THE SEARCH FOR A PLANET BEYOND NEPTUNE.—Herr T. Grigull, of Münster, Germany, describes in the October number of the *Bulletin de la Société Astronomique de France* his new contribution to the research which has for its object the discovery of another planet, beyond the orbit of Neptune.

In a previous paper (*Bulletin*, January, 1902, p. 31), Herr Grigull explained the hypothesis on which his calculations are based, and the elements of the hypothetical planet as deduced from the observations of the aphelia of three comets. In the present contribution, the elements given below have been calculated from the observed aphelia of twenty comets which appeared, and were observed and recorded, between the years 1490 and 1898. After giving due weight to the various cometary observations, the author has calculated these elements for the possibly existing planet:—

Epoch 1902.

$$\lambda = 357^{\circ}\cdot 54 \pm 1^{\circ}\cdot 867$$
$$\text{Dist. from sun} = 50\cdot 61 \text{ R.}$$
$$\text{Time of revolution} = 360 \text{ years.}$$
$$\Omega = 90^{\circ} \ (???).$$
$$\omega = ?.$$

A NEW MINOR PLANET.—In No. 3819 of the *Astronomische Nachrichten*, Prof. Max Wolf announces, along with other minor planetary observations, the discovery of another new minor planet, 1902 T.

COMET 1902 *b*.—A number of observations of this comet have been made.

A photograph taken on September 27 by Prof. Kononowitsch, Odessa, with three hours' exposure, shows a straight double tail extending in a southerly direction to a distance of 3°.

Prof. Nijland has published, in the *Astronomische Nachrichten* (No. 3817), a further ephemeris, from which the following extract is taken:—

1902.	app. α. h. m. s.	app. δ. ° ′	Brightness.
Oct. 16	18 16 24	+16 30·5	
17	9 55	14 9·1	
18	4 7	11 58·9	13·0
19	17 58 52	9 59·1	
20	54 6	8 8·9	
21	49 43	6 27·3	
22	45 41	4 53·6	10·9
23	17 41 56	+3 26·9	

The brightness of the comet on September 6 is taken as unity, and it was then estimated at 7·5m.

THE BRITISH ASSOCIATION AT BELFAST.

SECTION A.

SUBSECTION OF ASTRONOMY AND COSMICAL PHYSICS.

OPENING ADDRESS BY ARTHUR SCHUSTER, F.R.S., F.R.A.S., CHAIRMAN OF SUBSECTION.

OUR proceedings to-day constitute an innovation and require a few words of explanation. When, a few years ago, some astronomers felt that our Association bestowed an insufficient share of attention on their subject, an easy remedy suggested itself in the formation of a special subsection devoted to that subject. Such a subsection was accordingly organised at Bradford and Glasgow, but for reasons, which are perhaps not altogether to be regretted, the experiment was only partially successful. In the meantime the work of Section A became heavier and heavier, and, as it seemed necessary to find some way of relieving its meetings, it was decided to hand over to the already established subsection of Astronomy other subjects, such as Meteorology, Terrestrial Magnetism, Seismology, and, in fact, anything that the majority of physicists is only too glad to ignore.

When the Council of the British Association asked me to act as President of such an enlarged subsection, I was very doubtful whether I ought to accept the honour. In the first place, I felt incompetent, owing to my almost complete ignorance of most branches of astronomy, and in the second place I do not approve of the formation of subsections dealing with important branches of Physics. If I eventually consented, it was partly because I lacked the strength of mind to refuse an honour of this kind, but partly because I was glad to have an opportunity of raising the whole question of the organisation of our meetings. The ground for such a discussion has, however, to a great extent disappeared, because, when the Organising Committee of Section A met in the spring, there appeared amongst those present a sudden revival of interest in the subjects assigned to the subsection and it was decided that the main section should not meet at all to-day so as to allow its members to help us in our discussions. The parent section has, therefore, voluntarily submitted itself to absorption by its neglected offspring, which now has to show that Cosmical Physics obeys the laws of Terrestrial Physics and that good absorbers are also good radiators.

Gratifying as this reunion must be to us, it fails to realise one of the original objects for which we have been called into existence, because instead of lightening your work it has added to it by imposing upon you the burden of having to listen to a second Presidential Address. I will try to make this additional burden as light as possible by concentrating my general remarks into a few sentences and then introducing the business of the section by means of a contribution to its scientific work, which I otherwise should have made in the ordinary course of the meeting.

To make our meeting as fruitful as possible, we should make the fullest use of the opportunities it gives us of personal contact and interchange of ideas. This is not accomplished by dividing into separate camps as soon as we have come together, but rather by finding some common ground for our debates. We should not try to minister to the separate needs of the specialist in electricity, or in meteorology or in astronomy, but should impress upon each of these specialists that they must bring before us the results of their investigations in so far as they bear on the more general questions in which we all are, or ought to be, interested. If it is necessary to lighten the work of the section this should be done by excluding all papers which are of interest only to specialists, or by establishing subsections for such papers. Let us divide—if divide we must—according to the character of the contribution, rather than according to the subject it happens to deal with. The difficult and, perhaps, unpopular censorship which such a course would involve would probably be temporary only, as the character of the papers which are desired for the main section would soon become known, and the increased attraction and usefulness of our discussions would, I am convinced, in a few years compensate for the initial trouble. We all require, occasionally, to be reminded that the detail work which is necessary, and on which most of us are engaged, is only of importance or interest if it helps us forward towards the solution of the great problems of Nature.

Addressing myself more particularly to Astronomers, I should like to say that we shall always welcome them as members of Section A, and that the benefit we shall derive from their contributions will be great in proportion as they will consider themselves to be citizens of the general empire of that section rather than inhabitants of an independently governed State.

There is one minor reform, or perhaps I ought to call it a protest against one of the traditions of the Association, which I feel called upon to urge on you. Discussion is our principal aim, and we are always trying to find suitable subjects for discussion; yet we are prevented by the rules of the Association from discussing the Presidential address and the reports of Committees. Those who framed such a rule must have had some unfortunate idea that the dignity of the chair might be endangered if some criticism happened to be expressed in the discussion of the Chairman's address, or that the value of the report of a Committee might be endangered by some adverse comment coming from outside. But it seems to me that a scientific society or association, and especially one framed on a democratic constitution, ought not to take such a narrow and unscientific view. I can remember several Presidential addresses which might, and probably would, have given rise to most instructive debates had the rule not existed. Reports of Committees if not suitable for discussion should not be read at all; but if read they should be open to discussion.

I hope that to-day you will not feel yourself bound by ancient custom, but in order that, at any rate, the more scientific portion of my contribution to our proceedings should not be stained by the suspicion of immaculate conception, I will now ask the duly constituted President of our section to take his proper place.

The question I wish to bring to your notice to-day is an old one: if two events happen simultaneously or one follows the other at a short interval of time, does this give us any reason to suppose that these two events are connected with each other, both being due to the same cause, or one being the cause of the other? Everyone admits that the simple concurrence of events proves nothing, but if the same combination recurs sufficiently often we may reasonably conclude that there is a real connection. The question to be decided in each case is what is "sufficient" and what is "reasonable." Here we must draw a distinction between experiment and observation. We often think it sufficient to repeat an experiment three or four times to establish a certain fact, but with meteorological observations the case is different, and it would, *e.g.*, prove very little if on four successive full moons the rainfall had been exceptionally high or exceptionally low. The cause of the difference lies in the fact that in an experiment we can control to a great extent all the circumstances on which the result depends, and we are generally right in assuming that an experiment which gives a certain result on three successive days will do so always. But even this sometimes depends on the fact that the apparatus is not disturbed, and that the housemaid has not come in to dust the room. Here lies the difference. What is possible in a laboratory, though perhaps difficult, is not possible in the upper regions of the atmosphere, where some unseen hand has not made a clean sweep of some important condition.

When we cannot control accessory circumstances we must eliminate them by properly combining the observations and increasing their number. The advantage does not lie altogether on the side of experiment, because the very identity of condition under which the experiment is performed gives rise to systematic errors, which Nature eliminates for us in the observational sciences. In the latter also the great variety in the combinations which offer themselves allow us to apply the calculus of probability, so that in any conclusion we draw we can form an idea of the chance that we are wrong. Astronomers are in the habit of giving the value the "probable error" in the publication of their observations. Meteorologists have not adopted this custom, and yet their science lends itself more readily than any other to the evaluation of the deviations from the mean result, on which the determination of the probable error depends. We look forward to the time when weather forecasts will be accompanied by a statement of the odds that the prediction will be fulfilled.

The calculation of the probability that any relationship we may trace in different phenomena indicates a real connection seems to me to be vital to the true progress of Meteorology, and although I have on previous occasions (Cambridge *Phil. Trans.*, vol. xviii. p. 107) already drawn attention to this matter I should like once more to lay stress on it.

The particular case I wish to discuss (though the methods are not restricted to this case) is that in which one of the two series of events between which relationship is to be established has a definite period, and it is desired to investigate the evidence of an equal period in the other series.

Connections between the moon and earthquakes, or between sunspots and rainfall if proved to exist, would form examples of such relationships. The question to be decided in these cases would be, is there a lunar period of earthquakes, or an eleven years' sunspot period of rainfall.

Everyone familiar with Fourier's analysis knows that there is a lunar or sunspot, or any other period in any set of events from volcanic eruptions down to the birth-rate of mice; what we want to find out is whether the periodicity indicates a real connection or not. Let us put the problem into its simplest form. Take n balls, and by some mechanism allow them to drop so that each falls into one of m compartments. If finally they are equally distributed each compartment would hold n/m balls. If this is not the case we may wish to find out whether the observed inequality is sufficient to indicate any preference for one compartment or how far it is compatible with equality of chance for each. If we were able to repeat the experiment as often as we like we should have no difficulty in deciding between the two cases, because in the long run the average number received by each compartment would indicate more and more closely the extent of bias which the dropping mechanism might possess. But we are supposed to be confined to a single trial, and draw our conclusions as far as we can from it.

It would be easy to calculate the probability that the number of balls in any one compartment should exceed a given number, but in order to make this investigation applicable to the general problem of periodicities we must proceed in a different manner. If the compartments are numbered, it does not matter in which order, and a curve be drawn in the usual manner representing the connection between the compartments and the number of balls in each, we may, by Fourier's analysis, express the result by means of periodic functions. The amplitude of each period can be shown *on the average* to be $\frac{1}{m}\sqrt{\pi n}$. It is often more convenient to take the square of the amplitude—call it the intensity—as a test, and we may then say that the "expectancy" of the intensity is $4n/m^2$. The probability that the intensity of any period should be k times its average or expectancy is e^{-k}. We may apply this result to test the reality of a number of coincidences in periods which have been suspected. A lunar effect on earthquakes is in itself not improbable, as we may imagine the final catastrophe to be started by some tidal deformation of the earth's crust. The occurrence of more than 7000 earthquakes in Japan has been carefully tabulated by Mr. Knott according to lunar hours, who found the Fourier coefficient for the lunar day and its three first sub-multiples to be 10·3, 17·9, 10·9, 3·97; the expectancy on the hypothesis of chance distribution for these coefficients I find to be 19·3, 15·7, 10·6, 5·02. The comparison of their numbers disproves the supposed connection; on the other hand, the investigations of Mr. Davison on solar influence have led to a result much in favour of such influence, the amplitude found being in one series of observations equal to five times, and in the other to fifteen times the expectancy. The probability that so large an amplitude is due to accident in the first case is one in 300 millions, and in the second the probability of chance coincidence would be represented by a fraction, which would contain a number of over 70 figures in the denominator. We may, therefore, take it to be established that the frequency of earthquakes depends on the time of year, being greater in winter than in summer. With not quite the same amount of certainty, but still with considerable probability, it has also been shown that earthquake shocks show a preference for the hours between 9 a.m. and noon.

A great advantage of the scientific treatment of periodical occurrences lies in the fact that we may determine *a priori* how many events it is necessary to take into account in order to prove an effect of given magnitude. Let us agree, for instance, that we are satisfied with a probability of a million to one as giving us reasonable security against a chance coincidence. Let there be a periodic effect of such a nature that the ratio of the occurrence at the time of maximum to that at the time of minimum shall on the average be as $1+\lambda$ to $1-\lambda$, then the number of observations necessary to establish such an effect is given by the equation $n = 200/\lambda^2$. If there are 2 per cent. more occurrences at the time of maximum than at the time of minimum $\lambda = \cdot01$, and n is equal to two million. If the effect is 5 per cent., the number of events required to establish it is 80,000.

To illustrate these results further, I take as a second example a suggested connection between the occurrence of thunderstorms and the relative position of sun and moon. Among the various statistical investigations which have been made on this point, that of Mr. MacDowall lends itself most easily to treatment by the theory of probability. One hundred and eighty-two thunderstorms observed at Greenwich during a period of fourteen years have been plotted by Mr. MacDowall as distributed through the different phases of the moon, and seem to show a striking connection. I have calculated the principal Fourier coefficient from the data supplied, and find that it indicates a lunar periodicity giving for the ratio of the number of thunderstorms near new moon to that near full moon the fraction 8·17 to 4·83.

This apparently indicates a very strong effect, but the inequality is only twice as great as that we should expect if thunderstorms were distributed quite at random over the month, and the probability of a true connection is only about 20 to 1. No decisive conclusions can be founded on this, the number of thunderstorms taken into account being far too small. We might dismiss as equally inconclusive most of the other researches published on the subject were it not for a remarkable agreement among them, that a larger number of storms occur near new moon than near full moon.

I have put together in the following table the results of all investigations that are known to me; following the example of Koeppen, I have placed in parallel columns the number of thunderstorms which have occurred during the fortnight including new moon, and the first quarter and the fortnight including the other two phases.

Place of observation and author.	Time of observations.	Percentage of thunderstorms during the fortnight including New moon and first quarter.	Full moon and last quarter.
Karlsruhe (Eisenlohr) ...	1801 31	50·8	49·2
Gotha (Luedicke) ...	1867–75	72·5	27·5
Vigevano (Schiaparelli) ...	1827–64	46	54
Germany (Köppen) ...	1879–83	56	44
Glatz (Richter) ...	1877–84	62	38
United States (Hazen) ...	1884	56·5	43·5
Prag (Grüss)	1840–59	51	49
,, ,, ...	1860–79	52·5	47·5
Göttingen (Meyer) ...	1857–80	54	46
Kremsmunster (Wagner)..	1862–87	53·8	46·2
Aix la Chapelle (Polis) ...	1833–92	54·4	45·6
Sweden (Eckholm) ...	1880–95	53·8	46·2
Batavia (v.d. Stock) ...	1887–95	51·9	48·1
Greenwich (McDowall) ...	1888–91	54	46
Average	—	54·9	45·1

It will be seen that out of fourteen comparisons, thirteen show higher numbers in the first column, there being also, except in two cases, a general agreement as regards the magnitude of the effect. Two of the stations given in the table, Göttingen and Gotha, are perhaps geographically too near together to be treated as independent stations, and we may, therefore, say that there are thirteen cases of agreement, against which there is only one published investigation (Schiaparelli) in which the maximum effect is near full moon.

The probability that out of thirteen cases in which there are two alternatives, selected at random, twelve should agree and one disagree is one in twelve hundred. If the details of the investigations summarised in the above table are examined, considerable differences are found, the maximum taking place sometimes before new moon and sometimes a week later. There is, however, evidently sufficient *prima facie* evidence to render an exhaustive investigation desirable. The most remarkable of all coincidences between thunderstorms and the position of the moon remains to be quoted. A. Richter has arranged the thunderstorms observed at Glatz, in Silesia, according to lunar hours, and finds that in each of seven successive years the maximum takes place within the four hours beginning with upper culmination. If this coincidence is a freak of chance, the probability of its recurrence is only one in three hundred thousand. The seven years which were subjected to calculation ended in 1884. What has happened since? Eighteen years have now elapsed, and a further discussion with increased material would have definitely settled the question, but nothing has been done, or, at any rate, published. To me it seems quite unintelligible how a matter of this kind can be left in this unsatisfactory state. Meteorological observations have been allowed to accumulate for years, one might be tempted to say for centuries, yet when a question of extraordinary interest arises we are obliged to remain satisfied with partial discussion of insufficient data.

The cases I have so far discussed were confined to periodical recurrences of single detached and independent events, the condition, under which the mathematical results hold true, being that every event is entirely independent of every other one. But many phenomena, which it is desirable to examine for periodic regularities, are not of this nature. The barometric pressure, for instance, varies from day to day in such a manner that the deviations from the mean on successive days are not independent. If the barometer on any particular day stands half an inch above its average it is much more likely that on the following day it should deviate from the mean by the same amount in the same direction than that it should stand half an inch below its mean value. This renders it necessary to modify the method of reduction, but the theory of probability is still capable of supplying a safe and certain test of the reality of any supposed periodic influence. I can only briefly indicate the mathematical theorem on which the test is founded. The calculation of Fourier's coefficients depends on the calculation of a certain time integral. This time integral will for truly homogeneous periodicities oscillate about a mean value, which increases proportionately to the interval, while for variations showing no preference for any given period, the increase is only proportional to the square root of the time.

Investigations of periodicities are much facilitated by a certain preliminary treatment of the observations suggested by an optical analogy. The curve, which marks the changes of such variables as the barometric pressure, presents characteristics similar to those marking the curve of disturbance along a ray of white light. The exact outline of the luminous disturbance is unknown to us, but we obtain valuable information from its prismatic analysis, which enables us to draw curves connecting the period and intensity of vibration. For luminous solids we thus get a curve of zero intensity for infinitely short or infinitely long radiations, but having a maximum for a period depending on temperature. Gases, which show preference for more or less homogeneous vibrations, will give a serrated outline of the intensity curve.

I believe meteorologists would find it useful to draw similar curves connecting intensity and period for all variations which vary round a mean value such as barometric, thermometric or magnetic variations. These curves will, I believe, in all cases add much to our knowledge; but they are absolutely essential if systematic searches are to be made for homogeneous periods. The absence of any knowledge of the intensity of periodic variation renders it, *e.g.*, impossible to judge of the reality of the lunar effect which Eckholm and Arrhenius believe to have traced in the variations of electric potential on the surface of the earth. The problem of separating any homogeneous variation, such as might be due to lunar or sunspot effects, is identical with the problem of separating the bright lines of the chromosphere from the continuous overlapping spectrum of the sun. This separation is accomplished by applying spectroscopes of great resolving powers. In the Fourier analysis, resolving power corresponds to the interval of time which is taken into account, hence to discover periodicities of small amplitude we must extend the time interval of the observations.

I believe that the curve which connects the intensity with the period will play an important *rôle* in meteorology. It is a curve which ought to have a name, and for want of a better one I have suggested that of periodograph. To take once more barometric variations as an example, it is easy to see that just as in the case of white light the periodograph would be zero for very short, and probably also for very long, periods. There must be some period for which intensity of variation is a maximum. Where is that maximum? And does it vary according to locality? The answer to these questions might give us valuable information on the difference of climate. Once the periodograph has been obtained, the question of testing the reality of any special periodicity is an extremely simple one. If h be the height of the periodograph, the probability that, during the time interval chosen, the square of the Fourier coefficient should exceed kh is e^{-k}. If we wish this quantity to be less than a million, k must be about 11; so that in order to be reasonably certain that any periodicity indicates the existence of a truly homogeneous variation, the square of the Fourier coefficient found should not be less than 11 times the corresponding ordinate of a periodograph

I have calculated in detail the periodograph of the changes of magnetic declination at Greenwich, taking as basis the observations published for the 25 years 1871–95. It was not, perhaps, a very good example to choose, on account of the complications introduced by the secular variation, but my object was to test the very persistent assertions that have been made as to the reality of periodic changes of 26 days or thereabouts. The first suggestion of such a period came from Hornstein, of Prague, who ascribed the cause of the period to the time of revolution of the sun round its axis. He only discussed the records for one year's observations, but the evidence he offered was sufficient to impress Clerk Maxwell with its genuineness. Since Hornstein's first attempts, a great many rough and some very elaborate efforts have been made by himself and others to prove a similar period in various meteorological variations. The period found by different computors differed, but there is a good deal of latitude allowed if the rotation of the sun really has an effect on terrestrial phenomena, because the angular velocity of the visible solar surface varies with the latitude. Hornstein himself and some of his followers deduced a period not differing much from 26 days, while Prof. Frank Bigelow, using a large quantity of material, finds 26·68 days, and Eckholm and Arrhenius return to 26 days, or, as they put it more accurately, to 25·929 days. The two latter investigators do not, however, adopt the idea that this periodicity is due to the rotation of the sun. None of these periods can stand the test of accurate investigation.

As the result of my calculations, I can definitely state that the magnetic declination at Greenwich shows no period between 25·5 and 27·5 days having an amplitude as great as 6″ of arc. The influence of solar rotation on magnetic variation may therefore be considered to be definitely disproved.

The intensity of the periodograph increases rapidly with the period, and minute variations are, therefore, more easily detected in short than in longer periods. Six seconds of arc forms about the limit of amplitude, which can be detected in 25 years of observations, when the period is about 26 days; and from what has been said above, the amplitude which can be detected will be seen to vary inversely with the square root of the time interval. For periods of about 14 days, an amplitude of 3″ of arc is still distinguishable with the material I have used; and such an amplitude is actually found for a period which has half the synodic month as its time. The chance that this apparent variation is due to an accidental coincidence is one in two thousand; and I cannot, therefore, assert its definite existence beyond all possibility of cavil. But it is surely significant that of all the periods possible between 12·3 and 13·7 days, that gives the highest amplitude which coincides with half the synodic revolution of the moon. That it is at all possible to detect variations of 3″ of arc in the observations which are taken to 6″, with a probability of error of only one in two thousand, is, I think, a proof of the value of the method and the carefulness of the observations. The periodograph has another valuable use. It not only gives us the time necessary to establish true periodicities of given amplitude, but it also gives us an outside limit of the time beyond which an accumulation of material is of no further advantage. That limit is reached when the time is sufficient to discover the smallest amplitude which the instruments, owing to their imperfections, allow us to detect.

I am only concerned to-day with a purely statistical inquiry, and not with the explanation of any suggested relationship. To prevent misunderstandings, however, I may state that I consider the possibility of a direct magnetic or electric action of the moon excluded; as regards the latter, the diurnal variations of electric potential would be so much affected by a lunar electrification sufficiently strong to influence the outbreak of thunderstorms that it could not have escaped discovery. We must not, however, be dogmatic in asserting the impossibility of indirect action. The unexpected discovery of radio-activity has opened out an entirely new field, and we cannot dismiss without renewed careful inquiry the evidence of lunar action which I have given. Its reality can be decided by observation only. No—not by observation only—but by observation supplemented by intelligent discussion; and this brings me to my concluding appeal, which I wish to urge upon you with all the legitimate weight of strong conviction and all the illegitimate influence of presidential infallibility.

The subjects with which our subsection is concerned deal with facts which are revealed to us by observation more frequently than by experiment. There is in consequence a very real danger that the importance of observation misleads us into mistaking the means for the end, as if observation alone could add anything to our knowledge. Observation is like the food supplied to the brain, and knowledge only comes through the digestion of the food. An observation made for its own sake and not for some definite scientific object is a useless observation. Science is not a museum for the storage of disconnected facts and the amusement of the collecting enthusiast. I dislike the name "observatory" for the astronomical workshop, for the same reason that I should dislike my body to be called a food receptacle. Your observing dome would be useless without your computing room and your study. What you want is an Astronomical Laboratory, a Meteorological or Magnetic Laboratory, attaching to the word "laboratory" its true meaning, which is a workshop in which eyes and hands and brains unite in producing a combined result.

The problems which confront the astronomer being more definite than those of Meteorology, Astronomy has grown under the stimulus of a healthy tradition. Hence it is generally recognised, at any rate in the principal observatories, that the advance of knowledge is the chief function of the observer. Nevertheless, the President of the Astronomical Department of Section A last year (Prof. H. H. Turner) has found it necessary, in his admirable address, to warn against the danger there is that the astronomer should allow himself to be swallowed up in a routine work and mere drudgery. The descent is easy: You begin by being a scientific man, you become an observer, then a machine, and finally—if all goes well—you design a new eyepiece.

If such a danger exists in Astronomy, what shall we say about Meteorology? That science is bred on routine, and drudgery is often its highest ambition. The heavens may fall in, but the wet bulb must be read. Observations are essential, but though you may never be able to observe enough, I think you can observe too much. I do not forget the advances which Meteorology has made in recent years, but if you look at these advances, I think you will find that most of them do not depend on the accumulation of a vast quantity of material. The progress in some cases has come through theory, as in the applications of Thermodynamics or through special experiments as by kite and balloon observations, and when it has come through the ordinary channels of observation, only a comparatively short period of time has been utilised. It would not be a great exaggeration to say that Meteorology has advanced in spite of the observations and not because of them.

What can we do to mend matters? If we wish to prepare the way for the gradual substitution of a better system, we should have some one responsible for the continuation of the present one. For this purpose it should be recognised that the head of the Meteorological Office is something more than a Secretary to a Board of Directors; also that he is appointed to conduct Meteorological research and not to sign weather forecasts. The endowment of Meteorology should mean a good deal more than the endowment of the Telegraph Office which transmits the observations. Terrestrial Magnetism and Atmospheric Electricity are looked after at present by institutions already overworked in other directions and should be handed over to an enlarged Department of Meteorology. Seismology in this country now depends on the private enterprise and enthusiasm of a single man, and as long as Prof. Milne is willing to continue his work, we cannot do better than leave it with him, but some permanent provision will ultimately have to be made.

An improved organisation such as I have sketched out would do good, but could only very slowly overcome the accumulated inertia of ages. I should prefer a more radical treatment. Organisation is good, but sometimes disorganisation is better.

Most earnestly do I believe that the subjects of meteorology and terrestrial magnetism, and possibly also of atmospheric electricity, could be most quickly advanced at the present moment if all observations were stopped for five years, and all the energy of all observers and computors concentrated on the discussion of the results obtained and the preparation of an improved scheme of observation for the future. When we have made up our minds what to do with the observations, when we have actually done it; when we know where our present instruments require refining or supplementing, and especially when we have found out whether we have not spent much time and trouble on unnecessary detail, then the time will have arrived for us to draw up an economical, sufficient and efficient scheme of observations. At present we are disinclined to discontinue observations, though recognised as useless, for fear of causing a break. We make ourselves slaves to so-called "continuity," which is important, but, may be, and I believe is being, too dearly purchased.

There are no doubt some, though probably not very many, observations which it is necessary to carry on continuously over long periods of time. But at present we are groping in the dark, and go on observing everything, and always in the hope that some time the observations might prove useful. Our whole point of view in this respect wants altering. We should fix on our problem first and then provide the observations which are necessary for the solution of the problem. Let us restrict, in the first instance, the secular observations to the smallest number, and concentrate our attention, for short periods of time, on some special question. Let us have, for instance, two or three years of thunderstorm observations, all countries joining in concentrating their energies to the elucidation of all the various features of their phenomena. When that is accomplished, it will probably be found that thunderstorms may be left to shift for themselves for a while, and attention might be directed to some other matter. The whole question of lunar influence on meteorological phenomena might be settled in a comparatively short space of time if the civilised countries of the world could agree to record all observations during a few years according to lunar instead of solar coordinates. Other problems will readily suggest themselves to you, and several might possibly be dealt with simultaneously.

The great reform I have in view is this :—Before you observe, make sure that your observations will be useful and will help to answer a definite question.

I hope that, though my frankly outspoken criticisms may not command universal assent, you will agree that there is some foundation for them, and, if so, the time is obviously not well chosen when observational science can be separated from its mathematical and experimental sisters. We hope that cosmical physics may remain an integral portion of Section A, and, though we acknowledge our weaknesses, we claim to have also something to teach.

I hope that our proceedings this week may show that we can put aside observational detail and throw some light on the great and important problems with which our science is concerned.

MATHEMATICS AND PHYSICS AT THE BRITISH ASSOCIATION.

ALTHOUGH the number of communications made to the Section at Belfast was less than at Glasgow last year, there was no decrease in the interest of the meetings. The inclusion of cosmical physics in the subjects dealt with by the department for astronomy materially increased the attendance at the meetings of that department.

In the mathematical department, Miss Hardcastle described the ground covered by the second part of her report on the present state of the theory of point groups, and stated that a further communication would be necessary to bring the report up to the present time. In the absence of the author, Prof. Forsyth gave a short account of Mr. E. T. Whittaker's solutions of the partial differential equations of mathematical physics. Mr. Whittaker finds that an expression of the type

$$\int_0^{2\pi} f(z + ix\cos u + iy\sin u, u)\,du$$

is the most general solution of the potential equation of Laplace, where f is an arbitrary function of the arguments

$$z + ix\cos u + iy\sin u \text{ and } u, \text{ and } i = \sqrt{-1}.$$

It follows that Legendre's, Bessel's and other well-known solutions of the equation are special forms of Mr. Whittaker's. In the same way, the general solution of the equation of wave motion is of the type

$$\int_0^{2\pi}\int_0^{\pi} f(x\sin u\cos v + y\sin u\sin v + z\cos u + \frac{t}{k}, u, v)\,du\,dv,$$

where f is an arbitrary function. Mr. Whittaker points out that this solution may be analysed into plane waves, and therefore supports the conclusion arrived at by Dr. Johnstone Stoney in 1897, that all disturbances in the ether can be resolved into trains of plane waves.

In the department of physics, Lord Rayleigh brought forward the question of the accurate conservation of weight in chemical reactions. He considered the discrepancies found by experimenters too large to allow the law of conservation to be accepted as proved, and hoped that the experiments at present being carried out by Landolt and Heydweiller would soon lead to a definite conclusion. Prof. Morton described the experiments he and Mr. Hawthorne had carried out on the motion of a detached thread of liquid in a capillary tube. He concludes from them that there is some force of the nature of an attraction between the liquid and the material of the tube, which must be taken into account to explain completely the phenomena observed. He further detailed how he had, in conjunction with Mr. Vinycomb, repeated and extended the work of Raps on the mode of vibration of stretched strings, and investigated the effect of the rigidity of the support on the motion of the string.

Dr. Barnes, of Montreal, on continuing his experiments on the critical velocity of flow of water through tubes, has found that the velocity varies with temperature in the way anticipated from the viscosity term in the expression given by Prof. Osborne Reynolds in his classical paper on critical velocity. By applying in the case of mercury the method used in determining the specific heat of water, he has also found that the specific heat of mercury decreases at a rate which itself decreases slightly with increase of temperature. Lord Kelvin sent a short communication in which he suggested that the temperature of an animal surrounded by a saturated atmosphere hotter than itself was kept down by evaporation within the lungs.

Dr. J. Larmor, in a paper on the application of the method of entropy to radiant energy, showed that by defining the entropy of a given space containing radiant energy distributed in any arbitrary way, as the logarithm of the probability of the existence of that particular distribution, the law of distribution of the energy with wave-length, which was recently deduced by Planck by considering a space filled with electrical resonators, could equally well be established. According to it, the amount of energy between wave-lengths λ and $\lambda + d\lambda$ radiated by a perfectly black body at absolute temperature t is proportional to

$$\frac{1}{\lambda^5}\,\frac{1}{e^{\frac{a}{\lambda t}} - 1}$$

where a is a constant.

Mr. Petavel gave an account of the work he had done towards the production of a standard of light. He considered that the incandescent surface of a metal of the platinum group heated electrically furnished the best source, and proposed to fix the temperature of that source by the equality of the radiation transmitted by suitable thicknesses of two media, the absorption of one of which (water) increased, and of the other (black fluorspar) decreased, with increase of temperature of the source. Dr. C. S. Myers called attention to a variation of pitch of Galton and other high-frequency whistles when the wind pressure was changed, which he had not been able to explain.

Lord Rayleigh prefaced a description of his own experiments to determine whether double refraction was produced in isotropic transparent bodies by their motion through the ether, by an account of those of Michelson and Morley. The latter led to the conclusion that light travelled with the same velocity, whether the direction of transmission was coincident with, across or opposed to that of the motion of the body. Lord Rayleigh's arrangement would have enabled a change of velocity of 10^{-10} of the velocity of light to be detected, but no change was observed when the light was transmitted through water or carbon bisulphide. The experiments on solids are not yet concluded.

Dr. Johnstone Stoney forwarded a note in which he showed that by substituting for Huyghen's wave surface a wave film of finite thickness, within which the phases of the disturbances were given proper values, the disturbance propagated to a point outside the wave surface could be accurately calculated. In a second note, Dr. Stoney showed how his method of resolving the light traversing any isotropic medium into trains of plane waves might be applied to explain several optical phenomena which have not hitherto yielded to other methods.

Prof. E. Wilson described his experiments on the use of a magnetic detector in space telegraphy. His detector consists of an iron ring magnetised to instability by a current through a coil wound on the ring. The electric waves falling on the ring slightly disturb its magnetic state, and the disturbance is indicated by the sound produced in a telephone in series with a second coil wound on the ring. He finds such a detector very convenient and satisfactory in working.

Prof. Minchin has found that a coherer consisting of a carbon rod lightly supported in aluminium stirrups in an evacuated glass tube decoheres better than any other form he has tried, and is now engaged in applying the arrangement to long-distance transmission.

Dr. Marchant showed that the graphical method of determining the discharge of a condenser through a variable inductance gave results which agreed very closely with the calculated discharge in those cases in which the calculation could be carried out.

Mr. Butler-Burke gave a short account of his work on the phosphorescence produced in partially exhausted tubes by the passage of an alternating current round them. He concludes that it is due to the formation of groups consisting of a large number of molecules of gas within the tube.