The constitution of our fathers provided for representation in congress and in the electoral college according to population. This has led to vast results. A magnificent world of data is now spread before us by the census. Every man, woman, and child, and their interests, enter into it, and it has its lesson for each in all their various capacities and relations; but not more than a hundred thousand can possess it, and few can master the whole of it. It would be too much to come annually, and therefore cannot be frequent enough to meet every condition. Many statements should be annual. Our system of government affords an excellent opportunity to perfect a system of statistics parallel to the decennial census, and fitted to meet all demands.

Publicists have said much of the importance of the town-meeting as found in New England. An important characteristic of it is the bringing of all questions of public taxation and expenditure and policy to the consideration of all the citizens. This attention of all the citizens to the details of municipal action in large cities is impossible: therefore there are public reports and manifold statements. But should the town system of reports be everywhere adopted, and these be followed by county and state summaries, the nation could group these so as to give a variety of form and result sufficient for each according to his interest. The student and statesman would find them falling into appropriate classes, of sufficient frequency, and in connection with our decennial census of the nation would discover us in the very front rank with respect to knowledge of ourselves as a people. This is now done measurably for the subject of education. Each institution publishes its report or catalogue, most towns and cities their reports; many states gather up the data; and the national bureau, carefully avoiding improper complications, and solely for the purposes of information, issues an annual volume. The result is unique in the history of voluntary statistics. Were this system carried into every other great field, and the whole distilled into a single volume, and should each nation do the same, we should see the beginning of a solid foundation for internationalism, and the scientific method at last pervading the world of thought. It would determine the most far-reaching generalizations, and have an effect upon common life not now possible. Childhood would be ushered into new conditions, and alike the humblest and the highest would more easily find the truth.

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

PROCEEDINGS OF THE MATHEMATICAL AND PHYSICAL SCIENCE SECTION.

THE session of the British association in Montreal might be fairly designated as a 'section A' meeting, in view of the leading position in British science occupied by the representatives of that section, and the prominence which was accorded them and their section in the general meetings of the association. The retiring president of the association, who was to have been present, but was not, was a distinguished member of the section. His few duties were gracefully performed by another distinguished member of the section, Sir William Thomson, who also presided over the sittings of the section during the meeting. As representing the retiring president he introduced his successor, the president for 1884, in the person of Lord Rayleigh, another of the 'strong' men of section A. Two of the three evening lectures were given by members of the section, on subjects connected with physics and astronomy.

When it is remarked that the place of meeting offers no especial attractions to students of mathematical and physical science, it will be admitted that the roll of the section presented an unusual array of great names, including as it did such as Sir William Thomson, Lord Rayleigh, J. C. Adams, J. W. L. Glaisher, Henrici, Dewar, Preece, James Glaisher, Lodge, Rev. S. J. Perry, Osborne Reynolds, and many others.

As might be easily inferred from a glance at the above list, a large majority of the papers presented had to do with physics rather than with astronomy or pure mathematics. By a judicious action of the sectional committee, and one worthy of imitation, the papers were very fairly 'bunched' by subjects so that one was not required to remain during the entire week in order to listen to the treatment of a particular topic.

The first notable physical paper to be presented was, of course, the address of Lord Rayleigh as president of the association.

This address has already been placed before the readers of this journal, and no extended reference to it will be necessary. Although historical in the main, it was rich in valuable and timely suggestions such as could come only from one as thoroughly familiar with the topics referred to as its author. As a sample of these, may be quoted the remarks concerning the theory of the action of the telephone, which was declared to be "still in some respects obscure, as is shown by the comparative failure of the many attempts to improve it;" and in considering some of the explanations that have been offered, Lord Rayleigh said, "We do well to remember that molecular changes in solid masses are inaudible in themselves, and can only be manifested to our ears by the generation of a to-and-fro motion of the external surface extending over a considerable area. If the surface of a solid remains undisturbed, our ears can tell us nothing of what goes on in the interior."

The address of Sir William Thomson as presiding officer of section A must be carefully read and studied to be appreciated. One or two of the 'steps towards a kinetic theory of matter' may be usefully referred to. The as yet unsurmounted difficulty in the kinetic theory of gases is the explanation of what actually takes place during a molecular collision. It need hardly be said that physicists will not be satisfied until more or less of the obscurity surrounding this subject is dissipated. The mutual action at the moment of collision has been generally assumed to be repulsive by all who have written of or contributed to the kinetic theory. Sir William Thomson asks, May it not, after all, be attractive? Under certain conditions it seems that the appearance of repulsion may be the result of attraction. In general, two molecules approaching each other with a high velocity, assumed to be due to their attraction for each other, will approach obliquely, as the chances of a square 'hit' will be exceedingly small; they will then dash past each other in sharply concave curves around their centre of gravity, and fly asunder again, something, indeed, after the fashion of a comet passing around the sun. "A careless onlooker," says Sir William Thomson, "might imagine they had repelled one another." The idea that this mutual action might be attractive rather than repulsive had been in his mind for thirty-five years, but up to the preparation of this address he had never made any thing of it.

But, after all, the molecules must be infinitely small in order that they may *never* come in actual contact, so that we cannot evade the consideration of the effects of these real impacts when they do occur. Concerning these impacts, but two views seem to be open to us; the one is to imagine the molecule to be a little elastic solid; the other, to conceive it to be a 'configuration of motion in a continuous all-pervading liquid.' It is hardly necessary to say, that, in the opinion of Sir William Thomson, the latter must be the final hypothesis upon which we may rest.

But as a convenient intermediate station he suggested the conception of an elastic molecule, out of which we might not only construct a model of a gas, but, with some satisfaction, by linking these molecules together we might explain the elasticity of a solid. In a paper presented to the Royal Society of Edinburgh in March, 1883, of which the title only had been published, he had shown how an elastic system may be constructed, composed entirely of suitably disposed masses in motion. A system of four gyrostatic masses connected together by links was shown to possess all of the properties of an ordinary elastic spring, although composed of matter in itself entirely devoid of elasticity. By properly linking great numbers of such gyrostatic systems together, a model of an elastic solid results. Such a hypothetical solid lends itself easily to the explanation of such effects as the rotation of the plane of vibration of a wave transmitted through it, as in Faraday's celebrated experiment of the rotation of the plane of polarization of a ray of light in a magnetic field.

Sir William Thomson considered further the possibility of discarding entirely the postulate of rigidity in the materials under consideration, and showed how a hydrokinetic model of matter might be constructed in which all the effects of 'action at a distance' might take place. By means of this the model of a perfect gas might be produced, in which, however, there still exists the difficulty of explaining the case of actual impact of the particles. Some ingenious suggestions were made in the way of surmounting this difficulty; and the whole address was enriched by the most delightful digressions on the part of the author, during which the manuscript was neglected, and the section was afforded the pleasure of following, as best it could, the great physicist in his involuntary excursions into this most interesting but little-explored domain of physical science.

Lord Rayleigh, in his presidential address, had referred at some length to recent investigations concerning the theory of lubricants; and the section was therefore in a favorable mood to listen to the first regular paper on the programme, which was a theoretical consideration of that subject by Professor Osborne Reynolds.

The hitherto unrecognized results obtained by Mr. Tower in his experiments were referred to, Mr. Reynolds undertaking to show that they were in strict accordance with our knowledge of the laws of motion of viscous fluids. Mr. Tower had found, that when the rotating journal with its box was immersed in a bath of the lubricant, the resistance was not more than one-tenth of its value in ordinary oiling, and that the journal was less likely to heat at higher than at lower speeds. By boring a hole through the top of the box, it was found that the oil was forced through with considerable velocity; and on attaching a pressure gauge, as high as two hundred pounds per square inch was indicated. The oil appeared to be carried up by the motion of the journal, and to form a film upon which the box rested. Mr. Reynolds showed that there would necessarily result a difference of pressure on the two sides of the vertical line through the centre of gravity in the thin space between the box and journal; the maximum being on one side or the other, according as the rotation is one way or the other. Mr. Tower had found, that if, after running the journal for some time in one direction, a reversal were made, great heating would result. Owing to the difference of pressure above referred to, it was to be expected that this would occur; as, undoubtedly, the box and journal became adapted to each other for a certain direction of running, and when a reversal was made some time would elapse before a re-adaptation would be completed. This would explain why a new journal and box would *always* heat on first being run, however perfect they may be. Mr. Tower had likened the operation to a stroking of the fibres of the metal in one way by one direction of revolution, and the reverse stroking at the early part of a reversed motion; but this was not the true explanation, as the resistance was evidently a shearing resistance, the sliding of one layer of oil over the other. Sir William Thomson, in commenting upon the paper, called attention to the fact that one solid cannot slide over another without *tearing*.

Professor Reynolds also presented an interesting paper on a method of illustrating the second law of thermo-dynamics by means of kinetic elasticity. If a long flexible cord or chain be suspended with a weight at the lower end, the weight may be lifted a considerable distance by communicating a vibratory motion in a horizontal plane to the upper end of the chain. It then represents an absolutely reversible engine. If the weight, when at an elevated point, be removed from the chain, to straighten the chain out will require as great an expenditure of energy, not counting dissipation through friction, etc., as was consumed in raising the weight. It was shown that in this model the mean square of the velocity of the chain, multiplied by the weight per unit of length, corresponds to the heat in Carnot's engine. Another form of the device consisted of two vertical cords to which a number of horizontal bars of wood were attached at equal distances. In discussing the paper, Professor Fitzgerald described a very pretty illustration of the same principle by means of a 'balanced governor,' with a chain and weight attached in such a way as to be in equilibrium in all positions, the details of which are difficult to describe without the aid of a diagram.

The subject of the relative vapor tensions of a body in the liquid and solid state at the same temperature was discussed in a paper by Professor Ramsay and Mr. Sydney Young.

Professor James Thomson long ago pointed out that there must be a sudden change in the curve of vapor density of water at the point of solidification; and showed that this change was really to be detected in Regnault's results, but that Regnault himself had not thought such a break to occur, and had 'smoothed' his curve at this point; believing errors of observation to be sufficient to cover the discrepancies.

Messrs. Ramsay and Young, by means of ingenious devices, had overcome some of the difficulties of the experimental investigation, and had experimented upon camphor, benzine, water, and several other substances. The results were in accordance with the previously accepted views, and in the case of water were found to agree with those based upon Professor James Thomson's formula.

Radiation was the subject of two or three papers. Professor Dewar offered the methods and results of an investigation of the law of total radiation at high temperatures. The plan and arrangement of the apparatus for the research were ingenious and effective; and Professor Dewar stated that he had just learned from Professor Newcomb, that he had some time before devised and described an arrangement for the same purpose, identical in principle with that made use of in his own work.

For relatively low temperatures, Professor Dewar made use of an iron vessel containing mercury, into which a thermometer-bulb was pushed. The radiation measured was that from one side of the vessel, which was made of exceedingly thin iron; and the heat was received upon the face of a thermopile enclosed in a case properly screened, and arranged so that a steady current of water would be used to maintain constancy of temperature at one face. For these lower temperatures, the equation expressing the amount of radiation was of the ordinary parabolic form, the radiation being nearly proportional to the square of the temperature. The difficulty in dealing with high temperature is, that most substances undergo an alteration in the character of the surface when the temperature is very much raised. The arrangement finally adopted consisted essentially of a platinum air thermometer, the bulb of which was enclosed in a small furnace with a small opening through which the radiation took place. The walls of the platinum bulb were very thick, nearly a quarter of an inch in the actual experiment, and the bulb was connected with a mercury manometer for determining the pressure. Experiments were also made to determine the radiation when the thermopile was protected by an iodine screen. The results were as follows, the numbers being in arbitrary units: —

Radiation at 600° .	. 15.5,	screen used,	8.
" " 700° .	. 19.5,	" "	12.2
" " 800° .	. 29.0,	" "	19.5
" " 900° .	. 42.5,	" "	29.9
" "1000° .	. 60.5,	" "	44.0
" "1100° .	. 84.5,	" "	66.0

The assumption was made, that the radiation was represented by some power of the temperature; and this power was found in the first case to be 3.4, and in the second 3.3, thus showing a tendency to approach the fourth power; and attention was called to the fact that many of the results of Dulong and Petit were well represented by the equation $R=at^4$.

Mr. J. T. Bottomly offered a paper on the loss of heat by radiation and convection as affected by the dimensions of the cooling body, and on cooling in a vacuum; which, on account of the absence of the author, was read by Sir William Thomson. The paper was based on an extensive series of resistance measurements of copper wires under various conditions, accepting the well-known coefficient of increase of resistance of copper for increase of temperature. The conclusion was reached, that the emission power was greater for small wires than for large ones, and that it diminished with the pressure.

It would be almost impossible to give a perfectly clear idea of Sir William Thomson's paper on a gyrostatic working-model of the magnetic compass, without quoting the paper entire. What he proposes to accomplish may, however, be readily understood. At the last meeting of the association, at Southport, he had explained several methods for overcoming the difficulties which seemed to have defeated all previous attempts to realize Foucault's "beautiful idea of discovering with perfect definiteness the earth's rotational motion by means of the gyroscope." He had there shown that a gyrostat supported, without friction, on a fixed vertical axis, with the axis of the fly-wheel approximately horizontal, will behave exactly as does a 'magnetic compass,' only with reference to the true or rotational north rather than the magnetic north. A method was there presented for so mounting a gyrostat about such vertical axis as to reduce the friction to a minimum. The present paper was concerned principally in the presentation and discussion of another and simpler plan for realizing the same idea. The plan consisted essentially in suspending a gyrostat, properly constructed, by means of a very long and very fine steel wire attached to a torsion-head, capable of being turned about a vertical axis, at the top. The gyrostat being suspended, by successive adjustments of the position of the torsion-head, a position is found in which the position of the gyrostat, in relation to the torsion-head, shows that the wire is free from torsion. In this position the axis of the gyrostat will be in the true north-and-south line; and, if disturbed from this position, it will vibrate about it precisely as does an ordinary magnetic needle about the magnetic meridian.

The author pointed out several difficulties in the way of the complete realization of the idea, and closed by suggesting some possible methods of mounting, in a simple way, a gyrostat free to move about an axis rigorously or very approximately vertical. Regardless of any practical results which may come from it, the suggestion of a gyrostatic compass is singularly interesting as an example of how *motion* may effectively take the place of a directing *force*, although only one of the many which Sir William Thomson has furnished.

As was naturally to be expected, topics bearing upon the subject of electricity occupied a good share of the time of the section. Unfortunately one or two papers bearing upon this subject had been assigned to the chemical section, and were presented contemporaneously with the electrical discussion in section A. The paper by Professor Frankland, on the chemical aspects of the storage of power, was one which many members of section A would have been delighted to hear. While it was being read, however, section A was engaged in an extremely interesting discussion of the question of the seat of the electromotive forces in the voltaic cell, which was opened by Professor Lodge. For the first time in the history of the association, the experiment was attempted, of assigning a definite topic for general discussion; and the success was such as doubtless to lead to a permanent establishment of the custom. The selection of Professor Lodge to open the discussion was extremely fortunate. He is not only a ready and clear expounder of his own views, but he was fortunate, as a leader in the discussion, in that those views were not those which are generally accepted as being orthodox. His opening paper was largely historical; in fact, too largely so in the opinion of many of his hearers. He traced the history of the discussion from the time of Volta, declaring that the only really great contributions to our knowledge of the subject were those of Volta in 1801 and of Sir William Thomson in 1851. Of late years the so-called contact theory had been generally accepted. This theory, as generally understood, Professor Lodge could no longer accept. He did not believe that two metals in air or water or dilute acid, but not in contact, are practically at the same potential; or that two metals in contact are at seriously different potentials, or that the contact force between a metal and a dialectric, or between a metal and an electrolyte, is small. He did believe that by far the greatest part of the electromotive force of a voltaic cell exists at the zinc and liquid contact rather than at the zinc and copper contact, as generally supposed, although he believed that there was an electromotive force at the junction of every two substances. A summary of the argument may be briefly given as follows, which, as far as it goes, is in Professor Lodge's own words: —

Wherever a current gains or loses energy, *there* must be a seat of electromotive force; and conversely, wherever there is a seat of electromotive force, a current must lose or gain energy in passing it.

A current gains no energy in crossing from copper to zinc, hence there is no appreciable electromotive force there.

When a current blows from zinc to acid, the energy of the combination which occurs is by no means accounted for by the heat there generated, and the balance is gained by the current; hence at a zinc acid junction there must be a considerable electromotive force (say, at a maximum, 2.3 volts).

A piece of zinc immersed in acid is therefore at a lower potential than the acid; though how much lower it is impossible to say, because no actual chemical action occurs.

It was not to be expected that this statement of views, differing so greatly from those usually held, would be received without some protest, and particularly from Sir William Thomson, who has been regarded as the chief exponent and expounder of the metallic-contact theory. Professor Lodge was perfectly successful in inaugurating a discussion which was full of interest; although it can hardly be said to have contributed much to the discovery of a substantial basis of agreement, as he had evidently hoped. Sir William Thomson presented his own views at some length. The subject was one surrounded by great difficulties. He thought there could be no doubt as to a difference of potential at the zinc-copper junction, but the question of the electro-motive force of a voltaic cell might be separated from that of difference of potential at the junctions. He fully agreed with Professor Lodge in his view of the seat of the 'working-force' in the cell. The 'working-force' was essentially chemical force. Undoubtedly, in a certain sense, both the chemical and voltaic or contact views of the question were correct.

Professor Rowland, on being called upon to express his opinion, with characteristic frankness declared that he knew nothing about it. Professor Willard Gibbs called attention to the fact that this was a case similar to several other well-known physical problems, in which an attempt to determine the exact point or place at which a force resides had not been rewarded with success. In such cases much depends upon the standpoint from which the subject is viewed, and it sometimes happened that each of several quite different explanations of a phenomenon might be perfectly correct. This proposition came nearer affording a 'substantial basis of agreement' than any thing else; but it cannot be denied that the impression remained on the minds of many in the section, that the extreme views were nearly, if not quite, irreconcilable with each other. Among the interesting results of the discussion was the somewhat unexpected limitation put upon the generally accepted idea of the 'potential of a body,' by Sir William Thomson. This he defined to be the energy expended in bringing unit electricity from an infinite distance to a point in air extremely near the surface of the body.

Lord Rayleigh described a galvanometer of twenty wires which he had constructed, by joining the wires in multiple arc and also in series, so that the constant of one circuit was exactly ten times that of the other. The instrument was useful for the accurate standardizing of ammeters for measuring currents of from ten to fifty amperes. Professor Shuster discussed the influence of magnetism on the discharge of electricity through gases. He had found that this influence was very different upon the discharge from large electrodes from those usually observed when small electrodes are used. The construction of an apparatus of peculiar form, with large electrodes, had enabled him to obtain many curious effects by the introduction, between the electrodes, of electro-magnets of various forms. He had also found that none of the usual Crooke's effects are produced in mercury-vapor tubes, and this was connected in the theory of the operation with the fact that mercury was a monatomic substance.

In the discussion of a paper by Lord Rayleigh on telephoning through a cable, Mr. W. H. Preece related his experiences in telephoning the Dublin and Holyhead cable, a distance of sixty miles, which had been fairly successful; accurately heard conversation, however, could not be carried on beyond a distance of twenty-five miles. Other experiments had proved that it was at present impracticable to use underground wires in cities for distances of more than twelve miles. In every experiment telephonic circuits were made metallic. With an arrangement of double lines he said he had no difficulty in speaking through two hundred and forty miles on overground wires.

The much-talked-of, and one might justly say the much-abused subject, of the connection of sun-spots with terrestrial phenomena, received considerable attention in a discussion which was opened by Prof. A. Schuster. It is generally agreed that sun-spots have a periodicity; the length of their period being somewhat irregular, varying, indeed, from about eight years to fifteen or sixteen years, but the mean from maximum to maximum being about eleven years. This period might be the resultant of several periods superposed; and Professor Balfour Stewart had pointed out the fact that the irregularity observed could be fairly well accounted for by supposing the superposition of two periods of about ten and a half and twelve years respectively.

The first noticeable effect of this sun-spot cycle was the corresponding cycle in the daily variation of the magnetic needle, — a relation which was also generally admitted. From maximum sun-spot area to minimum sun-spot area, the daily variation of the needle changes in the ratio of about three to two; and in at least two instances brief but violent disturbances in the sun had been known to be accompanied, or at least followed closely, by similarly brief but marked disturbances of the magnetic needle. Such was undoubtedly the case in 1859, as observed by Carrington; and again in 1872, as observed by Professor Young. Professor Loomis has shown that there is an intimate relation between the sun-spot cycle and that of the aurora borealis; and, in fact, the practical agreement of those three cycles — the sun-spot, the magnetic, and that of the aurora — may now be considered as universally admitted. But although much time and great labor have been expended in this direction, it must be admitted that no other connection of solar disturbance as shown in sun-spots, with terrestrial phenomena, has been so completely proved as to command general confidence.

The question of accounting for the magnetic influence had been considered. Were the sun made of solid steel, and magnetized to saturation, it could not produce the effects upon the magnetic condition of the earth which are now justly attributed to it. Whether electricity is conducted in some way or other from the sun to the earth, is a question which cannot at present be answered, although it would be rash to affirm that the space between the sun and the earth does not contain enough matter to conduct electricity. It has been suggested, that variations in the amount of heat radiated from the sun might be shown to be an important factor; and some determinations of the total solar radiation have seemed to indicate that the total amount varied from time to time by as much as eight per cent. But the measurement of the sun's radiation is surrounded by the greatest difficulties, on account of the unknown and possibly varying absorption of the earth's atmosphere. Professor Schuster was convinced that the only mode of attempting the solution of the problem lay in the direction of evading this disturbance by establishing observing stations on the highest accessible points; and he suggested the Himalaya Mountains as offering, on many accounts, the most suitable locality. As the question now stood, he believed he was correct in saying that we know nothing of the variation of the sun's radiation.

The question as to the possibility of investigating the problem, through observed temperature effects upon the surface of the earth, had naturally been considered. In spite of the difficulties surrounding the subject, there could be no doubt that several different observers had proved that a connection existed between the sun-spot period and certain temperature effects upon the earth. Among these effects may be mentioned the agreement between this period and the best wine years on the Rhine; and also with the period of the increasing and decreasing number of cyclones upon the Indian Ocean.

As to a similar period in mean atmospheric pressure, the evidence was very contradictory; but it may be safe to add two other coincidences which seem to be established: the number of small comets about the sun seems to vary through a period about in agreement with that of the sun-spots; and it has been shown, particularly in photographs secured by Professor Schuster himself, that the appearance of the solar corona depends in some way on the same cycle.

Professor Schuster was followed by Mr. W. Lant Carpenter, who read a paper upon the same subject, prepared by Prof. B. Stewart and himself. It consisted, in the main, of a description of some very elegant methods which they had made use of in discussing the temperature observations of Toronto and Kew, for the purpose of detecting short periods common to solar and terrestrial phenomena. One of the results of this investigation was to show, that, in general, temperature phases make their appearance at Toronto eight days before they appear at Kew; while what might be called 'magnetic declination range weather' travels from Toronto to Kew in about one and six-tenths days.

The subject was further discussed by several members of the section, among whom was Rev. S. J. Perry, who took a very conservative view of the matter, and declared that much research was demanded before any thing really definite would be known.

The sun, at least as to its spectrum, received further attention from Professor Rowland and Rev. S. J. Perry, — the former exciting great interest in the section by an exhibition of several of his latest spectrum photographs, and a discussion of the remarkable advances in this direction which had followed, necessarily, the use of his diffraction gratings. Mr. Perry's paper was a discussion of observations on the spot spectrum from D to B.

Professor Carpmael described a new form of induction inclinometer which he had devised, which was a modified form of Lloyd's instrument, a bifilar suspension being substituted for an unifilar, and one or two other changes made. The instrument had only been in use a few weeks, but it promised to be of considerable value.

The Earl of Rosse described his method and machinery used for polishing specula, and with which he had completed at Parsonstown a three-foot and a six-foot speculum. He also described a device for securing electrical control of an equatorial driving-clock, which he had recently tried, and found to be very satisfactory. It was essentially a 'see-saw' escapement, with a piece of soft iron on an extension at one end, which moved between two electro-magnets, being held firmly by each, during a certain portion of the swing of the controlling pendulum. In this way, he had secured accuracy and certainty of control, even with crude apparatus.

Mr. Perry, in speaking of the great importance of accurate control of an equatorial, now that the spectroscope had come so to the front, said that it was interesting to know that Mr. Huggins, in producing some of the most perfect telescopic photographs that had yet been made, had not especially felt the need of a more perfect controlling device; since Mrs. Huggins was constantly at his side to regulate the position of the instrument, and that his splendid results were largely due to her precision and patience.

One of the most interesting and novel papers was that of Professor Douglass Archibald, describing his method of sending anemometers into the air by means of kites, and thus studying the velocity of the air at different heights. The carrying kite was seven feet in length; and this was raised and afterward somewhat steadied by a smaller one about four feet long attached to it. The anemometers were arranged at different points along the line of the larger kite, so as to record the velocity at various heights, in some cases extending up as high as six hundred feet. Although the experiments thus far made were only preliminary in their character, some interesting results had been obtained. The velocity increased with increased height, but at a diminishing rate. On being questioned, Professor Archibald declared that the most important thing about a kite was its tail. In his kite the tail was made up of cones of canvas arranged with their bases towards the wind, with the cord running along their axes. They were placed at a distance of three or four feet from each other, and six were used. Sir William Thomson said that after more than a century the kite was again being dedicated to science, first on one continent, and now on another. He was convinced that the device of Professor Archibald was sure to prove to be of great value in meteorological research.

Professor Archibald made a brief reference to the work already accomplished by a committee, of which he was a member, known as the 'Krakatoa committee.' Their object was to determine, if possible, whether the sun-glows or remarkable sunsets of the past year could in any way be attributed to the general diffusion of dust from the eruption of that volcano. They had succeeded in collecting much information, which had not yet been examined; and he could only say that nothing had yet appeared which was inconsistent with the Krakatoa theory.

Some further contributions to meteorology were made in a paper by Professor James Thomson on whirlwinds and waterspouts; in a note on internal earth temperature by Mr. H. S. Poole, in which the increase in temperature at Wolfville, N.S., was shown to be in fair agreement with other well-known determinations; and in a paper by Dr. H. Muirhead on the formation of mackerel sky. The latter was an extension of the explanation suggested many years ago by Sir William Thomson, by the introduction of a third stratum of moving air. The effect of one stratum moving over another, as Sir William Thomson had suggested, would be to produce 'waves' in the air, which might result in long lines of cloud-forms. A third stratum, moving in a direction at or near a right angle to the first, would tend to break these lines up into small patches, thus producing the peculiar appearance known as the mackerel sky.

Prof. Chandler Roberts interested the section greatly in presenting the results of some experiments which he had carried out to show the diffusion of metals; the cases specially considered being the diffusion of gold, silver, and platinum in lead. The rate of diffusion in these cases, and notably in the case of gold, seems to be enormously high compared with the rate of diffusion in liquids.

Mr. W. J. Millar read a paper on iron and other metals in a liquid and solid state, which started a lively and entertaining discussion of the question of the expansion of iron on solidification. Mr. Millar contended that iron did not expand on solidification; while Sir William Thomson, and other members of the section, protested that Mr. Millar's own experiments proved conclusively that it did.

The matter of the velocity of light of different colors was considered by Professor Michelson, and also by Professor George Forbes. Mr. Michelson explained, somewhat in detail, his method of determining the velocity of light, and gave the results of an investigation of the velocity of red and blue through a column of carbon bisulphide about ten feet long. The velocity of the mean ray through this medium had been found to be about 1.75 times its value in air, which was somewhat higher than theory would indicate; but the difference was doubtless attributable to errors in experiment. A measurable difference between the velocity of the red and that of the blue ray had been observed, agreeing very closely with that indicated by theory. Professor Forbes's paper was a discussion of the observations by means of which he, in junction with Mr. Young, had shown, apparently, that there was a measurable difference between the velocities of red and blue light in air. The paper was discussed by Sir William Thomson, Lord Rayleigh, Professor Newcomb, Professor Michelson, and several others; and the general opinion was quite decidedly against the view that such difference really existed.

On the last day of the session, the section was divided; and a number of papers on pure mathematics occupied the attention of a sub-section. No report of these papers can be made, as the *Science* reporter found it impossible to organize a sub-section to follow the mathematicians.

PROCEEDINGS OF THE SECTION OF CHEMICAL SCIENCE.

THE session opened at noon, Aug. 28, with the president, Sir Henry E. Roscoe, in the chair. Dr. Perkins, the retiring president, sat on his right hand; and Drs. Wolcott Gibbs, Gladstone, and Frankland, on his left. The room was filled to overflowing; and the address was listened to with marked attention and interest, and the comments upon it were uniform in their commendation. This is rather surprising when we recall the present state of feeling in England which the efforts to found a superior institution for technical instruction have aroused, but his views on chemical education are in conformity with those generally entertained in the United States. It will be seen by the papers presented at this session, that the particular phases of the recent advances in chemistry of which the president treated occupy at present the attention of many of the English chemists.

The first paper read was by Dr. Wolcott Gibbs, at the request of the section, and was upon the complex inorganic acids. It consisted of a *résumé* of the magnificent work which he has done in the field which he has discovered and explored.

It is impossible in the brief space at our command to do justice to this superb research; which is destined to revolutionize many of our chemical conceptions, and in which has been shown the cumulative power of the molybdenum and tungsten oxides, the existence of dominant and subdominant groups, and of different kinds of basicity prevailing within the same molecule, and of the production of isomerism by the orientation of the atoms.

Mr. H. B. Dixon exhibited tables in which Bunsen's, Horstmann's, and his own results on the effect of mass on the incomplete combustion of mixtures of carbon monoxide, hydrogen, and oxygen were compared; and the discrepancies were found to be due to differences in the temperature and pressure under which the experiments were conducted. Above four hundred millimetres, the pressure did not affect the results; and at temperatures between 60° and 140° constant results were also obtained. It is believed, that when the mixtures were exploded below 60°, the reaction was interfered with by the condensation of water on the sides of the tube. Further, it was found that mixtures of carbon monoxide and oxygen, in equivalent proportions, could not be exploded unless there were aqueous vapor, or some body containing hydrogen, present. With traces of hydrogen, hydrochloric acid, hydrogen sulphide, or a hydrocarbon present, the mixture could be exploded. It is supposed that the steam is reduced by the carbon monoxide, and that the liberated hydrogen burns, and re-forms steam, which again acts on more carbon monoxide. By a series of alternate reductions, a few molecules of steam serve to carry oxygen to the carbon monoxide just as the oxide of nitrogen acts in the sulphuric-acid chamber. By putting a dry mixture of carbon disulphide and oxygen into a dry mixture of carbon monoxide and oxygen, the first could be inflamed, then by introducing a little water the carbon monoxide and oxygen could be exploded.

Professors Liveing and Dewar read a paper on the spectral lines of the metals developed by exploding gases. Berthelot has recently investigated, by means of the chronograph, the rate of propagation of the explosion of mixtures of oxygen with hydrogen and other gases; and has found, that, with a mixture of hydrogen and oxygen in the proportion to form water, the explosion progresses along a tube at the rate of 2,841 metres per second, a number which is not far from the velocity of mean square for hydrogen particles, on the dynamic theory of gases, at a temperature of 2,000°.

This velocity, though far short of the velocity of light, bears a ratio to it which cannot be called insensible. It is, in fact, about $\frac{1}{105000}$ part of it. Hence, if the explosion were advancing towards the eye, the waves of light would proceed from a series of