THE LAWS OF NATURE*

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Contrast in Character of Motion and Temperature Laws

IT would, however, be premature to conclude that because the laws of temperature in this one respect follow more closely the outlines of bare experience, they are therefore necessarily purer or more worthy of preservation than the laws of motion. We must look more closely into the matter, for it may be that, when the necessary supplementary clauses, so to speak, have been added, the type of law which has been constructed to describe motions is, by its greater scope, more adaptable to further experiences than the type constructed to describe temperature changes.

To examine this point we may usefully return to our examples of a ball thrown into the air and bodies at different temperatures placed near one another, to see how the difference of description, not inherent in the essential experiences, is introduced in the chosen terms of expression. The ball, it will be remembered, could, and in fact did, reverse its path, while the temperatures of the two bodies moved only towards equality. How did this come about ?

'Bare experience' would have required the ball only to fall, for its tendency is to move towards the earth. Hence something must have interfered with its natural behaviour, and, in fact, we did throw it up, thereby making it take a path it would not by itself have chosen. In order to describe its motion completely, therefore, we must state not only the law of motion but also the initial occurrence which set it moving. No importance is to be attached to the fact that this was a human interference with natural processes, for the same thing might have happened without human agency ; for example, the ball might have been thrown up by a volcano. The essential fact is that the motion cannot be described by the law of motion alone, but needs the statement of the law of motion plus a particular event. According to the character of this event the motion can be in one direction or in the opposite one, and the law of motion by itself must therefore be broad enough to allow both possibilities if it is to cover all the motions that occur in Nature.

The same thing is true, of course, on the larger scale. The planets might have revolved round the sun in the opposite direction so far as the laws of motion are concerned. To explain why they move in the direction observed we must know something of the origin of the solar system. When the circumstances in which they became detached from the sun are known, their motion is completely determined by the laws of motion, and no room is left for the possibility that they might have moved differently. The laws of motion thus establish their freedom by shifting the onus of distinguishing between opposite courses on to the shoulders of a 'boundary condition' ; and when we deal with something so vast and ancient as the universe, this boundary condition lies so far in the past that we are apt to forget that it is there. Consequently we think that the course of development of the universe has the same freedom as the untrammelled laws of motion. In that, however, we are wrong. So far from releasing us from difficulties concerning the origin of things, the laws of motion make it essential that we shall face those difficulties, for they are framed in such a way that otherwise their requirements are necessarily ambiguous.

Now so far as possibilities are concerned, temperature phenomena are in precisely the same boat as the phenomena of motion. In saying that the ball could move up or down whereas the two bodies could only tend towards equality of temperature, I was in fact making an incorrect statement, and if you did not notice it, that is only an indication of our extreme susceptibility to error through accepting laws of Nature as equivalent to bare experience. It is quite possible for the hot body to become hotter and the cold body colder if we choose our initial circumstances properly. For example, a gas expanding suddenly may cool and give its heat to another gas originally hotter. Such circumstances occur much less readily on the earth than do movements against gravity, and we are therefore liable to overlook their possibility. But in principle there is no difference at all in this respect between motion and temperature exchange, and there is no fundamental reason why laws of temperature should not have been constructed permitting a reversible exchange of temperature and requiring supplementary boundary conditions to determine what would happen in a particular case. If that had been done, there would have been no formal difference between the modes of treatment of the two phenomena, and I should have had to choose another subject for this lecture.

It has not been done, however. Perhaps owing in some measure to the fact that in everyday experience the temporary flow of heat against a temperature gradient generally needs the establishment of highly artificial conditions, while the temporary movement of bodies against gravitation is a very common occurrence, a different approach to the problem has been made; and instead of describing a thermal phenomenon in terms of the rate of change of temperature with time, as we describe a kinematical phenomenon in terms of the rate of change of position with time, we describe it in terms of a new conception, *entropy*, which has no analogue in the present formulation of mechanical laws. The characteristic of entropy is that, no matter whether heat flows from the hot to the cold or from the cold to the hot body, the entropy at the end is always greater than the entropy at the beginning. (There is, it is true, a theoretical possibility that the entropy may remain unchanged, but in practice that can be ignored, and even theoretically it is impossible for the entropy to decrease.) This statement is true only if the *total* entropy is taken into account—the entropy of the hot and cold bodies and of any other body or system which has any influence at all on the process.

By this device we give direct and immediate expression to the one-way tendency of bare experience. We do not need first to give phenomena a round-trip ticket, and then to limit its validity to one direction only according to the ticket office at which it is presented. Whatever happens, whether bodies tread the natural path of temperature equalization or by violence are forced along the opposite course, the entropy of the whole system increases.

No such function as entropy, I have said, exists in the laws of motion, but there seems no fundamental reason why it should not be derivable. Every incidental motion in the universe contributes in its own measure to the grand one-way march of bare experi-

* *Continued from p.* 736.

ence, and should not be inherently incapable of representation in terms of an ever-increasing (or ever-decreasing, it does not matter which) function which for the purpose of reference we may call 'motion-entropy'. Motion-entropy would increase when an apple fell to the ground, and it would also increase when we threw our ball upwards—provided, of course, we took into account its change in ourselves and in the earth on which we pressed more heavily when we threw, as well as its change in the ball. If motion-entropy were formulated, we could destroy the formal difference between the laws of motion and those of temperature exchange in another way, by bringing the former into line with the latter, instead of by the reverse process.

On the face of it, this would seem the more desirable thing to do, for a direct expression of experience seems preferable to an indirect one. There is a penalty to pay, however. The conception of entropy is such that it has meaning only with reference to equilibrium conditions. Consider our two bodies again, and let us suppose that at the beginning their temperatures, though different, are steady. We can then evaluate their entropy—relative to an arbitrary zero, it is true, but that is of no importance here. Now let them interact with one another. After the lapse of a certain time their temperatures will have become equal, and then they will remain steady again. We can now again evaluate their entropy, and we find, according to the law, that it is greater than it was before. And that is all that the law can tell us. During the interval between the two steady states we cannot say that the entropy has steadily increased; we cannot, in fact, say anything at all about it, for the conditions essential to its significance do not exist. And since, in actuality, things are never in equilibrium, it follows that, in strict truth, we can never assign a precise entropy value to any actual system of bodies, let alone the whole universe. That is one reason why, as we saw, we could not apply our temperature laws to any system not enclosed within a boundary, for a boundary is necessary, though not sufficient, for equilibrium. Our assumption that our two bodies were originally at steady temperatures was an illegitimate one; there is no known process for keeping them so.

It should be understood that equilibrium here has a perfectly definite meaning, and is not subject to the arbitrariness arising from the possibility of changing our terms of expression. I mentioned earlier that a body at constant temperature could be regarded as an inert mass or as constantly interchanging energy with its surroundings, and that we could choose which form of expression we liked. That is true, but the laws of temperature I am speaking about now—in particular the law that entropy always increases—are framed after we have made our choice, and the choice is such that a state of constant temperature is a state of equilibrium so far as temperature is concerned, and a state of varying temperature is not a state of equilibrium. We cannot satisfy the condition for the significance of entropy by conceptually petrifying into equilibrium whatever state we may be confronted with.

The case again is not improved by the adoption of some physical picture of entropy, such as the very common and very convenient though very dangerous one which relates it to 'organization'. That representation was adopted after the discovery of the significance of entropy, and its validity is entirely teristics of entropy regarded as a simple mathematical function of certain thermal quantities. 'Organization' is a term of expression of a term of expression, and as such it has aspects which have no connexion with the facts of experience; it allows us, for example, to assign a precise measure of probability to occurrences which we have no reason to suppose are even possible. We can attach no weight to long-distance extrapolations of a process of disorganization which could not be reached also by extrapolations based on the original meaning of entropy, and we shall therefore not concern ourselves with them.

Strictly speaking, then, the entropy of an actual system can never be determined, but in practice we can often arrange to isolate a system sufficiently well for it to appear to be in equilibrium for a finite length of time, and we can then calculate its entropy. In such cases, in spite of its strict meaninglessness, the conception is extremely useful. We always find that the combined entropy of the system and its surroundings is increased when a change occurs, and our assumption that the entropy of the universe is continuously increasing is based on this fact. Such a conclusion, however, I must repeat, is rigorously a meaningless statement, and can only by courtesy be called an unprovable assumption.

The laws of motion do not suffer from this disability. They enable us to follow our ball throughout the whole duration of its flight, whether it is moving upwards or downwards, and make no demand that the system shall be in equilibrium. They have therefore much to compensate them for their excessive latitudinarianism, and make up by attention to detail what they lack in self-sufficiency.

The result of our analysis, then, is this. Our experience, both of the motions and of the temperature changes of bodies, shows that at present the processes going on in the universe tend in a certain direction and not in the opposite one. We describe this tendency for purposes of precise calculation by choosing terms of expression which form the alphabet of physical laws, and since time is included among these terms of expression, the laws allow us to extrapolate to the distant past and the distant future. The terms we choose for motion, however, differ in character from those which we choose for temperature, in that they lead to a different type of law. The former lead to reversible laws, which describe every detail of the changing process, but leave to an unknown 'original state' the task of determining why the process goes in one direction rather than the opposite one. The temperature laws, on the other hand, indicate the direction of the process without reference to an original state, but cannot be applied unless we can contrive or assume that the system in which we are interested is brought to a condition of equilibrium on two successive occasions: they then require that a certain quantity is greater on the second occasion than on the first, but can tell us nothing about the course from the first to the second condition of equilibrium.

Historical Aspect of the Laws of Temperature

Neither type of law affords grounds for dogmatizing about the distant past or future of the universe, and it is perhaps not altogether profitless to have achieved a realization of even that modest result. It is a matter of some interest to inquire why, when the bare experiences follow such similar lines, we should have chosen laws of such different types for describing

The earliest progress in science was naturally concerned with motion, for that is the phenomenon most easily examined with precise measuring instruments. Our theoretical scale of time measurement, for example, was chosen by assuming a body moving freely in space, and defining equal times as those in which it covered equal distances. A radiating body, such as the sun, might have been chosen instead of a moving body, and equal times defined as those in which it melted equal masses of ice, say, but the practical difficulties would have been greater. A kinematical measure was therefore chosen, *and then applied universally*—in particular, to the description of temperature phenomena when they came to be examined. Temperature thus, from the beginning, inherited terms of expression chosen originally for the analysis of motions instead of developing along its own intrinsic lines.

This procedure was further established by practical needs. We required to know how *work* (a concept belonging to mechanics, the science of motions) could be obtained from heat before we were very far advanced in the study of temperature phenomena themselves, and accordingly the additional terms of expression chosen for the laws of temperature were those found to be best adapted to the description of the transformation of heat into work, and not those best fitted for the study of heat for its own sake. The theoretical temperature scale, for example, was based on the amount of work obtainable from heat, and not on simple thermal effects alone. The result of this unnatural union between the representatives of motion and temperature was the birth of entropy, a quantity which was conceived in order to afford a measure of the availability of heat for transformation into work. Nothing parallel to this, of course, existed in the science of motion, and so no corresponding concept was created there.

It may be argued that this was a fortunate circumstance, and that the thermodynamic conceptions thus originated are more favourable for rapid and permanent progress than pure thermal conceptions would have been. From the point of view of the understanding, apart from the exploitation, of Nature, however, this seems to me very unlikely. Certainly our ultimate aim is to unite the sciences of mechanics and heat, but I think the soundest basis for a satisfactory union would lie in the existence of two strong independent sciences established on similar lines. We have brought about a union long before the science of heat has reached maturity, and so forced on it an unnatural development. The incompatibility between the motion and temperature laws is a result of this, and is not a happy augury for future connubial bliss. I venture to propose a return to first principles, and the creation of an independent science of heat, with concepts and laws of its own.

The obvious starting point is the phenomenon of radiation, for of all the modes of temperature exchange that occur, this is overwhelmingly the most important in the universe as a whole. It is indeed impossible to express how utterly insignificant all other temperature phenomena become when compared with it. To take a single example, the sun, a dwarf star, radiates some 4×10^{33} ergs of energy every second to space. On the other hand, the energy available, through all terrestrial processes—production of fuel, human and animal metabolism, etc.—for transformation into work does not exceed 5×10^{19} ergs per second, and if we suppose one tenth of this to be so converted, we see that the radiation from one dwarf star is about 10^{15}—a thousand million million—times the total terrestrial transformation of heat into work in the same time*. How many times the number of stars equivalent to the sun exceeds the number of planets on which work is artificially produced from heat we do not, of course, know ; but, whatever it may be, when we consider that the terms of expression and laws of temperature phenomena have been shaped by this terrestrial 'gnat' and then foisted on the multillions of stellar 'camels' throughout the universe, we begin to realize something of the anomaly which has occurred.

Thermal Relativity

There is, however, another reason why we should —indeed, I would go further and say why we *must*— reform our present treatment of radiation. Progress in the study of motion has in the last generation brought to light a fundamental principle which, it is generally acknowledged, is valid throughout the whole of scientific inquiry, and this principle is violated in our present theory of radiation. I refer to the fundamental justification of the theory of relativity, namely, the principle that our theories should not imply the possibility of observing what is, in fact, inherently unobservable. It was this principle that destroyed the materialistic ether doctrine of the nineteenth century. Motion through the ether eluded observation so consistently as to force acknowledgment of the idea that it was inherently unobservable, whereupon the whole science of kinematics was reformed in such a way as to require that any experiment made to observe such motion must necessarily fail. The only observable motion is motion of one body with respect to another, and accordingly the word 'motion' now carries with it the quality of relativity, so that we cannot speak of it without implying the existence of some locatable frame of reference.

Now absolute radiation is unobservable in precisely the same way as is absolute motion, but while we have dismissed the latter from the terms of expression of motions, we still retain the former among the terms of expression of radiation phenomena. Consider two bodies relatively at rest. We used to say that each had an absolute velocity, v, and that they were relatively at rest because their absolute velocities were equal. We have now discarded the idea of absolute velocities, and associate no motion with the bodies. But consider two bodies at the same temperature. We used to say, and we still say, that each has an absolute temperature, θ, as a result of which it radiates a certain definite amount of energy, and that we do not observe any effects of the radiation because each receives from the other the same amount of energy that it radiates. But this absolute radiation, just like absolute velocity, is essentially unobservable. We should, then, in accordance with our principle, cease to employ it in our theories, and express the laws of radiation in such a form that it has no significance.

This, of course, means a radical reform of the laws of radiation, but I can see no escape from its necessity unless we deny the basic justification of the theory of relativity. When we reflect on the matter, we see other points of resemblance between the sciences of motion and radiation, which, indeed, is not surprising in view of the fundamental parallelism of the bare experiences already pointed out. For example, the

* I am indebted to Sir Alfred Egerton for the data concerning terrestrial processes.

development of the mechanical theory of relativity showed that gravitational and inertial mass, two quantities which, according to the former view, just happened always to be equal, were, in fact, essentially the same thing. In the theory of radiation we have likewise two quantities, radiative power and absorptive power, which similarly just happen always to be equal. We might expect that these two quantities, in a reformulation of the theory of radiation, would also be revealed as the same thing. Again, there is in temperature, as in motion, a limit approachable only asymptotically by material bodies ; we call one the absolute zero of temperature and the other the velocity of light. The fact that in temperature there is only a lower limit, whereas the velocity of light is a limiting velocity for motion in all directions, is merely a characteristic of our method of measurement. We have already seen that a measurement of velocity in terms of the Doppler effect would have given us a finite limit in one direction and an infinite one in the other, and we could equally well choose a scheme of temperature measurement (Kelvin, in fact, at one time proposed such a scheme for his 'absolute' scale) which would place the 'absolute zero' at 'minus infinity'.

I have attempted[4] a re-expression of the phenomena of radiation, along the lines of the relativity treatment of the phenomena of motion, in which temperature is measured in terms of the rate of change with time of some observable characteristic of the radiating body, just as velocity is measured in terms of the rate of change with time of the spatial position of the moving body. The 'observable characteristic' could be the energy radiated by the body, expressed in terms of the readings of a suitably defined instrument, but I have found it more convenient to choose an instrument which records something analogous to the entropy change of the body ; I will denote it by the Greek letter η. That, however, is a detail ; the important thing for our present purpose is that temperature is measured in terms of a temporal process instead of by the reading of a thermometer in equilibrium, and the measurement of time involved is made by a thermal clock instead of a mechanical clock.

To understand the character of a thermal clock, let us look for a moment at the character of a mechanical clock. Here some specified body moves over a dial on which equal spaces are marked out, and equal times are those in which equal numbers of spaces are covered by the moving body. The fundamental clock is that in which the 'specified body' is a beam of light (this, of course, is only another way of expressing the familiar 'postulate of the constancy of the velocity of light' familiar to students of the theory of relativity), and clocks for practical purposes are constructed so as to give the same scale more conveniently. The time-scale suitable for the description of motion is thus one in which equal times are defined in terms of equal spaces, conformably with the measurement of motion by velocity defined as the rate of change of space with time. Similarly, the time-scale suitable for the description of radiation is one in which equal times are defined in terms of equal amounts of η, conformably with the measurement of radiation by temperature defined as the rate of change of η with time. Instead of the strictly specified body (a 'hand') moving over a space-scale (a 'dial'), we have a strictly specified body radiating to an η-measuring instrument, and equal times are those in which equal quantities of η are recorded by the instrument. We have to wind up the mechanical clock to keep its specified body moving continuously, and similarly we have, of course, to supply heat to keep the specified radiating body radiating continuously. In other respects also the two procedures are perfectly analogous.

In this way we provide a set of concepts, or terms of expression, for the description of radiation, which are intrinsic to the subject itself and not imported into it from without. The result is that just as mechanics has turned to advantage its choice of time measurement in terms of space by forming a unified conception of space-time, so the study of radiation along the lines suggested can turn to advantage its choice of time measurement in terms of η by forming a unified conception of η-time. To see the analogy, let us recall that in the ordinary theory of relativity the 'interval' between two events in the history of a moving particle is made up by combining a space increment with a time increment, and the relative magnitudes of these two components vary with the velocity of the co-ordinate system—that is, the velocity of the measuring instruments. When the co-ordinate system moves with the body, the interval consists entirely of the time increment, but for other velocities it is partly a time increment and partly a space increment. A similar thing is true of radiation. If we examine the radiation of a body with an η-measuring instrument and a thermal clock at the same temperature as the body, the former instrument records nothing and the thermal interval is wholly thermal time ; but if we use instruments at a temperature different from that of the radiating body, they both give finite records, and the transformation equations connecting the readings at one temperature with those at another (the thermal 'Lorentz equations') are such that a total thermal interval exists which is the same for all temperatures of the instruments.

The concept of η-time thus arrived at can be given a geometrical interpretation corresponding to that given to space-time by Minkowski. The null geodesic in space-time corresponds to motion with a velocity of $\pm c$, and that for η-time corresponds to radiation at a temperature of the absolute zero or infinity. There is the difference, however, that whereas space-time is 4-dimensional since space has three dimensions, η-time is 2-dimensional ; but, on the other hand, while the co-ordinate expression for the space-time interval is quadratic, that for the η-time interval is quartic. The geometries applicable to the two cases are thus different, and I have so far not been able to proceed beyond what I may call the 'special' theory, in which only constant temperatures are considered, just as in the special theory of mechanical relativity only constant velocities are considered. Constant velocities are appropriate to motions in a world of zero masses, and, similarly, constant temperatures are appropriate to radiation by bodies of infinite heat capacity. There is throughout a close parallelism between the new thermal and the current mechanical terms of expression, and we have an earnest of the possibility of ultimate amalgamation into a much more natural thermodynamics than the existing science of that name in the fact that thermal and mechanical time, though quite differently defined, give identical scales, and at any fixed temperature and velocity could be given the same unit. This is a necessary consequence, of course, of the experimental fact that a body at constant temperature radiates at a uniform rate according to our ordinary mechanical clocks.

The special theory of thermal relativity describes the same facts of experience as the current theory of radiation, but uses fundamentally different concepts for the purpose. So far as I can see, it entails nothing new in the field of observation, and in this respect it differs from the special theory of mechanical relativity which, when it superseded the earlier ideas based on absolute space and time, led to such discoveries as the dependence of mass on velocity, the equivalence of mass and energy, and other important relations. There are, however, two reasons for this. In the first place, pre-relativity mechanics was almost entirely concerned with velocities far from that of light, and the new requirements of the relativity theory thus related to conditions of which no experience had been obtained. The current absolute theory of radiation, however, is based on a relatively much wider range of experience, including that of phenomena at temperatures approaching the absolute zero. It has accordingly already adapted itself to the regions where anomalous results would be expected, so that the new theory is robbed of its chance of springing a surprise. Secondly, the general field of validity of the special theory of mechanical relativity —namely, the region of phenomena which can be discussed without considering the accelerating effects of gravitation—is a very large one, giving scope for many applications of the new principles. The special theory of thermal relativity, however, has scarcely any scope, for in all ordinary phenomena the effect of radiation is to lower the temperature of the radiating body very rapidly, and so to give variations of the radiation, the neglect of which is out of the question. Only bodies of very large heat capacity, like stars, come within its power, and we cannot make experiments with stars. New discoveries would therefore be expected to await the formulation of the general theory. But for our present purpose that is of little moment, for we are concerned more with the character and terms of expression of the laws than with the facts they represent, and these are as well exhibited in the special as in the general theory.

Whatever, then, may be the value of a thermal relativity theory from the heuristic point of view, it does, I think, show conclusively that it is possible to express the facts of radiation in terms totally different from the traditional ones, and so to preserve in our laws of motion and temperature the similarity of character inherent in the bare experiences. Each set of phenomena employs only concepts peculiar to itself—in mechanics, space and 'mechanical' time, and in temperature, η and 'thermal' time—and the measurements of the associated quantities are carried out by independent instruments. The connecting link is time, which, whether measured by the space covered by a moving body or by the η received from a radiating body, gives the same scale in the ideal cases of 'constant' velocity and 'constant' temperature. We can thus confirm the location of the rift in current physical theory in the arbitrary terms of expression chosen for our laws, leaving bare experience, if not necessarily quite homogeneous, at least sufficiently so to encourage the hope that a single scheme of law for all phenomena is a possible objective.

The One-Way Evolution of the Universe

Let us, in conclusion, look at the problem of the infinitely distant past and future from the point of view we have reached. We can say, first of all, that in so far as the problem arises from an extrapolation of existing laws, it is not a fundamental one. We can change the laws without violating experience. If they lead us into difficulties it may be necessary to do so, and if they lead us into contradictions or impossible situations we *must* do so. The fundamental difficulties are those which result from the trend of bare experience itself. If that leads us to an impossible situation, we must either fall back on the almost desperate expedient of trying to correlate our experiences without arranging them in a time order at all, or else capitulate and say that the universe is essentially irrational or (perhaps the same thing) beyond comprehension by human reason. I do not think, however, that we are yet reduced to that extremity.

In both its motion and temperature aspects the universe at present appears to be taking a one-way course. Its motions show a process of local consolidation and large-scale diffusion. This offers no problem for the future, for the diffusion can continue indefinitely and the consolidation tend asymptotically towards an eternally steady state. Working backwards to the past, however, we come by way of a large-scale consolidation and local diffusion to the idea of a single homogeneous mass, and, according to our present time-scale, this state would be located at a not infinitely remote epoch. The present discrepancy between the dates of this epoch yielded by the local and large-scale processes is a characteristic of our theories which can be ignored in our present considerations. What happened before this ? There is nothing that I can see to prevent that state having been reached by a gradual condensation of an originally infinitely diffused mass the rate of contraction of which, infinitely slow at first, accelerated until it culminated in a state of maximum density. After this a large-scale expansion could have proceeded, begun either by a rebound of a continuous mass from a state of compression, or by separate bodies simply continuing on their one-way journey after having made their closest approach to one another. Such a course of cosmic history would involve no problem of the distant past or distant future so far as motions are concerned.

What about temperatures ? The present trend is somewhat ambiguous. Our experience is so brief that we can detect no change in the temperatures of the stars, and any theories we may have on the matter are based on laws which we are now leaving out of account. Certainly there is a tendency towards equalization of temperature, shown by the fact of radiation from hot to cold bodies, but the probability is, nevertheless, that as we go backwards to remote times, the hottest bodies, the stars, become colder rather than hotter. I think it is in accord with present indications, so far as we can read them, to say that when we reach the time of maximum density of the universe, the stars, if they then existed, were cooler than now, and the temperature of the whole universe was more homogeneous. The tendency to local consolidation since that time has raised temperatures locally, and the large-scale expansion has checked the process of equalization. As we recede further into the past we contemplate a homogeneous mass of matter falling gradually in temperature step by step with its decrease in density, and there is no greater difficulty here than there is with the succession of motions. In the reverse direction, as we go from the present time towards the future, we presumably find the stars getting hotter up to a maximum temperature and then cooling, while all the time the tendency towards equalization of temperatures by radiation goes on inexorably until ultimately the universe approaches asymptotically a state of uniform temperature.

We may sum up this picture of cosmic history, then, as follows. In the infinitely distant past the universe consisted of an infinitely diffuse, homogeneous mass at uniform temperature. With the passage of time the mass became denser and hotter until a state of maximum density was reached, and perhaps about this time the mass broke up into units. Thereafter a process of expansion went on, with the units getting hotter at varying rates but tending by radiation to come to a state of common temperature again. Ultimately they will reach and pass a maximum temperature, tending finally to a universe consisting of a number of aggregations of units at a common temperature performing eternally unchanging motions.

It should not be necessary to say that this is not intended to be in the slightest degree a theory of cosmogony. My purpose has been simply to give a conceivable course of development not inconsistent with the present trend of our experience, and involving no contradictions or insuperable difficulties in the distant past or the distant future. There may be a thousand such possible courses, and I am not concerned with the task of choosing between them. What I have tried to show is that not only does our present dilemma concerning the origin of things arise from our arbitrary laws, but also no such dilemma exists in the requirements of experience itself. The practice of 'induction of principles from phenomena', the origination of which Halley saw and did so much to facilitate, is still an endeavour worthy of our utmost effort.

[4] In course of publication.

TRANSFORMATION OF PNEUMOCOCCAL TYPES

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GRIFFITH[1] was the first to show that an attenuated and non-encapsulated 'rough' (*R*) variant of one specific type of Pneumococcus could be transformed into a virulent and encapsulated 'smooth' (*S*) form of another specific type. The transformation was accomplished *in vivo* by injecting subcutaneously a small amount of living *R* culture of Pneumococcus Type II together with a relatively large inoculum of heat-killed organisms derived from a fully virulent *S* strain of Pneumococcus Type I or III. The living *R* strain alone failed to kill mice, whereas the addition of the heat-killed Type I or III organism caused a fatal bacteriæmia. The organism isolated from the heart's blood of the infected animals, however, was not a virulent Type II organism, but a virulent encapsulated Type I or III pneumococcus according to the type of the heat-killed *S* vaccine employed. The importance of this observation was soon recognized, and Griffith's findings were confirmed by Neufeld and Levinthal[2], Baurhenn[3] and Dawson[4]. Some time later, Dawson and Sia[5] succeeded in carrying out the transformation of *R* Type II pneumococci into a virulent *S* Type III organism by an *in tivro* procedure.

In order to bring about this change, it is essential that the *R* variant should be in a reactive phase, since it is only when in this condition that an *R* culture is found to respond to the transforming stimulus. Once the organisms have assumed the type-specific *S* characters, they remain true to form through serial transfers in ordinary media and through repeated animal passage. In this work the greatest care was taken that the *S* vaccines employed contained no viable organisms.

The earlier work of Dawson and Avery[6] and Dawson[4] showed that the conversion of *R* pneumococci to the *S* form of the same type could frequently be accomplished by growing the organism in anti-*R* serum. In effecting this transformation of type *in vitro*, anti-*R* serum is generally added to the culture medium, although it is recorded that the transformation can frequently be achieved in the absence of *R* antibodies. It is now known that to obtain repeatable results, sulphonamide inhibitors must be removed from the nutrient broth used as culture medium. The transformation has never been observed to occur in the absence of serum, and failure to induce the conversion of resting cells seems to indicate that transformation takes place only during the active reproduction of the cells. Attempts to use solutions of the *S* organisms, obtained by freezing and thawing and afterwards heating at 60°, in place of whole bacteria, were mostly without success. Alloway[7], using extracts of virulent Types I and III pneumococci, was successful in effecting the transformation of an *R* Type II culture into fully virulent Type I and III organisms respectively. The conversion of *R* Type II into *S* Type I was more difficult and usually required several sub-cultures before the conversion was finally established. Alloway showed that serum from the sheep, rabbit, guinea pig, horse or man could be employed irrespective of its content of anti-*R* immune-body, thus indicating that some property in serum other than the anti-*R* component is essential if transformation of type is to occur. The presence of the specific polysaccharide of a heterologous type failed to induce the conversion of *R* Type II organisms into the heterologous *S* form.

The transformation of *R* pneumococci into a virulent *S* form means that the organism has acquired the property of producing the specific capsular substance, which for the Type III pneumococcus has been shown to be a polysaccharide built up from 4-β-glucuronosidoglucose units[8]. It would appear, therefore, that in the presence of a specific factor contained in an extract of the *S* Type III organism, the *R* Type II pneumococcus develops the capacity to elaborate the Type III specific material. In a later paper, Alloway[7] showed that potent extracts, as active as the original *S* vaccine in causing the $R \rightarrow S$ transformation, were obtained by dissolving the *S* cells in sodium desoxycholate, and that the active factor could be freed from certain of the accompanying impurities by precipitation with alcohol, or by adsorption of the contaminating substances on charcoal.

After a lapse of rather more than ten years, progress on this subject has once again been brought to notice by the publication of a paper by Avery, MacLeod and McCarty[9] which describes the isolation and identification of the active transforming principle present in an extract of *S* pneumococci (Type III). The evidence of its nature is based on an examination by chemical, enzymatic and serological analysis, and by electrophoresis, ultracentrifugation and ultraviolet spectroscopy. Within the limits of the methods