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THE INTERNAL CONSTITUTION OF THE STARS[1]

LAST year at Bournemouth we listened to a proposal from the president of the association to bore a hole in the crust of the earth and discover the conditions deep down below the surface. This proposal may remind us that the most secret places of nature are, perhaps, not 10 to the n-th miles above our heads, but 10 miles below our feet. In the last five years the outward march of astronomical discovery has been rapid, and the most remote worlds are now scarcely safe from its inquisition. By the work of H. Shapley the globular clusters, which are found to be at distances scarcely dreamed of hitherto, have been explored, and our knowledge of them is in some respects more complete than that of the local aggregation of stars which includes the sun. Distance lends not enchantment but precision to the view. Moreover, theoretical researches of Einstein and Weyl make it probable that the space which remains beyond is not illimitable; not merely the material universe, but space itself, is perhaps finite; and the explorer must one day stay his conquering march for lack of fresh realms to invade. But to-day let us turn our thoughts inwards to that other region of mystery—a region cut off by more substantial barriers, for, contrary to many anticipations, even the discovery of the fourth dimension has not enabled us to get at the inside of a body. Science has material and non-material appliances to bore into the interior, and I have chosen to devote this address to what may be described as analytical boring devices—*absit omen!*

The analytical appliance is delicate at present, and, I fear, would make little headway against the solid crust of the earth. Instead

[1] Address before the Mathematical and Physical Science Section of the British Association for the Advancement of Science.

of letting it blunt itself against the rocks, let us look round for something easier to penetrate. The sun? Well, perhaps. Many have struggled to penetrate the mystery of the interior of the sun; but the difficulties are great, for its substance is denser than water. It may not be quite so bad as Biron makes out in "Love's Labour's Lost":

> The heaven's glorious sun;
> That will not be deep-searched with saucy looks;
> Small have continual plodders ever won
> Save base authority from others' books.

But it is far better if we can deal with matter in that state known as a perfect gas, which charms away difficulties as by magic. Where shall it be found?

A few years ago we should have been puzzled to say where, except perhaps in certain nebulæ; but now it is known that abundant material of this kind awaits investigation. Stars in a truly gaseous state exist in great numbers, although at first sight they are scarcely to be discriminated from dense stars like our sun. Not only so, but the gaseous stars are the most powerful light-givers, so that they force themselves on our attention. Many of the familiar stars are of this kind—Aldebaran, Canopus, Arcturus, Antares; and it would be safe to say that three quarters of the naked-eye stars are in this diffuse state. This remarkable condition has been made known through the researches of H. N. Russell and E. Hertzsprung; the way in which their conclusions, which ran counter to the prevailing thought of the time, have been substantiated on all sides by overwhelming evidence, is the outstanding feature of recent progress in stellar astronomy.

The diffuse gaseous stars are called *giants*, and the dense stars are called *dwarfs*. During the life of a star there is presumably a gradual increase of density through contraction, so that these terms distinguish the earlier and later stages of stellar history. It appears that a star begins its effective life as a giant of comparatively low temperature—a red or M-type star. As this diffuse mass of gas contracts its temperature must rise, a conclusion long ago pointed out by Homer Lane. The rise continues until the star becomes too dense, and ceases to behave as a perfect gas. A maximum temperature is attained, depending on the mass, after which the star, which has now become a dwarf, cools and further contracts. Thus each temperature-level is passed through twice, once in an ascending and once in a descending stage—once as a giant, once as a dwarf. Temperature plays so predominant a part in the usual spectral classification that the ascending and descending stars were not originally discriminated, and the customary classification led to some perplexities. The separation of the two series was discovered through their great difference in luminosity, particularly striking in the case of the red and yellow stars, where the two stages fall widely apart in the star's history. The bloated giant has a far larger surface than the compact dwarf, and gives correspondingly greater light. The distinction was also revealed by direct determinations of stellar densities, which are possible in the case of eclipsing variables like Algol. Finally, Adams and Kohlschütter have set the seal on this discussion by showing that there are actual spectral differences between the ascending and descending stars at the same temperature-level, which are conspicuous enough—when they are looked for.

Perhaps we should not too hastily assume that the direction of evolution is necessarily in the order of increasing density, in view of our ignorance of the origin of a star's heat, to which I must allude later. But, at any rate, it is a great advance to have disentangled what is the true order of continuous increase of density, which was hidden by superficial resemblances.

The giant stars, representing the first half of a star's life, are taken as material for our first boring experiment. Probably, measured in time, this stage corresponds to much less than half the life, for here it is the ascent which is easy and the way down is long and slow. Let us try to picture the conditions inside a giant star. We need not dwell on the vast dimensions—a mass like that of the sun, but swollen to much greater volume on account of the low density, often below that of our own atmos-

phere. It is the star as a storehouse of heat which especially engages our attention. In the hot bodies familiar to us the heat consists in the energy of motion of the ultimate particles, flying at great speeds hither and thither. So too in the stars a great store of heat exists in this form; but a new feature arises. A large proportion, sometimes more than half the total heat, consists of imprisoned radiant energy—ether-waves travelling in all directions trying to break through the material which encages them. The star is like a sieve, which can only retain them temporarily; they are turned aside, scattered, absorbed for a moment, and flung out again in a new direction. An element of energy may thread the maze for hundreds of years before it attains the freedom of outer space. Nevertheless the sieve leaks, and a steady stream permeates outwards, supplying the light and heat which the star radiates all round.

That some ethereal heat as well as material heat exists in any hot body would naturally be admitted; but the point on which we have here to lay stress is that in the stars, particularly in the giant stars, the ethereal portion rises to an importance which quite transcends our ordinary experience, so that we are confronted with a new type of problem. In a red-hot mass of iron the ethereal energy constitutes less than a billionth part of the whole; but in the tussle between matter and ether the ether gains a larger and larger proportion of the energy as the temperature rises. This change in proportion is rapid, the ethereal energy increasing rigorously as the fourth power of the temperature, and the material energy roughly as the first power. But even at the temperature of some millions of degrees attained inside the stars there would still remain a great disproportion; and it is the low density of material, and accordingly reduced material energy per unit volume in the giant stars, which wipes out the last few powers of 10. In all the giant stars known to us, widely as they differ from one another, the conditions are just reached at which these two varieties of heat-energy have attained a rough equality; at any rate one can not be neglected compared with the other. Theoretically there could be conditions in which the disproportion was reversed and the ethereal far out-weighed the material energy; but we do not find them in the stars. It is as though the stars had been measured out—that their sizes had been determined—with a view to this balance of power; and one can not refrain from attributing to this condition a deep significance in the evolution of the cosmos into separate stars.

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Study of the radiation and internal conditions of a star brings forward very pressingly a problem often debated in this section: What is the source of the heat which the sun and stars are continually squandering? The answer given is almost unanimous—that it is obtained from the gravitational energy converted as the star steadily contracts. But almost as unanimously this answer is ignored in its practical consequences. Lord Kelvin showed that this hypothesis, due to Helmholtz, necessarily dates the birth of the sun about 20,000,000 years ago; and he made strenuous efforts to induce geologists and biologists to accommodate their demands to this time-scale. I do not think they proved altogether tractable. But it is among his own colleagues, physicists and astronomers, that the most outrageous violations of this limit have prevailed. I need only refer to Sir George Darwin's theory of the earth-moon system, to the present Lord Rayleigh's determination of the age of terrestrial rocks from occluded helium, and to all modern discussions of the statistical equilibrium of the stellar system. No one seems to have any hesitation, if it suits him, in carrying back the history of the earth long before the supposed date of formation of the solar system; and in some cases at least this appears to be justified by experimental evidence which it is difficult to dispute. Lord Kelvin's date of the creation of the sun is treated with no more respect than Archbishop Ussher's.

The serious consequences of this contraction hypothesis are particularly prominent in the case of giant stars, for the giants are prodigal

with their heat and radiate at least a hundred times as fast as the sun. The supply of energy which suffices to maintain the sun for 10,000,000 years would be squandered by a giant star in less than 100,000 years. The whole evolution in the giant stage would have to be very rapid. In 18,000 years at the most a typical star must pass from the initial M stage to type G. In 80,000 years it has reached type A, near the top of the scale, and is about to start on the downward path. Even these figures are probably very much overestimated. Most of the naked-eye stars are still in the giant stage. Dare we believe that they were all formed within the last 80,000 years? The telescope reveals to us objects not only remote in distance but remote in time. We can turn it on a globular cluster and behold what was passing 20,000, 50,000, even 200,000 years ago —unfortunately not all in the same cluster, but different clusters representing different epochs of the past. As Shapley has pointed out, the verdict appears to be "no change." This is perhaps not conclusive, because it does not follow that individual stars have suffered no change in the interval; but it is difficult to resist the impression that the evolution of the stellar universe proceeds at a slow, majestic pace, with respect to which these periods of time are insignificant.

There is another line of astronomical evidence which appears to show more definitely that the evolution of the stars proceeds far more slowly than the contraction hypothesis allows; and perhaps it may ultimately enable us to measure the true rate of progress. There are certain stars, known as Cepheid variables, which undergo a regular fluctuation of light of a characteristic kind, generally with a period of a few days. This light change is *not* due to eclipse. Moreover, the color quality of the light changes between maximum and minimum, evidently pointing to a periodic change in the physical condition of the star. Although these objects were formerly thought to be double stars, it now seems clear that this was a misinterpretation of the spectroscopic evidence. There is in fact no room for the hypothetical companion star; the orbit is so small that we should have to place it inside the principal star. Everything points to the period of the light pulsation being something intrinsic in the star; and the hypothesis advocated by Shapley, that it represents a mechanical pulsation of the star, seems to be the most plausible. I have already mentioned that the observed period does in fact agree with the calculated period of mechanical pulsation, so that the pulsation explanation survives one fairly stringent test. But whatever the cause of the variability, whether pulsation or rotation, provided only that it is intrinsic in the star, and not forced from outside, the density must be the leading factor in determining the period. If the star is contracting so that its density changes appreciably, the period can not remain constant. Now, on the contraction hypothesis the change of density must amount to at least 1 per cent. in 40 years. (I give the figures for δ Cephei, the best-known variable of this class.) The corresponding change of period should be very easily detectable. For δ Cephei the period ought to decrease 40 seconds annually.

Now δ Cephei has been under careful observation since 1785, and it is known that the change of period, if any, must be very small. S. Chandler found a decrease of period of $\frac{1}{20}$ second per annum, and in a recent investigation E. Hertzsprung has found a decrease of $\frac{1}{10}$ second per annum. The evidence that there is any decrease at all rests almost entirely on the earliest observations made before 1800, so that it is not very certain; but in any case the evolution is proceeding at not more than $\frac{1}{400}$ of the rate required by the contraction hypothesis. There must at this stage of the evolution of the star be some other source of energy which prolongs the life of the star 400-fold. The time-scale so enlarged would suffice for practically all reasonable demands.

I hope the dilemma is plain. Either we must admit that whilst the density changes 1 per cent, a certain period intrinsic in the star can change no more than $\frac{1}{800}$ of 1 per cent., or we must give up the contraction hypothesis.

If the contraction theory were proposed today as a novel hypothesis I do not think it would stand the smallest chance of acceptance. From all sides—biology, geology, physics, astronomy—it would be objected that the suggested source of energy was hopelessly inadequate to provide the heat spent during the necessary time of evolution; and, so far as it is possible to interpret observational evidence confidently, the theory would be held to be definitely negative. Only the inertia of tradition keeps the contraction hypothesis alive—or rather, not alive, but an unburied corpse. But if we decide to inter the corpse, let us frankly recognize the position in which we are left. A star is drawing on some vast reservoir of energy by means unknown to us. This reservoir can scarcely be other than the sub-atomic energy which, it is known, exists abundantly in all matter; we sometimes dream that man will one day learn how to release it and use is for his service. The store is well-nigh inexhaustible, if only it could be tapped. There is sufficient in the sun to maintain its output of heat for 15 billion years.

Certain physical investigations in the past year, which I hope we may hear about at this meeting, make it probable to my mind that some portion of this sub-atomic energy is actually being set free in the stars. F. W. Aston's experiments seem to leave no room for doubt that all the elements are constituted out of hydrogen atoms bound together with negative electrons. The nucleus of the helium atom, for example, consists of 4 hydrogen atoms bound with 2 electrons. But Aston has further shown conclusively that the mass of the helium atom is less than the sum of the masses of the 4 hydrogen atoms which enter into it; and in this at any rate the chemists agree with him. There is a loss of mass in the synthesis amounting to about 1 part in 120, the atomic weight of hydrogen being 1.008 and that of helium just 4. I will not dwell on his beautiful proof of this, as you will no doubt be able to hear it from himself. Now mass can not be annihilated, and the deficit can only represent the mass of the electrical energy set free in the transmutation. We can therefore at once calculate the quantity of energy liberated when helium is made out of hydrogen. If 5 per cent, of a star's mass consists initially of hydrogen atoms, which are gradually being combined to form more complex elements, the total heat liberated will more than suffice for our demands, and we need look no further for the source of a star's energy.

But is it possible to admit that such a transmutation is occurring? It is difficult to assert, but perhaps more difficult to deny, that this is going on. Sir Ernest Rutherford has recently been breaking down the atoms of oxygen and nitrogen, driving out an isotope of helium from them; and what is possible in the Cavendish laboratory may not be too difficult in the sun. I think that the suspicion has been generally entertained that the stars are the crucibles in which the lighter atoms which abound in the nebulæ are compounded into more complex elements. In the stars matter has its preliminary brewing to prepare the greater variety of elements which are needed for a world of life. The radio-active elements must have been formed at no very distant date; and their synthesis, unlike the generation of helium from hydrogen, is endothermic. If combinations requiring the addition of energy can occur in the stars, combinations which liberate energy ought not to be impossible.

We need not bind ourselves to the formation of helium from hydrogen as the sole reaction which supplies the energy, although it would seem that the further stages in building up the elements involve much less liberation, and sometimes even absorption, of energy. It is a question of accurate measurement of the deviations of atomic weights from integers, and up to the present hydrogen is the only element for which Mr. Aston has been able to detect the deviation. No doubt we shall learn more about the possibilities in due time. The position may be summarized in these terms: the atoms of all elements are built of hydrogen atoms bound together, and presumably have at one time been formed

from hydrogen; the interior of a star seems as likely a place as any for the evolution to have occurred; whenever it did occur a great amount of energy must have been set free; in a star a vast quantity of energy is being set free which is hitherto unaccounted for. You may draw a conclusion if you like.

If, indeed, the sub-atomic energy in the stars is being freely used to maintain their great furnaces, it seems to bring a little nearer to fulfilment our dream of controlling this latent power for the well-being of the human race—or for its suicide.

So far as the immediate needs of astronomy are concerned, it is not of any great consequence whether in this suggestion we have actually laid a finger on the true source of the heat. It is sufficient if the discussion opens our eyes to the wider possibilities. We can get rid of the obsession that there is no other conceivable supply besides contraction, but we need not again cramp ourselves by adopting prematurely what is perhaps a still wilder guess. Rather we should admit that the source is not certainly known, and seek for any possible astronomical evidence which may help to define its necessary character. One piece of evidence of this kind may be worth mentioning. It seems clear that it must be the high temperature inside the stars which determines the liberation of energy, as H. N. Russell has pointed out. If so the supply may come mainly from the hottest region at the center. I have already stated that the general uniformity of the opacity of the stars is much more easily intelligible if it depends on scattering rather than on true absorption; but it did not seem possible to reconcile the deduced stellar opacity with the theoretical scattering coefficient. Within reasonable limits it makes no great difference in our calculations at what parts of the star the heat energy is supplied, and it was assumed that it comes more or less evenly from all parts, as would be the case on the contraction theory. The possibility was scarcely contemplated that the energy is supplied entirely in a restricted region round the center. Now, the more concentrated the supply, the lower is the opacity requisite to account for the observed radiation. I have not made any detailed calculations, but it seems possible that for a sufficiently concentrated source the deduced and the theoretical coefficients could be made to agree, and there does not seem to be any other way of accomplishing this. Conversely, we might perhaps argue that the present discrepancy of the coefficients shows that the energy supply is not spread out in the way required by the contraction hypothesis, but belongs to some new source only available at the hottest, central part of the star.

I should not be surprised if it is whispered that this address has at times verged on being a little bit speculative; perhaps some outspoken friend may bluntly say that it has been highly speculative from beginning to end. I wonder what is the touchstone by which we may test the legitimate development of scientific theory and reject the idly speculative. We all know of theories which the scientific mind instinctively rejects as fruitless guesses; but it is difficult to specify their exact defect or to supply a rule which will show us when we ourselves do err. It is often supposed that to speculate and to make hypotheses are the same thing; but more often they are opposed. It is when we let our thoughts stray outside venerable, but sometimes insecure, hypotheses that we are said to speculate. Hypothesis limits speculation. Moreover, distrust of speculation often serves as a cover for loose thinking; wild ideas take anchorage in our minds and influence our outlook; whilst it is considered too speculative to subject them to the scientific scrutiny which would exercise them.

If we are not content with the dull accumulation of experimental facts, if we make any deductions or generalizations, if we seek for any theory to guide us, some degree of speculation can not be avoided. Some will prefer to take the interpretation which seems to be most immediately indicated and at once adopt that as an hypothesis; others will rather seek to explore and classify the widest possibilities which are not definitely inconsistent with the facts. Either choice has its dangers; the first

may be too narrow a view and lead progress into a cul-de-sac; the second may be so broad that it is useless as a guide, and diverges indefinitely from experimental knowledge. When this last case happens, it must be concluded that the knowledge is not yet ripe for theoretical treatment and speculation is premature. The time when speculative theory and observational research may profitably go hand in hand is when the possibilities, or at any rate the probabilities, can be narrowed down by experiment, and the theory can indicate the tests by which the remaining wrong paths may be blocked up one by one.

The mathematical physicist is in a position of peculiar difficulty. He may work out the behavior of an ideal model of material with specifically defined properties, obeying mathematically exact laws, and so far his work is unimpeachable. It is no more speculative than the binomial theorem. But when he claims a serious interest for his toy, when he suggests that his model is like something going on in Nature, he inevitably begins to speculate. Is the actual body really like the ideal model? May not other unknown conditions intervene? He can not be sure, but he can not suppress the comparison; for it is by looking continually to Nature that he is guided in his choice of a subject. A common fault, to which he must often plead guilty, is to use for the comparison data over which the more experienced observer shakes his head; they are too insecure to build extensively upon. Yet even in this, theory may help observation by showing the kind of data which it is especially important to improve.

I think that the more idle kinds of speculation will be avoided if the investigation is conducted from the right point of view. When the properties of an ideal model have been worked out by rigorous mathematics, all the underlying assumptions being clearly understood, then it becomes possible to say that such properties and laws lead precisely to such and such effects. If any other disregarded factors are present, they should now betray themselves when a comparison is made with Nature. There is no need for disappointment at the failure of the model to give perfect agreement with observation; it has served its purpose, for it has distinguished what are the features of the actual phenomena which require new conditions for their explanation. A general preliminary agreement with observation is necessary, otherwise the model is hopeless; not that it is necessarily wrong so far as it goes, but it has evidently put the less essential properties foremost. We have been pulling at the wrong end of the tangle, which has to be unravelled by a different approach. But after a general agreement with observation it established, and the tangle begins to loosen, we should always make ready for the next knot. I suppose that the applied mathematician whose theory has just passed one still more stringent test by observation ought not to feel satisfaction, but rather disappointment—"Foiled again! This time I *had* hoped to find a discordance which would throw light on the points where my model could be improved." Perhaps that is a counsel of perfection; I own that I have never felt very keenly a disappointment of this kind.

Our model of Nature should not be like a building—a handsome structure for the populace to admire, until in the course of time some one takes away a corner stone and the edifice comes toppling down. It should be like an engine with movable parts. We need not fix the position of any one lever; that is to be adjusted from time to time as the latest observations indicate. The aim of the theorist is to know the train of wheels which the lever sets in motion—that binding of the parts which is the soul of the engine.

In ancient days two aviators procured to themselves wings. Dædalus flew safely through the middle air across the sea, and was duly honored on his landing. Young Icarus soared upwards towards the sun till the wax melted which bound hs wings, and his flight ended in fiasco. In weighing their achievements perhaps there is something to be said for Icarus. The classic authorities tell us that he was only "doing a stunt," but I prefer to think of him as the man who

certainly brought to light a constructional defect in the flying machines of his day. So too in science. Cautious Dædalus will apply his theories where he feels most confident they will safely go; but by his excess of caution their hidden weakness can not be brought to light. Icarus will strain his theories to the breaking-point till the weak joints gape. For a spectacular stunt? Perhaps partly; he is often very human. But if he is not yet destined to reach the sun and solve for all time the riddle of its constitution, yet he may hope to learn from his journey some hints to build a better machine.

A. S. EDDINGTON

THE HAWAIIAN OLONA

IN SCIENCE (N. S. 48: 236–38, September 6, 1918) was published a paper by the writer, entitled "The Olona, Hawaii's Unexcelled Fiber Plant." This was later reprinted by the *Literary Digest,* and evidently aroused widespread interest concerning this remarkable fiber. The writer received letters from many parts of the world, requesting further information. Since his previous account he has been furnished with the following statement, by Dr. N. Russel, of Olaa, Hawaii, and originally published in the report of the Hawaii Agricultural Experiment Station for 1902. As this report is out-of-print and unavailable to most students, Russel's excellent account is presented herewith:

Some fifty years ago about 1,000 natives were living on the margin of the virgin forest and pahoe-hoe rock along the trail connecting Hilo town with the crater of Kilauea, island of Hawaii, in a spot corresponding to the present 22-mile point of the volcano road. Making of "kapa" (native cloth) out of "mamake" bark (*Pipturus albidus*), of olona fiber for fishing nets out of *Touchardia latifolia,* and capturing "O-U" birds for the sake of the few precious yellow feathers under the wings, of which luxurious royal garments were manufactured—those were the industries on which they lived.

For the reasons common to all the native population of the islands, viz., the introduction of new germs of disease—syphilis, leprosy, tuberculosis, smallpox, etc.—this settlement gradually dwindled away, and in 1862 the few surviving members migrated to other localities. At present only patches of wild bananas, taro, and heaps of stones scattered in the forest indicate the places of former habitation and industry. I have heard, however, that as late as the seventies Kalakaua still levied a tax in olona fiber from the natives of Puna and Olaa districts, which fiber he sold at high prices to Swiss Alpine clubs, who valued it for its light weight and great strength.

Touchardia grows abundantly in Olaa forests, presenting a kind of a natural plantation. It very successfully holds its own in competition with ferns and other elements of the undergrowth in the shade of "ohia" trees (*Metrosideros polymorpha*). The deep shade, very porous soil, considerable moisture, with a yearly rainfall of 180 inches pretty evenly distributed, are the natural conditions. By removing some of the undergrowth, scattering seed, and probably by planting cuttings, the number of plants on the same area could be greatly increased with but very small expense. Since plants of medium age (about 18 months old) supply the best fiber, natives in gathering used to turn down the older ones with the foot, laying the whole plant on the ground to force new shoots and sprouts.

I was familiar with the plant and its properties for years, but did not pay any further attention to it as a possible object of industry for the reason that to all appearances the same difficulties in mechanical extraction of fiber will be met as in the case of ramie, for which no satisfactory machine has been found. Recently my interest in the matter was again aroused by Mr. Jared G. Smith, of the Hawaii Experiment Station. Considering that *Touchardia* seems to be free from resinous matter, upon his suggestion I decided to examine the subject more in detail. For this purpose an old native, born and raised in the settlement above mentioned, was interviewed. Together with him I proceeded into the forest along twenty-two miles side trail. In my presence