agents often act more intensely than would be expected from the behavior of each taken singly, it would appear advisable to determine the combined effects of inorganic ferments and ultra-violet light.

The second problem, which would be easier of attack if we had a satisfactory theory of catalytic agents, is that of the transmutation of the elements. This is now admitted to be distinctly a scientific problem, though not one in which we have made much progress. It is usually assumed that it is a very difficult problem. While this may be true, we have not yet reached the point where we are justified in being certain of it. No one has ever attacked the problem systematically and all we can say is that the rate of change has been small under any conditions that we have yet realized. That is not surprising. We should naturally expect a low reaction velocity. The rate of change of radium is so slight that it could not be detected by any ordinary methods. The fact that we have never observed any transmutation of the elements does not prove that none has taken place. We had been making diamonds artificially for years, even for cen-

turies, but nobody thought of looking for them in cast iron until after Moissan made his experiments a few years ago.

If we accept Lockyer's conclusions as to the state of things in the sun, we could undoubtedly break up many of the elements if we could hold them long enough at 6000° C. One difficulty is to get the temperature, and of course we must be cautious about conclusions based on simplified spectra. Many people have thought that radium was to be the catalytic agent which was to change all the elements; but the recent work of Rutherford seems to put an end to this idea. If radio-active lead, tellurium and bismuth are merely these elements plus the radium emanation or one of its decomposition products, there is very little evidence to show that any of our well-established elements are undergoing any change from contact with radio-active substances.

Another possibility which has been suggested is that we could change our elements if we could pump energy into them and change their energy content. This would have to be done electrically if at all. I have been told that Stas was busy during the last years of his life trying to change sodium into something else by an electrical process. The difficulty is to pump energy into the element. Passing a heavy current through a metal produces no effect that we know of other than to raise the temperature. Taking the element in the state of gas enables us to employ a higher potential difference, but here the effectiveness of the method is limited by the appearance of the arc. The first stage in the problem would, therefore, be the attainment of the highest possible potential difference without causing arcing. In view of the remarkable insulating action of gases under high pressure, it seems as though the silent discharge through compressed gases were the thing to try. The difficul-

ties people had in proving the dissociation of water at high temperatures makes us realize the possibility that we might decompose our elements and never know it, owing to the recombination taking place at once. If we are to simplify our elements by pumping energy into them, it appears that we should work with gases under high pressure, with the highest potential difference compatible with the absence of sparking, and with some application of the principle of the hot-cold tube.

While the methods of extremely high temperature and of high electrical stress have much to commend them on paper, they are liable to fail owing to the difficulty of attaining the proper temperature or the proper electrical stress. The ideal method would be to find a catalytic agent which would accelerate the rate of change and which would eliminate what we should then call the instable elements. Since there is no immediate prospect of our being able to predict the suitable catalytic agent and the conditions under which it is to be used, we must ask ourselves what is the scientific method of attacking the problem of the transmutation of the elements.

The answer is a simple one. We must start with the simplest case, study that thoroughly, and work up gradually to the more difficult tasks. We should begin with the cases in which we know a change is possible and should study the allotropic forms of the elements. At present our knowledge of these is disgracefully incomplete. We know a little about sulphur, phosphorus, carbon, selenium and tin; but even for these few elements our knowledge is incomplete and it is especially unsatisfactory in matters bearing on the rate of change. In most cases the change from one allotropic form to the more stable one is fairly slow. It is not even easy to get large amounts of gray tin. On the other hand, Saunders discovered, quite by ac-

cident, that there were a number of substances, notably quinoline, which convert amorphous selenium into the more stable, black, metallic modification. It is probable that similar results could be obtained with other elements. Kastle has shown that the rate of change of yellow mercuric iodide into the red form varies enormously with the nature of the solvent. The first thing that we need is a systematic study of the allotropic forms of the elements, considering reaction velocity as well as equilibrium. We next take up cases where the change from one form to another can be made increasingly difficult. The three disubstituted benzene compounds, as I have already said, are to be considered as different modifications, only one of which can be stable as solid phase at any given temperature and under atmospheric pressure. According to the text-books *o*-phenol sulphonic acid changes readily into *p*-phenol sulphonic acid on heating. When bromine acts on phenol in the cold, *p*-bromphenol is formed, while *o*-bromphenol is formed when the reaction takes place at 180°. I have not been able to find any record of the *p*-brom compound changing into the *o*-brom compound on heating; but the experiment is worth trying. When we come to the three dibrombenzenes, we have a case where we know that the three forms are identical in composition and where there is certainly some sort of an equilibrium at the time of formation because the relative amounts of the modifications can be changed by varying the conditions of preparation. In spite of all this we know no way of converting two of these compounds directly into the third. We could undoubtedly do it if we could raise the temperature high enough, just as we could also convert the elements. It is as yet impossible to attain the temperature at which the elements change rapidly, while secondary reactions interfere in the case

of the organic compounds. So long as we can not change the two less stable forms of any disubstituted benzene compound into the most stable form, there is no reason why we should expect to succeed in what may, perhaps, be the impossible task of simplifying the elements.

Summing up, the future developments in physical chemistry will comprise a theory of concentrated solutions, further applications of the phase rule and of the theorem of Le Chatelier, a systematic study of organic chemistry, and a theory of catalysis.

WILDER D. BANCROFT.

REPORTS OF COMMITTEES.

THE following reports of committees were presented to the council. They were accepted and ordered printed:

On the International Congress of Americanists.

The International Congress of Americanists held its fourteenth biennial meeting in Stuttgart, Germany, August 18-23, 1904. On June 1, 1904, I received a communication from you announcing my appointment as the representative of the American Association for the Advancement of Science at this meeting. The designation was gladly accepted, as it had already been arranged that I should attend the congress on behalf of the Smithsonian Institution.

I now have the honor to report that the meeting was in every way a most gratifying success and that the representation of the American Association was duly recognized and published in the official bulletins of the congress. The attendance was largely German, but representatives from a dozen other countries were present and took an active part in the proceedings. The papers presented related almost exclusively to American history and anthropology and especially to South American subjects. The Germans as well as the French have given very great attention to investigations on that continent.

The next meeting of the congress is to be held at Quebec in August, 1906.

Very respectfully,

W. H. HOLMES.

On Anthropometry.

The committee beg to report that individually and as a committee they have been carrying on