scientific investigations, published by those societies during the year ending June 1, 1897, was received.

One of the most important functions of the Association is represented by the work of its committees. In addition to the three reports to the Council many very valuable reports were presented at Toronto before the sections, which will be noticed in special articles on the work of the sections to be published in subsequent issues of Science.

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

*LONG RANGE TEMPERATURE AND PRESSURE VARIABLES IN PHYSICS.**

METHODS OF PYROMETRY.

The endeavor to provide suitable apparatus for high temperature measurement is one of long standing. The student of the subject is fairly overwhelmed with the variety of devices which have been proposed. There are few phenomena in physics which have not in some way or other been impressed into pyrometric service, often indeed by methods of exquisite physical torture. I cannot, of course, even advert to many of these this afternoon, as my purpose will have to be restricted to such devices as have usefully survived. Thus a whole group of 'intrinsic thermoscopes,' as Lord Kelvin calls them — apparatus in which some property of the substance is singled out for measurement—will be overlooked. Pyrometry will some day receive substantial aid from the phenomena of solid thermal expansion, dear to the hearts of old Wedgewood, of Professor Daniells, of the citoyen Guyton-Morveau, and recently to Professors Nichols, Joly and others ; but even the 'meldometer,' which has received Ramsay's encouragement and recent heroic attempts to measure the expansion of platinum, have not yet entered the arena to stay.* The same may be said of vapor pressure, ebullition and certain dissociations, of which the former is entirely too liberal in dispensing pressure, and the latter too negligent in readjusting it. Little has been done with heat conduction regarded as subservient to the measurement of high temperatures ; little with color and the spectrum, even though Draper and Langley in this country and many others abroad have paid tribute; little with polarization. The wave-length of sound has told Cagniard Latour and our own A. M. Mayer much about high temperature, but it did not tell them enough.

Throughout the history of pyrometry, *fusion* seems to have come forward for journeyman duty. What is more convenient than to find whether the degree of red heat is too low or too high from the fusion of prepared alloys. As far back as 1828 Prinsep, aware of the golden opportunity, with his golden air thermometer determined the melting point of some equally precious alloys of gold, silver and platinum, and determined them very well. Other alloys were afterwards substituted and graded mixtures made of quartz, chalk, kaolin and feldspar for the purpose. Efforts to obtain more accurate values are due to Becquerel; but the absolute values most widely used until quite recently, namely, the melting points of silver (958°), gold (1035°), copper (1054°), palladium (1500°), platinum (1775°), iridium (1950°), are due to the researches of Violle.

Interest in high temperature fusions has of recent date rather increased than abated. The demand for more accurate data has been met by the Reichsanstalt, and we have now a set of values for silver, copper, gold, nickel, palladium and platinum in terms of

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* Address by Professor Carl Barus, Vice-President and Chairman of Section B (Physics).

* Noteworthy attempts to replace mercury by a liquid potassio-sodium alloy in glass thermometers are among the novelties.

the air thermometer standard of that institution. Data have also been supplied by Callendar. Among these values there is as yet considerable confusion and the end is not yet. Long ago I suspected that the Violle melting points were probably too low, whereas the assumed zinc boiling point is probably too high. This surmise has been partially borne out by the Reichsanstalt, though Le Chatelier even now prefers Violle's values.*

Thermoscopes based on a *specific heat* have an advantage over fusion thermoscopes in not being discontinuous. They are quite as 'intrinsic' and much less convenient in practice. Guyton-Morveau, at the beginning of the century, pointed out the pyrometer importance of specific heat, and a host of observers followed him. But the critical discussion of the subject is due to Pouillet (1836), who determined the thermal capacity of platinum between 0 and 1200° absolutely, and found a value so nearly constant as to place this method of pyrometry in a very favorable light. Other observers followed with new data, and the bulk of our knowledge to-day is again due to Violle. Violle used Deville and Troost's exhaustion air thermometer and determined the law of variations of specific heat and temperature throughout a large pyrometric interval, with a number of metals, silver, gold, copper, palladium, platinum, iridium, among them. It was by prolonging this law as far as fusion that the melting points of the metals, to which I have already alluded, were obtained. This verges on extrapolation, but it is not extrapolation gone mad.

The importance of calometric high temperature measurement has recently been accentuated in connection with the remarkable high temperature accomplishments of Moissan. Furnace temperatures in the case of such technological operations as are used in connection with iron, glass and porcelain manufacture rarely exceed 1,400°, except perhaps in the Bessemer process, where the temperatures are wont to exceed 1,600° and even reach 2,000°. In Moissan's furnace, which is essentially an electric arc enclosed by non-conducting lime, a totally new order of high temperatures is impressed. There was thus a call for at least an approximate measurement of their values, which was answered by Violle, assuming that the specific heat of carbon above 1,000° approaches a limit. The sufficiency of this hypothesis is not unchallenged, however; for instance, Le Chatelier finds that, up to 1,000°, the specific heat of carbon continually increases, having no certain limit. Admitting Violle's results, Moissan's furnace temperatures exceed 2,000° even at 30 amperes 55 volts; at 360 amperes and 70 volts tin and zinc oxides melt and boil; they exceed 3,000° at 500 amperes and 70 volts, where lime melts, and often boils. Moissan, however, went as far as 1,000 amperes at 50 volts.

The striking novelty of Moissan's work is rather of chemical interest, and a large part of it is so fresh in our memory that, in view of Moissan's forthcoming book,* I need merely glance at it. A range of fusibilities, among which platinum lies lowest, while chromium, molybdenum, uranium, tungsten, vanadium, follow in order, and of ebullitions beginning with silica and zinc oxide, is rather breath-taking. Finally, his structural investigations on the occurrence of carbon, and his long series of carbides, many of them commercially valuable, have staggered even the sensational press.

Leaving other intrinsic thermoscopes for the moment, I will ask your attention in

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* The following table contains a brief summary :

Ag.	(Violle) 954°	(Barus) 986°-985°	(Callendar) 982°	(H.&W.) 971°
Au.	1045°	1091°-1093°	1037°	1072°
Cu.	1054°	1096°-1097°		1082°
Ni.	——	1476°-1517°		1484°
Pd.	1500°	1585°-1643°		1587°
Pt.	1775°	1757°-1855°		1780°

* Le four électrique, par Henri Moissan ; Paris, Steinheil.

this place to the development of the only fruitful method of absolute pyrometry which has yet been devised. I refer, of course, to the gas thermometer, or, in other words, to the manometric methods of measuring the thermal expansion of gases. Efforts have, indeed, been made to use gaseous viscosity for absolute high temperature work. It has been definitely pointed out, inasmuch as viscosity in gases is independent of pressure, while both viscosity and volume increase with temperature, that the transpiration rates of gases through capillary tubes of platinum glazed externally would necessarily be an exceedingly sensitive criterion of the variation of high temperatures. The small volume of the transpiration pyrometer as compared with the clumsy fragile bulb and appurtenances of the air thermometer is further to the point. But modern kinetics has as yet failed to fathom the law of variation of viscosity with temperature, and even the researches of O. E. Meyer in this direction do not seem to have quite touched bottom. Gaseous transpiration pyrometry is thus still much in the air, surveying the horizon of a glorious future.

Returning from this digression to the air thermometer, we find the first thoroughgoing piece of high temperature work carried out by Prinsep (1829), by the aid of a reservoir of pure gold to which I have already alluded. Prinsep's manometer was filled with olive oil, and the volume issuing at constant pressures was found by the balance. In view of the pure olive oil, probably available in 1829, these experiments must have been comfortably appetizing, and I dare say Prinsep's good humor in the matter may have contributed to the remarkable excellence of his results. Prinsep's researches were not superseded until Pouillet, in 1836, published his paper on pyrometry. Pouillet constructed an air thermometer bulb of platinum and was thus able to reach the farthest pyrometric north of the day and long after. His results are many sided; they contain the first definite data in radiation pyrometry and in calometric pyrometry. His constant pressure manometer, afterwards further perfected by Regnault, is the best apparatus for the purpose to-day. Pouillet did not suspect, indeed he remained quite unaware of, the permeability of platinum to furnace gases; perhaps for this and other reasons he failed to detect the thermo-electric anomalies in the platinum-iron couple which he so carefully calibrated.

It was thus a great step in advance when Deville and Troost long after replaced platinum by glazed porcelain, availing themselves (1857–60) of Dumas' famous vapor density method for measuring temperature. Unfortunately for the rapid progress of pyrometry, Deville and Troost used iodine vapor in their bulbs, a heavy gas indeed, but a gas, as was afterwards found, whose low temperature molecule dissociates at higher temperatures. Thus they unwittingly committed an even greater error than Pouillet in gliding over permeable platinum, and their data for the boiling points of zinc and of cadmium were about 100° too high. In fact, these results were challenged not long after by Becquerel (1863), who had fallen heir to Pouillet's platinum air thermometer, had used it to calibrate a platinum-palladium thermocouple of his own, and had found data for the boiling points of zinc and cadmium upwards of 110° below those of Deville and Troost. I cannot here enter upon the discussion which thereafter arose between these active observers further than to state that in the course of it both parties frequently repeated their measurements (Becquerel even substituting a porcelain bulb for Pouillet's thermometer) without removing the decrepancy between their values.

Later researches have decided in favor

of Becquerel's results, and his original research, with its applications to fusion, to radiation, to thermo-electrics, etc., is one of the noteworthy accomplishments in the history of pyrometry. Nevertheless, it must not be forgotten that to the investigation of Deville and Troost our knowledge of the perviousness of iron, platinum and other metals to gases is due. We are also indebted to Deville for the great discovery of dissociation, the very phenomenon which he was here so loth to acknowledge. This is the case of a man stumbling in his own footprints. Victor Meyer was, I believe, the first to point out the probable dissociability of the iodine molecule, suggesting a fruitful subject of research which has since been extended to many other molecules.

In 1863 Deville and Troost began a new series of high temperature researches, the feature of which is the perfected form of porcelain bulb. This was a hollow sphere and long capillary stem adapted for use with Regnault's standard air thermometer. Great difficulties were encountered in the endeavor to glaze the bulbs within. They were finally overcome by making bulb and stem separately and then soldering them together with feldspar before the oxy-hydrogen blow pipe. Elaborate measurements on the thermal expansion of Bayeux porcelain accompany these researches which, undertaken together with M. Gosse of the Bayeux works, occupied them intermittently for about seven years. A full summary of their data did not appear, however, until 1880, when, together with a new vacuum method of high temperature air thermometry, they communicated the results of twenty-seven measurements on the boiling point of zinc. Their new results are in good accord with the data of Becquerel already cited and the more recent results of Violle and others for the same landmark in the region of high temperatures. Measurements between 0° and 1500° had thus reached a degree of precision of about 15° in 1000°.

The further development of pyrometry took a somewhat different direction. Regnault (1861) had already made use of a displacement method, in which the measuring gas is removed bodily into the measuring apparatus by an absorbable gas. But the method was independently revived by Professor Crafts, of the Boston Institute of Technology. These methods are not of especial excellence below 1500°; but above that temperature, when most solids tend to become viscous, their importance increases (as Crafts duly pointed out) in proportion to the rapidity with which the measuring operations can be completed. One or two minutes may suffice and different gases may be tested consecutively. It is in this way that Victor Meyer and his pupils, after demonstrating the dissociation of iodine and chlorine molecules, succeeded in penetrating quantitatively to very much less accessible heights of temperature. A particular desideratum was a rigid test as to the stability of the molecule of the standard measuring gases (oxygen, hydrogen, nitrogen). The results were favorable, inasmuch as for these and for many gases like CO_2, SO_2, HCl, Hg, etc., the expansions obtained were linear functions of each other.

In their final work, temperatures as high as 1700° were reached, the air thermometer for this purpose being tubular in form, consisting of very refractory fire clay with an interior and exterior lining of platinum and with two end tubulures of platinum for influx and efflux of gases. Among many results of great chemical interest their researches showed that metallic vapors, phosphorus, sulphur, etc., at high temperatures tend to pass into the monatomic or the diatomic molecular structure.

Sometime after (1887) a series of experiments furthering the line of research of

Deville and Troost were made, with a geologic aim in view, in the laboratory of the U. S. Geological Survey. Finally, porcelain air thermometry was taken up with great vigor by the Reichsanstalt. These results, due to Holborn and Wien, are now almost exclusively quoted and carry the stamp of the great institution from which they emanated. They have been wisely made commercially available by the deposition, with Heräus, in Hanau (Germany), of a platinum rhodium alloy definitely calibrated for a temperature range of 1400°.

Apart from this, these researches contain no essential novelty, except, perhaps, a more detailed attempt to investigate the stem error of the thermometer bulb; their procedure otherwise is identical with the method developed in this country. I am not, therefore, inclined to yield to it the unhesitating deference which has become customary. There can be no doubt, in view of the splendid facilities due to the cooperation of the Royal Prussian porcelain works—facilities which those who have been baffled by porcelain technology, or have had to coax unwilling manufacturers into reluctant compliance, will appreciate—that the data of the Reichsanstalt will eventually be standard. For the present, however, I should be more impressed by some sterling novelty either in the direction of a larger range of measurement or of method. Conceding that an accuracy of 5° at 1000° has been reached, all results above 1500° remain none the less subject to increasingly hazardous surmise.

A beautiful method of absolute thermometry, albeit as yet only partially developed, is due to Töpler. In this the densities of communicating columns of gas are compared very much as in Dulong and Arago's classical methods for liquids, by the gravitation pressures which correspond to these unequally hot columns. To accomplish such extremely fine pressure measurement, Töpler invented the 'Druck libelle,' an inversion, as it were, of the common level, in which, therefore, the motion of the bubble (or of a thread of liquid) indicates a change of pressure conditioned by the invariable horizontality of the instrument.

The development of the practical forms of continuous intrinsic thermoscopes (the *radiation*, the *thermo-electric* and the electric *resistance* methods) went, more or less, hand in hand with the development of the air-thermometer, although the latter is decidedly the more recent. Aside from pioneering experiments of Müller (1858) and others, the well-known Siemens resistance pyrometer (1871) was the first instrument in the field. It was based upon data obtained from platinum, copper and iron by the calometric method of calibration. This instrument has been remarkably perfected by Callendar and Griffiths, using specially pure platinum calibrated by comparison with the air-thermometer as far as about 600°. Notwithstanding these improvements, the resistance pyrometer is inferior, in my judgment, to the thermo-electric pyrometer, from the greater bulk and fragility of the exposed parts and the tendency of platinum to disgregate or waste itself gradually at high temperatures. Its upper limit of temperature measurement is thus limited; for, even if the difficulty of selecting suitable terminals for the coil is set aside, the difficulty of finding an insulator at very high temperatures would remain. According to Holborn and Wien resistance is seriously subject to the influence of furnace gases, and permanence of the low temperature constants does not imply a like permanence of the high temperature constants of the metal.

Radiation pyrometry, curiously enough, is the most venerable method within the whole scope of the subject. It was introduced by Newton (1701) in his *scala graduum caloris* in connection with his well-known law of cooling. Not to mention minor workers, it was successively attacked and revived in most of the noteworthy high temperature investigations. Pouillet and Draper have studied it; Becquerel, Crova, Violle, Le Chatelier, Langley, Nichols, Paschen and others have advanced it. It remains to-day the most promising as well as puzzlingly fascinating subject for pyrometric research. One needs merely advert to its broad scope in relation to the temperature of the heavenly bodies to acknowledge this. Here I can only allude to Becquerel's principle that the radiation of opaque bodies is spectrometrically alike at the same temperature, a result which has Crova's more recent assent; to Violle's photometric measurements of the total emission of platinum; to the more recent work in the same direction of Violle and Le Chatelier, in which consistent results were obtained for oxide of iron and platinum as far as 1500° to 1700°; to Stefan's law as proved by Boltzmann, and the variety of discussion it has elicited; to H. F. Weber's collateral equation; to the Johns Hopkins measurements, etc. Another school of observers, including Langley, Paschen and others, has undertaken the promising but much more laborious method of bolometric measurement of the distribution of spectrum energy in its relation to temperature. Without doubt, however, the whole subject is yet *in primis rudimentis;* the results are confessedly 'intrinsic.' Indeed, vagueness in the nature of the radiating source lowers with sufficiently threatening aspect to chill the fondest hopes. When one is told by Violle working on Mont Blanc that the temperature of the sun is 2500°; thereupon by Rossetti that it is 9965°, by Le Chatelier that it is at least 7600°, by Paschen that it is below 5000°, by Wilson and Gray that it is 8000°, etc., one wisely concludes that more may yet be learned about it. Our sympathies naturally go with those who, like Lummer and Wien and the Johns Hopkins people, are beginning fundamentally with the search for an absolutely black body. Less superstructure and more sub-cellar is, perhaps, the watchword in radiation pyrometry.

Turning, now, to the last and most important of the methods of practical pyrometry, we find a curiously meandering evolution apparent. I have already indicated that Pouillet (1836) was the first to complete a legitimate piece of calibration work. Pouillet might have condemned the method, but for some reason Tait's thermoelectric anomalies of red-hot iron were not detected. Regnault (1847), who was the next to take up the subject as it happened with the same couple, made his condemnation sweeping enough. It was not the real perversity of the platinum iron couple which provoked Regnault, for of this neither he nor Pouillet became aware. Regnault's objection (as we should put it to-day) lay in the fact that the thermocouple obeyed Ohm's law, which in that early day lay somewhat beyond the great physicist's range of interest. Fortunately, but none the less long after, Becquerel followed with his palladium and divers platinum couples, carefully calibrated and efficiently used. What these platinum couples were is not stated. They cannot have been very sensitive or they would have been preferred to the palladium-platinum couple. Indeed, the metallurgy of platinum alloys did not reach a degree of refinement until Deville and Debray (1875) overhauled the chemical separation of platinum metals with particular reference, both to iridium and to rhodium. Recently Mylius and Förster at the Reichsanstalt further contributed to platinum metallurgy. But in view of the toils in which the whole subject of high temperature measurement languished in Becquerel's day, his results were not sufficient to remove the discredit which Regnault had thrown upon thermo-electric pyrometry. And so it happened that the return to the method in recent date was of the nature of a resuscitation.

It is amusing to note, as we pass on, the pranks of custom as it bore down upon pyrometry. Following Deville and Troost, every worker (I might mention at least five) felt in duty bound to redetermine the boiling point of zinc—rather a difficult feat in its way. Thus we find boiling zinc inseparably associated with the destiny of the calibrated thermo-couple. Le Chatelier broke this law of fateful sequence by ignoring the need of calibration at the outset, and then using the couple so dignified to determine the melting points of silver, gold, palladium and platinum. But these are Violle's melting points. Hence the pyrometric feature of Le Chatelier's platinum-rhodium couple was in its inception due to Violle.

Meanwhile, accompanying the geologic inquiries of Clarence King, an extensive series of pyrometric investigations which had been in progress in this country since 1882 were completed (1887). These contained a full examination of divers efficient methods of pyrometry and a study of the porcelain air-thermometer with particular reference to the calibration of thermo-couples. In the course of this work the admirable pyrometric qualities of the platinum iridium alloy were exhibited by detailed and direct comparisons with the air thermometer. It was shown that the calibration could be made permanent by referring the thermo-electromotive forces to a Clark's cell; that the character of their variation with temperature is uniformly regular, and that the thermal sensitiveness of the couples increases as the higher degrees of red and white heat are approached. Finally, it was pointed out that couples destroyed by silicate corrosion or in similar ways could be restored by fusing over again on the lime hearth with merely negligible changes of constants. Elsewhere, Le Chatelier's clever combination of the platinum rhodium couple with the D'Arsonval galvanometer, then a comparatively new instrument in the laboratory, secured immediate favor. Professor Roberts-Austen, ever on the watch to waft good things across the channel from Gaul into Albion, hailed the new comer with no uncertain sound. Sometime after, the platinum-rhodium couple entered Germany and was there definitely calibrated (1892) for the first time, as already stated, at the Reichsanstalt.

Of the three available couples, palladium, platinum-rhodium and platinum-iridium, the former is excluded from competition by reason of its low fusibility. Between platinum-iridium and platinum-rhodium, the latter has been more extensively advertised, but is otherwise inferior to the older platinum-iridium alloy. In other words, platinum-iridium, when suitably alloyed, can be made more sensitive than platinum-rhodium in the ratio 100 to 76. Beyond this the alloys are much alike; both are tenacious, resilient, refractory metals, and their thermo-electric forces under like conditions of temperature show a constant ratio even at extreme white heats. The thermo-electric activity of these two alloys is exceedingly remarkable. Among over fifty different platinum alloys examined no similarly sensitive combinations were found. Moreover, whereas platinum alloys of extremely large electrical resistance are not unusual, such metals are not apt to be distinguished thermo-electrically.

To conclude: the small dimensions of the sensitive point of the thermo-couple, the independence of the intermediate temperatures between the junctions (apart from small corrections due to the Thomson effect) and therefore the removal of the terminal difficulty, the high upper limit of the measurable temperatures, the permanence of its constants in relation to Clark's cell in the lapse of time, the instantaneity of its indications, the easy reproduction of destroyed couples, their relative insensitiveness to furnace gases, the regular and simple character of the temperature function, the sustained sensitiveness throughout all temperature ranges even quite into the fusion of platinum—all these facts are a sufficient if not overwhelming recommendation of the method.

In speaking of long range temperature variables one is hardly permitted to overlook the remarkable work which has recently been done in the direction of low temperature, but with these subjects I am less familiar and can therefore only refer to in passing. The progress made in the subject is sufficiently evidenced by the growth of large low temperature laboratories throughout the world, laboratories which undertake 'the cold storage' of 'cold storage,' as it were, like those of Pictet in Berlin and Paris, of Dewar in London, of Kamerlingh Onnes in Leyden, of Olezewski in Krakau, and others. Dewar and Fleming have added to our knowledge of the probable constitution of bodies at the absolute temperature. Olezewski has found the critical temperature of hydrogen at —230° and its atmospheric boiling point at —243°. Dewar and Moissan have liquefied fluorine. There is much here which I must reluctantly forego. The hydrogen thermometer, the platinum balance (Callendar) and the thermo-couple are again doing excellent work in thermometry.

APPLICATIONS OF PYROMETRY.

Turning now to the applications of recent pyrometry, we meet first many series of valuable data on melting points and similarly valuable data on the dissociation temperatures of chemical compounds. To these I merely refer, not being qualified to enter into chemical interpretations. High temperature boiling points have also been treated, and I will especially consider the case of the variation of metallic ebullition with pressure. The relation of vapor pressure to temperature has thus far defied the counsels of the wise, even though such men as Bertrand and Dupré have given the matter close scrutiny. One would suspect the simplest relation to hold for metallic boiling points, and investigations have therefore been undertaken in which the temperature of ebullition of Hg, Cd, Zn, Bi, were studied for pressures decreasing from one atmosphere down indefinitely.

The results so obtained show an effect of pressure regularly more marked as the normal boiling point is higher, so that the attempt to express the phenomenon for all these bodies by a common equation is roughly successful. By far the most rapid reduction of boiling point occurs when the pressure decreases from $\frac{1}{10}$ atmosphere indefinitely. For the case in which the normal boiling point is to be predicted from a low pressure value in case of a metal which, like bismuth, boils with great difficulty, very high exhaustion is essential.

Igneous fusion, by which I mean the fusion of rock-forming magmas, is particularly interesting in its relation to pressure. This has been again recently pointed out by Clarence King in his discussion of the age of the earth. If the earth is solid within, as is now generally admitted, such solidity can only result from superincumbent pressure withholding fusion. To study the relation of melting point to pressure directly is out of the question when white heat is the condition of fusion. In this respect the laboratory in the interior of the sun or even of the planets has some salient advantages, but we cannot comfortably put such a laboratory under strict surveillance of protoplasm.

Fortunately, the Clapeyron equation, successively improved by James Thomson and by Clausius, is here usefully available. To measure the melting point, the difference of specific volumes of the solid and the liquid body and the latent heat of fusion at this temperature, with the aid of Joule's equivalent, is to measure also the relation of melting point to pressure implicitly. Based on the first and second laws of thermodynamics, this equation is generally true, no matter what specific properties may characterize the body. The process has thus far been completely pushed through for diabase only. Thermal change of volume may be measured by enclosing the rock in a platinum tube of known expansion, and the contraction of the contents from liquid to solid found by an electric micrometer probe reaching within the tube. Given a furnace fully under control, then, as experiment has shown, the cooling can be made to take place so slowly that platinum remains rigid relatively to its charge of red-hot magma, and under these conditions the contraction can actually be followed into the solid state. At the same time the temperature at which marked change of volume occurs is the melting point. Apart from difficulties of manipulation, the latent heat may be found from measurement of thermal capacity on either side of the temperature of fusion by a modification of known methods.

The rate at which fusion is retarded by pressure computed from these data in the manner specified showed an increase of the melting point of a silicate of about 0.025°C. per superincumbent atmosphere. But this datum falls within the margin (.02 . . . .04) of corresponding data much more easily and directly derived for organic bodies. One may, therefore, argue that if the melting point pressure rate is so nearly constant on passing from the class of silicious to the thoroughly different and much more compressible class of organic bodies the rate would probably be more nearly constant in the same body (silicious or organic), changed only as to temperature and pressure. This surmise was verified for naphthalene within an interval of 2,000 atmospheres.

The endeavor to interpret the change during fusion of the volume of the chemical elements in terms of the periodic system has been begun with much success by Max Töpler for low temperatures. It would be of great interest to complete this diagram for high temperatures in view of the specifically molecular character of the fusion phenomenon, by repeating such experiments as have just been described for rock magmas.

The heat conduction of rocks has been investigated in many cases for temperatures lying below red heat. Among recent observers we need only instance the extensive investigations of Ayrton and Perry. No trustworthy experiments, however, have yet been carried into the region of essentially high temperature; and yet what is chiefly of interest in the geological applications of such experiments is the change of conduction which accompanies changes of physical state, whether induced by pressure or by temperature.

Experiments in heat conduction are difficult under any circumstances. They become insuperably so when conduction at white heat is to be studied under pressure, and that is what the geological conditions actually imply. Some notion of a body respectively solid and liquid at a given temperature may be obtained by observing the behavior of bodies which are capable of being undercooled. A number of such bodies are known, thymol being a conspicuous example. Experiments with this body were made by measuring the volume expansion, specific heat and heat conduction in parallel series both for the solid and liquid state at like temperatures. They showed, for instance, that the increment of absolute heat conduction, encountered in passing isothermally from the solid to the liquid state, when referred to solid conductivity is about 15%, and when referred to a liquid conductivity is about 15%. Similarly the change of thermometric conductivity under like conditions is an increment of 36% and 56% respectively. Now, since in most questions relating to thermal flow thermometric conduction enters exclusively, the importance of this large coefficient is obvious whenever a body passes from the solid to the liquid state.

Solid conduction is thus 40 or 50% in excess of liquid conductivity for the same body at the same temperature and pressure. It is reasonable to infer that a corresponding decrement of conduction will accompany any rise of temperature of a solid body. Measurements which have somewhat recently been made for relatively small intervals at Zurich, at Glasgow and at Harvard, upon typical rocks, all bear out this surmise. The immediate incentive to these experiments was a strong paper by Professor Perry in which Lord Kelvin's estimate of the age of the earth is shown to be insufficient for an earth in which the interior conductivity is systematically greater than the surface conductivity. Indeed, he deduces the percentage increment of the square root of the age of a Perry earth over that of a Kelvin earth to be one-fifth of the percentage decrement of conduction for each 100° C. So far as the effect of terrestrial temperature alone is concerned, the measurements just mentioned show that Perry's correction is negative or that Perry's earth would be less long-lived than the 100 x 10^6 limit of years set by Lord Kelvin.*

To estimate the effect on heat conduction of the increase of pressure which accompanies the increase of temperature with the depth below the surface is a much more serious matter. In the laboratory, pressure experiments are limited to 3,000 or 4,000 atmospheres; compared with earth pressures these scarcely amount to a scratch on the surface; yet even for this limit the determination of heat conduction at high temperatures is out of the question. A tentative method of arriving at a conclusion is given by Clarence King in his discussion of the age of the earth, the consequences of which have been quite overlooked. What King endeavored to accentuate long before Perry's contribution to the subject was precisely the fact we *cannot* assume greater conductivity for the interior than for the surface. Since heat conduction decreases isothermally from solid to liquid, it is assumed that in one and the same substance the viscosity could be taken as an index of the thermal conduction. Therefore, if temperature and pressure were made to vary in such a way (both increasing) as to leave viscosity constant, it was inferred that heat conduction would also remain constant. Now the isometrics or lines of constant viscosity of a viscous body for variable pressure and temperature are much more easily found than the isometrics of conduction. In fact, it has been shown that a burden of at least 200 atmospheres would have to be brought to bear in order to wipe out the decreased viscosity due to the rise of a single degree (Centigrade) of temperature. The depth at which this ratio is reached, as King points out, for a given surface gradient of temperature and depth, depends on the initial excess of the temperature of the earth considered, and on the age of the temperature distribution resulting. But no matter whether the Kelvin earth with an initial excess of 3,900° and an age of 100 x 10^6 years, or whether King's solid earth with an initial temperature of fused platinum and 25 x 10^6 years of life be taken, in all cases the temperature effect predominates

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* The text of Kelvin's recent address at the Victoria Institute, in which an age of thirty million years is maintained, has not yet reached me.

throughout those depths within which change of temperature with depth is the marked feature of the temperature distribution. In other words, if, for example, we consider the Kelvin earth, the strata above .035 of earth radius will be strata of smaller conduction than the surface strata. From the surface downward as far as .035 radius, conduction will decrease to a minimum. Below this, conduction will increase again, due to preponderating pressure, finally to exceed the surface value. But the computed temperature distribution of Kelvin's earth is such that at .035 radius the initial temperature excess of 3,900° has been reached to within one or two per cent. Below this in depth Perry's correction will begin to apply, but the further changes of temperature are so nearly negligible that the consideration of conduction is superfluous. From this point of view, therefore, the staggering force of Perry's clever argument is removed. Of course, I am fully aware that an argument from the supposed parallelism of physical properties of a given body (in the present case heat conduction and viscosity) is not inevitably convincing; but in physical geology, for some time to come I dare say, the question will be not one of rigorous proof, but rather one of forming a rational opinion.

In passing I will indicate the importance of an increased knowledge of the isometrics of liquid and solid matter, relations which have thus far been found simpler in character than other thermo-dynamic properties, as I shall again point out in the course of the address.

I want finally to add a few words on the electro-chemistry of magmas. The physical chemistry of molten rock has already been somewhat extensively considered, but I am hardly competent to review it. In this country, Joseph Iddings and, more recently, George F. Becker have discussed the natural differentiation of magmas from different points of view. Here I will merely include certain pyrometric experiments on the electric conduction of fused glasses which seem to give promise of throwing light on the chemical constitution of complex silicates and to be suggestive in other ways. In such measurements, if the magma is made to pass from the solid to the liquid state, the observed electric conduction contains no evidence either of a melting point or of polymerism. The law of thermal variation is easily derived and it agrees closely with the corresponding behavior of a zinc sulphate solution, for instance, observed through a range of temperature. In a general way, electric resistance decreases in geometric progression when temperature increases in arithmetical progression. Considered relatively to the composition of the magmas, electric conduction shows a marked and regular increase with the degree of the acidity of the magmas. The less fusible acid magmas are better conductors than the basic magmas at the same temperature. Curiously enough, conduction thus runs in an opposite direction to fusibility. However viscous a magma may be, therefore, and however cogent the arguments, such as those launched by Becker against the differentiating importance of ordinary diffusion may prove, it is fair to conclude that a thorough change of chemical structure through *ionic* diffusion, whether directed by an electric field or otherwise, must be an easy possibility for a sufficiently hot but otherwise solid magma. The results point specifically to the desirability of repeating Hittorf's brilliant experiments on the migration of the ions for a silicious medium. This ought not to be difficult, seeing that such a menstrum need not even be liquid to be compatible with a high order of electric conduction.

Further consideration of the subject shows the probable passage of conduction through a maximum when acidity is continually increased. Finally quartz appears like an insulator in the same rôle as water in ordinary aqueous solutions. In all these cases I wish to keep in mind the results of Alexeëff and their recent repetition for metallic alloys, together with the interpretation of these results due to Masson. In a crust subject to variable magnetism, traversed by earth currents, sustained by semimetallic carbides of the Mendeleëff-Moissan type, containing piezo-electric and thermoelectric sources, who can say that electric fields are absent? Again, the character of the changes contemplated in Gibbs' famous 'phase rule,' as interpreted by Le Chatelier, would here be ionic rather than molecular.

A question of somewhat allied interest in the action of hot water under pressure on rock-forming silicates. Investigations of this kind have been described in the well-known and fascinating book of Daubrée. Daubrée's work, however, is qualitative in character, like that of many others in the same line, and the furtherance of the subject is to be looked for in the quantitative direction. Some time ago Becker suggested experiments on a huge mass of granulated rock under the action of steam at exceptionally constant temperature. But no thermal effect of the action of water could be detected. True, the boiling point of water is a temperature comparatively low for the purpose; yet similar experiments made with liquid water at over 200° under pressure were equally negative as to results. Experiments of this kind are not very conclusive. The insufficient sensitiveness of the measuring apparatus, the rate at which heat is carried off compared with the rate of generation and other obscure causes mar the results. The question may, however, be approached in a somewhat different way: If water is heated under pressure in glass tubes, the volume of water contained decreases as the square, whereas the chemically active area, *i. e.*, the inside surface of the tube, decreases as the first power of the diameter. Hence, in proportion as the tube is more capillary, the action of water on the glass will produce accentuated volume effects. Thus it was shown that the behavior of hot water is profoundly modified by its continued action on glass, inasmuch as its compressibility increases at a very rapid rate with the time of action even at 180°, until, with the approach of solidification, the observed compressibility is fully three times its isothermal value at the inception of the experiment. Even more striking is the simultaneous and continual decrease of the length of the column of water. Clearly, therefore, the confined volumes of glass and included water must undergo contraction at 180° in forming an eventually solid aqueous silicate, while increasing compressibility is due to the increasing quantity of silicate dissolved. Now, in nearly all cases the effect of solution is a decrease of compressibility. Hence the increased compressibility observed is to be referred to a precipitation of the dissolved silicate, in response to the action of pressure, a result borne out by the appearance of the tube and by varied correlative experiments. It is, however, the volume contraction which is particularly interesting, because of its far-reaching geological application. In the first place the measurements show that about .025 cubic cm. of liquid water is absorbed per square centimeter of glass surface at 180° C. per hour.* The effect of this absorption is a contraction of bulk amounting to 18% per hour. So large and rapid a contraction is presumably accompanied by the evolution of heat. Hence, under conditions given within the first five miles of the earth's crust, *i. e.*, if water at a temperature above 200° and under sufficient pressure to keep it liquid be so circumstanced that the heat produced

* This is an initial rate of about 180 kilograms per square meter per year.

cannot easily escape, the arrangement in question is virtually a furnace whose efficiency accelerates with rise of temperature or increase of terrestrial depth.

PIEZOMETRY.

It is not feasible to make much progress in pyrometry without feeling the need of a corresponding development in high pressure measurement. This has already appeared in the preceding parts of my address. It will not be expedient to look into the history of the subject as comprehensively as I did in the case of pyrometry, partly because the literature is more diffuse, and partly because the real development of piezometry is of recent date and virtually begins with pressures of the order of several thousand atmospheres. So understood, although we gladly pay homage to Oersted, to Regnault, to Grassi and many others, our historical summary may be abridged.

As is often the case in physics, the great advances in the subject are associated with the name of one man; for, though many able investigators have contributed effectively to the progress of piezometry, the overshadowing importance of the results of Amagat have superseded all researches coextensive with his own. For over twenty years Amagat has been laboring on this definitely circumscribed subject. Year after year his prolific experimental ingenuity has put forth results, each of which in its turn constituted the highest attainment in accuracy and the greatest breadth of scope which high pressure measurement had reached at the time. It is impossible to give any adequate view of this sustained labor in an address. The subject is highly specialized and demands special treatment; but we owe to Amagat the bulk of our knowledge of the properties of a gas regarded not as an ideal fluid, but as a physical body; some of the most far-reaching results in the thermo-dynamics of liquids, and some of the best data in the elastics of solids.

Amagat investigated gases within an interval of pressure which at times reached 4,000 atmospheres, with a view to interpreting their divergence from the laws of ideal gaseity. Indeed, we may note in passing that just as the advanced astronomy of the day is being enriched with unexpected discoveries from a discussion of mere errors of observation, so refined physical measurement gleans new harvests in carefully tracing out the all but rigorous sufficiency of established laws. The product of pressure and volume, nearly constant in the ordinary isothermal behavior of gas, shows, under higher pressures, a well-marked passage through a minimum in the case of all gases except hydrogen. Hence, below a certain definite pressure, varying with the character of the body (say 40 atm.), gases are more compressible than Boyle's law asserts, and above this pressure they are continually less compressible and begin to resemble hydrogen in this respect. The sharpness of the minimum diminishes as temperature increases, and probably ultimately vanishes. Cailletet, it is true, had undertaken a study of the same subject simultaneously, but his results were not in the same degree correct. Again, the coefficient of expansion of gases, considered in its isopiestic behavior for temperatures not too far above the critical point, increases with pressure to a maximum, which seems to occur at the same pressure for which the volume-pressure product is a minimum. This thermal maximum also decreases with temperature and finally vanishes. To specify the conditions further than this would be to exceed the limits beyond which verbal statement ceases to be lucid. The value of Amagat's work, however, is not merely the formulation of such general laws for gases as a whole, but rather the investigation of sharp and specific results for each gas individually. Thus, if one uses these data for a given gas to compute the contestants in Van der Waal's law, one is actually able to predict remote critical conditions of the gas in question with a fair order of accuracy.

Whenever pressure measurements are to be made through such large intervals as are here in question, the elastic constants of the apparatus become of increasing moment. Amagat, however, treated these incidental measurements as of like importance with the rest of his labors. The starting point of his investigation into high pressures was the open mercury manometer first erected along a staircase near Lyons, finally in the shaft of the St. Etienne mine, about 380 meters deep. This apparatus was used for graduating the closed manometer, preferably containing nitrogen. In later experiments for excessively high pressures the closed manometer was replaced by the 'manomètre à pistons libres,' a sort of inverted Bramah press, in which the small pressures of an open mercury manometer acting on a large piston compensate the relatively large pressures of the piezometer acting on a small piston. The ingenious feature of Amagat's apparatus is the rotation of both pistons just before measurement, a device by which friction is rendered harmless. Equipped with this instrument direct determination of the bulk modulus for glass and metal was actually feasible. In the case of glass no serious variation of the compressibility could be ascertained within 2,000 atmospheres, and even 200°, an observation of great value in practical research. Poisson's ratio was similarly determined and the data used in computing Young's modulus. But the most important result of these researches, a result to which Professor Tait also contributed, is the datum found for the absolute compressibility of mercury. This will enable all future observers in piezometry to standardize the apparatus with ease and nicety.

Time prevents me from dwelling upon the remaining investigations of Amagat in a measure commensurate with their value. These contain a counterpart for the liquid state of the results already announced for gases. The change of volume throughout enormous pressures and about 200° of temperature is considered in detail for a number of important liquids. Only in one case, and that the rather remarkable one of carbon tetrachloride, are evidences of solidification encountered and the conditions determined. Amagat believes the absence of solidification to be due to the occurrence of the lower critical temperature below the isothermal of compression. In my own judgment, however, the pressures necessary to reach this lower critical point will be enormous even in units of 1,000 atmospheres, for which reason it is not in any case liable to be an easy conquest.

Special mention finally is due to the thermal position of the maximum density of water, which Tait had already studied. Amagat shows definitely that the temperature of maximum density moves towards the freezing point with increasing pressure, so that at high pressures, as well as at high temperatures, the behavior of water loses its anomalous character. In general, compressibility and expansion decrease with pressure for all normal liquids, and expansion increases rapidly with temperature. Other anomalous properties of water have been investigated, among which the diminished viscosity of water under pressure at ordinary temperature studied by Röntgen, Cohen and others may be stated.

After this cursory and wholly inadequate mention of the work of Amagat and the physicists who, like Tait, Cailletet and others, have been engaged in closely allied researches, it will repay us to look at some of the other as yet less splendidly developed contributions to piezometry. At the outset it is well to make mention of the forms of pressure gauges which have come into use. As far as 1,000 atmospheres, the Bourdon gauge, if well constructed, does good service, though in a somewhat rough way. The corrected nitrogen closed manometer is more accurate for a smaller range. A theoretically simpler pressure gauge was proposed by Tait and Cailletet. In this case a straight cylindrical elastic tube under internal or external pressure is substituted for the Bourdon tube and the expansion or compression is directly measured. With due precautions against changes of temperature and the choice of a solid of constant bulk modulus and rigidity, this gauge can be used as far as about 2,000 atmospheres with convenience.

Above 2,000 atmospheres Amagat's Bramah press manometer, already mentioned, is the only reliable gauge, and, though somewhat cumbersome, has the advantage of giving absolute results. However, a gauge based on the change of electric resistance of mercury with pressure, a constant now fairly well known from Palmer's measurements, will, in my judgment, do good service for pressures which exceed even the range of the manometer. With regard to methods for producing high pressures, the force pump, with a small steel plunger and the screw advancing bodily into a closed barrel filled with a liquid, have not yet been superseded. The efficiency of such apparatus depends essentially on the means used for obviating leakage. These must, of course, be very perfect.

Amagat's work with liquids was extended chiefly in the direction of high pressures Experiments have since been made by others throughout higher temperatures (310°) and, of course, a smaller range of pressures (500 atm.). Leaving out the less perspicuous results, I will here merely allude to the probable existence of a remarkable law which these researches have developed. Dupré (1869) and afterwards Lévy (1878), reasoning from thermo-dynamic premises, were the first to suspect that the isometrics or lines of equal volume of liquids are straight. In other words, if there is to be no change of volume then pressure increments must vary proportionately to the temperature increments ($p = a\theta - b$), a result which implies that the internal pressure of a body kept at constant volume is proportional to its temperature. Lévy's deduction was, however, declared to be theoretically unwarrantable by Clausius, Boltzmann and others. Sometime after, the same law reappeared in experimental form in a series of brilliant researches on critical temperatures due to Ramsay and Young. Fitzgerald, reasoning from Ramsay and Young's results, then proved that for such liquids as possessed straight isometrics specific heat is a temperature function only, and energy and entropy are each expressible as the sum of a mere temperature function and a mere volume function. This is curiously like the position from which Dupré and Lévy started. Ramsay and Young's work, however, applied specifically to vapors, and for high temperatures (200°) their pressures did not exceed 60 atmospheres. The law has since been tested for liquids as far as 1500 atmospheres and over 200° conjointly, and found in reasonable accordance with experiment. Hence we infer that if the thermo-dynamic change of a body is such that volume remains constant, pressure and temperature will vary linearly with each other, the increments being about 0.1° C. per atmosphere. Now, although any law relating to the liquid state would be welcome, these volume isometrics are particularly so. In the geology of the earth's crust, for instance, they would, in a great measure, determine the conditions of possible convection; and it is curious to note that, from the known values of bulk modulus and of the expansion of solid glass, the isometrics would here again be given by corresponding increments of about .1° per atmosphere. For solid metals the isometrics are of a different order.

Another line of research for liquids to which I attach supreme importance has only just been begun: I refer to the systematic study of the *entropy* of liquids. Among the first results on the heat produced in suddenly compressing a liquid are those of Tait. They are of too limited a range, however, and not in good accord with the more recent and extended data of Galopin. Generally speaking, the change of temperature produced per atmosphere of compression increases with temperature in a marked degree, as one may infer from Kelvin's equation. For organic bodies this increment at ordinary temperatures is of the order of $\frac{1}{50}$° = .020° per atmosphere. In case of liquid metals the order of values is decidedly different, being about $\frac{1}{10}$ this value, recalling correspondingly divergent results observed for the isometrics of volume. Quite recently (1896), the same subject has been taken up by Tammann (to whom we also owe results for the correlative compressibility) particularly for solutions and with reference to the theory of solutions. Tammann's data are of the order .001° per atmosphere at 0°, and in better keeping with the thermo-dynamics of the subject than the earlier experiments. Much more, however, must be done before anything like a degree of critical accuracy is approached or a broad survey taken. Pressure intervals are to be chosen wider and the temperature measurement given with greater certainty.

Finally, I wish to touch upon the relations of melting point and pressure in their more recent development. Obviously, the classical work of Andrews on the continuous passage of a liquid into the gaseous state will find some counterpart in the manner in which the analogous passage from the solid into the liquid state takes place. The character of these phenomena may be shown from direct observations of melting point and pressure, as was done by the earlier observers. Full knowledge, however, can be obtained only by mapping out the isothermals throughout the region of fusion very similarly to the method pursued by Andrews himself for vaporization. This has thus far been attempted for a single body only, naphthalene, within 130° and 2,000 atmospheres. Six isotherms (63°, 83°, 90°, 100°, 117°, 130°) were traced within these intervals, along each of which, excepting the first, the body passed from the liquid to the solid state under the influence of pressure only. An exhibit of these data shows strikingly that in all cases the change of physical state takes place in accordance with a cyclic law, *i. e.*, a larger pressure is necessary to change the body from the liquid to the solid state, at a given temperature, than the pressure at which the body at the same temperature again spontaneously melts. Freezing almost always seems to take place at once; the corresponding fusion is apt to be prolonged, and in its gradual occurrence traces the contours of James Thomson's well-known, doubly-inflected isothermals much more fully than does the allied case of vaporization.

The appearance of the cyclic parts of these isothermals is suggestive, and may be described in terms of their dimensions in the direction of volume and of pressure respectively. The former dimensions indicate the probable fate of the volume increment. They show that, as pressure and temperature increase, the volume increment tends more and more fully to vanish, and they thus imply a lower critical temperature at which the solid would change into the liquid continuously as far as volume is concerned. It does not follow that other properties of the body would here also vary continuously. For naphthalene this point would lie in a region of several thousand atmospheres and several hundred degrees Centigrade—therefore, in a region too remote to admit of actual approach.

Again, the breadth of the cycles, measured along the pressure axis, decreases from the center of the field both in the direction of increasing and decreasing pressures. The tenor of these results is an additional indication of the recurrence of a lower critical temperature at which cycles must necessarily vanish. The decrease of the breadth of the cycles in the direction of decreasing pressure suggest the possible occurrence of a point in the region of negative external pressure, so circumstanced that beyond it the substance would solidify at a lower pressure than that at which it fuses. This may be interpreted as follows: The normal type of fusion changes continuously into the ice-type of fusion through a transitional type characterized by the absence of volume lag.

An independent discussion more searching in character has quite recently been given by Tammann. Tammann points out that for the normal case of fusion and for increasing pressure the two determinative factors of the Clapeyron equation—the volumes and latent heat of fusion—will not in general simultaneously become and remain zero. He argues that the volume constant will at the outset decrease with pressure passing through zero to negative values. Hence the curve representing the relation of melting point to pressure must initially rise to a maximum when the melting point pressure ratio is zero, and then decrease. Contemporaneously, the latent heat of fusion, decreasing continually with pressure, eventually also reaches zero, but at a much later stage than the volume constant. At this stage, therefore, since the melting point and the volume constant now have definite values (the latter negative), the melting point and pressure ratio is negatively infinite. Hence the curve expressing the relation of melting point to pressure decreases with increasing pressure from the maximum specified, as far as the pressure at which latent heat is zero, and there drops vertically downward. Thus Tammann's melting point pressure curve, with its initial and final ordinate in the direction of temperature, maps out a field of pressure and temperature within which the body is solid. Outside of this region the body is liquid and cannot by pressure alone be conceivably converted into the solid state. Any thermo-dynamic change involving a march through the boundary of this region is accompanied by the discontinuity of fusion, of viscosity, etc. A march through the final ordinate (for which latent heat is zero) is probably not accompanied by such discontinuity. For a given temperature there may be two fusion pressures. At a temperature sufficiently below the melting point the continued increase of pressure should, therefore, move the normally fusing body from the solid into the liquid state continuously. This is a somewhat anomalous result of close reasoning, but it must not be forgotten that in the depth of our ignorance of the actual occurrences above several thousand atmospheres the term anomaly is a misnomer. Indeed, if we regard the melting pressure curve beyond the stated maximum as characterizing the ice type of fusion (which Tammann does not do) our difficulties would in a measure be reconciled.

Tammann finally points out that the term lower critical temperature is not justified by the character of the phenomenon. Data for melting point and pressure due to Damien seem directly to corroborate the occurrence of zero values in the ratio of melting point and pressure increments, but Damien's tests are restricted to a pressure interval much too small to be trustworthy.

Of the two bodies which have been tested throughout long pressure intervals, naphthalene shows a linear melting point and pressure ratio for 2,000 atmospheres, while the carbon tetrachloride of Amagat, though initially concave upward, soon also becomes linear. Clarence King has, therefore, in geological considerations so represented it. To conform with Tammann's inferences the interior of the earth would have to be a fluid.

One point of issue, however, in these cases is clear: At Andrews' critical temperature both the difference of specific volumes and the latent heat of fusion vanish simultaneously wherever observed. Under corresponding conditions of change from liquid to solid the internal pressures are of tremendously greater value for both states, and the passage of the solid into the liquid molecule may involve an immense transfer of energy without any corresponding change of volume, for the density of the molecule itself eludes observation. The manner of this isothermal change from one state to the next is in all cases along the characteristic doubly-inflected contour first pointed out by Thomson for vapors, and since elaborated by Van der Waals, Clausius and others. We may, for brevity, call this a *volume lag* and measure it in terms of the pressure or the volume interval subtended. The liquid can exist even above the critical temperature, which would mean that even here pressure must be reduced below the critical pressure in order to rupture the liquid molecule.

Pronounced as these phenomena are for the change from gas to liquid, they become much more remarkable, indeed often formidable, for the change from liquid to solid. In this case a volume lag subtending more than 100 atmospheres is the rule. In other words, it takes much greater pressure to solidify a liquid at a given temperature than to liquefy the solid. Among all these cases there is a group of well-known bodies in which, while the solidification pressure is of marked intensity, the isothermal pressure of spontaneous fusion may even be below zero or be in the region of negative pressure. Take the single example of thymol among many: This body between zero Centigrade and its melting point at 53° can be kept in either the solid or the liquid state of pleasure. Given at about 50° in the liquid state it would require more than 2,000 atmospheres to solidify it. If solid it must obviously remain so even if pressure be wholly removed. But thymol may be similarly treated beginning with the undercooled liquid state at 28°, *i. e.*, 25° below its melting point. Even here at least one thousand atmospheres are needed to condense it (400 have been tried quite ineffectively). Once solid it would require about 1,000 atmospheres of *negative* external pressure again to melt it. In other words, it could not be melted again on the same isothermal.

If we but knew more about the physical constants involved in these transformations we could predict the results along the lines of J. W. Gibbs' splendid theory of the equilibrium of heterogeneous mixtures; but, with the dearth of our concrete knowledge of long range physical phenomena relating to liquids and solids, we must be content with humbler methods.

I have always regarded the significant behavior instanced for the case of thymol as capable of a broad interpretation. Professors J. J. Thomson and Fitzgerald abroad, and Elihu Thomson in this country, have recently sought for atomic dissociation in the electrolyzed vacuum of a Crookes' tube. Speaking to the same point, I would venture to assert that we may reasonably look to the volume lag for a rational account of the genesis of atoms. We have already met with two orders of volume lag: the first at the mergence of gas into liquid being usually a few atmospheres in isothermal value; the second at the mergence of liquid into solid, a hundred or even one thousand times as large in isothermal value, and characterized by the fact that, whereas freezing pressures may be enormous, the corresponding isothermal melting pressure may even be markedly negative.

If, then, we further inquire as to what will happen if we indefinitely compress the solid along a suitable isothermal, I think it is logically presumable that, with the succeeding and profoundly accentuated volume lag, we shall reach the next atom in a scale of increasing atomic weights.

However enormous the condensation pressure for this purpose may be, it is supposable, in the light of the examples already given, that along an accessible isothermal the disintegrating external pressure of the new atom may be permanently negative. Hence the new atom will persist within the pressure and temperature range available in the laboratory.

But the last stage is virtually identical with the first, or the inherent nature of these changes is periodic. The inference is therefore that, under suitable thermal conditions and continually increasing pressure, the evolution of atoms, of molecules, of changes of physical state, again of atoms and so on indefinitely, are successive stages of periodically recurring volume lag.

CARL BARUS.

BROWN UNIVERSITY.

THE AMERICAN MATHEMATICAL SOCIETY.

THE American Mathematical Society held its fourth annual meeting at Toronto, Canada, on Monday and Tuesday, August 16th and 17th. All conditions were exceptionally favorable for the success of the meeting, but the result far exceeded all anticipations. Forty-four members of the Society were registered as in attendance, forming by far the largest body of mathematicians ever brought together on this continent. Among those present were Professor Simon Newcomb, President of the Society, and former Presidents Dr. G. W. Hill and Dr. Emory McClintock. Several distinguished visitors also attended the meeting, among them Professors A. R. Forsyth, A. G. Greenhill and O. Henrici. Twenty-three papers were presented, all by members of the Society. Although the great length of the program necessitated a severe limitation of time for the reading of each paper, the hearty cooperation of the authors with the plans of the committee enabled the Society to conclude the sessions in the two days allowed without the least friction. The meeting was characterized not only by great scientific interest, but also by a cordial spirit of good feeling between hosts and guests.

At the meeting of the Council on Monday evening three persons were elected to membership in the Society and three nominations were received. It was decided to hold the next summer meeting of the Society at Boston, Mass., at or about the time of the meeting of the American Association for the Advancement of Science. Resolutions were adopted by the Society in recognition of the generous hospitality of the University of Toronto and its officers.

A portion of the afternoon session on Tuesday was devoted to a general discussion of the topics mentioned below. The members of the Society were afterward received by President and Mrs. Loudon, of the University of Toronto. On Tuesday evening the members of the Council were entertained at the Toronto Club by Professor Alfred Baker.

The following is a complete list of the papers presented:

MONDAY MORNING.

1. *Upon the representation by ruled surfaces of the curves drawn by mechanisms. Preliminary report, illustrated by models:* DR. EDWIN M. BLAKE, Purdue University.