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SOME PROBLEMS OF COSMICAL PHYSICS, SOLVED AND UNSOLVED[1]

By LORD RAYLEIGH, F.R.S.

OF the activities of our section, the Cape has perhaps been more identified with astronomy than with any other branch. In the middle of the eighteenth century, when exact astronomy of the southern hemisphere may be considered to have begun, there were few, if any, other places in a considerable southern latitude where an astronomer could work in safety with the necessary help of trained artisans. This tradition worthily begins with Lacaille (1750–51). Other landmarks were the foundation of the Cape Observatory (1821), the expedition of Sir John Herschel (1833–38) and the forceful and energetic career of Sir David Gill, who was the life and soul of our organization on its visit to South Africa in 1905. Shortly afterwards he retired, and I then had the privilege of friendship with him in London. Indeed, I have taken these few facts and dates from the copy of his "History of the Cape Observatory," which he gave me very shortly before his death. Although past his prime at the time I knew him, he was still vigorous and keenly interested in scientific developments, though if one brought anything new to his notice a severe cross-examination as to the validity of the evidence had to be faced.

It is partly on account of this association of South Africa with astronomy that I have chosen to lean as far towards this direction as I feel able, and to pass in review some subjects lying on the border-line between astronomy and physics.

After the first period of success in identifying the origin of the spectral lines of the sun and stars with

[1] Address of the president of Section A—Mathematical and Physical Sciences, British Association for the Advancement of Science, Cape Town, South Africa, July 24, 1929.

terrestrial materials, certain outstanding cases remained which were obviously important but in which the identification could not readily be made.

The first of these cases to yield was that of helium, which was unraveled while some of the pioneers in astronomical spectroscopy were still active. Although in my youth I was privileged to see the discovery of helium at close quarters, I shall not go back so far. When we hear of the gas being used in millions of cubic feet for inflating large airships, we have to realize that its discovery is an old story.

Kindred to the hypothesis of helium, so triumphantly vindicated by terrestrial experience, were the hypotheses of nebulium, geocoronium and coronium. The problems epitomized by the two former words have now been solved, though the solution has taken quite a different turn from what was expected by the older generation of astrophysicists.

THE NEBULAR SPECTRUM

In the nebulae are spectrum lines which have never been observed terrestrially. These are not faint members of otherwise complex spectra, such, for instance, as we have in nearly all remaining unidentified lines of the solar spectrum, but they stand out, bold and challenging, on a dark background, presenting a puzzle that was the more intriguing from its apparent simplicity. According to spectroscopic experience, now made precise and rational, simple spectra are due to light elements. This, taken with the fact that lines known to be due to hydrogen and helium accompanied the nebular lines, strongly suggested that they too were due to light elements of the class which terrestrially are known as permanent gases. But the fact remained that no one had succeeded in observing them in the laboratory, and as time went on the originally convenient resource of relegating them to an unknown element had become less convenient. For the scheme of the elements became definite, and there was no room in it for new light elements. This was one of the many cases in science where the method of frontal attack has been exhausted in vain. More systematic knowledge of spectra in general, and of the spectra of the light elements in particular, was wanted before the question could be resolved.

The clue was afforded by the circumstance that important nebular lines occur in pairs, obviously associated by their closeness and their constant relative intensity in different nebulae and in different parts of the same nebula. The consideration of such pairs or multiplets has more than once proved an advantageous point of attack on spectroscopic problems. It was in this way that Hartley, examining the diffuse triplets of magnesium, first established the constancy of frequency intervals, thus suggesting for the first time that addition and subtraction of frequencies was the proper method of analyzing spectra—an idea which appeared at that time sufficiently paradoxical. Again the recognition of the frequency intervals of multiplets afforded the clue by which complex spectra such as manganese and iron were first unraveled.

It is found then that the frequency difference of the green pair of lines originally discovered by Huggins, and known as N_1 and N_2, is 193 waves per centimeter. I. S. Bowen, to whom we owe the final elucidation of this enigma, sought for an equal interval in the spectrum of doubly ionized oxygen which he was analyzing and found it in the interval between the low-lying levels designated as I^3P_2 and I^3P_1.

This is hardly enough in itself to establish the suggested origin; to do that it is necessary to fix, not only the interval between the nebular lines, but their position as well. The lines were attributed to intercombination between one singlet upper level and two lower levels belonging to a triplet, the third being excluded by the rule of inner quantum numbers. To fix the differences of the terms concerned it was necessary to connect the singlet and triplet levels by an intercombination line observed in the laboratory spectrum of doubly ionized oxygen. This was done by A. Fowler, who, combining Bowen's laboratory data with his own, was able to get a fairly satisfactory check on the observed position of the nebular pair. Practically no doubt remains, in view of the fact that other less well-known nebular lines can be similarly explained as due to singly ionized nitrogen and singly ionized oxygen.

The identification of these lines was made by ignoring so far as convenient the rules of the quantum theory which had been evolved from laboratory experience, and given some theoretical basis by Bohr and his followers. These rules forbid certain lines which might occur according to the combination principle. When a state of excitation of the atom is such that it can not directly pass to a lower state without breaking one of these rules, that state is called metastable; and this is the case which we have in the nebular lines. I shall return presently to the consideration of metastable states and "forbidden" lines.

THE AURORAL SPECTRUM

The next cosmical problem I wish to refer to is the long outstanding one of the green line of the aurora. This was first seen by A. J. Ångström at Upsala in 1868, and he recorded the observation in one of the supplementary notes at the end of his great paper in which an extensive scale of wavelengths for the solar spectrum was first established. In this case the enigmatic line is even more isolated than in the case of the nebulae, since, except in the case of unusually bright auroras, one can see nothing else in the spectrum at all. For some years I took every available opportunity of looking at this spectrum, and never did so without a deep sense of mystery. The origin of the line was not in this case in the depths of space, but in our own atmosphere at the distance of a short railway journey from the observer. Yet an apparently exhaustive study of the spectra to be obtained from terrestrial gases by the combined efforts of very many experimenters gave no clue to its origin.

As is well known the clue was eventually found by McLennan, who was able to produce the line by heavy electric discharges in a mixture of oxygen and helium, or, better, oxygen and argon. Oxygen is essential, and there is now no doubt that the aurora line is an oxygen line, but the function of the inert gas is not very clear, though various more or less plausible guesses may be made. To have established that the line is due to oxygen is an immense step forward. There is, however, yet more to be done, for we do not know how to get the green line alone or with only the negative nitrogen bands as we see it in the sky. In the artificial spectrum the ordinary oxygen lines and the lines of the inert gas, helium or argon as the case may be, are conspicuous.

The wave-length of the auroral line could not be foreseen or calculated from our present knowledge of the arc spectrum of oxygen. In this case we have only a single line to deal with, and are thus without the invaluable clue afforded in the case of the nebulae by the frequency separations of a doublet or triplet. There is, however, no difficulty in finding a conjectural place for it in the scheme of the oxygen arc spectrum as given by Hund's theory. This theory, which may be regarded as a generalization of all our knowledge of line spectra, affords a kind of frame into which we may confidently hope to fit new empirical knowledge as it accumulates.

McLennan, arguing from the fact that nitrogen bands do not appear in the spectrum of the night sky, which, however, shows the green line, takes the excitation potential as less than 11.5 volts. This condition excludes very many possibilities. Indeed, if we are to be bound by the selection rules, it excludes *all* the possibilities. So with the example of the nebulae before him, McLennan waives these rules, and assigns the green line to a transition from one or other of the low-lying metastable states which the theory indicates.

The lowest state of all should be a triplet, and owing to the absence of companions to the green line this may very probably be excluded.

If so, only one alternative remains, and the successful determination of the Zeeman effect carried out in McLennan's laboratory is in harmony with the choice so arrived at. An independent investigation by L. H. Sommer, published immediately afterwards, covered exactly the same ground and led him to the same choice. This is satisfactory so far, but the position will be much strengthened and consolidated when we have an independent determination of the levels in question, giving the means of calculating a theoretical wave-length for comparison with that observed. To do this will require a fuller survey of the Schumann region of the arc spectrum than has yet been made. For the aurora line we have the experimental production from oxygen but not the numerical spectroscopic relation. For the nebular lines our position is exactly the reverse.

The origin of the green auroral line has thus been definitely cleared up, at all events in so far that it is attributable to the arc spectrum of oxygen. There are, however, other features of the auroral spectrum which are still obscure. I will limit myself to discussion of one of them—the red line of the aurora. Red auroras are comparatively rare, and when they do occur the distribution of color presents very curious features. In some cases the ends of the streamers are tipped with red, while the greater part of the length is green. The only reddish aurora which I have been privileged to observe at my home in the south of England (May 14, 1921) was of a different character, the color ranging rapidly through various shades of purple. The light was distributed in irregular patches high up near the zenith, though predominantly in the north. At the same time its position was highly unstable, and the general impression produced was reminiscent of high potential discharges in highly exhausted vacuum tubes. Vegard has described cases where the whole sky suddenly turned crimson. He has obtained good small-scale spectrograms of the red line, which give the position as $\lambda 6322$, which, however, is subject to a probable error of at least $\pm 1A°$. A determination by V. M. Slipher, of the Lowell Observatory, gave $\lambda 6320$.

So far as can be judged from the evidence available, no pair of the low-lying levels of the oxygen arc scheme which McLennan has discussed in connection with the aurora are suitably placed to yield this red line by combination. We naturally turn to the consideration of nitrogen spectra, which, as is well known, are represented in the blue and violet regions of the auroral spectrum.

I described in 1922 a spectrum in which one of the first positive bands of nitrogen $\lambda 6323$ was very much intensified relative to the neighboring red bands, which ordinarily are of comparable brightness. This spectrum was produced by adding a large excess of helium to the nitrogen afterglow, and the source had a visual red color dominated by this band. In describing this work it was suggested as a possibility that this was the origin of the red auroral line, and somewhat similar ideas have been revived by McLennan in his recent Bakerian lecture. But there are difficulties to be met. Photographically two yellow nitrogen bands come in with intensity equal or superior to the red one, and these have no counterpart in auroral spectra. Moreover, the wave-length data for the red auroral line are far from being accurate enough for an identification depending on a single coincidence only. One of the most urgent problems in auroral work is an adequate wave-length determination of this red line from a large-scale spectrogram.

Coronium

A problem which has generally been classed with those we have been discussing is that of the lines in the sun's corona, attributed to a hypothetical coronium. In the light of our present knowledge it is not probable, perhaps we may say not possible, that an unknown element coronium exists. Attempts have not been wanting to identify these lines with known elements. The latest is by Freeman, working in the Ryerson Laboratory of Chicago, who seeks to attribute the lines to argon. He thinks, for instance, that the strong visual green line, from which the conception of coronium arose, may result from two different transitions in the argon atom, being in reality double. One of his proposed transitions would give the fifth line of a possible series, and the other the ninth member of an actual series. But none of the earlier members of either of these series is seen in the corona, and this seems fatal to the identification proposed. We could not assign an observed line at $\lambda 3771$ as H_ι (H iota) if H_α, H_β and H_γ and the other earlier members of the series were missing, yet this would be an analogous case.

I think we must consider the origin of the strong lines of the corona as an unsolved problem. The possibility of their being in reality heads of molecular bands must be kept in view.

Excitation of the Various Spectra

We have discussed these cosmical spectra so far chiefly from the standpoint of the spectroscopist. It will now be of interest to consider the probable mode of excitation of some of them.

Let us consider first the polar aurora; this, as is well known, is closely bound up with exceptional conditions of magnetic disturbance, and these in turn are conditioned by solar influence. As regards the nature of this influence, the theory of Birkeland, elaborated by Störmer, still holds the field. The sun was regarded by them as emitting localized streams of electrically charged particles from limited areas of its surface.

The unrivaled advantages of this theory are that it allows the solar action to be emitted in a highly specialized direction, thus accounting for the sudden commencements of magnetic storms all over the globe, and their tendency to recur after the twenty-seven days of a solar rotation have passed, and further, that by the earth's magnetic field the action can be got round to the night side of the earth. But this theory in its original simplicity has required a good deal of patching, and it is difficult to feel much satisfaction with the special *ad hoc* hypotheses which have had to be introduced into it.

A stream of particles with a charge of one sign only is open to the criticism, first put forward by Schuster, that the stream will dissipate itself by electrostatic repulsion, and loses the hard outline which is one of the most essential features. Lindemann has proposed to get over the difficulty by making the stream neutral on the whole, still consisting, however, of charged particles of both signs. Here, however, we lose too much of the magnetic flexibility of the stream. Chapman proposes to retain a slight excess of charge of one sign, and in this way is able to arrive at a tolerable compromise. But one feels that more experimental guidance is badly needed before we can venture with confidence into these theoretically dark regions. The search for direct evidence might not seem at first sight very hopeful, but not long ago a sensational suggestion was made by Störmer. His attention was drawn by Hals to echoes heard after short-wave (131 meters) wireless signals sent out from Eindhoven in Holland. These echoes have been found by Störmer and Hals at long intervals up to as much as fifteen seconds after the original reception.

Now, if we bear in mind that with the velocity of light the longest terrestrial distances only give intervals about one seventh of a second, it seems inevitable that some extra-terrestrial reflector should be looked for. Störmer finds this in the corpuscular stream as bent round by the earth's magnetic force. Though the boldness of the idea is staggering, it is difficult to suggest any alternative view. Störmer states that "the variability of the phenomenon indicated by the observations agrees well with the corresponding variability of aurora and the magnetic registrations."

T. L. Eckersley has made an observation on electrical disturbances of natural origin which he interprets as analogous to Störmer's. A click is heard in a telephone attached to a large aerial, which is followed at an interval of about three seconds by a "whistler" or musical note of short duration. Further whistlers follow at intervals of 3.8 seconds, each more drawn out than the previous. The musical notes are regarded as due to the spreading action of a dispersive medium on an electrical impulse. It is only at times of magnetic storm that these phenomena are frequent.

Further development of observations of this kind will be awaited with keen interest.

To return to the nebular spectrum: although the main problem has been cleared up in the way described, it would still be an important step to imitate the spectrum in the laboratory, not so much to confirm the origin of the lines as to get direct information about the conditions under which they may be excited. No success has yet been obtained in this direction, but it is fairly clear how the attempt should be made. We must have conditions capable of exciting the lines of doubly ionized oxygen and attempt to work in a large volume at high rarefaction.

A large volume and high rarefaction (rarity of collisions) is suggested by the nebular conditions, and was plausibly held by Bowen to be an essential. It must be allowed, however, that such experimental evidence as we have at present on passage downwards from metastable states does not definitely point in this direction.

In such attempts what Darwin called "fool experiments" and what prospectors for oil call "wild-catting" are not to be discouraged. Indeed, many of the most fruitful discoveries are really made in this way. The logic is put in afterwards. That is what happened in the case of the three-electrode thermionic valve.

Thanks to the work of Wright, Hubble and others the source of excitation in the bright line nebulae no longer appears inexplicable. We have the cardinal fact that in nearly all cases stars of early type, capable of affording radiations of high frequency, are involved in the nebulae. The two or three apparent exceptions, though deserving of the closest scrutiny, do not at present seem to have enough weight to upset a generalization which rests on a great number of cases. It is true that we can not observe these short waves, the maximum of intensity in the spectrum being hidden from our view by the layer of ozone overhead, of which I shall say more presently. But we can confidently infer their existence by extrapolating from what we can see and correcting for what we know of atmospheric absorption. The cases of some of the central nuclei of the planetary nebulae are specially satisfying from the definite relation of the star to the nebula and the adequate character of the star itself. W. H. Wright, writing in 1918, before these views had emerged, and without any thesis to maintain, expressed himself as follows:[2]

> I can not but believe that this wonderful richness in ultra-violet light which gives the spectra of nebular nuclei their characteristic appearance, in spite of the great difference which they exhibit in the matter of bright bands, is the dominating peculiarity which must be regarded as the distinguishing mark of this group of objects.

[2] Lick Observatory, vol. xiii, p. 252, 1918.

It has been suggested that the general penetrating cosmic radiation of which we have heard so much of late stimulates the nebular spectrum, but upon the facts available this hypothesis hardly seems necessary or helpful.

THE DARK PATCHES IN THE NEBULAE

There is another aspect of the diffuse galactic nebulae which remains obscure in more senses than one. It is seen to special advantage in such objects as the "trifid" nebulae in Sagittarius. Dark regions are, as it were, interlarded with the bright ones in a way which strongly suggests that we have to do with complementary aspects of the same phenomenon somewhat in the same way that, for instance, the emission of a fluorescent body is connected with its absorption. Yet it is very difficult to pursue this line of thought into satisfactory detail. The opacity is quite unrelated to the emission, and indeed it presents the baffling peculiarity of having no peculiarity. For apparently every part of the spectrum of the stars lying beyond is obscured in the same ratio. Experimenters in the field of optics know how difficult it is to secure a result of this kind in the laboratory, particularly when the ultra-violet spectrum has to be included.

Even fairly opaque gases like iodine vapor which are at our disposal show markedly selective absorption, and in terrestrial experiment recourse is usually had to the partial action of a solid obstruction such as a spinning sector or a wire gauze not seen in focus. The astronomical equivalent of these devices is a swarm of meteorites, and it may be necessary to invoke their aid, but the rare gaseous atmosphere required to give the line spectrum of hydrogen, helium, nitrogen and oxygen can not be considered to blend harmoniously with a swarm of meteorites or to have anything like a complementary relation to it, and it is particularly difficult to understand from this point of view how it can come about that the bright nebular lines are often seen on a profoundly dark background almost or quite free from continuous spectrum.

COMETS

A kindred problem is that of the luminosity of comets. This has been discussed by Zanstra in a recent paper.[3] He takes the view that the Swan bands of carbon are resonance bands excited by light from the sun in the visual spectrum, the gases being at an ordinary temperature such as prevails at the earth's surface. If these are really the conditions, the prob-

[3] M. N., December, 1928.

lem of imitating the comet seems ideally easy from the laboratory point of view. The Swan spectrum should appear in absorption of suitable carbonaceous gases contained in a vessel at the ordinary temperature, and it should be observable in lateral emission. I can not help thinking that if nothing more than this was necessary, the thing would have been done before now. In the case of the D line sodium, treated by Zanstra as quite analogous, it has of course been done long ago in the phenomenon described by R. W. Wood, and called resonance radiation.

METASTABLE STATES

In discussing the nebular and auroral spectra, we encountered the idea of "metastable states." At present this conception is not in a very satisfactory condition. The original idea was of a state which did not allow of direct transition by emission of radiation to the stable ordinary state. Let us compare the level of the atom to the stories of a building and the optical electron to a man inside the building. The ordinary state of the atom is represented by the man being on the ground floor, and the metastable state by placing him on the first floor. But the internal architecture of our building must be pictured as peculiar. A staircase connects the first floor with the second floor, and another staircase connects the second floor with the ground floor: but there is no connection between the first floor and the ground floor except by going up higher and coming down again.

Such, I say, was the original conception, but facts which have since come to light require some revision of it.

In the nebulae the electron manages somehow to escape from its prison-house, and descend to the level below not by the legitimate route of going upstairs and down again, but by illicitly breaking through the floor, contrary to the rules of the establishment.

Abandoning the metaphor, and the attempt to use popular language, the selection rule which forbids transitions not involving a change in the azimuthal quantum number is violated in all such cases. The inner quantum number rule, which requires that the inner quantum number should not change from 2 to 0 or from 0 to 0 is also violated in one class of cases, and rather meticulously observed in another. This rule permits only the pair of green nebular lines in doubly ionized oxygen which we have discussed; and in deference to it only two are observed, instead of the three which apart from this might have been expected from the triplet ground state.

Yet we find the blue singlet line $\lambda 4363$ of this ion violating the same rule, and the same applies to the analogous case of the aurora line, if we adopt McLennan's view of its position in the scheme of the arc spectrum.

In the case of the mercury spectrum, which lends itself well to experimental observation and of which much detail is known, we have laboratory examples of the violation of this rule, as originally shown by experiments of Takamine, Fukuda and other Japanese physicists. The lines were originally obtained under conditions where a strong electric field was acting, and this was sometimes urged in mitigation for breaking the rule. Again, the lines were of low intensity, and this too was thought to be a partial excuse.

Whatever might have been thought of these apologies originally their irrelevance was, I think, clearly shown in some experiments of my own, in which one of the "forbidden" mercury lines was obtained as the second strongest line in the entire mercury emission spectrum, in the vapor passing through a discharge, but altogether away from the region in which the discharge itself was taking place, and consequently in the absence of an extraneous electric field.

In another experiment I was able to obtain the other forbidden line as an absorption line in unexcited mercury vapor, and thus apparently in the absence of any disturbing conditions. In this experiment the quantity of vapor used was very large, about ten million times the amount required to bring out the resonance line of mercury in absorption. The probability of the transition thus indicated is very low, and for the other forbidden line it is apparently still lower. But for all that, as we have seen, this forbidden line can be got in considerable intensity in emission. The necessary condition in the mercury experiments appears to be a large accumulation of mercury atoms in the relevant metastable state, so that even with a low probability of transition for the individual excited atom a considerable number of transitions occur.

It has even been proposed to define a metastable state as one with a low probability of transition. This takes us far from the original conception, and makes "metastability" merely a question of degree. Some recent results which I hope to bring before the section at a later stage in our proceedings seem to indicate that even the normal excited state may possibly persist for a much longer time than has hitherto been supposed. If this conclusion is accepted, a far-reaching revision of our present notions may become necessary. The general softening of outline in our picture of atomic events resulting from the substitution of wave groups for particles seems likely to afford what is required and to allow the occasional transition downwards from a metastable state.

OZONE

The case of the nebular spectrum affords an illustration of how spectroscopic theory, working on laboratory data gained in the remote ultra-violet region, enables us to some extent to turn the difficulties which arise from our inability to examine this region in celestial spectra. The veil of atmospheric ozone overhead cuts off the spectra of the sun and stars and thus hides much of the ultra-violet, constituting a great obstacle to astrophysical research. On the other side of the account we may remember that it protects our persons from the harmful ultra-violet rays, and that without it we might not be here to conduct research at all. It has been suggested by Cario, R. W. Wood and others that on the view that atmospheric ozone is generated by the absorption of short wavelengths in the sun's spectrum, it may be absent in the Arctic during the polar night. This possibility has been put to the test by Rosseland, but with negative results. It is doubtful whether his station was far enough away from the sunlight to make his result absolutely final. But other omens are unfavorable. Thus Chalonge has found that the amount of ozone present in the night (using the moon as a source) is notably more than by day; and Dobson, Harrison and Lawrence have found that when the meteorological conditions are such as to bring air from the Arctic, the ozone content goes up, and that this is particularly marked in spring, when the Arctic has been without sunlight for months. The point is, however, of great interest in itself, and makes the question acute of how the ozone is generated. For in view of these facts it seems hard, as Dobson has pointed out, to regard it as the product of the sun's ultra-violet radiation.

No search seems yet to have been made for ozone in the planetary atmospheres. In the cases of Mars, Jupiter and Saturn, at all events, the problem is not at first sight specially difficult. It would not be easy to establish a positive result, however, unless the atmospheres of these planets possess an ozone stratum at least comparable in effective thickness with the terrestrial one.

POSSIBILITY OF UNKNOWN ELEMENTS OF HIGH ATOMIC WEIGHT

Although we are no longer at liberty to postulate unknown light elements, we are free up to the present to postulate heavier ones than any known terrestrially. Jeans, as is well known, has made use of this hypothesis to explain the origin of stellar energy. In common with other authorities he provides it by the destruction of matter, with radiation of the equivalent quantity of energy (MC^2) demanded by the theory of relativity. So far there seems to be fairly general agreement. The difficulties arise when we come to the question of stability, and here agreement is not general. Jeans considers that the source must liberate energy at a rate independent of the temperature. I am not qualified, and shall not attempt, to discuss this point. The object of postulating unknown heavy elements is to endow them with the property of going out of existence spontaneously at a rate which is independent of external condition, except in so far as ionization, which involves the removal of some of the electrons from the neighborhood of protons, tends to hinder the process.

In the known radioactive elements we have, of course, instances of unstable forms of matter, and Jeans regards these as transitional, but it must be admitted that substances which undergo spontaneous disintegration do not at first sight form an altogether satisfactory halfway house between those which are quite stable on the one side and those which spontaneously go out of existence on the other. Then we have to explain why these heavy atoms are not found on the earth, which, it is generally agreed, originally formed part of the same mass as the sun. Jeans mentions this difficulty, and gives reasons for thinking that the heavy elements would sink to the interior of the mass, so that the earth, formed from the exterior part of it, would not contain them. That a *vera causa* is here appealed to can not be doubted, but there seem to be some difficulties in assuming that it operates with enough precision to secure the desired result.

The list of known elements ends with uranium, and we must notice that the occupants of the ninety-two places up to and including uranium in the list nearly all answer to their proper numbers when the roll is called. The only exceptions are 85 and 87. And he would be a rash philosopher who attached much importance to these vacant places, which may be filled up any day. Roughly speaking, we may say that the elements up to uranium are all present, and the higher members assumed to exist in the stars are all absent. It is putting a heavy burden on the mechanism of gravitational separation to expect it to achieve this result. The inventors of ore-dressing machinery would, I should imagine, despair of accomplishing anything like it.

Nature works on a vast scale and with plenty of time at her disposal, and it may well be urged that we must be careful of measuring her possible achievements by our own. We may ask, however, whether the more direct indications available suggest that she has in fact made this separation. If there is this cut between the atomic numbers 92 and 93, we should expect most of 92 to have gone into limbo in order to ensure *the whole* of 93 having done so. Yet 92 (uranium) is a relatively abundant element compared with most, being in fact number 25 on the list of abundance in igneous rocks, according to the estimate of Clark and Washington. Again, we happen to be in a position to say that on the earth, at least, uranium, so far from having sunk to the center, is concentrated near the surface. This is inferred from the known outflow of heat from the earth, which is difficult to reconcile with the observed amount of radioactive matter near the surface and impossible to reconcile with the existence of a comparable amount in the interior.

Assuming that uranium exists on the sun as on the earth, then, as first pointed out by Lindemann, there are strong grounds for thinking that it must be in course of formation there, for the life of uranium is too short in comparison with the probable age of the sun to allow us to suppose otherwise. Those who remember the early development of radioactivity will recall that a parallel argument was successfully used by Rutherford to prove that radium must have originated on the earth before the fact was directly proved that it is being generated here. Radium (it was later shown) is generated from a parent body of higher atomic weight, namely uranium. Jeans would regard the origin of uranium itself as analogous, and if this analogy is accepted it would require the presence of an element of still higher atomic weight, capable of undergoing radioactive disintegration, but, it is to be observed, incapable, *ex hypothesi,* of dissolving entirely into radiation.

No doubt these are very deep waters, and we can hardly expect at present to fathom them. What would really be most helpful would be a theory of atomic structure in sufficiently definite agreement with experiment as regards known elements to enable us to proceed to investigate the properties of elements of higher number than 92 with confidence. On the general question of whether the evolution of elements has proceeded from the simple to the complex, or from the complex to the simple, it does not seem to me very much to the purpose to appeal to evolutionary doctrine and the analogy of organic evolution, in favor of the former alternative. Is it not more to the point that the only cases we can observe (radioactive changes and those induced by radioactive bombardment) are of the latter class? At present this is a question of scientific taste. Perhaps it is not irrelevant to remark that even in organic evolution degeneration of organisms sometimes occurs, and I do not know whether our biological colleagues are in a position to assert that the whole course of organic evolution may not at some future time be reversed by a change of conditions. At all events, it is something to have formulated the more restricted question of whether uranium now comes into being on the sun by a synthetic or an analytic process. It would seem that this is a well-framed question, and that the answer can hardly be either *both* or *neither.*

CONCLUSION

The great success of theoretical investigations in recent times naturally leads enterprising spirits to use them not only in interpreting what we know or can verify by observation, but to lead us into regions where experiment is not available as a check. I believe that this does nothing but good in times like ours, when there is no danger of the doctrines even of a master being unduly pressed, if the evidence of observed fact turns against them. At the same time, we must not expect too much of pure intellect unchecked by observation. Theories that do not stand the test of time pass for the most part into complete oblivion, and we are apt to forget how appallingly large a mass of wreckage the total of them represents. The next generation remembers chiefly those that survive, and does not take full advantage of the lesson of how easy it is for an apparently inevitable conclusion to be wrong. Unless the argument carries its own verification by some accurate and previously unforeseen numerical coincidence, it is hard indeed to tell if we are on the right track.

Though some of the problems we have been discussing have been only partially or not at all resolved, yet many possible points of approach are opening to our view.

The attack on nature's secrets is now conducted along a long line of battle. No sooner does the defense show signs of crumbling at any point than an eager crowd of combatants, not restrained by any undue respect for the traditional modes of scientific thinking, are ready to throw themselves into the breach. The great array of trained workers in pure science existing in the modern community is powerfully reinforced by workers in applied science, who are backed by the resources of the industrial and financial world and hand back to the physical laboratory the devices which had their birth there in a form infinitely strengthened in power and convenience of application. Thus rearmed with weapons of greater power and precision, pure science advances again to the attack of fresh territory, and so the process goes on at an ever-accelerating rate. How long this acceleration is destined to continue it is impossible to say. It shows few signs of abating at present. But for all that I for one am not afraid that our successors will be able to complain that we have left them no more worlds to conquer.