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Cosmic Ages
More About Theoretical Entities
Testing Significance
The Modern Theory of Liquids
Zone Melting
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Singing Sands
Research Report

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EDITED BY A. W. HASLETT

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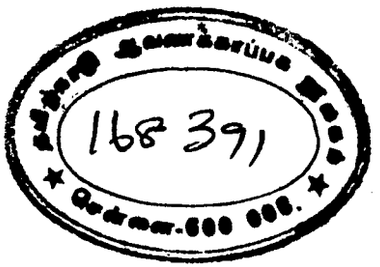

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COSMIC AGES

P. REASBECK

UNTIL very recently a curious anomaly existed in our experimental information about the age of different parts of the universe. Thus the earth appeared to be twice as old as the universe of which it formed a part, while some of the meteoritic bodies which fell on the earth seemed to be twice as old again. This naturally provided a very serious obstacle to any theory which strove to draw, from observational data, a logical and comprehensive picture of the way in which the universe evolved to its present state, and seems to have led to two widely different theories of the way in which the universe has been formed. On the one hand there are the ideas built around the theme of an expanding universe, which had a definite beginning in past history and has continued to evolve ever since, and on the other hand, the concept of a universe which had no beginning and no limit, in which atoms, gas and dust clouds, stellar bodies and galaxies are being continuously and spontaneously formed out of the void. The theory of an expanding universe could not contain the fact that a few component bodies were very much older than the rest of the universe, whilst the theory of continuous creation could.

To examine the picture in more detail, it is necessary to consider first the different kinds of objects to which it is possible to apply experimental and observational methods of age-determination. The obvious first choice is the earth, since this easily provides much material for direct experimental investigation, and, in fact, a very large amount of work has been done on determining the age of different strata of the earth's crust by many different methods. The only other cosmic material which is as yet available for direct investigation in the laboratory is that from meteorites which have fallen on the earth. It is also

possible to look further afield, and study the solar system, the sun and planets, chiefly by means of optical observations, for any facts which give an estimate of its age. Next we must go much further afield, first to the stars, then to the galaxies, and finally to the universe as a whole, for further information.

However, it would be as well at this stage to look briefly at the two different concepts which have been put forward to try to explain the observed facts about the composition of the universe, and since these two seem to be in direct contradiction, we may be permitted to label them as 'Evolution *versus* Permanence'.

On the one hand, the key to the concept of cosmic evolution was provided about thirty years ago by E. Hubble's discovery that the galaxies, or families of stars, which make up the universe are in a state of rapid dispersion (the expanding universe). This naturally leads to the idea that, at some time in the past, all the matter in the universe must have been squeezed together in a highly compressed mass and has been expanding and evolving from this time.

On the other hand, the alternative concept seems to be that the universe has existed in about the same state throughout eternity (the steady-state universe), and that the 'empty spaces', produced as a result of the continuous expansion of existing galaxies, are filled in by the creation of atoms, gaseous nebulae, and new stars in intergalactic space by a process of continuous creation.

In order to help in obtaining an informed estimate of which of these two concepts best fits the observed facts, it is necessary to collect all available information about the probable age of the basic parts and features of the universe.

THE AGE OF THE EARTH

There are many different ways of 'dating' various parts of the earth, but there is space here for only a few of them. Throughout, the term 'billion', taken to mean a thousand million, is used for convenience. The first thing to do is to see if we can set an age for the atoms themselves from which all material is built. It seems that the natural radioactive elements can give us some

clue here. A radioactive isotope will 'decay' at a certain rate peculiar to that isotope regardless of any known chemical or physical influences which may be applied to it. Thus, it seems that if the radioactive elements had been formed too far back in time they would all have decayed completely by now. Further, the observed relative abundances of the various radioactive elements can give us an estimate of how far back in time they were formed. Thorium, and the common isotope of uranium, uranium-238, are not appreciably less abundant than the other heavy elements, for example, gold, mercury, and bismuth. Now the half-lives (i.e. the time taken for a radioactive element to decrease its original quantity to one-half, or from one-half to one-quarter, etc.) of thorium and uranium-238 are 14 billion

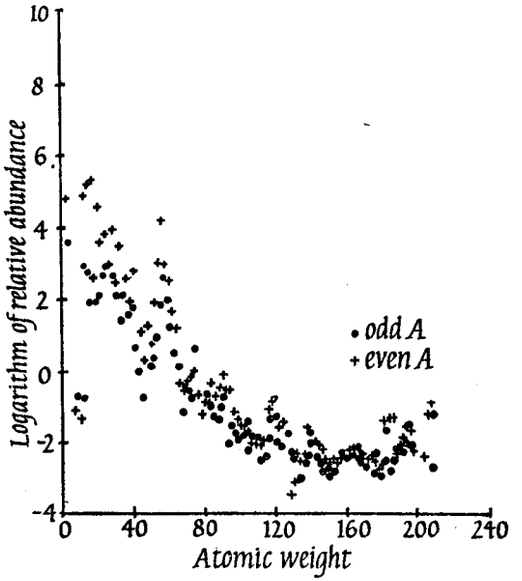

Fig. 1. Relative abundance of the elements plotted against atomic weight.

and 4.5 billion years respectively. If the atoms had been formed 30 billion years ago, the abundance of thorium would only have decreased to a little under one half of its original value, whilst the uranium-238 would have decreased to less than one-hundredth of its original amount. Conversely, if this time had elapsed since the formation of the atoms, the abundance of uranium-238 would originally have been extremely high, which would not have agreed with the way in which the abundances of the heavier elements fall off with increasing atomic weight (Figure 1). Thus, an age of a few billion years rather than a few tens of billions of years seems preferable. This conclusion is more strongly emphasized by considering the instance of the much rarer isotope, uranium-235, which has a half-life of 0.71 billion years, that is, its quantity would be decreased by a half every 0.71 billion years. It must then have taken about seven such periods, about 5 billion years, to bring the uranium-235 down to its present abundance of 0.72 per cent ($(\frac{1}{2})^7 = 0.78$ per cent). Had the age of the atoms been only twice this value, namely 10 billion years, the original abundance of uranium-235 would have been over 16,000 times its present-day amount, which again does not seem likely. Further, a consideration of Table I shows that no natural radioactive elements with a half-life shorter than 0.71 billion years have been found. This argues that the formation of the atoms must have taken place not much less than a few billion years ago.

TABLE I: *Naturally occurring radioactive elements*

Element	Half-Life (years)	Isotopic Abundance (%)
bismuth-209	3×10^{17}	100
neodymium-144	5×10^{15}	23.9
indium-115	6×10^{14}	95.8
rhenium-187	4×10^{12}	62
samarium-147	1.3×10^{11}	15.1
rubidium-87	6.2×10^{10}	27.2
lutecium-176	2.4×10^{10}	2.6
potassium-40	1.2×10^{9}	0.012
uranium-235	0.71×10^{9}	0.72

From these figures, it seems that the isotopes with a half-life long compared to 5 billion years are still present in high abundance, while those with a half-life smaller than about 5 billion years are now in low abundance. It must be pointed out that there is no evidence to show that all isotopes of a given element were produced in exactly equal amounts, and this, in fact, is not to be expected. However, from the above considerations, it seems that the most acceptable figure for the age of the atoms themselves is about 5 billion years.

The next aspect that can be considered is the age of the earth's crust, and a very great deal of work has been done on this subject. In this case, as well as attempting to find the oldest possible minerals in order to determine how old the crust of the earth may be, it is of great interest to geologists to determine the age of various rock strata, in order to build up a chronological picture of the development of the crust itself. Many different methods, again depending on radioactive processes, have been used to catalogue the ages of the rocks; and only a few of the methods are mentioned here.

The uranium-lead and thorium-lead method depends upon the fact that the two uranium isotopes, uranium-235 and uranium-238, produce the stable lead isotopes, lead-204 and lead-206 respectively, as the end product of their disintegration, while thorium decays finally into lead-208. These radiogenic lead isotopes differ from the common lead isotope, lead-204, which is not the product of decay of any natural radioactive element. As long as the material containing the radioactive elements is in the molten state, various chemical and physical processes may separate the radiogenic lead from the molten substance, but as soon as the material solidifies, the lead remains locked in at the place of its origin. By determining the amounts of radiogenic lead isotopes and the amounts of the lead-producing radioactive element (i.e. the ratios lead-206/uranium-238, lead-207/uranium-235, lead-208/thorium-232) and, knowing the corresponding decay rates, one can estimate the time at which a given radioactive ore was formed. In this way, results similar to those in Table II have been arrived at:

TABLE II: *The ages of some ores by methods using radiogenic lead*

Mineral	Locality	Age (billions of years)	Geological Period
pitchblende	Colorado (U.S.A.)	0.058	Tertiary
pitchblende	Belgian Congo (Africa)	0.580	Pre-Cambrian
uraninite	Wilberforce (Canada)	1.035	Pre-Cambrian
uranite	Manitoba (Canada)	1.985	Pre-Cambrian

This method gives an estimate of the period for which the radioactive deposits have been held in the same place, that is, the time at which the uranium-containing rocks solidified. However, by a method which is too lengthy to detail here, Professor A. Holmes was able to extrapolate back the results obtained from uranium-238/lead-206 and uranium-235/lead-207 ratios beyond the time of rock solidification to the time when the material constituting the earth was formed, and gives this date as about 3.5 billion years ago.

Several other methods have been applied to dating the rocks of the earth's crust, and, of these, the measurement of the strontium/rubidium ratios in lepidolites and biotites are of special interest. The rubidium-87 decays to strontium-87, so that the age of the rubidium-bearing rock can be determined in a way analogous to the lead method. However, by this method, the greatest ages yet found for rocks have been reported (Table III):

TABLE III: *The Ages of some Pre-Cambrian rocks by the strontium/rubidium method*

Mineral	Locality	Age (billions of years)
lepidolite	Winnipeg River (Canada)	3.36
biotite	Saskatchewan (Canada)	3.23
lepidolite	U.S.A.	3.57
lepidolite	South Africa	3.80

These dates are important, since, if correct, they imply a greater age for the earth's crust than has been considered hitherto.

Another method of approach which does not involve radioactivity is that of computing the age of the oceans. The salinity of the oceans is mainly due to the salt washed into them by the rivers from the surface of the earth. If one divides the amount of salt in the oceans by the amount brought down yearly by the rivers, one arrives at the figure of 300 million years needed for the oceanic salt to have accumulated. However, this figure is on the low side, since the present rate of oceanic salt deposition is known to be exceptionally high. During the greater part of the earth's history between the periods of mountain raising and erosion the surface of the continents was quite flat. It is estimated very roughly that during the flat periods, the erosive action of the rivers must have been only one-tenth of what it is at present. This would then give us an age for the oceans of a few billion years.

The age of the earth's atmosphere has also been investigated. It is considered that virtually all of the argon-40 in the earth's atmosphere has been produced by the decay of radioactive potassium-40 in the earth's crust. Now, it is possible, from seismic and geological evidence, to form an estimate of the amount of the earth's crust which has been remelted by volcanic activity, or has been eroded, since the earth first assumed a form in which it was able to retain the gases of its atmosphere. From a knowledge of the potassium-40 content of the earth's crust, and the half-life of this isotope (i.e. its rate of production of argon-40), it is possible to arrive at a figure for the time taken to generate the amount of argon-40 present in the atmosphere today, and this turns out to be about 5 billion years.

THE AGE OF METEORITES

The meteoritic material which falls on the earth from some as yet unknown source varies from iron containing up to 15 per cent nickel, through varying proportions of iron-silicate mixtures, to masses of silicate material only (Inset 7 and 8). These meteorites vary in size from masses of many tons, which are

thought to have produced craters such as the one in Arizona (Inset 11) with a diameter of one mile and a depth of 600 ft, to those smaller than a pea, most of which probably never traverse the earth's atmosphere, and even those which appear as meteoritic dust.

The age of these objects, that is, the time since which they last solidified, was first investigated by the helium method. The very small amounts of uranium and thorium which a meteorite contains can be measured, and both uranium and thorium emit alpha particles, or helium nuclei, as they decay to lead. The very small amount of helium gas thus generated in the meteorite, of the order of one-millionth of a cubic centimetre per gramme of meteorite, can be accurately measured (Inset 9), and the time during which this helium has built up can be ascertained from the decay rate of the uranium and thorium. This method originally gave an age of 7.5 billion years for the oldest meteorite, older than the estimate for any other part of the universe. However, during the last few years it was realized that cosmic radiation in outer space, which consists chiefly of very energetic fast-moving protons (hydrogen nuclei), could probably cause 'disintegrations' of the atoms of iron, silicon, etc., in colliding with these meteorites. Many of the resulting fragments would be in the form of helium nuclei, of mass 3 and 4 in roughly equal proportions, whereas any helium nuclei produced radiogenically would all be of mass 4. When the isotopic composition of meteoric helium was measured, it was found to contain up to 30 per cent of helium mass 3, so that it was immediately obvious that in many meteorites, particularly irons, the major proportion of the helium was produced by cosmic rays. When the best possible allowance for this effect is made, it appears that the age of iron meteorites should be lowered to about 0.2 to 0.3 billion years, whilst the age of stone meteorites is at least 3.5 billion years.

Another method of determining the age of stony meteorites depends on the fact that the isotope of potassium, potassium-40, decays to argon, argon-40. Here again, the amount of potassium-40, the volume of argon, and the half life of potassium-40 have been measured, and by this method the age of stone

meteorites appears to be up to about 4.5 billion years. Thus, the newest determinations on the ages of meteorites remove the previous difficulty of an age greater than that of any other part of the universe, but pose a new question to be answered; namely why is it that the iron meteorites seem to be much younger than the stones? One possible explanation would be that meteorites are fragments of a large planetary body which was somehow disrupted late in the history of the solar system and in which iron masses had remained molten long after the higher melting silicates had solidified.

THE AGE OF THE MOON

The recession of the moon from the earth was demonstrated by the work of George Darwin, and is at present at a rate of about 5 inches per year. The gravitational attraction between the moon and earth results in tides, so that the friction caused by the flow of water on the earth slows down its speed of rotation, and, in accordance with the law of conservation of angular momentum, causes the gradual increase in the distance of the moon from the earth. From the relationship connecting the rate of recession with the present distance between the two (about 240,000 miles) the moon must have been practically in contact with the earth about 4 billion years ago.

We need not choose between the different hypotheses which strive to account for the formation of the moon from the earth. For example, one suggests that the material of the moon was drawn out from the earth by the tidal attraction of the sun, perhaps leaving behind the hollow which is now the Pacific basin; another that the earth and the moon were both thrown out together by rotational forces as lumps from one of the larger primeval masses of which the majority remaining formed one of the larger planets. In spite of this uncertainty, the ages of the moon and earth seem unlikely to be much greater than the time which has elapsed since they were almost or actually in contact, namely 4 billion years.

Proceeding now somewhat further afield, the information that we have about the properties of the planets and their

satellites is such that we are unable to produce good estimates of their age. For a further age determination, we have therefore to look at the sun in conjunction with the other stars.

THE AGE OF THE SUN AND OTHER STARS

We can arrive at an idea of the age of the Milky Way, the great collection of stars of which our sun is merely a humble member, by considering the energy source from which the stars derive their heat and luminosity. It was recently found by H. Bethe and C. F. Von Weizsäcker that one particular series of nuclear reactions could account satisfactorily for the maintenance of the sun's expenditure of radiant energy. This is known as the carbon-nitrogen cycle, and results in the building up of hydrogen atoms into helium atoms, with subsequent release of energy, atoms of carbon and nitrogen virtually playing the role of 'catalysts' (Figure 2).* Now, from a knowledge of the energy released in this process, from the mass of hydrogen in the sun and from the energy liberated by the sun, it is possible to calculate that the sun would take 50 billion years to use up all the hydrogen available as fuel. But stellar brightness increases as the cube of the mass of the star. Thus a star twice as big as the sun uses up fuel 8 times as fast, and therefore will only take 1/4 as long to burn up all its fuel.

This difference in life-span with size provides a convenient way to estimate the ages of the stars. Observational evidence indicates that a profound difference exists between those stars which are lighter than four sun-masses and those heavier. Stars of the first kind form the main proportion of the population, and are quietly behaved; stars of the second kind are much less numerous and behave in an excited way, spinning wildly and ejecting streams of hot material from their tidal bulges. Further, the stars with a mass of just about 4 sun-masses appear to be very unstable, some swelling up to an enormous size and pulsating with varying brightness; the so-called Cepheid variables. Others apparently give outright explosions, varying from the

*See also the article by C. M. Bondi and H. Bondi on 'The Energy of the Stars' in *Science News 34*.

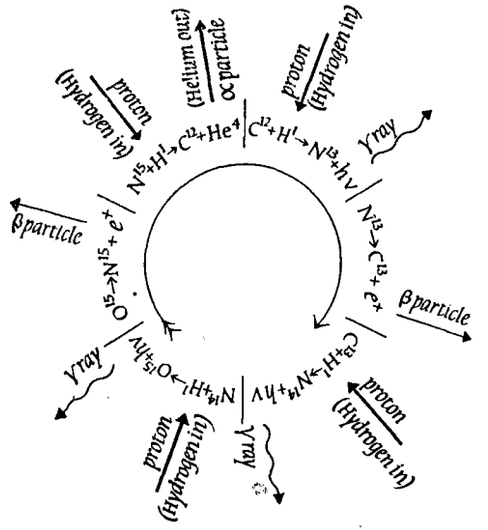

Fig. 2. The cyclic nuclear reaction converting hydrogen to helium in the sun, and releasing vast amounts of energy.

periodic minor outburst of U Geminorum stars up to the spectacular explosions (supernovae) which make a single star temporarily as bright as the entire galaxy to which it belongs (Inset 10). It seems that these convulsions are a sign of the fact that a star has used up all its hydrogen fuel and is 'dying'. Applying the previously mentioned relationship between stellar mass and life-span, we find that these stars which are now nearing their thermal end must have a life-span of 3 to 5 billion years.

Again, we can derive the apparent ages of various other sections or features of the universe. For example, if we consider the galactic clusters, that is, groups of stars which are close together and moving in the same general direction, we find that such groups cannot hold together indefinitely, but will gradually be scattered apart as a result of gravitational interaction with passing stars. Calculations show the average time which such groups

can hold together is between 1 billion and 10 billion years, and since there are of the order of several hundred of these groups still existing in our Milky Way, its age cannot be greater than a few billion years. There are still other ways of arriving at an estimate of the age of the Milky Way and in each case the answer would appear to be about the same, namely a few billion years.

THE AGE OF THE UNIVERSE

There remains for consideration the one method which attempts to measure the age of the universe as a whole. This method depends on the behaviour of a type of star, mentioned earlier, whose brightness changes periodically in a regular manner. These stars, known as 'Cepheid variables', change their luminosity because of periodic pulsations of their giant bodies. A simple relationship exists between the period of these pulsations and the absolute luminosity of the star; the longer the period, the brighter the star. By measuring the pulsation period of a star, we can thus arrive at a measure of its absolute brightness. This fact, in conjunction with its visual brightness, tells us the actual distance of the star.

As well as being familiar objects in our Milky Way, 'Cepheid variables' have been found at greater distances. This discovery, made by E. Hubble, led to still further progress in our knowledge of the universe. Up to this time, spiral nebulae, which always have well-defined shapes, consisting of a central body with a pair of spiral arms winding round it, had been generally thought to have their locations in the Milky Way, and to be possible examples of stars evolving their own planetary system according to the classical concept of I. Kant and P. S. Laplace. However, the discovery by Hubble of Cepheid variables in the arms of these nebulae immediately enabled their distances to be calculated. The Andromeda nebula was found to be 1 million light years away, that is, about a hundred times the diameter of the entire Milky Way, and other nebulae to be at even greater distances. Thus, if they are so far away, they must be very much larger than previously suspected, and are in fact about as large as our own galaxy. W. Baade was later able to resolve photo-

graphically many of the myriads of stars which make up the Andromeda nebula.

Further, the 'reddening' of the light emitted by these nebulae could now be correctly interpreted to give their speed of recession. In terms of the Döppler effect, when the source of light is receding from us, the light waves are lengthened, and all colours are shifted towards the red end of the spectrum (Figure 3). By measuring the 'degree of reddening', the speed of recession could

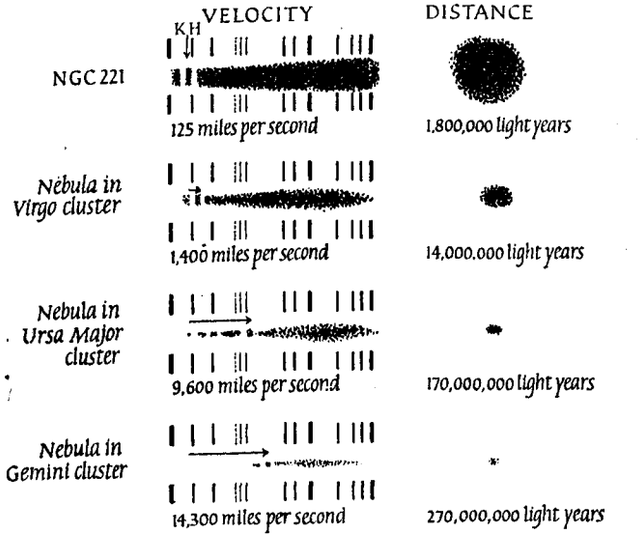

Fig. 3. To show the velocity-distance relation for extragalactic nebulae. The arrow marked KH in the top diagram points to the H and K spectral lines of calcium in the nebular spectrum, which is the blurred central portion in each diagram. The regular lines above and below this are a comparison spectrum of helium. In the second, third, and fourth diagrams, the horizontal arrows show the displacement of the H and K lines towards the red end of the spectrum, the corresponding speed of recession being given. The diagrams on the right (all on the same scale) illustrate the decrease in size and brightness with increasing velocity or red shift.

be obtained. Thus the new picture emerged that the whole of space was populated by billions of galaxies like our own, the whole in a state of rapid expansion, with all the members flying away from each other at great speeds.

From a knowledge of the speed of recession and the distance away of each of the galaxies, it was found that, in all cases, these two properties are interrelated by what is known as Hubble's Law, which states that the speed of recession of a galaxy is directly proportional to its distance away. Furthermore, in order to find out how long ago this expansion started, we need only to divide their present mutual distances by their velocity of recession.

Since the recession velocities are directly proportional to the distances the result of the division is always the same, no matter whether we take two neighbouring galaxies, or two that are far apart. Now, with the knowledge available up to the last few years, the date at which the expansion started came out to be about 2.5 billion years ago. As has been stated previously, this was a very serious discrepancy at the time, since several objects in, or sections of, the universe seemed to be about twice as old as this, with some meteorites a good deal older still, and consequently gave rise to misgivings about the view that the date of the beginning of expansion represented the date of the origin of the universe in its present form. However, it was observed by W. Baade in 1946 that there were in fact two different kinds of Cepheid variables, and not just one. Type I Cepheids steadily brighten and dim, but type II Cepheids 'brighten, dim a bit, level off, and then dim a great deal more before they repeat the process'. Now, as mentioned previously, the distance of the Andromeda nebula had been used as a yardstick to measure all other astronomical distances; but, with this new knowledge, it was found that the Cepheids in Andromeda, used to establish this distance, are actually of type II and not type I. This, of course, alters the relationship between distance and period of pulsation, that is, between distance and brightness, and the net result is, that, with present knowledge, the distance of the Andromeda Nebula is just doubled, and hence, all the distances of the

galaxies are doubled. From this, it is immediately obvious that the constant in Hubble's Law must be decreased by a factor of two, and that the age of the 'expanding universe' must be doubled, to almost 5 billion years. And now, it seems that satisfactory agreement is emerging between the estimated ages of the various components of the universe. This may be taken as a strong point in favour of the theory of the expanding universe which suggests that about 5 billion years ago the whole of the matter in the universe was in an extremely highly compressed form and at a very high temperature, probably about 15 billion degrees absolute. Since that time, the universe has been expanding with increasing rapidity, the temperature falling to its present value in interstellar space of about 100 degrees absolute.

The processes which may have been involved in the production of atomic species at the beginning of the expansion, when the temperature was still very high, and the processes involved in the condensation of gases and agglomeration of particles leading to the formation of the stars and planets are somewhat beyond the scope of an article on cosmic ages. It has been argued that such a rapid expansion as would be necessary to explain the present rate of recession would rule out any chance of condensation of gases and agglomeration of particles needed to initiate star building, and that such a picture of the origin of the universe is untenable. This, however, seems to be an open question. The view put forward by Sir James Jeans half a century ago that a gas subjected to gravitational forces and filling an unlimited space is intrinsically unstable and is bound to break up into separate giant gas clouds, leading to a condensation and agglomeration process, seems equally feasible.

However, to see how the idea of a steady state universe would fit into a consideration of cosmic ages, let us first consider the mechanism of the steady state model itself. It has been suggested that atoms are continuously being created out of nothing all through interstellar space. These atoms then enter into the process of condensation, forming huge gas clouds and then stars and galaxies, steadily replacing the older galaxies which are receding at an ever increasing rate. This creation is said to 'drive'

the universe, because the new material produces an outward pressure which leads to the steady expansion. If we were able to take a series of photographs at widely spaced time-intervals of all the galaxies which can be observed with our largest telescope, and these photographs were then run together to form a continuous film covering a period of thousands of millions of years, the overall picture would be the same at the beginning of the film as at the end. As the older galaxies moved farther and farther away from us, and vanished altogether, their place would be continuously filled by new galaxies appearing out of nowhere, and also receding from us. Conversely, if the film were run backwards the universe would appear to be contracting, with galaxies appearing on the fringe of the picture as faint objects which came nearer and nearer to us until they vanished before our eyes. The stars of the galaxy would evaporate back into gas, the gas would evaporate into the background of separate atoms, and the atoms would disappear into nowhere. Also we could run our film forward or backward for ever and the overall features of the universe would remain unchanged. In such a concept as this, then, any general agreement between the ages of various parts of the universe would not be possible since different galaxies, and even, perhaps, different groups of stars in a galaxy, would have different ages. Thus, all possible ages of galaxies would be represented, from very young ones up to 'infinitely old' ones. Of course, we would not be able to observe any infinitely old ones, since our biggest telescope can only detect galaxies up to a distance of about 1 billion light years away, and, with the biggest telescope that could ever be made, the maximum possible distance would only be 2 billion light years, since galaxies at this distance away would be receding from us at the speed of light, so that the light from any galaxies farther away than this distance would never be able to reach us. Consequently, any galaxies of more than a certain age would long ago have receded from our view.

The idea of atoms being continuously created out of nothing is probably no stranger than the idea of a definite time of 'creation' of the universe, with the expansion beginning from the

highly compressed and heated form. Although theoretical arguments have been advanced both for and against the steady-state universe, one piece of observational evidence which has been cited as an argument against such an hypothesis is as follows:

Several years ago, J. Stebbins and A. E. Whitford decided to study colour distribution in far-off galaxies and picked out for the purpose four groups of galaxies in the constellations Virgo, Coma Berenices, Corona Borealis and Boötes (about 12, 80, 240, and 480 million light years distant, respectively). It was discovered that the farther away these galaxies were, the redder they looked. This general change of colour, which is different from the Döppler effect of a red shift in spectral lines, might have been explained by the presence of dust in intergalactic space, in a similar manner to the reddening of the setting sun near the horizon, but would, in fact, need so much intergalactic dust as to be incompatible with other astronomical observations. Further, the most recent observations indicate that only certain types of galaxies, elliptical (armless) galaxies, show this reddening and spiral galaxies do not, whilst, if the reddening were due to dust, all galaxies would show some reddening effect.

It is now thought that this observed reddening of distant galaxies must be due to the different relative numbers of red and blue stars of which they are composed. But, if we look at a galaxy which is 480 million light years away, we see it as it was 480 million years ago. Since it is to be expected that the stellar populations of galaxies will evolve with time by the process of burning up their fuel supplies, as was outlined earlier, it is also to be expected that the galaxies could have contained more red giant stars in their youth than they do now, so that the galactic populations are evolving in a regular manner with increasing age. That is, the far distant galaxies which we are now seeing as they were in their youth contain the highest proportion of red giants, whilst the nearest galaxies which we now see at a much more advanced age have lost a much larger proportion of their red giants by the evolutionary change to white dwarfs. However, if such an explanation of the observed reddening is accepted (and no alternative has yet been suggested), this points to the

TABLE IV: *Cosmic ages, classified by subjects and methods used*

Subject	Method	Age (billions of years)	Author and Date
earth	isotope abundances	5	
	composition of terrestrial and meteoritic leads	4.5	F. G. Houtermanns (1953)
			C. C. Patterson.
			H. Brown & G. Tilton (1953)
	Sr-Rb ratios in minerals	4	L. O. Nicolayson, L. T. Aldrich & J. B. Doak (1953)
	Age of oceans	few billion years	A. Holmes (1937)
	A^{40} content of atmosphere	5	M. A. Shillibeer & R. D. Russell (1955)
earth-moon	Tidal friction	4	H. Jeffreys (1952)
meteorites	A^{40}-K^{40} content	4.5	J. Wasserburg & R. Hayden (1955)
	Helium/U-Th content	at least 3.5	P. Reasbeck & K. I. Mayne (1955)
stars	Theory of evolution of stars	5.1	A. R. Sandage (1953)
	Age of Galactic Clusters	few billion years	B. J. Bok (1946)
	Equipartition of kinetic energy	about 2.5	F. Gondolatsch (1948)
universe	Rate of expansion	5	E. Hubble & W. Baade (1950)
	"	3.5 to 7.8	A. R. Sandage (1950)

fact that the general properties of galaxies in the past differed from those of galaxies today, which would contradict the hypothesis of a steady state universe.

Thus it would seem that, wherever observational evidence can be interpreted in such a manner as to give an indication of the age of a part of the universe, all such age determinations arrive at a figure fairly close to 5 billion years for the time since the universe began to evolve in its present form, with ages younger than this for features which must have evolved at a later stage, such as the solidification of the meteorites or the surface rocks of the earth. Some of these figures are summarized, together with their authors, in Table IV. The chronological time-scale for the evolutionary process has now been put in order, and the previous contradictions have been resolved.

ZONE MELTING

C. H. L. GOODMAN

KNOWLEDGE of a new research technique often takes a considerable time to spread from the specialized field in which it was first applied to other fields in which it may be of at least equal value. The new set of techniques discussed in the present article appear to be a case in point. They have been developed, and applied, almost exclusively in connexion with semi-conductors, though their more general application in metallurgy and in inorganic and organic chemistry would seem to be of the greatest promise, particularly for obtaining materials of extremely high purity.

The techniques of distillation, to which the new techniques are in some respects analogous, are restricted in their application to relatively volatile materials, which rules out their use for the purification of a vast range of involatile materials – most metals and other crystalline solids, including compounds of high molecular weight. Just as distillation is based upon the progressive boiling and condensing of a liquid, the new techniques are based upon the progressive melting and freezing of a solid, and thus can be applied to many of the involatile materials mentioned above. They are conveniently termed 'zone melting techniques' since they depend upon the passage of molten zones along a bar of the solid being treated. They can be applied to any solid which can be melted without appreciable decomposition, and by their use materials can be obtained in a very pure state, often purer than is possible using more conventional methods.

So far some fifteen metallic or semi-metallic elements have been purified by zone melting, among them: copper, zinc, aluminium, titanium, iron, lead, and tellurium. With many of these elements it has proved possible to reduce the amounts of impurities present to well below the limit of direct spectrographic

detection. (Impurity concentrations of the order of one part in one hundred million (0.000001 per cent) have been achieved in some cases.)

Relatively few compounds have been treated by the new techniques. Most of them are semi-conductors; indium antimonide (InSb) and cadmium telluride (CdTe) are examples, and it has been found that materials of extremely high purity can be obtained. In organic chemistry an example of the advantages of zone melting is afforded by recent work on the purification of naphthalene. Small traces of anthracene, of the order of 0.01–0.1 per cent, markedly affect the fluorescent properties of this material. A very simple zone melting procedure reduced the anthracene content below the one part per million level (0.0001 per cent). This was achieved without the use of solvents or chemical reagents, merely by slowly passing two molten zones along a glass tube filled with the compound.

APPARATUS AND PROCEDURE

Apparatus for purification by zone melting can be very simple, especially if the material to be treated has a low melting point. For most reasonably low-melting substances all that is required is a glass tube to hold the material, a heating coil to form the zone, and a means of moving this coil slowly along the tube. A single apparatus on these lines is sketched in Figure 1. The heating coil is made from 'Nichrome' or 'Kanthal' wire thick enough to be self-supporting (i.e. 15 gauge). It is conveniently fed from a low-voltage transformer. A power input of about 200 watts is adequate for forming a zone in metals melting below about 600° C, provided that the area of cross section of the bar or rod to be treated is not more than about 2 square centimetres; for most organic compounds, which have lower thermal conductivities than metals and relatively low melting points, much less power is required. The heating coil is moved by a screw thread which is driven through reduction gearing by a constant speed motor. The gearing can be altered so that the heating coil moves at speeds varying from about one-quarter of an inch up to several inches an hour. The material to be treated, in the form

of powder or a solid bar, is enclosed in a sealed evacuated hard-glass tube.

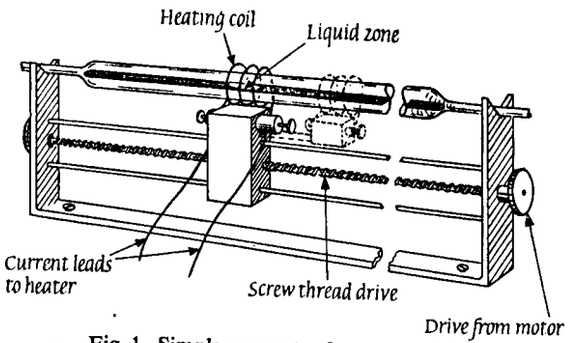

Fig. 1. Simple apparatus for zone melting.

Variations on the scheme shown can readily be made to suit circumstances. For example, if the material to be treated attacks glass it can be held in a suitable inert container or trough within the glass tube, while a stream of protective gas can be passed over it if required. For materials of rather higher melting point (up to 900–1,000° C) a tube of transparent silica, rather than of glass, is required, while external lagging of the heating coil to reduce radiation loss is essential. A certain amount of lagging is in any case advisable in order to minimize sudden changes in temperature due to draughts which by altering the size of the zone would affect the zone melting process (see below). At still higher temperatures, pick-up of impurities by reaction with the containing tube becomes increasingly a limitation. One way of getting round this difficulty is by dispensing with a container and relying on surface tension to keep the zone intact. Thus in the 'floating-zone' technique a rod of the material, secured at both ends and held vertically, has a small zone melted in it. If the surface tension of the liquid is sufficiently high and the radius of the rod small enough, the zone so produced is stable and can be passed up or down the rod as required. This technique has been used with silicon (melting point 1,420° C). The latest development on these lines is the 'caged-zone' technique. If a rod of solid, which has longitudinal ribs or fins machined in it, is heated to form a zone the projecting ribs lose heat by radiation more readily than the main body of the rod, which thus is at a higher temperature than the ribs. By suitably adjusting the power input the main body of the rod can be heated above the melting point without melting the ribs, so that a molten zone held by surface tension inside a 'cage' of ribs is obtained. This technique is largely limited to temperatures at which radiation loss is very high, and has been applied to titanium (melting point 1,660° C).

Other variations are in the driving movement and method of heating. Provided that movement is smooth and steady and can be adjusted in the required speed range, other methods of drive may be more convenient (e.g. a water-clock arrangement using the descent of a weighted float in a tank of water). Again, a simple heating coil is not always the best method of producing a zone, particularly at high temperature. Eddy-current heating, for example, offers particular advantages in the case of metals, and its use can be extended to non-metals if a radiating pick-up ring, of graphite or tungsten for example, is used to heat the zone. Such a ring, however, requires protection from atmospheric oxidation. As a final practical point, if a number of zones are to be passed along the bar to be treated, it is often most convenient to couple more than one heater to the advance mechanism, so that two or more zones are passed in a single run.

THEORY OF ZONE MELTING

The fundamental fact upon which zone melting techniques are based is that, generally speaking, impurities are more soluble in a material when it is liquid than when it is solid (exceptions to this rule will be discussed later). If a quantity of liquid containing an impurity is gradually made to solidify, the impurity tends to stay in the liquid phase while the solid which first separates out contains less impurity than did the original liquid. The case of water with sodium chloride dissolved in it can be used as a simple example. If such a solution of salt is partially frozen the ice that is formed contains very little salt while the remaining

liquid is found to be saltier than before. (The increase in salt concentration in the liquid comes about since the salt lost from the volume of water frozen is 'rejected' to the liquid as the ice crystallizes). For relatively dilute solutions it is found that the concentration of salt in the ice is a fixed fraction of that in the liquid it crystallizes from, provided that crystallization takes place sufficiently slowly for equilibrium to be approached. This fixed fraction, or partition constant as it is often termed, is characteristic for the system salt and water, being independent of the concentration (so long as this is not large). One consequence of this is that as a dilute salt solution is progressively frozen the ice crystallizing out from it will contain increasing amounts of salt because of the increase in salt concentration in the remaining liquid.

The effects described for the system salt in water can in fact be generalized. Thus they can also be observed to occur in metals (e.g. copper as an impurity in aluminium) and in organic compounds (e.g. anthracene as an impurity in naphthalene). However, unless the partition constant is small, progressive freezing cannot be used to provide efficient purification. This can be seen from Figure 2, which shows how the impurity concentration in the solid varies as the liquid gradually solidifies. The curves shown are for three different values of the partition constant K, and the concentration scale is such that the initial concentration of impurity in the liquid before any solid separates out is unity. Where K is much smaller than 1, as in the case $K=0.01$ (the bottom curve in the figure), the solid crystallizing out contains but little impurity until nearly all the liquid has solidified. If the last 10 per cent of the liquid is rejected, it will, for $K=0.5$, contain only about 32 per cent of the original impurity, but for $K=0.01$ as much as 99.8 per cent; in the latter case the solid left is nearly 500 times as pure as was the starting material. For still lower values of the partition constant, the purification effected will be still greater.* Where K is of the

*The widely practised technique of crystallization from a suitable solvent rather than from melted solid can be considered as a device for achieving suitably low values of partition constant.

order of 0.01 or less, this simple process of progressive solidification can give a useful degree of purification.

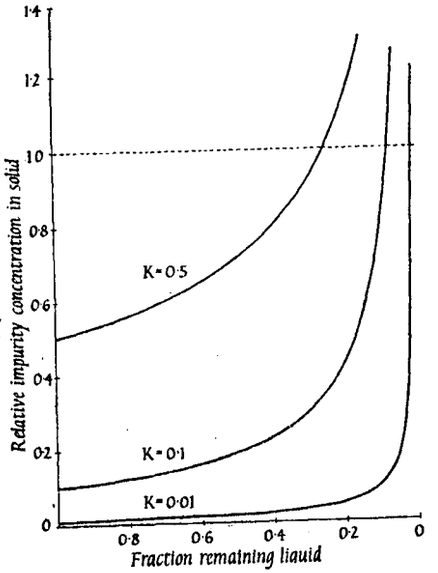

Fig. 2. Variation of impurity concentration in a solid during gradual solidification from a liquid. Curves are shown for three values of the partition constant K.

The obvious drawback to this simple method of purification is that, if repeated, as is usually necessary if great purity is wanted, it is wasteful of material since some must necessarily be rejected in each operation. The great interest of purification by zone melting is that it offers a way out of this difficulty, so that greater purification can be achieved without such losses.

In the simple zone melting process a liquid zone is produced at one end of a long uniform bar of the solid being treated by means of a small heater or furnace. This zone is then passed slowly along the bar by moving the source of heat. When the zone begins to move along the bar, which for simplicity is

assumed to have an absolutely uniform concentration of impurity in it, it melts the solid in front of it and crystallizes out solid behind it, the volume of the liquid in the zone remaining constant. Now the solid crystallizing out contains less impurity than the liquid it crystallizes from, the impurity being preferentially retained in the zone to an extent determined by the partition constant for the system. Thus (as in the case of the progressive solidification of a liquid already described) the impurity concentration in the liquid increases as solid crystallizes out during the passage of the zone along the bar, and since the partition constant is a constant, the concentration of impurity in the solid deposited increases in proportion. The overall effect can be better understood from Figure 3. It is assumed that the partition coefficient in this case is 0.5 (i.e. the impurity concentration in the solid is always half that in the liquid) and the zone length is 1 inch. The figure shows the way in which the impurity concentration varies along the bar during the passage of the zone. Figure 3*a* shows the situation when the zone has moved its own length along the bar, Figure 3*b* when it has moved five times as far. Two points should be noted: The first is that the zone carries or sweeps some of the impurity along the bar; the second is the way in which the impurity concentration in the treated section increases along the bar, asymptotically approaching the value for the untreated solid. This is due to the gradual increase in impurity content of the zone, which is limited to a maximum of $1/K$ times the original impurity content of the solid. At this limit the material melted into the zone is of the same composition as that solidifying from it, and the zone cannot gain impurity. Hence as it moves along the bar the zone gradually loses effectiveness in achieving purification. The degree to which this occurs depends upon the partition constant K. As in the case of the progressive freezing of a liquid, the smaller the value of K, the more effective is the process. The dotted line in Figure 3*b* shows the impurity distribution for $K=0.1$ and it can be seen that the impurity content rises much more slowly in this case than when $K=0.5$, while more impurity is retained by the zone and swept to the end of the bar.

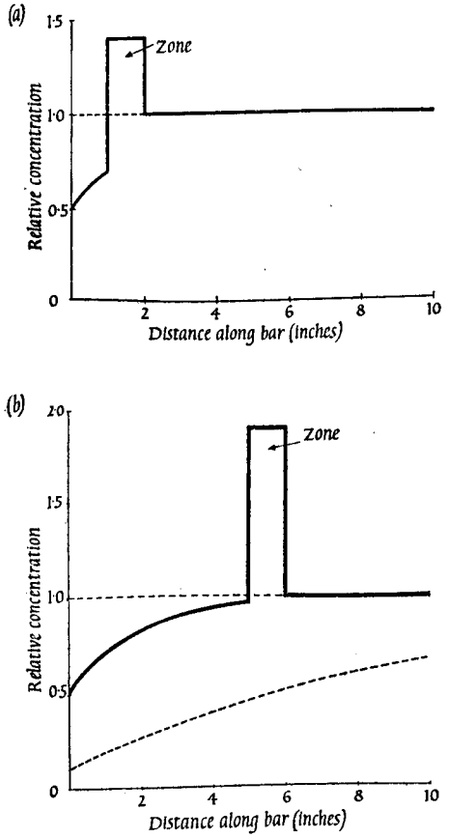

Fig. 3. To show the effect of the passage of a molten zone along a bar in the usual case when the impurity concentrates in the liquid: (*a*) when the zone has moved its own length (1 inch) along the bar; (*b*) when it has moved 5 inches. The continuous curves are for $K=0.5$ in both figures; the dashed curve in (*b*) shows the distribution of impurity for $K=0.1$ when one zone has passed along the bar.

It is occasionally, though not often, found that purification works the other way about, the impurity tending to remain in the solid rather than the liquid; in other words, K is greater than unity. The impurity is not then swept along by the zone but instead becomes concentrated at the end of the bar from which the zones start. Such behaviour is found only where there is some close relationship in valency or structure between the impurity and the material being treated. Nickel as an impurity in copper, and β-naphthol in naphthalene, both with values of K greater than unity, are examples of this. A simple rule which is often useful is that K is less than one for impurities which depress the freezing point and greater than one for those which elevate it. Generally speaking, however, values of K greater than one appear to be rare; and, unless otherwise stated, the following discussion refers to the more usual case in which the impurity tends to remain in the zone.

The particular importance of zone treatment for the purification of materials, even of those with impurities for which the partition constant is not very small, is that the process can be repeated as often as desired. Each time a zone is passed along a bar of the material being treated it tends to sweep along with it some of the impurity present. There is, in fact, a limit to the process, since there is a limiting impurity distribution. This distribution depends upon three factors: the length of the bar being treated, the length of the zone employed, and K. Figure 4 (due to W. Pfann) shows approximate curves for limiting distributions for several values of K in the practically useful case of the zone being one-tenth the length of the bar. It should be noted that the concentration scale is logarithmic, the value zero being the level before treatment. Such limiting distributions begin to be approached after the passage of a relatively small number of zones, of the order of ten to twenty, provided that contamination from the container or the atmosphere does not maintain a higher level of impurity, a very real possibility if K is 0.1 or less. The lowest concentrations are not, in fact, attainable, since the arguments used in deriving Figure 4 are essentially statistical. Such arguments are valid only for large numbers; in the present case they cannot be applied where the number of impurity atoms or molecules is small. In practice, the effects discussed have been observed down to concentrations of the order of 1 in 10^{10} for some materials. While the very great purification attainable by zone melting is extremely attractive, the speed of operation is necessarily slow. There is a speed limit of the order of an inch an hour on the rate of movement of a zone. This will be discussed later. It must be pointed out also that Figure 4 is derived in a way which neglects certain effects at the very end of the treated bar of material. The limiting distributions would be displaced towards higher impurity concentrations at the right-hand side of the diagram if this were corrected for.

The curves of limiting distribution for the case in which K is greater than unity, and the impurity tends to become concentrated at the end of the bar from which the zones start, have

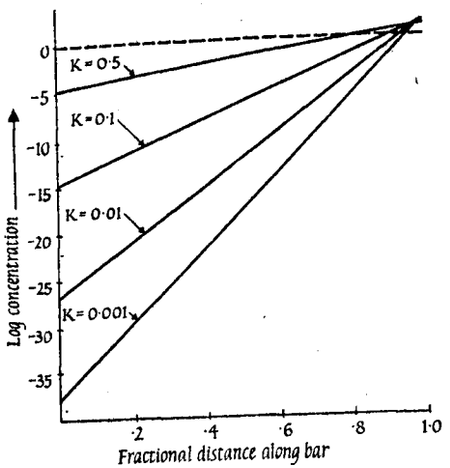

Fig. 4. Calculated approximate limiting distributions of impurity for values of K from 0.001 to 0.5 (after W. Pfann); the concentration scale is logarithmic ($\log_{10}$), and the lower values of impurity concentration (below 10^{-10}) are not attainable in practice.

been worked out but will not be illustrated here. It is possible also for a given material to have some impurities present for which K is less than unity and others for which K is greater than unity. In such a case the impurities are concentrated at both ends, on repeated zone melting, and the purest material is found in the middle of the treated bar.

UNIFORM DISTRIBUTION OF IMPURITIES

So far only an absolutely homogeneous initial distribution has been considered. In practice however such a distribution is rare, for appreciable fluctuations in impurity concentration are frequently encountered. Figure 5*a* indicates an arbitrary distribution in a bar of material in which the impurity concentration varies markedly. If one postulates a partition constant of 0.5 for the system, this distribution is altered to that of Figure 5*b* by passing a zone one inch long from left to right along the bar. It can be seen that the sharp fluctuations shown in Figure 5*a* have largely been smoothed out. (The steep, smooth rise in impurity concentration at the right of Figure 5*b* is due to the final solidification of the zone itself on reaching the end of the bar, when it behaves as a mass of progressively solidifying liquid, the impurity distribution then following the appropriate curve in Figure 1.) A still smoother distribution is obtained if a zone is now passed in the reverse direction, from right to left along the bar; this is shown in Figure 5*c*. (Figures 5*b* and 5*c* were derived by assuming that the zone moved forward in steps of 0.2 inch; the true curves for continuous movement of the zone would be rather smoother.)

Incidentally, had the second zone been passed in the same direction as the first more impurity would have been carried to the right-hand end of the bar, and further such treatment would continue the process, the impurity distribution tending to the limit previously described for the case of an initially homogeneous distribution. In fact it can be shown that, whatever the initial distribution, the passage of zones in one direction will tend to give the limiting distribution.

Returning to Figure 5 it should be noted that the impurity

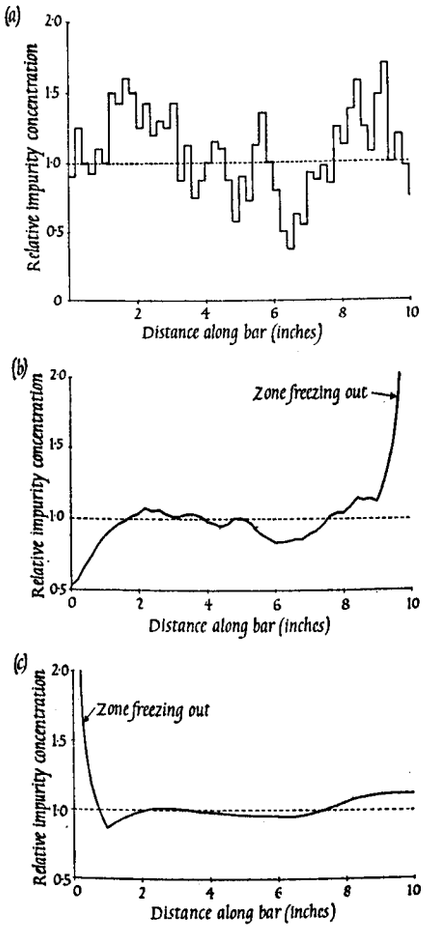

Fig. 5. To show the smoothing effect produced on an initially irregular distribution of impurity by zone melting: (*a*) arbitrary initial distribution; (*b*) after passing a 1-inch zone from left to right ($K=0.5$); (*c*) after, in addition, passing a zone in the reverse direction.

distributions shown indicate an important property of a moving zone: that it acts as a buffer to changes in impurity concentration, tending to smooth them out. It follows from this that the repeated passage of zones in alternate directions along a bar tends to give an entirely level impurity distribution. Such a process is sometimes referred to as 'zone levelling', and can be used to obtain a specific uniform concentration of one material in another.

It may occur that a solid bar with a given homogeneous impurity content is required in a case where the starting material has the correct impurity content but is in the wrong physical form, i.e. it may be reclaimed scrap, or a powder. The zone levelling technique could be used successfully, though many to-and-fro passages of the zone would be required; however, there is a more elegant and economic solution to the problem. A zone is formed at the end of the mass of material, held in a tube of silica or glass, but the impurity content of this zone is increased by the direct addition of impurity to K times its initial value. Such a 'loaded' zone is equivalent to one which has passed along an infinitely long bar of the material and so can neither gain nor lose impurity if it is now moved along the tube. Thus a uniform bar of solid can be obtained in this case with but a single passage of the zone.

A rather different type of 'loaded' zone is also possible. In this case what is added to the zone is not the impurity, nor the material being treated, but a third substance, for which the value of K is very small. Such a substance is more familiar if described as a solvent (liquid) for the material being treated. The advantage of adding such a solvent is that it depresses the freezing point of the solid so that the zone can be kept liquid at temperatures below the melting point. It is also possible that a more favourable value of K for the impurity will be obtained for such a solvent-loaded zone than for a zone of melted solid. The use of such loaded zones enables zone melting techniques to be applied to materials which decompose on melting, or which can only be melted with difficulty. They can be used in most cases in which the familiar process of recrystallization from a solvent is applied.

PRACTICAL LIMITATIONS

Certain assumptions were implicit in the discussion of zone melting processes, and it is necessary to consider these briefly in order to appreciate certain practical limitations of the new techniques.

An important assumption which is not entirely valid is that the liquid in a zone is always perfectly mixed, no concentration gradients existing in it. If the impurity tends to remain in the liquid of the zone rather than in the solid crystallizing from it, the impurity concentration in the immediate vicinity of the solid must tend to be higher than in the rest of the zone. Diffusion of the impurity takes place at a finite rate, so that the more rapidly the zone moves and solid crystallizes from it the more marked this local increase in impurity concentration becomes. In effect a 'bow wave' of impurity is set up in the liquid immediately in front of the advancing solid. Since the solid must contain K times the impurity of the liquid it crystallizes from, it thus has a higher impurity content than if it had crystallized from a perfectly mixed zone. Alternatively one can say that the effective value of K has increased. (In those cases where K is greater than unity the 'bow wave' is a depletion layer and the effective value of K is decreased.) For the techniques described no great change in K can be tolerated, which sets a practical limit to the speed of advance of the zone. This limit differs from case to case, but is usually of the order of one inch per hour. It is this low speed of advance that is the main limitation to the large-scale application of zone melting techniques.

Again, the assumption that the partition constant is in fact constant is strictly valid only for infinitely slow advance rates (equilibrium conditions) and for dilute solutions. At advance rates of the order of those used in zone melting there is in fact no evidence of any appreciable variation in the value of K which cannot be understood in terms of the bow-wave mechanism described above. For appreciable concentrations of impurity (greater than a few per cent) K is found to vary slightly. This variation is usually small and can often be neglected.

No mention has been made of the effect of altering the size of the zone. A large zone tends to reach the limiting impurity concentration (see Figure 4) more slowly than a small one, and thus will sweep more impurity with it; nevertheless the use of a number of small zones rather than one or two large ones of equal total size is more efficient. Any rapid fluctuation in the size of the zone is usually undesirable as it changes the rate of growth of the solid, and hence of K, and in addition alters the slope of the impurity distribution. It is advisable therefore to protect a zone melting apparatus from draughts, sudden changes in gas flow (if a protective gas is used) or fluctuations in power supply. It is only in the 'zone levelling' and 'loaded zone' techniques, however, that absolute constancy of zone size is essential; for purification a certain amount of fluctuation can be tolerated, and the loss in efficiency can be compensated by one or two additional passes of a zone.

Since the impurity distribution in a treated bar of material depends upon both the zone size and the effective value of K it is possible to vary it between wide limits by altering the power input to and the speed of advance of the zone.

CONCLUSION

At present the various zone melting techniques can deal with but small quantities of material, of the order of one kilogramme at most. It is possible, however, that some development on the lines of the recently devised continuous flow zone melting process (which enables a stream of material to be purified, in contrast to a single bar, but at the cost of increased complexity of apparatus) will allow appreciably larger amounts of material to be handled. Till then zone melting must remain primarily a research technique, one of great value.

FURTHER READING

General Theory

The fundamental paper is:

Pfann, W. G. *Journal of Metals*, **4**, 747 (1952).

For a more recent discussion which extends this paper, see:

Goodman, C. H. L. *Research*, **5**, 168 (1954).

The results of many detailed calculations, done with an electronic computor, are given in:

Burris, L., Stockman, C. H., and Dillon, I. G. *Journal of Metals*, **7**, 1017 (1955).

Techniques

Floating zone:

Keck, P. H., Green, M., and Polk, M. L. *Journal of Applied Physics*, **24**, 1479 (1953).

Caged zone:

Brace, P., Cochardt, A., and Comenetz, G., *Review of Scientific Instruments*, **26**, 303 (1955).

A detailed study of the purification of aluminium:

Albert, P., Montariol, F., Reich, R., and Chaudron, G. *Proceedings of the 2nd Radio-isotope Conference, Oxford 1954*, pp. 75–84.

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MORE ABOUT THEORETICAL ENTITIES

BERNARD MAYO

THIS article is a statement of a view about the logic of scientific inquiry, and is designed to meet a certain line of criticism which has appeared in correspondence about my earlier article 'The Existence of Theoretical Entities', published in *Science News 32*. The critics I have in mind are not, I think, practising scientists, at any rate in the physical sciences, but those who claim to put an interpretation (in my opinion a false one) on the procedures of the theoretical physicist. Their general trend is to reaffirm the very thesis which I rejected in my article, namely that there is 'no essential difference' between common sense and the most abstruse scientific procedures. For, it is alleged,* we theorize about everyday occurrences such as cars failing to start, and our remote ancestors theorized about supernatural agencies; and theorizing in science differs, if at all, only in its greater degree of systematization.

This kind of argument misses the point which was not so much stated in my article as presupposed by it: namely that there is a special use of the term 'theoretical' which is required for the theorizing of the physicist about fundamental particles, and for which there is no place in the 'theorizing' of detectives, historians, or primitive men. This does not mean that there is a complete discontinuity between common sense and science. Certainly science, as an historical fact, emerges out of practical activities. Certainly there are inquiries which are hard to classify as either common-sense reasoning or as scientific reasoning. But not all cases are borderline cases; and those who wish to insist on

*Reference here and at points similarly marked is to a letter by R. C. Traill, published in *Science News 38*, pp. 117–19.

the similarity, at all levels, can only do so by misrepresenting the nature of scientific theorizing. The point of my earlier article was that we ought not to ask whether theoretical entities exist or are real. The point of the present one is to explain what a theoretical entity is.†

Roughly speaking, a theoretical entity is something which is unobserved *and unobservable*, and which can be defined only by reference to the job it performs in a particular scientific theory. They are most prominent in physical theory; but physics is by no means the only theoretical science. (Lord Rutherford's much-quoted remark, that the 'sciences' can be divided into 'physics and stamp-collecting', can only be based on professional prejudice.) Equally good examples of theoretical entities are the biologist's gene, and the psychologist's super-ego. These entities are unobservable not because of practical difficulties (like the mountains on the other side of the moon) but because their unobservability is a logical consequence of their other properties. The unobservability of the electron, for example, follows from Heisenberg's principle.

THE LOGIC OF EXPLANATION

Now in order to understand why the scientist has recourse to unobservables, we need to ask rather fundamental questions about the general nature of his activity. And the only answer that is sufficiently general to apply to all fields of science is that he is trying to explain something. (This is not true of the applied scientist, who might reply that he is trying to make something happen. But the only way he can make an event of a certain sort occur – except by accident – is by finding out how events of that sort do occur, that is, by being able to explain them; and this is the job of the 'pure' scientist.) What, then, is explanation, and what is scientific explanation?

Explanation is often defined as relating something new or unfamiliar to something old or familiar. We do not ordinarily 'explain' why eating a meal satisfies our hunger, but we do want

†I am much indebted to Peter Alexander, of Leeds University, for valuable criticism.

to explain why eating a certain meal made us ill. Explanation is relative to what is familiar, and also to what is interesting. We know that eating satisfies hunger, but we do not know the connexion between eating and illness. Also the latter is interesting, where the former is not; though, to a physiologist or biochemist, it may become so.

But this will not do for scientific explanation. Very often the event to be explained is more familiar than that which explains it. Burning a candle is more familiar than molecular processes. We are dealing too much with the psychological aspect of explanation. We need to attend to its logical features. They can be stated quite precisely. Let us take first an example of scientific explanation:

> We begin with the case of the Sun: let M_0, R_0, L_0 be its observed mass, radius, and luminosity.
>
> Consider then a quantity of matter of mass M_0 (to be called *the* matter) left to itself remote from all other matter. Suppose we deduce what kind of a body the matter will form, and use for our deductions nothing but the known laws of terrestrial physics. Further, suppose we find that the deduced body is also a sphere of radius R_0 and that it also emits radiation at the rate L_0. Then we can almost certainly say that the constitution of the Sun is the same as that of this body.... We should have demonstrated that physical laws require the Sun to be what it is: such is all that we can mean by a physical understanding of the Sun.†

Explaining has to do with deducing. Take next a more ordinary explanation: we explain Mr X's illness by saying that there was arsenic in his meal. The logical force of this is: we supply a premiss for a deduction. We show that 'X is ill at time $T+t$' – which we shall call proposition (c) – follows from the propositions (a) 'X eats a meal containing n grammes of arsenic at time T', and (b) 'Persons who ingest not less than n grammes of arsenic become ill after time t'. In practice we supply only the proposition (a) ('There was arsenic in the meal') because we already take for granted (b) ('Arsenic is a poison'). But somebody who innocently put arsenic in the meal, not knowing it to be a poison, would have to be supplied with (b), taking (a) for granted. The full explanation, however, logically requires both. In the case of the sun, we have to be told both (a) that the sun has M_0, and (b) that any matter having M_0 also has R_0 and L_0.

†W. H. McCrea, 'The Constitution and Evolution of Stars', in *A Century of Science* (1951).

To explain an event E is to show that the occurrence of an event of type E is deducible from something we are prepared to accept independently of E. Now it is notable that we can deduce the occurrence of E from (a) and (b) not only after the event, but also before. This yields not an explanation but a prediction ('X will be ill at time $T+t$'). We can also deduce E even if E never happens at all: if, for example, (a) happens to be untrue. This yields a conditional statement ('If X were to eat, or had eaten, such a meal, he would be, or would have been, ill'). Explanation, prediction, and hypothetical statement all involve a common deductive pattern.

Deduction has three main elements. Of the above propositions, (b) is a general statement about what happens in all cases of a certain sort; (a) is a statement that a particular case is a case of that sort; and (c) is a statement about what, accordingly, may be expected in this particular case. These are, in fact, the essential features of the Aristotelian syllogism, and are so general that they can be found in all cases of the application of knowledge. The practical application of knowledge can be represented as a prediction: if I intend to poison X, I predict what will happen (c), if I put arsenic in his meal (a), because of the toxicity of arsenic (b). Now it is part of the meaning of 'prediction' that we cannot have (c) without (b): that is, the mere assertion of a future statement, without having some general proposition (b) from which to deduce it, does not count as a prediction (but merely, say, as a prophecy or guess). On the other hand, we can very well have (b) without (c): that is, we can always contemplate a general proposition, without also contemplating its possible applications in practice. Chemists are not, after all, would-be poisoners, nor are they doctors. Knowledge may be sought for and possessed for its own sake, as well as, or even instead of, for the sake of its practical applications.

'THEORETICAL' VERSUS 'PRACTICAL'

The word 'theory', in its wider sense, originates at just this point. The capacity to formulate and entertain general propositions is perhaps the most striking feature of the human intellect; and the Greek philosophers gave to the word *theoria* (which originally meant visual contemplation and its objects) the meaning it still bears: theory is intellectual contemplation. They also established and perpetuated a fundamental dichotomy between the theoretical life and the practical life, to the detriment of the latter.

Now it is true that this antithesis between two 'lives' or fields of activity is somewhat bogus. Practical life is seldom completely unreflective, and theory is seldom without some relation to practical affairs. Indeed, Dr Conant in his excellent book *Science and Common Sense* has shown convincingly that the practical knowledge of the craftsman is one of the main strands in the historical development of the sciences. (The other two, he thinks, are mathematics and theology.) Nevertheless we must insist that science is irreducibly separated from practical life by the simple logical fact that all its propositions are general. They all resemble (*b*) rather than (*a*) or (*c*).

Even if practical activities are the foundation of science, it will not do to say that they must therefore involve an element of rudimentary or un-self-conscious theory.* A man who opens a wooden box with a crowbar has not necessarily studied the laws of levers; the only kind of understanding he requires is an understanding of what crowbars are for, and this 'understanding' may well consist entirely of his manipulative skill. It is not in the least 'theoretical'. (Neither, we may add, is a student of mechanics necessarily much good at opening boxes.) The case for a 'theoretical understanding' is somewhat more specious in such an example as the motor mechanic. He may have studied electrical theory. He may explain the failure of the car to start by saying that air is a non-conductor. But in practice he will pre-

*Here, and for subsequent text references marked with an asterisk, see footnote on p. 42.

fer to say that the ignition lead is broken. And in fact the reason why it appears plausible to link motor mechanics with electrical theory is easy to explain. Motor cars, unlike wooden boxes, are the products of applied science; an enormous amount of scientific theory, mechanics, physics, chemistry and so on, is 'built in' to them. Even so, this is merely a historical fact about motor cars. There is no logical connexion between an ability to detect electrical faults and a grasp of the theory of electro-magnetic induction. Persons quite ignorant of theory make quite good drivers and even mechanics.

So far I have suggested that the distinction, and connexion, between theory and practice is that an actual event E may be of a type, the occurrence of an instance of which can be deduced from certain propositions including a general one, such deduction providing the theoretical explanation of the event. But the arsenic example, in which the deduction takes only a single step, is much too simple to illustrate more than the basic pattern. A theoretical statement may itself require explanation. Why is arsenic poisonous? The pattern of explanation is still the same, though what is being explained is no longer an event, but a generalization about the toxicity of a class of substances. We explain (if we can) the effects of arsenic by deducing them from chemical properties, biochemical reactions, changes in the blood stream, and so on.

One difference between scientific explanation and explanation in practical life is that the former is always explanation of generalities. The scientist is not interested in the question, why Mr Smith was ill on 1 August at his home in Hampstead. This is a question for the practical man, including the forensic pathologist. The scientist is interested in explaining the uniformities which are already known, and expressed in general propositions. Why is it that people always get ill after a certain dose of arsenic? Why do plants grow towards the light? Why does ice form on the surface of water? Such general propositions are called empirical generalizations, to distinguish them on the one hand from statements reporting particular occurrences, and on the other hand from general statements about unobservable entities.

The pattern of explanation is always: deduce a proposition which is to be explained (*c*) from other propositions (*a*, *b* . . .). The deduction may be short and simple, or long and complex. All but the simplest deductive steps will involve mathematical symbols; indeed mathematics can be regarded as a simplifying notation for carrying out deductions which would otherwise be intolerably prolix.

EXPLANATION AND INVENTION

We must now notice a distinction familiar to logic students as the distinction between truth and validity. I shall express it as a twofold criterion of satisfactoriness in explanation, namely adequacy and truth. We require an explanation to be adequate: that is, (*a*) and (*b*) must be sufficient to entail (*c*). Illness must really be a consequence of whatever is alleged to explain it. But, further, we require an explanation to be true: that is, (*a*) and (*b*) must not only entail (*c*), they must also be themselves true. The above explanation of *X*'s illness, if he did not in fact take any arsenic, is adequate, but not true. On the other hand, if we say that he ate some stale bread (and he did) this 'explanation' is true, but not adequate.

Now when we wish to explain something, we cannot always refer at once to the relevant propositions from which it can be deduced. We may not know them. Even the motor mechanic cannot always say at once why the engine will not start. He checks the ignition, the petrol, the carburettor. In so doing he is testing certain hypotheses: that the ignition lead is broken, the petrol tank empty, or the feed pipe blocked. These propositions are not asserted as true; they are merely entertained as possibilities to be ruled out. He finds the ignition is in order, the petrol tank is full. Two hypotheses are false. But he finds an obstructed feed pipe: the third hypothesis is true. This illustrates what happens when the propositions required for an adequate explanation are not already to hand. We have to invent them. And what we invent has to satisfy both the adequacy criterion and the truth criterion.

Similarly when we are trying to explain a set of phenomena

or an empirical generalization instead of a particular occurrence. We need a general proposition from which the first is deducible. We may have to invent one. Sometimes we may be able to make do with another empirical generalization. For example, we explain the phases of satellites by showing what a rough-surfaced sphere looks like when illuminated from different directions. Here the 'invention' consists in the formulation of a general proposition about facts already accessible (illuminated spheres) and the demonstration of a deductive step from those facts to the phenomena in question (from spheres to satellites). But the facts invoked in the explanation are plain facts, just as broken ignition leads and empty petrol tanks are.

'THEORETICAL' VERSUS 'EMPIRICAL'

Much more characteristic and important, however, are the cases where we invent propositions which are not empirical generalizations and which no longer describe phenomena at all. We invent not only the propositions, but also what the propositions are supposed to be about. Theory in the narrower sense begins at just this point. Theorizing in this special sense is not the contemplation of general propositions as opposed to practical activity. It is the invention of non-empirical concepts: that is, of concepts of objects and properties which are unobserved and unobservable. The sole justification for these concepts is that they occur in the formulation of certain propositions; and the sole justification for the propositions is that they supply premisses from which empirical propositions can be deduced, and thereby explained.

A theoretical physicist, H. J. Bhabha, has put forward the same view†:

> The aim of theoretical physics must be to find a complete set of mutually consistent mathematical postulates or axioms from which the properties of nature, defined as the result of every conceivable experiment, can be deduced in the form of a number of theorems. In order, however, to compare the mathematical statements of the

†In his article 'The Present Concept of the Physical World' in *Science News 21*, p. 14 (1951).

theorems with the results of observation, the basic mathematical postulates must be supplemented by a set of prescriptions about the interpretation of the mathematical formalism. It is clearly not sufficient that the postulates should be consistent. Their correctness from the point of view of physics can be demonstrated only by an agreement between the deductions and the results of experiment.

Theoretical entities are not, as they are often called, inferred entities: for theoretical propositions are not inferred, that is deduced, from the phenomena: on the contrary, theoretical propositions are invented in order that the phenomena may be deduced from them. What Bhabha suggests is that one may first develop a piece of mathematics and later give it a physical interpretation, thus arriving at a physical theory and theoretical entities. Now to quote Bhabha again:

> A consequence of this approach is that any newly discovered fact of nature which does not fit into the existing scheme of physics may necessitate a complete change of the fundamental postulates.

Evidently if the 'fundamental postulates' are changed, then the theoretical entities introduced under the old scheme are liable to be modified or replaced under the new one.

Because theoretical propositions do not describe phenomena, we cannot test them for truth and falsity as we can test the mechanic's propositions about ignition leads. And this means that theoretical explanations are peculiar in that they are not subject to the two-fold criterion of adequacy and truth. (It is significant that Bhabha, in the passage first quoted, spoke not of 'truth' but of 'correctness from the point of view of physics'.) Such explanations must of course be adequate, but we can no longer insist that they be also true. For we have no kind of access to what they purport to describe; no test we can apply to them, except that they shall lead by deduction to statements about phenomena which *are* true. But this is only the adequacy criterion. (It is a commonplace in logic that the truth of an inferred proposition does not establish the truth of that from which it was inferred.) But an adequacy criterion alone will not do. For there is no limit to the number of propositions which may be invented and from which a given one is deducible. In

practical explanation most of these are ruled out as false. But if in theoretical explanation we have no truth criterion, how are we to adjudicate between rival theories which are all adequate? The answer is that there are other criteria, and some hints will be given below; but a proper answer is outside the scope of this article.

GRAVITY: A NON-EMPIRICAL CONCEPT

But what are we to understand by the invention of non-empirical concepts? Let us take a very familiar example. Newton wished to explain the phenomena of motion. Why do terrestrial bodies always fall towards the earth, but not celestial ones? Earlier thinkers had been content with the Aristotelian explanations, which required two separate theories, one for terrestrial bodies and another for celestial ones. This dualism accorded well with medieval theology, but Newton wished to find a unified theory applicable to both kinds of body. He achieved his object by inventing a theoretical entity which he called 'gravity'.

The concept of gravity is a non-empirical concept. Gravity is not a phenomenon. Newton did not discover facts unknown to his predecessors; the facts he attended to were already familiar. The weight of bodies can be felt; the downward acceleration of their fall can be observed by eye within certain limits, and so can the revolutions of the heavenly bodies. But gravity cannot be seen or felt. It is a purely theoretical construct. More precisely, it is a property which Newton's Laws *ascribe* to 'bodies' or 'particles'. These 'bodies' are themselves theoretical entities too. They are quite different from any objects in the ordinary world. For instance, they have no size (they are treated as points); they have no shape, colour, or texture. They have only mass, motion, and position. 'Gravity' is ascribed to them as a further property, namely their tendency to accelerate towards each other in accordance with the inverse square law. Gravity is not an observable property of actual bodies, as weight is: it is a theoretical property of theoretical bodies; and the justification for accepting the theory is that it enables us to make deductions which are found to be in close accordance with the facts of motion.

It is true that Aristotle, too, had invented theoretical concepts. He proposed to speak of 'natural' and 'violent' motion in order to explain the difference between the various motions of terrestrial bodies; and he explained the apparently circular motion of the celestial bodies by inventing a special celestial substance, the quintessence or 'fifth substance' (the other four being the terrestrial 'elements', Earth, Water, Air, and Fire). All these entities are theoretical, in the sense that they are not phenomena, but are invoked to explain the phenomena by deduction, and are given whatever properties are required to yield the right results. But there the similarity between Aristotle and Newton, between Greek and modern exemplars of theoretical explanation, ends. Aristotle's theory, being formulated in terms of a purely qualitative distinction, did not yield deductions which could be compared with precisely determined observations of measurable quantities. It could not yield predictions of future events, such as solar eclipses. Thirdly, it required a total distinction between the earthly and the heavenly, which, though plausible to common sense and theology, could also be regarded as arbitrary, and which Newton showed to be unnecessary. And finally, Aristotle did not regard his concepts as purely theoretical, since he certainly believed that they stood for real agencies or elements in a teleologically ordered universe; whereas Newton claimed no more than that the laws of gravity enabled one to explain and predict the movements of bodies. Gravity was not an 'agency' whose origin and cause were also liable to explanation, at any rate within science. His celebrated remark, 'I frame no hypotheses' (to explain gravity), did not mean that he did not invent any hypothesis (since logically speaking his Laws are hypotheses), but that he did not invent hypotheses which were not testable by the deduction of observable consequences, as the Laws are.

'CORRESPONDENCE WITH REALITY'

All the theoretical terms of science are, in essentials, of the same logical nature as Newton's term 'gravity'. Electron, light-wave, temperature, magnetic moment, entropy – the list is endless.

Many of these terms refer to theoretical properties, many others to a tendency to behave in accordance with certain laws. (The distinction is largely unreal because to have a property – such as magnetic moment – *is* to behave in accordance with certain laws.) A comparatively small number – such as 'electron' – *seem* to stand, not for properties or behaviour, but for things which have the properties and exhibit the behaviour. But this is an illusion. For we cannot ask, 'What *is* the thing called an electron, apart from its properties?' And if anyone suggests here that the same is true of ordinary objects like chairs and potatoes, the answer it that it is not. For ordinary things have whatever properties we observe them to have, and these can be multiplied without limit. A chair is not only brown and hard, but oaken, owned by Mr Jones, and so on. But an electron has precisely those properties which are postulated of it, and no others; and the list is very short. The constant properties of an electron are: electric charge, rest mass, angular momentum, and magnetic moment; the variable properties are: position and velocity. It makes no sense to ask 'What colour is it?' or to speak of something 'having' the properties, but distinct from them. If we had occasion to suggest that two electrons might have changed places, it would not be legitimate to say so. We could only say that the situation was exactly as before.

The temptation to insist that there must be something in a 'real objective world'* which 'corresponds with our concept' arises partly because certain of the theoretical concepts are expressed by non-abstract nouns (such as 'electron') and we normally take such nouns to stand for things. Other concepts are expressed in abstract nouns, such as 'gravity', and we all know better than to look for a thing corresponding to such a noun. Indeed it may already have been felt that my choice of gravity as an example of a theoretical entity was unfair: is gravity an entity at all? But my point was to emphasize the similarity in logical function between 'gravity' and 'electron': compared with this the differences are minor ones, and depend on the peculiarities of certain properties such as mass, position, and velocity. As Samuel Glasstone puts it, 'if a close examination is made

into the significance of the description "particle", it is seen to mean that experiments are designed with the object of obtaining information concerning momentum'† – that is, the product of mass and velocity. But the difference between existents and non-existents can hardly turn on any special privilege attaching to the properties of mass and velocity.

Another source of our worry about 'reality' is our habit of requiring an (ordinary) explanation to be not only adequate but true. A true statement normally does correspond to something in the 'real objective world': it states a fact or describes a phenomenon. But a theoretical explanation cannot be true in this sense. It can only be (*a*) adequate, and (*b*) generally accepted in preference to other possible explanations in accordance with criteria of preference which are universally adopted by scientists, and in accordance with which we prefer (for example) the Newtonian system to the Aristotelian. To insist on a further 'correspondence with reality' is only an empty claim to something over and above the explanatory success and the prestige of an established theory: empty, because there is no possible test for such 'correspondence'.

We must now deal with the objection that theoretical entities do correspond to reality because they may, after all, be observable. We *see* when an electron hits the fluorescent screen*; we *see* the track of an alpha particle in the cloud chamber. The answer to this is that we no more 'see' the electron than we 'feel' gravity. What we see is a flash of light.

Next it might be urged, more plausibly, that the electron is at any rate the cause of the scintillation, and if an effect is real, surely its cause must be? But what we take to be the 'cause' of the flash depends on what we regard as a satisfactory causal explanation; the causal explanation in this case is a theoretical one; and the theory in question is so well established that the cause of the flash is thought of as if it were just as plain a fact as the flash itself. What we see in cloud-chamber experiments are certain lines on a photographic plate; but to interpret one of

†*Textbook of Physical Chemistry*, p. 18.

them as the track of an alpha particle depends, again, on a certain theory. A more naïve claim is that particles must be real because they are 'parts' of things,* or what matter is made of. Cakes are made of flour, and the flour and the making are just as observable as the cake. But flour is not, in turn, 'made of' atoms. The constitution of cakes is an empirical question; the 'constitution' of matter is a theoretical one.

Any residual doubt about the metaphysical status of theoretical entities can, I think, be disposed of by showing that their mode of existence would be not only unseen and unseeable, but inconceivable. Rutherford and Bohr thought of the electron as a planetary particle; Heisenberg's account is purely mathematical; Schrödinger replaces the electron by a cloud of electric charge, while Born interprets it as a system of probabilities of a particle occurring at given points in space. It seems generally admitted that for certain theoretical purposes the electron must be thought of as a particle, for others as a cloud. But it cannot *be* both a particle and a cloud. Again, Sir William Bragg solved the early dilemma about X-rays by suggesting that they 'partake of the nature of both' waves and particles†; later de Broglie suggested that the same was true of electrons. But there cannot *be* things which are both waves and particles. The inconceivability of such a co-existence causes no embarrassment to the physicist who is concerned merely to ascribe to his theoretical entities whatever properties may be required to yield deductions in accordance with the phenomena. It does not matter in the least that these properties may be such as it would be self-contradictory to ascribe to actual objects.

†Kathleen Lonsdale, 'Neutrons and Atom Patterns in Crystals', *Science News 37*, 25 (1955).

THE MODERN THEORY OF LIQUIDS

G. H. A. COLE

THE properties of the molecules of which all matter is composed, whether gas, liquid, or solid, are now well known in their general features. That is to say, the structure can be determined, as well as the force which one molecule exerts on another. These molecular properties determine the features of matter with which we are familiar in everyday life. It is usual to call the molecular properties of a system its microscopic properties, and those properties which we recognize directly with our several senses, its macroscopic properties. We say, then, that the macroscopic properties of a given physical system are determined uniquely by its corresponding microscopic properties. The problem now arises of calculating the macroscopic properties from the known microscopic properties; this is the aim of that branch of theoretical physics known as the kinetic theory.

The kinetic theory of gases and the kinetic theory of solids can claim considerable success, and it is now possible to calculate many of the properties of these two states of matter. The kinetic theory of liquids has for a long time fallen behind, largely owing to very severe mathematical difficulties. In the last ten years, however, new attempts have been made at building up a kinetic theory of liquids. These attempts have been most successful, and are explained in general principle in the present article.

STRUCTURE OF LIQUIDS

Although we usually find no difficulty in recognizing a liquid when we see one, it is by no means easy to define exactly what a liquid is in terms of characteristic properties. The most characteristic features of a liquid are perhaps its very small compressibility, and its almost complete lack of rigidity. These two features

mean respectively that the actual volume (and so also the density) of a given mass of liquid is very little affected by the application of external pressure; nevertheless the *shape* of the volume depends entirely on details of these external influences. But these two properties are not unique to liquids. Solids have a very small compressibility, and gases have essentially no rigidity. It appears, then, that no clear-cut macroscopic distinction can be made between the three states of matter, and this conclusion is confirmed by the existence of substances which lie between a true liquid and a solid (such as pitch, or glass), or between a true liquid and a gas of low density (conventionally called a dilute gas).

If only macroscopic properties of liquids are of interest this difficulty of definition is not of vital importance, as is witnessed by the great success of the theory of hydrodynamics. This theory, which is now regarded as belonging to applied mathematics rather than physics, allows the macroscopic flow properties of liquids, or gases, collectively called fluids, to be calculated by an application of the laws of motion, either Newtonian or relativistic, to small fluid volumes.

In calculating the properties of a liquid from molecular data, however, it is essential to distinguish clearly the liquid state from the other two states of matter. For this purpose it is now customary to appeal to considerations of the arrangement of the constituent molecules in space. On this basis a liquid is characterized by local ordering only. This means that the molecular arrangement about any representative liquid molecule as centre is highly ordered for only a small distance, i.e. a few molecular diameters, after which the order disappears. Each molecule of a liquid is, in this way, the centre of only local or short-ranged order; in contrast a solid is characterized by long-ranged order, and a gas by virtually no order at all.

Experimental Identification of the Structure

The order of molecules in space has been identified experimentally by the well-established techniques of X-ray diffraction; very recently the diffraction of neutrons has alternatively been

used for this purpose.* In these experiments a beam of known wavelength (for X-rays), or energy (for neutrons), is focused on to the specimen, and the form of the transmitted beam is analysed. A large amount of the incident beam passes through the specimen unaffected, but an appreciable fraction is deflected from its original path. It is this latter beam, the diffracted beam, that allows the spatial arrangement of molecules in the specimen to be determined, since its form is completely characteristic of this arrangement. The experimental set-up is shown schematically in Figure 1.

For a liquid the diffracted X-ray beam shows a ring pattern, with one, somewhat diffuse, principal ring, possibly accompanied by one or more weaker rings. This pattern resembles that produced by a fine powder of crystallites, where rings also occur, although these are usually rather sharper than those for a liquid.

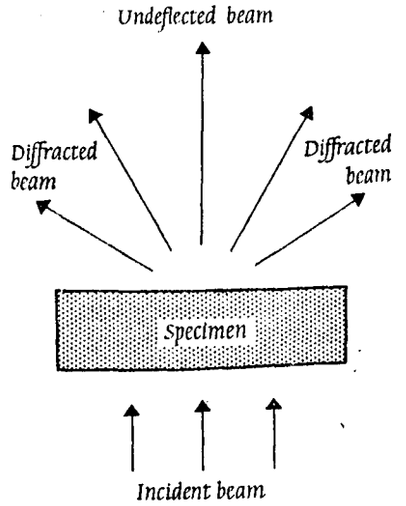


Fig. 1. Schematic diagram of X-ray apparatus.

*See, for example, the article by Professor Kathleen Lonsdale, F.R.S., in *Science News 37*.

The liquid diffraction pattern is quite unlike those for crystalline solids, or gases, where no such rings appear. Apparently diffraction experiments do allow a unique specification of the liquid state to be made in terms of the spatial molecular arrangement.

Interpretation of the Experiments

The first interpretations of the X-ray diffraction experiments were made by stressing the similarity between the curves for liquids and crystal powders. At first a liquid was regarded as a particular kind of crystal powder consisting of groups of small numbers of highly ordered molecules, the many groups comprising the liquid being able to move relative to each other with great ease. This is the hypothesis of 'cybotactic groups' proposed by G. W. Stewart in 1930. A more satisfactory interpretation of the data, which still retained the concept of the crystal lattice, was subsequently proposed by J. Prins, and considered in detail by him and H. Peterson, and independently by J. D. Bernal. According to these authors the liquid molecules are to be regarded as centred at the corners of a virtual crystal lattice, although not sufficiently strongly to prevent their executing considerable random motion about the equilibrium lattice sites. Although this random motion has little effect on the molecular ordering in the immediate neighbourhood of any molecule chosen as centre, at larger distances it blurs the virtual crystal order until it eventually disappears.

A more realistic liquid model, which retains the feature of molecular packing, is the cell model which was proposed by E. Guggenheim in 1932. It is known empirically that a molecule in a liquid is constrained by its neighbours to remain in a small volume of space, called a cell. Although unable to leave its cell, each molecule is able to perform random motion within it. If the position of each constraining molecule is known, along with the law of force between molecules, the force on the molecule in the cell can be calculated at any instant of time.

A more general interpretation of the diffraction experiments which does not involve the concepts of crystal physics has been made in terms of the probability of the occurrence of pairs of

molecules in the liquid. This interpretation, which stresses the importance in liquids of the interaction of pairs of molecules, is a characteristic of the modern theory. The probability function for the occurrence of pairs of molecules is usually called the pair distribution. In general, liquid molecules are not perfectly spherical, so that the pair distribution depends not only on the molecular separation distance but also on the orientation of the line of centres with some standard direction. This is the case for water, for example. There is, however, an important group of liquids, typified by liquid argon, in which the molecules are completely spherically symmetric, consisting of a single atom. These liquids are called monatomic liquids, and their inherent simplicity makes them particularly important from a theoretical point of view. The law of force between molecules is now independent of the orientation angles, and for this reason the molecular interaction is said to be due to a central force. In this case the pair distribution depends only on the radial distance, and is in consequence referred to as the radial distribution function.

This function, which is usually denoted by $g(r)$, is defined as follows. If we denote the number of liquid molecules per unit volume (the number density) at distance r from a chosen molecule as origin by the symbol $\rho(r)$, and the mean number density by ρ_0 (which equals the total number of molecules in the system divided by the total volume of the system) then the radial distribution function is the ratio of $\rho(r)$ and ρ_0:

$$g(r)=\rho(r)/\rho_0 \qquad \text{(Eqn 1)}$$

The same expression applies also if the force is not central, except that now the orientation angles also appear. Since the order of the liquid is short-ranged $\rho(r)$ must become ρ_0 after a relatively short distance, so that, from Equation 1, $g(r)$ must approach the value 1 as the distance r becomes large. Written mathematically this condition is:

$$\lim_{r\to\infty} g(r)=1 \qquad \text{(Eqn 2)}$$

The radial distribution function is related to the experimental diffraction pattern in a definite way, so that once the experimental data are available, the radial distribution function can

be uniquely calculated. The radial distribution function for a typical monatomic liquid is sketched in Figure 2. The curve is to be interpreted physically as showing that at certain distances (such as *A*, *B* in Figure 2) there are more molecules, while at others (such as *C*) there are fewer molecules, than the average number. The experimental data described so far form the basic material on which any kinetic theory of the liquid state must be based.

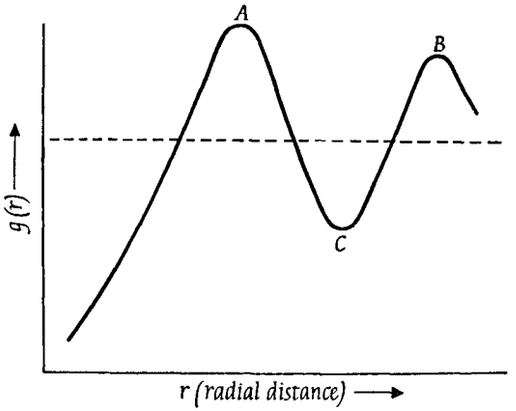


Fig. 2. Typical radial distribution function for monatomic liquids.

Points about the Theory

In carrying through the aims of the kinetic theory it has been found necessary to treat equilibrium systems (that is systems where there is no motion, and where the temperature is everywhere the same) quite differently from non-equilibrium systems (where there is motion, or a non-uniform temperature distribution). This is a general feature whether the system be a solid, liquid, or gas. It follows that the theory of liquids falls naturally into two parts, viz. theories of equilibrium, and theories of non-equilibrium. The remainder of the present article is divided in this way.

The modern theory has been most developed for classical

liquids; the corresponding theory of quantum liquids so far not having made very much generally agreed headway. Every indication so far is that an understanding of quantum liquids does not require any principles of physics that are not also needed for an understanding of classical liquids. In the present article, therefore, we restrict the argument to classical liquids, no mention being made at all of the very interesting quantum liquid helium-II.

Because monatomic liquids present the essential features of all liquids and at the same time have the simplest (central) type of interaction force between molecules, the modern theory has been particularly concerned with the study of these liquids, of which the classical liquid, argon, is treated as typical. The kinetic theory has so far been applied to numerical calculations almost entirely about these liquids, and interest will therefore be centred on them in what follows. This is not to say that the techniques now to be described cannot be applied to more general polyatomic liquids. It is clearly advisable in developing a theory to keep the subject-matter as completely characteristic as possible, adding frills only when the theory is already well established; for classical liquids the frill stage is possibly just being approached. It must, of course, be borne in mind that although quantitative calculations for general polyatomic liquids have not yet been made, it is nevertheless possible to say a great deal theoretically about these liquids on the general basis of the available calculations for monatomic liquids. We come now to the recent work on the theory of liquids.

EQUILIBRIUM LIQUIDS

The macroscopic properties of any equilibrium system, whether it be a solid, liquid, or gas, are related to each other by the formulae of thermodynamics.* These formulae are firmly related to the corresponding microscopic (molecular) properties,

*See, for example, the article 'A Hundred Years of Thermodynamics' by J. A. V. Butler in *Science News 18* (for a general introduction); and 'Irreversible Changes: New Thermodynamics for Old' by R. O. Davies in *Science News 28* (which gives some formulae).

and in particular to the intermolecular force, by the well-established principles of Statistical Mechanics. These principles apply equally to classical or quantum systems, although we shall be concerned here only with the classical theory.

The Partition Function

The principles of statistical mechanics specify the macroscopic physical system in terms of the momentum and position of each constituent molecule. A model of the actual system (solid, liquid, or gas) is constructed consisting of N interacting particles (representing the molecules) contained in a volume V. The interaction between the particles is taken as being the actual force between the molecules. The state of each particle in the model is specified by its momentum and its position. The state of the total system of N particles is, therefore, completely specified if the state of each of the particles is known. There are, however, an indefinitely large number of states of the system, because each particle can have any value of the momentum in an indefinitely wide range of values, and can also be at any point within the volume V. There is a definite probability for the occurrence of each of the possible states of the total system; it is convenient to denote this probability by the symbol $f^{(N)}$. This function depends upon both the total energy and temperature, and provides a complete description of the physical system. The average of $f^{(N)}$ over the momenta and position coordinates of all the N particles is called the partition function, and is denoted by Z. The partition function plays a very important role in the kinetic theory of matter in equilibrium.

It is sometimes sufficient to have only a partial knowledge of the state of the system, i.e. a knowledge of the state of small groups of, say, two, three, four ... particles in the model. This information is denoted by appropriate probability functions – respectively $f^{(2)}$, $f^{(3)}$, $f^{(4)}$. ...

The Thermodynamic Quantities

The thermodynamic functions, such as the pressure, the internal energy, the chemical potential, or the entropy, can be

derived from the partition function by an application of the formulae of statistical mechanics. These formulae equate the several functions to the change of the logarithm of the partition function, log Z, due to changes in particular parameters such as the volume, temperature, or number density (number of particles per unit volume) of the system. One formula relates the pressure to the change of log Z with the volume; another relates the total energy to the change of log Z with the temperature; and yet another relates the chemical potential to the change of log Z with the number density. Once the partition function has been evaluated for a given physical system the thermodynamic functions are found by inserting it into the formulae of statistical mechanics.

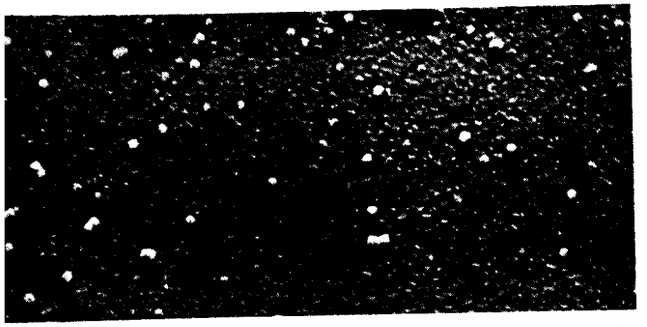
The Evaluation of the Partition Function

It is seen, therefore, that the evaluation of the partition function (Z) is the central problem of the equilibrium theory. Z contains the total energy, which is the sum of two terms, viz. the kinetic energy and the potential energy. The kinetic energy involves the momentum and not the position of each particle; the potential energy involves the position and not the momentum of each particle. In carrying through the averages appearing in Z there are two separate operations involved, the averaging over the momenta and the averaging over the position. Z can, in this way, be written as the product of two terms; one involving the averages over the momenta, denoted by Z_p, and the other involving the averages over the position, denoted by Z_r. Written explicitly this is:

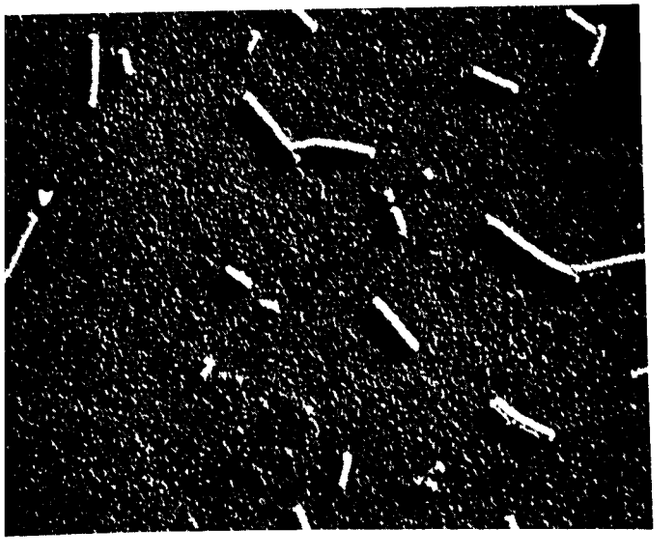
$$Z = Z_p Z_r \qquad \text{(Eqn 3)}$$

Z_p is readily evaluated whether the system of interest be a solid, liquid, or gas.* The evaluation of Z_r is, however, a far more difficult task for an assembly of molecules in interaction.

*Its value for all systems is $\sqrt{(2\pi kT/m)}$, where k is Boltzmann's constant, T is the absolute temperature, and m is the mass of the particle.

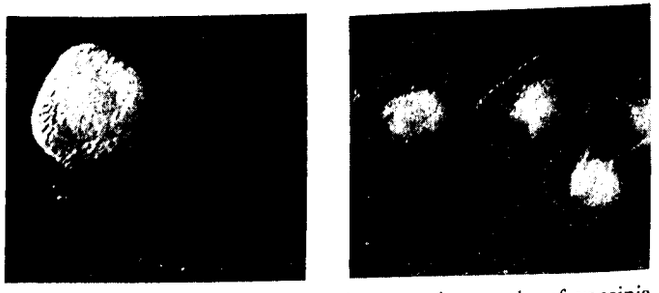


. Rebuilding a virus: from two preparations of tobacco mosaic irus (a nucleoprotein) there were separated respectively protein ragments, here shown, and nucleic acid; neither component retained etectable activity as a virus.



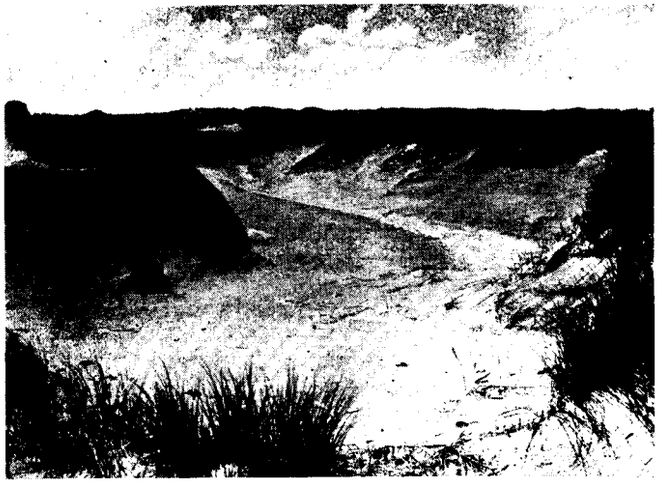
2. Rebuilding a virus: the protein and nucleic acid components have been put together in a 'rebuilt' nucleoprotein which is active as the virus. *(Electron micrographs from the Virus Laboratory, University of California.)*

3. Crystals of an animal virus: these crystals of poliomyelitis virus are the first crystals of a human- or animal-infecting virus observable under a light microscope. *(Virus Laboratory, University of California.)*



4. Structure in an animal virus: electron micrographs of vaccinia virus before and after digestion with pepsin *(I. M. Dawson and A. S. McFarlane)*. Magnification ×65,000. If the poliomyelitis virus consists of a single substance, as seems probable, the vaccinia virus does not.

NATURE CONSERVANCY

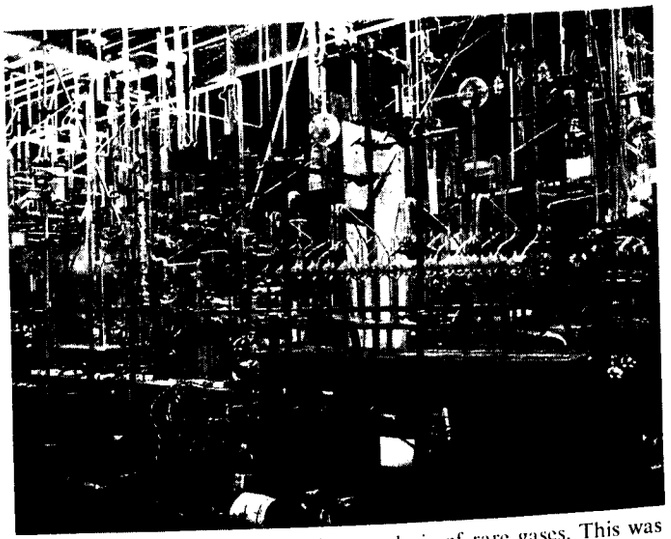


5. Newborough Warren and Ynys Llanddwyn, Anglesey, was declared a national nature reserve during last summer.

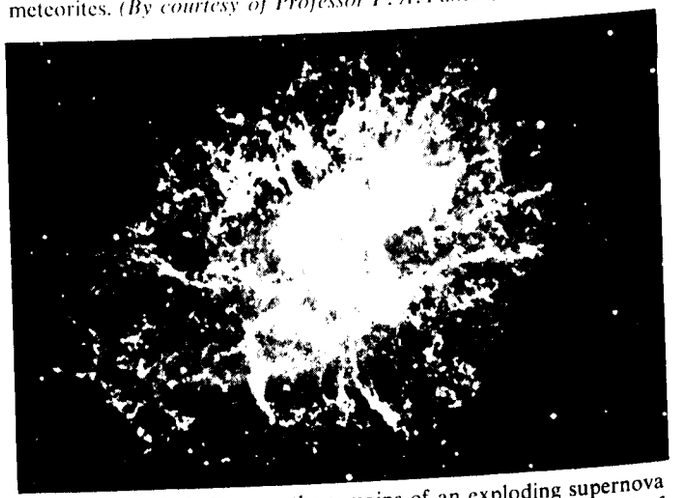
6. Field station in the conservancy's Moor House, Westmorland, nature reserve.

7. Iron meteorite: polished and etched slice from 30-kg. mass found at Otchinjau, Angola, in 1919. *(British Museum photograph.)*

8. Stone metorite: section from stone which fell at Beddgelert, North Wales, on 21 September 1949. *(British Museum photograph).*



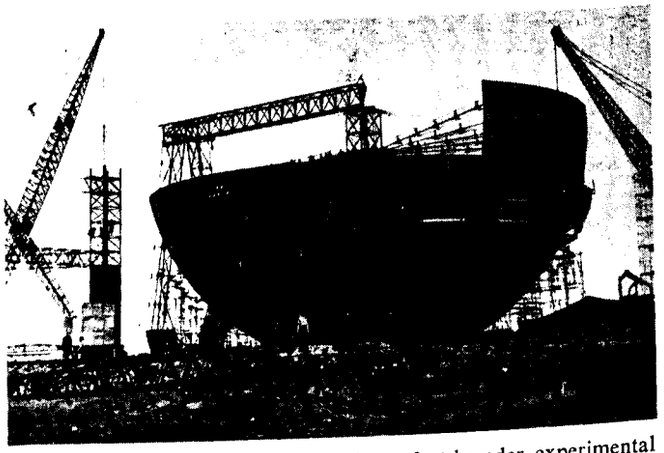
9. Part of apparatus for the micro-analysis of rare gases. This was chiefly built to measure the helium, argon, and neon content of meteorites. *(By courtesy of Professor F. A. Paneth, F.R.S.)*



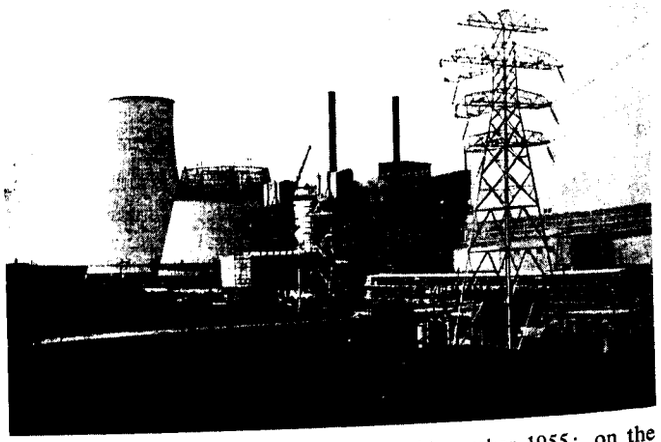
10. Crab nebula in Taurus, the remains of an exploding supernova photographed about 900 years after the explosion; present speed of expansion about 600 miles per second.

11. The Arizona Meteorite Crater at 'Crater Mound', 20 miles west of Winslow; the crater is 600 ft deep and 4,000 ft in diameter.

ATOMIC ENERGY



12. Progress in the construction of the fast-breeder experimental nuclear reactor at Dounreay, Caithness: in November 1955 the protective steel sphere, 135 ft in diameter, was about half assembled. *(Copyright, U.K. Atomic Energy Authority.)*



13. Calder Hall atomic power station in November 1955: on the left, cooling towers; centre, one of the two reactors; right, turbine generating plant. *(Copyright, The Times.)*

Only in special cases is a direct evaluation possible, those in which the interaction between the molecules has a sufficiently simple form to allow the complete averaging procedure over all the molecules to be replaced, at least to good approximation, by the product of the individual averages for each molecule. Such an evaluation has been possible for real dilute gases, where the molecular interaction is sufficiently weak to be neglected to a good first approximation; an evaluation has also been possible for solids where the extreme long-range order noted previously from the diffraction experiments introduces important simplifications into the calculations. The short-range order of a liquid makes it quite impossible to evaluate Z_r directly, owing to the complete lack of simplifying features associated with the molecular interaction in this case. Even the use of the most advanced electronic calculating machines does not make a direct evaluation possible for a liquid. Consequently the formulae of statistical mechanics cannot be applied directly to liquids using a correct expression for the partition function Z. The theory of equilibrium liquids resolves itself, therefore, to the problem of obtaining an approximate evaluation of this function which, although not exact, is nevertheless very near to the correct expression. We turn now to consider the solutions that have been given to this problem.

The Cell Model

We saw earlier that one way of interpreting the diffraction experiments involved the concept of cells in the liquid, each cell containing one molecule. Knowing the force law between the molecules and the positions of the molecules forming and surrounding the cell the force on the molecule in the cell can be calculated. The molecule in the cell moves under the action of this force, and its energy can be calculated in a simple way. The interaction part of the partition function, Z_r, can now be evaluated using only elementary mathematics, and this means that the partition function also is known. The thermodynamic properties then follow. This procedure was first used by Sir J. Lennard-Jones and A. Devonshire in 1937. Although simple the accuracy

of the method is not great since it over-idealizes the liquid state in order to simplify the calculations. Improvements have been made by later workers, and the method is still being used. These improvements have involved the contents of the cell. For instance the possibility of there being more cells in the liquid than there are molecules, some cells therefore being unoccupied, has been taken into account; also the case in which some cells contain more than one molecule. Further, the interaction between two neighbouring cells has been included in the theory. Finally the extension of the work to quantum mechanics has recently been made. The method certainly has the merit of simplicity, but has the disadvantage of not being based on any single unambiguous model of the liquid state.

The Pair Distribution of Equilibrium Liquids

A more reliable alternative to the cell model is the method which bases itself on the pair distribution, which we saw earlier is the most general way of interpreting the experimental diffraction data. The method of the pair distribution, developed by M. Born and H. S. Green, and independently by J. G. Kirkwood and his colleagues, involves the rearrangement of the general formulae of statistical mechanics into a form showing explicitly a dependence on the interaction between pairs of molecules. The pair distribution can be calculated even though the full partition function cannot. We denote the pair distribution in configuration (position) space for the molecules numbered 1 and 2 by $n^{(2)}(1, 2)$, and it is derived from $f^{(N)}$ by averaging over the momentum of all the molecules and the positions of all molecules other than 1 and 2. By considering the change of $n^{(2)}(1, 2)$ with the separation distance between the molecules 1 and 2 an equation is derived (the Born and Green equation) that contains $n^{(2)}$ on the left-hand side, and $n^{(3)}(1, 2, 3)$ on the right-hand side under an integral sign. In order to use this equation for calculating $n^{(2)}$ it is necessary to know $n^{(3)}$ explicitly in terms of $n^{(2)}$. This information cannot be obtained from the theory at present, and to make progress it has been necessary to introduce an approximation into the theory. The additional hypothesis

accepted so far is the superposition approximation, first suggested by J. G. Kirkwood, according to which the probability of the occurrence of three molecules in a given configuration is the product of the probability of the occurrence of the individual pairs. In this approximation the triplet distribution $n^{(3)}(1, 2, 3)$ is equated to the product of the pair distributions $n^{(2)}(1, 2)$, $n^{(2)}(1, 3)$, and $n^{(2)}(2, 3)$. Written explicitly the superposition approximation asserts that the Born and Green equation is to be supplement by the condition:

$$n^{(3)}(1, 2, 3) = n^{(2)}(1, 2) \times n^{(2)}(2, 3) \times n^{(2)}(1, 3) \quad \text{(Eqn 4)}$$

With the relation expressed as Equation 4 inserted into the right-hand side of the Born and Green equation, an equation results which depends only on the function $n^{(2)}$, and which may be solved for this function. If the intermolecular force is central (independent of the orientation angles) $n^{(2)}$ becomes identical with the radial distribution function determined experimentally.

In order to determine the thermodynamic functions from a knowledge of $n^{(2)}$ it is necessary to rearrange the equations for these functions that involve the partition function into a form that shows explicitly the dependence on the interaction of pairs of molecules. This was done by H. S. Green, who has given formulae for the pressure, and the internal energy in terms of both $n^{(2)}$, and the potential of the intermolecular force. (The force on a molecule is the space rate of change of this potential.) Green's equations contain two terms, the pressure and internal energy being made up of two contributions. The first term of each equation relates to the motion of single molecules and applies to a gas; the second term relates to the interaction of pairs of molecules and applies to a liquid. The relative importance of each term depends upon the density, the equations applying generally to fluids.

Experimental Test of the Equilibrium Theory

The experimental test of the cell model approach is slightly different from that of the pair distribution. For the cell model the calculation of the thermodynamic functions involves the construction of a partition function Z which, when inserted into the

correct equations of the theory, allows the experimental data to be repeated as nearly as possible. The initial model is slowly changed, in the manner explained earlier (on page 65), until the best agreement with experiment is obtained. By this method satisfactory agreement with experiment has been obtained in the high temperature region where the liquid approaches the gaseous phase.

The experimental test of the theory involving the pair distribution can be more stringent since in this case a definite model is used. Given the intermolecular force the pair distribution can be calculated by solving the Born and Green equation, or an equivalent equation due to Kirkwood, and the result compared with the experimental diffraction curves. With this function once determined its insertion into the equations of H. S. Green provides unique values of the thermodynamic functions.

The Born and Green equation was solved analytically by H. S. Green, and independently by A. Rodriguez, but the extreme difficulty of the mathematics made it imperative to introduce further approximations not based on physics into the calculations. The resulting pair distribution was accordingly not entirely reliable. J. G. Kirkwood and his colleagues solved the Born and Green equation, and also the Kirkwood equation, by numerical means using an intermolecular force law applicable to liquid argon; a pair distribution resulted that agrees with the experimental curve in its main details, but the two solutions differ slightly as to the exact value of the first maximum of the curve, point A in Figure 2. The accuracy of the experimental curves is not sufficient to allow a choice between the two calculated curves to be made.

When the calculated pair distribution $n^{(2)}$ is inserted into Green's equations, values of pressure and internal energy result that agree with experiment fairly well at the higher temperatures, although the agreement gets steadily worse as the temperature is reduced. The cause of this discrepancy appears to lie in the introduction of the superposition approximation (Equation 4). Although the accuracy of the calculations could doubtless be improved if a more accurate approximation than Equation

4 were used, there is at present no useful hint at all as to what the improved expression should be.

Summing up, we can say that the theory of equilibrium liquids is now well established, but that its application to actual liquids faces very great mathematical troubles that still have not been completely overcome.

NON-EQUILIBRIUM LIQUIDS

If the macroscopic properties of a physical system differ from one region to another (e.g. if the pressure or the temperature differs from region to region) the system is said to be in a non-equilibrium state. Such a state is not permanent, any non-equilibrium system rearranging itself in such a way as to eliminate completely all non-uniformities and so reaching equilibrium. This empirical fact is expressed by the statement that non-equilibrium systems (solids, liquids, or gases) move irreversibly towards the equilibrium state. The movement to equilibrium is brought about by the transport of momentum, energy, mass, electric charge, and so on, from one region to another. In gases this transport is due to the movement of molecules between collisions. For liquids, where there are no molecular collisions, the transport is brought about by the action of the forces between the molecules. The ease, or difficulty, of transport is conveniently described by a set of transport coefficients such as those of viscosity, thermal conductivity, diffusion, electrical conductivity, and so on. These coefficients describe the rate at which non-uniformities disappear from the system. The primary aim of the kinetic theory of non-equilibrium systems is the calculation of the transport coefficients from the known properties of the molecules.

Distinction between Theories of Equilibrium and Non-equilibrium

It has been assumed that the same properties of the molecules control the movement to equilibrium as control the equilibrium condition itself. In particular, the intermolecular force can be expected to play an especially important role.

In calculating the properties of non-equilibrium systems it is not possible to apply the principles of statistical mechanics that were seen earlier to yield the properties of equilibrium systems. This arises from the fact that non-equilibrium systems contain an element of irreversibility that equilibrium systems do not, and the principles of statistical mechanics are quite unable to account for irreversible features. Accordingly in treating non-equilibrium systems it is necessary first to enunciate principles whose validity is comparable to those of statistical mechanics in the field of equilibria. No general principles of this type applicable to all non-equilibria are yet available, but special solutions of limited validity have been given. The best known is possibly that applicable to dilute gases. In 1946, J. Kirkwood gave another special solution applicable to liquids.

Irreversibility in Liquids

Kirkwood introduces irreversibility into the theory by considering the force acting on a representative liquid molecule due to its neighbours at different times. If two consecutive values of the force are determined separated by a time interval τ it is asserted that there is a correlation between the two values only if τ has a value smaller than some characteristic value τ_c; for values greater than this no correlation exists. This characteristic value τ_c need not be stated exactly; it is enough to require that it is microscopically large (i.e. large compared with the times associated with molecular interaction), and yet macroscopically small (i.e. small compared with times characteristic of the large-scale physical system).

The Kirkwood hypothesis,* as we may call it, establishes the existence of a friction constant which controls the dissipative processes. It must, however, be realized at once that this friction constant in no way refers to a frictional force in the normal sense. The appearance of a friction constant in theories of non-equilibrium is characteristic, and has its macroscopic counterpart in the hydrodynamical Stokes' friction constant. The Kirk-

*This hypothesis is quite distinct from the Kirkwood superposition approximation met with earlier in the equilibrium theory.

ood hypothesis allows for the first time the macroscopic tokes' friction constant to be expressed in terms of molecular roperties, and this is an important result of the modern theory. xtreme mathematical difficulties have so far prevented the ploitation of this result, for it has not yet proved possible to valuate the expression to obtain a value of the friction con-ant for even the simplest liquid system. Such an evaluation ust be one of the major aims of future work.

air Distribution in Non-equilibrium Liquids

The Kirkwood hypothesis allows the specification of the ructure of non-equilibrium liquids in terms of the probabilities the occurrence in phase (momentum and position) of single olecules, and of groups of two, three, four ... liquid mole-ules. The importance of the interaction between pairs of mole-ules in the equilibrium theory makes it natural to assume, at ast at first, that the corresponding interaction will also prove gnificant in the theory of non-equilibrium liquids. It appears far that the pair interaction is indeed of particular impor-nce.

J. Kirkwood showed in 1946 that the distribution of pairs of olecules in a non-equilibrium liquid can be calculated by ing an equation of the same type as that used in the theory of e Brownian motion. (It will be remembered that the Brownian otion was one of the earliest demonstrations of the molecular mposition of matter.) This equation, known rather formidably the Fokker-Planck equation after its originators, A. Fokker d Max Planck, relates the change in the distribution function the physical factors. It is essentially a rearrangement of the uation of Liouville to include the extended interaction suf-red by each liquid molecule: the left-hand side of the equation scribes the change in the pair distribution due to changes in ne, position, and velocity; the right-hand side relates the ange in the pair distribution to phase increments during the ne interval τ, representing the cumulative effect of a large mber of weak encounters with neighbours.

In order to determine the pair distribution in a liquid, then,

it is necessary to solve the Fokker-Planck equation; the solution is made to represent a specific physical system by requiring it to satisfy certain conditions at large or at small distances. Restrictions of this type are called the boundary conditions; these form an important part in any problem of theoretical physics.

The problem of solving the Fokker-Planck equation is much simplified for systems in which the friction constant is sufficiently large. In this case the equation of the Brownian motion (in phase space) can be replaced to very good approximation by an equation in position (configuration) space due to von Smoluchowski, and used in the theory of diffusion. The solution procedure for the Smoluchowski equation, although still difficult, is simpler than that for the more correct Fokker-Planck equation, and has been used in calculations by several authors.

Viscosity and Thermal Conduction

Formulae have been given for the rate of flow of momenta, or of energy, in a liquid in terms of the intermolecular force, the pair interaction, and the appropriate solution of the Fokker-Planck or Smoluchowski equations. The initial formulae were due to J. Kirkwood, and independently to M. Born and H. S. Green, but although they agreed in their expression for the momentum flow, the expressions for energy flow were not exactly the same. This discrepancy has very recently been resolved by R. Eisenschitz. The momentum and energy flows eliminate non-uniformities in the liquid. When the deviations from equilibrium are small, a condition found in practice, the flow of momentum is proportional to the gradient of velocity, and the flow of energy is proportional to the gradient of temperature in the liquid. By definition the factors of proportionality in these expressions are respectively the coefficients of viscosity and thermal conductivity. In this way these coefficients have been related to molecular data. Expressions for the viscosity and for the thermal conduction of a liquid have been given explicitly by J. Kirkwood, and independently by R. Eisenschitz. To evaluate these expressions it is necessary to know the details of the intermolecular force, and also to have available

the solution of the Fokker-Planck equation, or alternatively the Smoluchowski equation, corresponding to the appropriate boundary conditions. The expressions due to these two authors differ in the boundary conditions to be applied. Essentially, boundary conditions used by R. Eisenschitz are chosen for reasons of physics (from the observed isotropy of liquids, or the condition that the heat flow must not be too great) whereas those used by J. Kirkwood are chosen from considerations of mathematics. The expressions contain explicitly the friction constant, so that any complete numerical test of each equation must involve the calculation of the friction constant.

Although such a calculation has not yet been achieved, a preliminary check of the equations has been made. Using an appropriate expression for the friction constant, J. Kirkwood and his colleagues have calculated the viscosity and the thermal conduction for liquid argon at one temperature; they find values of the correct magnitude, although the (rather unreliable) numerical factor is too low. R. Eisenschitz has shown that his formulae are of the correct magnitude, and further they show the correct dependence on temperature. In particular it appears that his formulation leads at once to a strong temperature dependence of the viscosity, and a rather weak dependence for the thermal conduction. This difference of temperature dependence for the two transport coefficients has long been recognized as an empirical distinction between liquids and gases, for the latter case the temperature dependence is rather weak in each case. Elementary theories have been quite unable to explain the experimental result for liquids, and it is gratifying that the modern theory predicts the result in a natural and consistent way through the difference in the boundary conditions that must be applied in each case. One point that emerges is that the empirical exponential dependence of liquid viscosity on temperature is not confirmed by the theory; the predicted strong temperature dependence has no simple analytical form.

The theory reported so far has been extended by R. Eisenschitz and the present author to include also orientation effects of the molecules. The formulae derived can be arranged so as

not to include the friction constant, so that absolute values can be calculated. Agreement with experiment is satisfactory, and this is encouraging since more elementary theories cannot account for the effect.

CONCLUDING REMARKS

It seems that principles are now available that allow the calculation of the macroscopic properties of liquids from the corresponding microscopic properties in a completely consistent manner. The methods so far developed have been applied to monatomic liquids with encouraging success. Although application has not yet been made to more complex liquids, such an application would appear to provide problems of computation rather than any new problems of physics. It can, therefore, be claimed that the beginnings of an understanding of the liquid state have at last been achieved. As electronic computors become more widely available, more rapid progress may be possible. Although a large amount of work still requires to be done, it can be confidently expected that the present methods will ultimately lead to a full understanding of the liquid state.

FURTHER READING

Article by the present author in 1956 Volume of *Reports on Progress in Physics* (Physical Society, London), 'The Kinetic Theory of Monatomic Liquids at Ordinary Temperatures for Uniform or Non-uniform Conditions'.

MNEMONIC SYSTEMS AND DEVICES

IAN M. L. HUNTER

In the year 1581, an Italian philologist by the name of Muretus heard of a young Corsican student who boasted an outstanding memory. To test this claim, Muretus invited the student to his house in Padua where he and his friends called out a long list of words which were not only unrelated to each other but came from a variety of languages. When the list was finished, the Corsican fixed his eyes to the ground for a short time and then repeated every word accurately and in its correct order. He amazed his audience further by repeating the list backwards and then naming each alternate word in the list. On hearing that the Corsican had been taught this memory trick by a Frenchman, one man asked if he too might learn the art. This man, Molinus by name, apparently had a very poor memory, yet, after only ten days of study, he could repeat accurately 500 words in any order required. This anecdote is not unique, for there exist scores like it. But it shows, first, that some people have the ability to perform remarkable feats of memorizing and, second, that this ability can be transmitted from one person to another.

How did the Corsican contrive to recall these lengthy lists of words? Unfortunately, Muretus does not tell us. One thing, however, is certain. The Corsican must have employed a 'mnemonic system', that is, a set of rules formulated with the explicit intention of assisting memorization. Over the centuries, there have been many others whose recall performances have rivalled that of the student. These performances have been used for the amusement of friends and, often, for unabashed commercial gain. Into the present day, so-called memory experts have packed halls with audiences which have paid to witness

feats of memory. Many of these experts have used a system (in which case they may be called 'mnemonists') and made a substantial livelihood by offering, usually under an oath of secrecy, to impart their technique to others.

This article will survey in brief such mnemonic systems as have found their way into print. Numerous as these systems are, it will be feasible to place each into one of three categories, namely, that of the visual-symbol type, the digit-letter type, and the successive-comparison type. An attempt will then be made to discuss the intellectual processes which these systems exemplify. In making this attempt, mnemonic devices will be introduced. These are techniques which assist memorizing yet cannot be said to constitute elaborately planned systems. They are used, often unwittingly, by everyone in day to day learning, but only used by mnemonists as incidental to their deliberately contrived systems proper. Lastly, suggestions will be hazarded about the relevance of these systems and devices to everyday learning and remembering.

THE SYSTEMS

Visual-symbol systems

The earliest system on record is that devised, around the year 500 B.C., by the Greek poet Simonides. According to Cicero, the outline of this system occurred to Simonides under most dramatic circumstances. It seems that a Greek called Scopas had won a wrestling victory at the Olympic Games and was giving a banquet at his house by way of celebration. After the fashion of the times, he invited a poet – Simonides – to provide a recitation as part of the entertainment. Almost immediately after delivering his eulogy, Simonides was called away to speak with two young men who were waiting on white horses outside. He had scarcely left the house when the floor of the banquet room collapsed, killing the host and all his guests. Naturally, relatives wished to sort out the bodies. But these were so mutilated as to be unrecognizable. However, Simonides had observed, during his recital, the position occupied by each person in the room and, by searching the wreckage in the appropriate places, he was able to identify

the bodies. He could recall *who* was present by recalling *where* they were: recalling the guest's position in space aided him to recall the identity. This incident set Simonides thinking. If such were the case with places and people, surely names, objects, and even ideas could be better memorized by assigning them fixed positions in space. He would imagine, as vividly and with as much detail as possible, a room. And each item he wished to commit to memory he would visualize as being placed in a certain part of this room. The first item would always be placed, say, on the middle of the far wall, the second on the top left-hand corner of the window, and so on. Then, when he wanted to recall these items, he would systematically explore this imaginary room and find each item located in its particular position. Simonides found that a technique of this sort was, indeed, of considerable help in memorizing. He reduced this method to a system and appears to have taught it to others, since both Quintillian and Cicero confess themselves indebted to it. These orators used to prepare their speeches by thinking of each division in connexion with a specific, visualized locality. Then, since the order of these localities was well known, the speaker had only to imagine each in turn to recall the divisions in their correct sequence. For obvious reasons, this type of system was called the 'locality' or 'topical' system. (The word 'topical' comes from the Greek word meaning 'a place'. It is of interest that we still speak of 'topics' in connexion with discourses and that Simonides's system is believed by some to have furnished the origin of such expressions as 'in the first place'.) Since the time of Simonides, many spectacular mnemonists have employed variations of the locality system. Rooms, houses, public buildings, the human body, the backs and palms of the hands – all of these have been broken up into distinctly visualized localities so that items to be memorized can be associated with them.

In the mid seventeenth century, a Cambridge man, Henry Herdson, devised what might be thought of as a logical development of the locality system. Up to this time, mnemonists had represented sequence by standard visual images occupying different parts of an imagined spatial whole. What Herdson did

was, very simply, to dispense with the spatial aspect of the model and employ merely the images themselves. He represented each numeral by one of a variety of objects as follows. 1 = candle, or any elongated object; 2 = swan, or any 2-shaped object; 3 = trident, or any object with three parts; 4 = dice, or any object with four parts; 5 = hand, or any object with five parts; 6 = tobacco pipe, or any 6-shaped object; 7 = open razor, or any 7-shaped object; 8 = spectacles, or any object involving two round shapes; 9 = burning glass, or any 9-shaped object; and 0 = orange, or any round object. An intending mnemonist would decide for himself which particular object he would use to represent each numeral. He would, incidentally, be likely to represent each numeral by only one object: Herdson's system is atypical in using more than one object for each number. Having made his decisions, he would then familiarize himself thoroughly with his code. And at this point he would be in a position to employ his system in exactly the same way as a locality system proper. He need not, of course, limit himself to the first ten numerals. He could extend his code indefinitely, e.g. ten might be a policeman, eleven a pillar-box and twelve a clock. In practice, most mnemonists seem to extend their systems to cover numbers up to thirty at least and often up to a hundred. Neither need he be bound to any particular code. He is free to represent any numeral by any symbol he cares to select. The very variety of symbols used by different mnemonists argues that there is nothing magical about Herdson's choices. One symbol seems to be as appropriate as any other; it matters little which is used. What is important is that one definite series should be decided on and should be made completely familiar before being put into use.

The first type of system, then, involves the representation of sequence by a succession of predetermined visual symbols. It is these symbols which are associated with the items to be memorized. Now, what about this association part of the procedure? Are any especially effective techniques involved? In so far as mnemonists have offered advice on this all-important aspect of the process, it is simply this: first, visualize the two objects

together as vividly as possible; and second, never compare more than two items at a time. Suppose Herdson's symbols are being used, and it is wished to memorize, in order of size, the largest cities in the world. From largest to smallest, the first three cities are London, New York, and Tokyo. London's position might be fixed by visualizing Big Ben surmounted by a candle round which thousands of people are clustering for warmth. A great flock of swans might be imagined waddling amid the skyscrapers of New York and trailing havoc in its wake. Three slant-eyed Japanese might be seen perched on an elaborately carved three-legged stool. Each object is visualized as concretely as possible: the candle is a particular candle with distinct individual features. Further, the paired objects are not simply juxtaposed. They are brought into a definite relation with each other, and the more ridiculous, far-fetched and 'striking' this relation, the better. Once the two objects have been thus visualized together, the mnemonist is free to attend to the next pair. It is unnecessary for him to attempt to keep the first pair of objects in mind while dealing with the second. In fact, any effort to attend to more than one pair at a time is detrimental to memorizing. The rule, often cited by nineteenth-century mnemonists, is: never compare more than two items at a time. The above two pieces of advice, on visualizing and comparing, are so frequently alluded to in the writings of different mnemonists that they must be assumed sound. And, for what it is worth, the writer has tried out these rules for himself and become convinced of their practical worth. Practice is, of course, essential for the effectiveness of these, as for any, techniques. With practice, it becomes a simple matter to visualize two objects clearly and bring them together in a lively and dramatic way. It becomes easier, too, to dismiss this scene from awareness and attend fully to the next pair. There may, however, be some people for whom such an accomplishment would never become easy. There undoubtedly exists that minority for whom vivid visual imagery is an impossible task.* It follows that, for them, any system of

*See, for example, the article 'The Measurement of Mental Images' by P. L. Short in *Science News 24*.

the visual-symbol type would be totally ineffective. Some of them might even find it inconceivable that such a system could be used by anyone at all.

In the practical application of the visual-symbol system, mnemonists do not seem to rely on exclusively visual relationships. The examples which they give of their associations reveal that the key object is often related to the given object in verbal terms as well. Consider the task of associating 'Tokyo' with 'three-legged stool'. In addition to visual relations, the Japanese might be thought of as wearing toques, as having a toe stuck in a bottle of Tokay wine, and as crying 'Oh, my toe is stuck in the Tokay, oh!'. Here the two items are related together in an imaginary situation which has verbal as well as visual characteristics. Examples employing both modes of relating are by no means uncommon in the literature. Mnemonists, it seems, are shrewd enough not to pass by any helpful method in the interests of preserving the purity of their chosen system. A system only dictates, as it were, the strategy. The tactics it leaves to the individual mnemonist.

Before leaving this first class of system, the question of interference should be mentioned. If a candle is visualized along with Big Ben and then, shortly after, it is associated with an elephant and, again, with an egg, might not the elephant be recalled when, in fact, Big Ben would be appropriate? The risk of such interference certainly seems plausible. In practice, however, it does not seem to occur any more than the ordinary reader is confused to find the same word occurring in the contexts of different sentences. Mnemonists claim that they do not suffer by this embarrassing possibility and can memorize two completely different lists, one immediately after the other, without confusion. This means that the relating of the visual-symbol to the given item cannot be the whole story. The mnemonist must also have learned something about the given series as a whole. The probability is that he has learnt the series as having been presented to him at a particular time under particular circumstances and also that he has learnt some fragmentary relations between the items themselves. Psychologists who study learning under controlled

conditions in the laboratory know that such forms of learning occur with great regularity, even although the learner himself may be quite unaware of it. Despite their not acknowledging them, mnemonists must form relations between items and their context. Otherwise they would lose the identity of their lists and be unable to recall now this series and now that. Whether the lists would still be distinct a week or a month later is another matter. It is to be expected that, without rehearsal, not only would items from each list be forgotten but also items from one list would intrude into the other. The correctness of this expectation is something which mnemonists neither confirm nor deny since they are rarely interested in long-term remembering. They are concerned, rather, with effecting a single astonishing performance of more or less immediate recall.

While the visual-symbol system is designed primarily for the memorization of objects, it has occasionally been applied to lists of digits. The usual procedure is to represent each digit from 0 to 9 by its appropriate symbol and represent the serial position of each digit by the symbol appropriate to the number 11 and upwards. The two symbols, one for the digit and one for its position, are then related together in the usual way. For example, suppose the sequence to be memorized is 621 . . . , and the symbols for 11, 12, and 13 are policeman, pillar-box, and clock, respectively. Using Herdson's code, the mnemonist would visualize 'policeman and pipe', then 'swan and pillar-box', and then 'candle and clock'. The simultaneous use of two different parts of the code is necessary in order that both the digit and its position can be represented. It is easy enough to associate a swan and a pipe. It is more difficult to visualize these objects in such a way that their sequence is apparent. And the outcome of such an attempt might well be an inability to recall whether the original number was 26 or 62. The situation becomes even more difficult with larger numbers. In leaving the visual-symbol system, it should be said that it is not only the earliest and most elaborate of the systems: it is also much the most popular and, after the time of the Greeks, seems to have been in constant use since the early fifteenth century.

Digit-letter systems

The second type of system is explicitly designed to assist the memorization of digit sequences. It was in 1684 that Stanislaus von Winckelmann, a German, published his 'most fertile secret' of symbolizing digits by letters of the alphabet. These letters were then built up into easily remembered words or sentences. In the following century, variations on Winckelmann's system appeared, including a version by the philosopher Leibniz. But the system seems to have gained its greatest popularity in the nineteenth century. By way of illustration, one of these systems of a century ago may be cited in detail. It is that devised by an English clergyman and schoolmaster called Brayshaw. His code was as follows:

1	2	3	4	5	6	7	8	9	0	00
B	D	G	J	L	M	P	R	T	W	St
C	F	H	K		N	Q		V	X	
			S			Z				

Applying his system to education rather than to theatrical entertainment, Brayshaw published, in 1849, his *Metrical Mnemonics* which contained a collection of rhymes embodying over two thousand dates and numerical facts drawn from history, geography, physics, astronomy, etc. These rhymes were clearly intended for use in schools and were, presumably, learned by the pupils of Keighley Grammar School where Brayshaw was headmaster. The method of this facile rhymester is apparent in the following typical example which gives the 'dates' of English sovereigns:

1066 By *men*, near Hastings, William gains the crown:
1087 A *rap* in Forest New brings Rufus down.
1100 Gaul's *coast* first Henry hates, whose son is drowned;
1135 Like *beagle*, Stephen fights with Maude renoun'd.
1154 A *cloak*, at Becket's tomb, sec'nd Henry wears:
1189 And *brave* first Richard oft Saladin dares.
1199 John's *act at* Runnymede England pleased avows:
1216 His *face, in* Parliament, weak third Henry shows.

The verse continues in this way up to the last line which gives the date of Queen Victoria's accession:

1837 Lastly, *our hope* rests on Victoria's will.

Thus, Brayshaw inserts in each line the sovereign's name, some outstanding fact about him, and the date of his accession to the throne. This date is embodied in the second word or in the second and third words combined, it being understood that the dates begin at the year 1000.

None of the other versions of the digit-letter system vary from Brayshaw's in more than three minor respects. First, they may use a different code, e.g. 'one' may be represented by P or W or any other letters of the alphabet. Second, the letters may not be formed into words but may be used initially to construct a phrase in which each successive word begins with the appropriate letter. Thus, 1855, the date of Lord Raglan's death, is represented by the letters C–R–L–L; these may be embodied in such a phrase as 'Courageous Raglan Lamented Lies'. The third variation concerns, again, the code itself, phonic elements being used in place of letters. Here each number is represented by a sequence of sounds. Such a system would obviously be preferred by those mnemonists who possessed strong auditory imagery, who, in other words, tended to think about events primarily in terms of how they sounded.

It should be apparent that these digit-letter systems would, like visual-image systems, gain greatly in efficiency by being practised. It is, however, noteworthy that they have played no appreciable role in the stage performances of mnemonists. This is no doubt due, in part, to the lesser interest shown by audiences in an ability to recall numbers as opposed to sequences of objects. But chiefly it may be attributed to the greater difficulty of fitting the letters together into words or phrases. With these systems, it is impossible to compare merely two items at a time: four or five letters must be related together simultaneously. Accordingly this type of system lends itself but poorly to the rapid-fire conditions of the stage. It has, instead, been employed by educators who could take leisure to devise the happy phrase which others could learn and interpret as occasion demands.

This raises the question of whether Brayshaw and his like-minded colleagues of the nineteenth century were correct in assuming that theirs was the most effective method of teaching a large body of numerical data. It must be remembered that their educational programme involved vast amounts of rote memorization. Their pupils had to learn hundreds of dates and distances, to say nothing of the areas and populations of countries. Faced by such an undertaking, pupils may well have found their metrical mnemonics of assistance. The modern pupil, however, would not find it worth while to master a digit-letter system since, for good or ill, the educational emphasis has swung away from rote memorization. School history, for example, is no longer synonymous with lengthy lists of dates.

Successive-comparison systems

About the third type of mnemonic system relatively little need be said since it has, in part, already been described. This type lacks the formal structure of the other two in that it does not involve the initial mastery of any code. It might even be debated whether it should be classified as a system at all. In discussing Simonides's system, reference was made to the rule: never compare more than two items at a time. The third system is, in essence, an exploitation of this rule. In memorizing a list of objects, the mnemonist compares the first word with the second, bringing them together into some related whole. He then dismisses this whole from awareness and attends to comparing the second and third words. This done, he relates the third word with the fourth, and so on. The important thing is that at no point is he concerned with more than two words: he relates each pair successively. It is for this reason that the system may be called that of successive-comparison. The first word in the series must, of course, be learned by itself. But in recall, this word is thought of in association with the second which, in its turn, is recalled as linked with the third. The actual method of relating any two words is highly variable. They may be brought together by any of the myriad relationships which used to be described so minutely by philosophers of the British Associationist School.

The objects for which the words stand may be visualized together; the words may rhyme; they may suggest some causal sequence, either mundane or absurd; they may be related through one or more intermediate words. These are but a few of the ways in which associations might be forged. As with other systems, the mnemonist becomes increasingly skilful with practice in relating one item with another.

This successive-comparison technique seems to have been used mostly with lists of unrelated words. But it could obviously be employed with any sort of material. It could, for example, be used in applying the visual-symbol system to the memorization of digits. As the mnemonist hears each digit, he could visualize its code object and associate this with the object representing the neighbouring digit. In spite (or perhaps because) of the wide applicability of this third system, it seems to have enjoyed little popularity among mnemonists. It appears to have been used more as a device to facilitate the use of the other two systems than as a system in its own right.

PROCESSES EXEMPLIFIED BY THE SYSTEMS

Repetition and observation

At this point, it might be instructive to pause, look back at the three systems and ask: have we learned anything from them about the basic processes of efficient memorizing? The survey has, in the writer's view, demonstrated the necessity of three conditions without which the more complex kinds of human learning cannot take place. In short, it has illustrated the necessity for: good observation; organization of material; and repetition. These three conditions have, of course, long been recognized as important. They were much emphasized by mnemonists and philosophers alike from at least the sixteenth century onwards. And they were, almost inevitably, discussed by Aristotle. It is now proposed to discuss, in broad terms, the major processes underlying these three conditions – processes which manifest themselves not only in mnemonic systems but in learning generally. One point, however, should first be made clear. Intellectual activity proceeds as a complex whole. It is not

compounded of this or that process tacked on to some other. This means that the following discussion will be, to some extent, artificial in that aspects of a unitary whole will be split off for the convenience of discussion. Processes which interact intimately and simultaneously will be separated out and treated consecutively. In so abstracting the parts, some violence will be done to the whole which they constitute; but not, it is hoped, so much violence as to render the attempt at analysis unworthwhile.

In reviewing the three mnemonic systems, all the emphasis was placed on methods of organizing the material to be memorized. Accurate observation was not mentioned as such, nor was repetition. If mnemonists ignore the factor of repetition, it is largely because the theatrical conditions under which they work deny them the opportunity for it. But wherever their systems have been advocated for use in long-term remembering, the importance of repetition has invariably been stressed. There is no need, here, to exemplify the value of repetition for learning: we are all aware of its worth in our everyday lives. If some learned performance is rehearsed often enough, it need never be forgotten. This truth seems to be known to the veriest child who departs on an errand chanting over and over again to himself the message he must deliver. As for the factor of accurate observation, mnemonists have tirelessly, and rightly, accorded it an essential role without, however, giving any explicit advice on how it may be improved. Nevertheless, their systems can be said to provide, unintentionally, the conditions under which observation can be sharpened through practice.

Consider the visual-symbol system. The very intention of using this system gives the user a set to learn. It prepares him to attend to the words supplied by his audience and to ignore irrelevant features of the situation. Further, the use of the system demands that the user know exactly which words are being spoken. If a word is indistinctly heard, he is acutely aware of it and demands that it be repeated rather than merely letting it pass as he so often does under ordinary circumstances. The system also demands that he think about the word in a certain

way. He must visualize the object for which it stands and do this in such a fashion that it can be related to the appropriate visual symbol. The system provides him, in short, with a technique of selective observation. It is a fact that observation can be sharpened by possessing an intention to learn and a knowledge of what to look for. We can demonstrate this fact for ourselves in the common situation of remembering 'names and faces'. The technique is simple. First, have an intention to learn. We must be aware that people's names are things to be learned; we must be prepared to accept an introduction to someone new as presenting a real memorization problem. Second, look for those distinctive aspects of the name and face which will enable them to be related together. The name must be got right even if it has to be repeated. We must ask ourselves if the name is strange or if it is the same as that of someone we already know. We must look for any peculiarities and also any of the many associations to which these differences and similarities may give rise. As with the name, so with the person. We must ask ourselves: what does he look like? Third, the material must be organized. Relations must be found between the name and its owner even if these are bizarre to the point of being downright uncomplimentary to the subject of our scrutiny. It should be mentioned that, with a little practice, this entire process takes much less time to perform than it has taken to describe. The fourth and last point is that the association between the person and his name should be given the advantage of repetition. In conversation, we should address our new acquaintance by name as often as politeness permits. This, then, is the technique. If we try it out, it will convince us that observation, organization, and repetition form a potent triumvirate. And it will give us some notion of the force with which our observation of a situation is sharpened by deliberately setting ourselves the task of organizing our experiences of it.

Assimilation

What about the organization process itself? Despite the many ways in which organizing can be achieved, it seems possible to

subsume all instances under the two broad headings of 'assimilation' and 'recoding'.

By 'assimilation', or more properly 'assimilation to schemata', is meant the interpretation of new experiences in terms of old, the relating of the unfamiliar to the familiar, or, to put it metaphorically, the filing away of novel experiences in the cabinet of past experiences. It is the phenomenon which philosophers of previous times referred to as 'apperception'. And its influence is ubiquitous throughout all human perceiving, learning, and thinking. About the *modus operandi* of the phenomenon little, as yet, is known. But about its existence and importance there can be no doubt, as the following illustration may demonstrate. If a trained scientist and an undergraduate student are asked to read a paper from one of the technical journals, the scientist will read it more quickly than the student and will, in addition, be able to tell us much more about it afterwards. Why should the two men differ so in the efficiency of their learning? There may, of course, be a number of reasons, but far and away the most important one is that the scientist brings to his reading a larger body of organized knowledge to which he can relate the contents of the new article. While reading, he might well make such comments as the following: this is exactly what so-and-so did; that's an interesting variation on such-and-such a procedure; what a good idea to shift the emphasis from this to that aspect of the problem; and, this finding flatly contradicts so-and-so's results. Throughout, the scientist is comparing what he reads with what he has read or thought or talked about at different times in the past. He is relating his present experiences to an organized body of past experience. He is assimilating to a schema. The student, on the other hand, possesses no such body of knowledge or, at best, very little. He cannot lay aside the paper and recall what he has read in terms of its similarities and differences to what he has read in the past. He must simply read it and try to remember it as it stands. He is at the disadvantage of having, as it were, no framework within which the new experiences can be set. Now, the advantage which the scientist, or any other specialist, gains by possessing a body of knowledge

is similar to that gained by the mnemonist through possessing a mnemonic system of either the visual-symbol or digit-letter variety. What the system does is to provide a schema to which new experiences are deliberately assimilated, a familiar framework in which to lodge the unfamiliar items.

It is in terms of a more than ordinarily well-organized and extensive body of past experience that we must consider the performances of 'memory-men' as distinct from mnemonists. The latter, as has been seen, specialize in the immediate recall of new and unrelated material. This they accomplish by means of a well-practised mnemonic system. The former, on the other hand, specialize in answering factual questions which usually concern some relatively restricted field such as historical dates or sport. Mnemonic systems are of no direct assistance in this type of performance. What is required is the ability to call readily on a vast store of information which has been built up over the years. The memory-man facing his audience is in much the same position as the well-prepared student facing his examiners and may, indeed, be little better than the latter in the range and spontaneity of his answers. He also has a slight advantage (which many students would doubtless wish to share) in that he can often ignore certain questions. During his performance, requests for information are being fired at him from several parts of the audience and he is, to some extent, able to select those questions to which he knows the answers. This in no way detracts from the sometimes truly amazing extent of his knowledge. It is merely one of the many techniques which all stage performers use to enhance the effectiveness of their 'act'. The main point, as far as we are concerned, is that the knowledge displayed has not been acquired by either super-normal means or through the use of mnemonics but by prolonged study of some subject in which the memory-man is, for some reason or other, profoundly interested.

The very possession of an extensive body of knowledge makes it easier to memorize any new material which can be assimilated to it. For example, Salo Finkelstein, the Polish 'calculator', was studied some thirty years ago and found to have a digit memory

span of twenty (the average adult has a span of about seven). His familiarity with figures was such that, for him, digit sequences 'made sense'. He immediately perceived them as mathematical functions such as powers, roots, logarithms, and prime numbers, as historical dates, as telephone numbers, as numerical characteristics of literary works such as the number of paragraphs contained, etc. And if an occasional number had no apparent association, it acquired memory value by very virtue of being unusual. As compared with the average person, his performance regarding numbers is comparable to that of a man who understands some foreign language which another man does not.

Before leaving the assimilation process, it might be appropriate to mention a mnemonic device advocated in the early nineteenth century by an American, Pliny Miles, under the name of 'homophonic analogies'. Difficult or unknown words are thought of in terms of similar sounding but more familiar words. Thus, the battles of Fontenoy, The Nile, Corunna, Quatre Bras, and Warsaw might be memorized in terms of Funny boy, a Nail, a Cow running, a Quart of brass, and a War of sorrow. For Miles, these analogies were apparently easy to remember and could be used by him to suggest the originals (they were doubly useful in that he combined them with a digit-letter system). This technique is mentioned here because it is an example of assimilation, relating, as it does, the unfamiliar to the familiar. But for all that, it can hardly be regarded as a system comparable in deliberate construction to, say, the visual-symbol system. It represents, rather, a fairly elementary trick which everyone has probably used at one time or another. The writer has certainly found himself using it in the past with some success – and a number of failures as when he once recalled the name Wolfenden (Wolf-in-den) as Foxinlair.

Recoding

We may now turn to the second method of organizing and, in so doing, encounter some of the mnemonic devices which illustrate it. We are all familiar with the verse which begins: 'Thirty

days hath September'. It is by means of this verse that most of us first memorized, and still recall, the number of days in each month of the year. From this example it can be seen that 'recoding' differs from assimilation in that items to be remembered are not translated into items of a different or related sort. The numbers are not, for example, converted into letters. The items themselves are left relatively intact, and it is the relationships between them which are altered in such a way that the whole can be more easily remembered. In brief, whereas assimilation aids memorizing by relating new items to others which are already known, recoding relates the new items to each other. As a further example of recoding we may cite a happy but little known jingle which one small girl had written specially for her (for obvious reasons) by a distinguished father, namely, Robert Southey, the poet. The verse runs:

A cow's daughter is called a calf;
A sheep's child is a lamb.
My darling must not say 'I are',
But always say 'I am'.

As in the two examples just mentioned, recoding has greatly exploited the organizational advantages of rhythm and rhyme. This is attested by the myriad verses (including Brayshaw's) which have been composed to help us learn the more cumbersome facts of history, foreign languages, and so on. But recoding does not rely solely on the relationships of rhyme and rhythm. There is, for instance, the time-honoured device of the acrostic. The idea of the acrostic is at least as old as the 119th psalm. This composition consists of twenty-two eight-verse sections corresponding to the twenty-two letters of the Hebrew alphabet. The first word of every verse in each section always begins with the same letter, i.e. the letter for the first section is 'aleph', and for the second section 'beth'. In general terms, the acrostic arranges the material, often in verse form, in such a way that the first, last, or middle letters of the lines combine to spell out a word, phrase, or alphabetical sequence. A good example is one which is known to many medical students. There are six major infectious diseases which produce a rash. However, one produces it on

the first day after infection, another on the second day, and so on. To ease the task of memorizing the incubation period of each disease, the student learns the sentence, 'Very sick people must take no exercise'. The position of each word represents the day on which rash appears and the first letter of each word is the same as that of the infection – except in the case of 'no', which indicates that no infection produces a rash on the sixth day.

Other examples of recoding were met in the discussion of digit-letter mnemonic systems. Thus, once the number 62174 had been translated into the letters M–D–C–P–S, these letters would be recoded into a word such as 'madcaps'. It is noteworthy that, in cases like this, it cannot be said that the organizing process is one of either recoding or assimilation. Both processes are present, as is also the case in the example of remembering that stalactite comes from the ceiling and stalagmite from the ground. This illustrates what was said earlier about the intimate interaction of intellectual processes. However distinct any two of these may be, there inevitably comes a point at which they shade imperceptibly the one into the other.

There is one last variety of recoding which is of much greater importance than any of the others. It is, indeed, one of the most powerful tools of scholarship and science since it provides the basis of all generalizations, rules, and laws. Suppose we want to remember how far a body has fallen through space when it has been falling freely for a given time. We could attempt to memorize all the individual distances. After one second, the body has fallen 16 ft, after two seconds, 64 ft, etc. However, in memorizing those figures, we would be making our task unnecessarily difficult. We would be ignoring the fact that these numbers are not independent of each other, that there is, in the language of 'information theory', a redundancy in the series. And this redundancy can be eliminated by recoding the above figures into the statement that the distance fallen at the end of t seconds is $gt^2/2$ ft. Since the value of g is approximately 32, all that need be remembered is the formula $16t^2$. Having recoded thus, we are in a position to 'memorize' thousands of measurements by simply memorizing this formula. Scientists are for ever on the alert for

dependencies of this sort. And these dependencies, if they are important enough, attain the title of scientific laws. The difference between the law and the original data does not lie in the information contained: it lies in the way the information is organized. The law, as it were, packages up the data into a form which can be more easily remembered.

It should be realized that the utility of recoding is not, by any means, limited to the field of remembering. Recoding, along with assimilation, plays a notable part in thinking. But a consideration of these aspects would fall outside the scope of the present discussion. Unlike assimilation, however, recoding has never been used as the central foundation of any mnemonic system. Memory-men have, none the less, used recoding, in the form of rules, as the basis of their performances. A favourite feat consists of naming the day of the week on which any given date fell. A member of the audience might call out 'October 21st, 1805', to be told, after a few seconds, that this particular date fell on a Monday. This is impressive but is not, in fact, a memory feat proper, since there is no question of the days and dates having been memorized as such. The performance is accomplished by means of a set of relatively simple rules which make both easy and possible the necessary calculations. The arithmetic involved is simple and anyone of average intelligence could perform the feat after half an hour of study and a little practice.

Reintegration

Our survey of mnemonic systems and devices is now reasonably complete. There has, however, been one omission – the keeping of mementos. A memento is an object which prompts recall. As an object, it is not in itself a technique for assisting memory. It is only when we provide ourselves with such an object that we can be said to employ a mnemonic device. The photograph of a distant friend, the treasured keepsake from some bygone day – these are not devices. What is a device is our having acquired them with the intention that they should, in the future, prompt us to recall certain past events. Memento-keep-

ing differs from the other mnemonic devices in that it does not exemplify any process of organization. Rather, it represents an instance of 'reintegration', that is, the process whereby some object or event in the here and now serves to bring about recall of that larger situation of which it previously formed a part. We must all have experienced reintegration in our everyday lives. Instances of it frequently occur to motorists. If they travel to a town which they have visited only once, or perhaps twice, before, they often have vivid recollections of their former visit. The experiences of that earlier time are recalled, what was seen or conversed about. And all this occurs without, on their part, any explicit intention to recall: it is as though the recall were forced on them from without. The memento, then, serves to reintegrate in the same way. The knot in the handkerchief reintegrates the situation in which we tied it. It prompts us to recall the reason for its being there. Likewise, Jews, at least in ancient times, wore a shawl during prayers in the synagogue and at each corner of the shawl was a string knotted five times to call back to awareness the five books of Moses. So also did the medieval clergy fashion their *memento mori*, their reminders of death. These gruesome effigies of decomposing bodies are still to be seen in many cathedrals. Their purpose was to remind their owners constantly of their mortality. In discussing the visual-symbol system, it was seen that reintegration made a vital contribution to its effectiveness. The mnemonist would set himself to visualize, say, a candle and find himself visualizing this candle along with, say, Big Ben. Without such reintegration, his mnemonic system would be worthless.

FINAL COMMENTS

Examples of all the major mnemonic systems and devices have, the writer hopes, now been given. And an attempt has been made to relate them to some of the normal intellectual processes which we all share in common. The question remains: of what use are they in everyday living? This question can only be answered tentatively because we vary so much from one another in the store we set by literal recall. In general terms, however, it

could be said that mnemonic systems are of little or no practical worth. These systems provide a technique for nothing more than the rapid memorization of long sequences of unrelated items. And the small need which most of us have for such memorizing in real life situations does not justify the effort involved in acquiring a system. Nor can the use of systems be defended on the ground that it is a useful intellectual exercise which will assist memorizing in other situations where the system is not employed. This notion of general 'memory training' is a myth which has carried over from the days of faculty psychology and phrenology. It is, for example, a gross oversimplification to claim that we can learn better what we read in a text-book by dint of practice memorizing with lengths of poetry or the names painted above the shops which we pass in the street. Mnemonic devices, on the other hand, do appear to be useful on occasion, as witness the frequency with which they are employed. The fact that they are sometimes ineffective is no argument to the contrary. It is true that we sometimes attempt to memorize something by means of a mnemonic device only to find that we can recall the device and nothing else. But if the device has not helped, it need not necessarily have hindered. The chances are that if we did not learn with the device we would not, without greater effort, have learned without it anyway. These instances of failure seem to be more than offset by the successes, and most of us possess items of knowledge which we would never have remembered without the aid of some mnemonic device. The device, then, can fairly claim to play a useful, if minor, role in memorizing. It would be surprising if it did not, for the methods it employs are coextensive with those employed in 'unaided' learning.

If the device has only limited usefulness and the system virtually none, it might be asked: how can we improve our memorizing abilities? Are there no rules? To this it can be answered that there do exist rules and that a substantial number of psychologists have, for a generation past, been engaged in evolving them. But these rules are not simple and the extent to which any one applies can vary markedly from one learning

situation to another. They cannot be summarized in a few words and handed out as a recipe. It would be unrealistic of us to ask the psychologist for a simple answer to all the problems of learning. It would be as unrealistic as asking, say, the physician to give a simple remedy for all bodily ills. In many situations, it is true, our teachers can simplify our learning task. They can tell us what to look for, they can organize the material for us and, in various ways, arrange more effectively the conditions under which we learn. But in the ordinary pursuits of life, little assistance of this sort can be expected from others. It is only through the individual's own successes and failures that he can acquire techniques of observation and organization. To be told what good memorizers do is of small help to us in developing our own memorizing skills. Just as, in the last analysis, we can only learn to drive a car by sitting at the wheel and trying to drive, so our intellectual skills can only be elaborated by our own intellectual efforts. As was once irreverently remarked about a famous American women's college: you can lead a girl to Vassar but you can't make her think.

That skill in learning can be vastly improved should be obvious from a glance at ourselves and our fellows. Humans and lower mammals alike can learn how to learn. But it should be equally obvious that practice and skilful teaching are not the only factors at work. Inherited potentialities, however flexible they may be, impose their limitations. It would be difficult to account otherwise for the achievements of such men as Lord Macaulay. This historian was a voracious reader and, it is reported, could remember everything he had ever read. Exaggerated though this claim may be, it is true that he could recall pages and pages of hundreds of volumes as a result of having read them only once. Now, it seems most unlikely that many of us could emulate Macaulay's performances. His feats of memory are different from those of most mnemonists and memory-men in that they are not the fruits of practice alone but also appear to depend on a native capacity for rapid and permanent learning which is out of the ordinary. The mnemonists, with their practised systems, provide one category of memory

feat. The memory-men, with their years of specialized study, provide another. And geniuses such as Macaulay furnish yet a third category about which the psychologist is able to say little. What it is that distinguishes the genius from the ordinary man or the idiot is something which, so far, has defied explanation. There is little that can be done except to point to the differences and say that they exist.

FURTHER READING

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TESTING SIGNIFICANCE AND MAKING DECISIONS

J. A. NELDER

HARDLY a scientific paper with quantitative data in it appears today that does not contain statements such as 'The differences are not significant', 'the effect of treatment A is significant at the five per cent level', or 'the linear effect of nitrogen is highly significant', and so on. The layman may be pardoned for recoiling at the jargon, and frequently he has good reason for bafflement. What he is witnessing is part of the intrusion into almost every field of science of the significance test, a statistical technique which, broadly speaking, is used in assessing the validity of experimental data. Like any other technique, it is capable of being useful or 'abuseful', and I shall try to assess in this article how far the significance test meets the demands of the scientist (and his readers) in analysing his results, and to outline a more general theory, that of decision functions, which goes beyond that of significance tests.

Significance tests arise from the existence of random variation, and random variation comes into experimental work from the inability of experimenters to measure exactly what they want to. This is not to cast a slur on experimenters for not being able to achieve perfection, but simply to say that their control over their material cannot be complete. This is obvious in the biological sciences; for example, if you are investigating questions of growth in plants, your plants will be individually variable unless you can guarantee that the constituents of the different seeds are identical, and that the environment that they grow up in is identical. Of course you cannot do this, and your plants will show differences even if treated alike (as far as you can manage). Even if you are growing bacteria, where the environment can sometimes be very closely controlled and colonies formed whose ancestry is identical, in that they are all descended from the same cell, 'mutations' occur, apparently spontaneously, and alter the response of some cells to their environment. In the physical sciences unwanted variation comes more frequently from inaccuracy of instruments, although this occurs also in biology; and at the atomic level we seem to be in the presence of a more fundamental kind of uncertainty related to the fact that we cannot observe anything without acting on what we observe, and, further, acting on it in an undiscoverable fashion. Refinements in technique often make it possible for an experimenter to measure something more accurately, i.e. to reduce experimental variation, but he can never eliminate it entirely.

It is convenient to consider experimental variation under two headings, systematic error and random error. Systematic error or bias does not average out as the number of readings taken is increased; random error does. Systematic error is relatively easy to eliminate in comparative experiments, that is, where only the difference between two experimental treatments is required to be estimated: where absolute values, such as, for instance, the mass of an electron, the velocity of light, or the gravitational constant, are to be determined, the elimination of systematic error may be far from easy. Significance tests arise chiefly in comparative experiments and we shall be concerned with these in what follows.

William of Ockham's dictum 'Entities must not be multiplied without necessity' is a basic working rule for scientists. One form of it may be stated: 'Where two hypotheses explain the same set of data, choose the simpler'. This is admittedly rather crude, because there is no rigorous definition of the simplicity of an hypothesis. Jeffreys' statistical version runs: 'All variation must be taken as random until there is positive evidence to the contrary'. That some rule of this kind is essential for the experimenter may be seen from the following considerations. Suppose an experimenter is comparing a new formulation of a drug with a standard one. He will probably do several trials with different batches of subjects and each trial will give him a

measure of the relative efficacy of the two drugs. Now if there is really no difference between the effect of the two formulations, the results will show sometimes formulation *A* more effective than *B*, and sometimes the reverse, depending on the luck of the draw in the distribution of the test subjects between the two drugs. The simplicity rule says that the two drugs are to be assumed the same until proved different, and if the difference in a trial is no more than might have happened by chance this is ascribed to random variation. Without the rule the experimenter would have to conclude after each trial that the relative value of the two drugs was first perhaps that *A* was better than *B*, then perhaps *B* better than *A*, and so on according as the trials turned out. In fact he sets up the hypothesis '*A* has the same effect as *B*' as an Aunt Sally and waits for the results of the trials to knock it down or not as the case may be.

LIMITS OF RANDOMNESS

How do we know when the variation has gone beyond the limits of randomness? The answer is simple – we never know for certain. But we can say that more or less probably it has, and this is the basis of a significance test. Suppose I toss a coin 10 times and obtain 3 heads and 7 tails – is this enough to make me abandon the hypothesis that the coin is unbiased? (We assume that the throws can be regarded as independent, i.e. each exerting no effect on succeeding ones.*) The simplicity postulate says that the chance of a head equals that of a tail (i.e. each is $\frac{1}{2}$) until proved otherwise. The 10 throws can give any of 11 possible results, and, on the assumption that the coin is unbiased, the probabilities attached to each can be evaluated from the binomial expansion of $(\frac{1}{2}+\frac{1}{2})^{10}$. These probabilities are shown in Table 1.

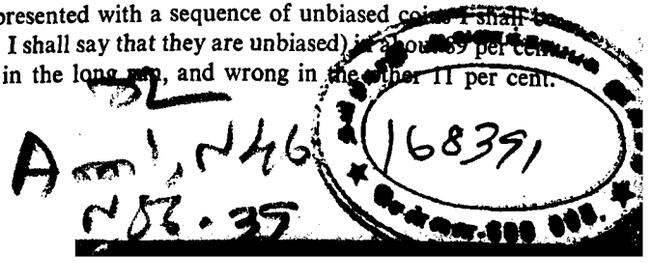
*The reversion to coin-tossing as an example is now traditional. It has the great advantage of simplicity in stating the problem, but the drug formulation problem can be carried on by the reader in an analogous fashion. An unbiased coin corresponds to a drug that is 50 per cent effective on the average, and so on.

TABLE 1: *Probabilities in tossing an unbiased coin 10 times*

Result heads	Result tails	% probability (approx.)
0	10	0.1
1	9	1.0
2	8	4.4
3	7	11.7
4	6	20.5
5	5	24.6
6	4	20.5
7	3	11.7
8	2	4.4
9	1	1.0
10	0	0.1
		100.0

We see that 5 heads and 5 tails (5, 5) is the commonest result, but that (4, 6) and (6, 4) are almost as probable. Furthermore about 89 per cent of the probability belongs to those results with a number of heads (or tails) between 3 and 7 inclusive. Thus, if the coin is unbiased, there is an 89 per cent probability that the number of heads (or tails) will lie between 3 and 7 inclusive. So if I make it a rule to accept any coin as unbiased if I get between 3 and 7 heads inclusive in 10 throws, I shall be right in 89 per cent of cases in the long run *when the coin is really unbiased.* If I get the extreme values of 0, 1, or 2 heads I reject the hypothesis of unbiasedness and say that the probability of getting a head 'is significantly less than $\frac{1}{2}$ at the 11 (=100 – 89) per cent level', and if I get 8, 9, or 10 heads I say that it is correspondingly greater than $\frac{1}{2}$.

To see how effective this rule is, let us suppose that I am given a number of coins, allowed to toss each 10 times, and then obliged to decide whether the coin is biased or not biased. If I am presented with a sequence of unbiased coins I shall be right (i.e. I shall say that they are unbiased) in about 89 per cent of cases in the long run, and wrong in the other 11 per cent.



Saying that a coin is biased, when really it is not, is called 'an error of the first kind', and my procedure makes them with a frequency of 11 per cent. Now suppose I am given a set of biased coins to toss where the chance of a head is 7/10 instead of 5/10. The probabilities corresponding to Table I are now (approximately) as in Table II.

TABLE II: *Probabilities in tossing 10 times a coin biased 7 to 3 in favour of heads*

Result heads	Result tails	% probability (approx.)
0	10	0.0
1	9	0.0
2	8	0.1
3	7	0.9
4	6	3.7
5	5	10.3
6	4	20.0
7	3	26.7
8	2	23.4
9	1	12.1
10	0	2.8
		100.0

We see that the probability of getting between 3 and 7 heads inclusive is about 61.6 per cent. This means that with my rules I shall say that 61.6 per cent of the biased set is unbiased, and even that 0.2 per cent are biased towards tails instead of heads. I shall be right about the biasedness in the remaining 38.2 per cent of cases. Saying that a coin is not biased towards heads, when it really is, is called 'an error of the second kind'.

The most obvious characteristic of Table II is the relative inefficiency of the procedure in picking out biased coins. A similar procedure with the two drug formulations would lead us to be relatively effective in discarding formulations that were no improvement on the standard, but relatively ineffective in

picking formulations considerably better than the standard. The idea of throwing out a 'winner' so frequently is not likely to appeal to the chemist developing the new formulation; how, then, can this be avoided? One way would be to restrict the range of values allowed for unbiasedness say to 4, 5, or 6 heads. In this way the chance of an error of the second kind is reduced from 61.6 to 34.0 per cent. But the price is an increase of the chance of an error of the first kind from 11 to 34 per cent. If the price is considered too high, there is only one way out, and that is to do more experiments. For example, if an unbiased coin is tossed 100 times instead of 10, the number of heads obtained lies between 43 and 58 (or 42 and 57) with a probability of about 89 per cent. This is a span of only 15 in 100, as compared with 4 in 10 when 10 throws were used. Furthermore the chance of getting a score as low as 58 with a biased, 0.7-probability coin is less than 1 in 200. Thus with 100 throws per coin there is very little chance that I shall fail to detect the biased coin at the same level of significance (11 per cent). This example shows that if the significance test procedure is to be useful, the chance of detecting a difference big enough to interest the experimenter must not be too small, otherwise the phrase '*A* is not significantly different from *B*' means, not 'it is unlikely that *A* and *B* differ by an amount of any importance', but rather 'there are not enough data to decide whether *A* and *B* differ by an important amount'.

This ambiguity in the expression '*A* is not significantly different from *B*' is important because it is at the root of the doubts of the usefulness of significance tests quite often found among scientists. For the significance test as often presented is essentially a two-decision procedure; we decide *A* is or is not different from *B*. Now this, of course, is not the way a scientist works. A much closer approximation would be to say that after an experiment he may decide that *A* is greater than B, *A* is less than *B*, or he may suspend judgement and do another experiment. He stops experimenting when it is very probable either that *A* is greater than *B*, *A* is less than *B*, or the difference between *A* and *B* is zero or too small to be important. During the second World

War a type of sampling, 'sequential sampling', was developed which made use of this 3-decision technique in controlling the output of production lines. It is particularly suited to the sort of situation where further observations can be quite easily obtained at each stage as required. A scientist working with crops, however, who can only get one set of data a year as a rule, would not find it so immediately applicable. But any significance test can make room for the suspended judgement if the probability of errors of the second kind is considered as well as errors of the first kind. However, even this 3-decision procedure leaves us with a great deal of arbitrariness in the particular test we choose. How do we decide what difference it is important to detect, and what probability we should choose for errors of the first kind (promoting a dud) or errors of the second kind (ignoring a winner)? There is a theory to cover these questions, but before describing it I had better say straight away that it is of precious little practical value at the moment in any possible application to everyday experimenting. The theory, that of 'decision functions', is due to Abraham Wald. In this theory we first establish 'loss functions'; these represent the loss we shall incur from wrong decisions, that is the loss from deciding that a treatment difference is x, when in fact it is y. For any given actual treatment difference we can assess the probability of making an error of any size in determining that difference and the loss consequent on making such an error. Weighing all these losses with their appropriate probabilities and taking into account the cost of experimenting we can derive a 'risk function' for the detection of our treatment difference, with the particular decision procedure we are using. If we further knew the probability that the treatment difference lay in different ranges (the 'prior probability distribution' of the unknown difference) we could average our risk functions to give a total risk. The problem then is to choose a decision function or method of experimenting in such a way as to minimize the risk, and the solution of this would fix the amount of experimentation, how it should be done, the significance levels of tests, and so on.

The most obvious snag in all this is that the prior probability

distribution is quite unknown to us; we simply do not know beforehand the chance that our new drug formulation is so much better than the original. We may have vague notions that it is probably not much worse than the original, may be slightly better, is unlikely to be phenomenally better, and so on, but these 'hunches' are impossible to put into figures. We cannot, therefore, work out the total risk. Wald's suggestion for overcoming this difficulty (which he did not regard as necessarily the final answer) was to work out the prior distribution giving the maximum total risk and to act in such a way as to minimize the maximum risk. This is the so-called minimax procedure. This idea has been criticized as assuming that a universal law of cussedness is in operation, with nature always being as awkward as possible to the experimenter. The experimenter thus assumes the worst, i.e. that the risk is a maximum, and tries to minimize that. The procedure is, therefore, extremely conservative, but whether any better will come to light remains to be seen.

A further snag, which will probably have occurred to the reader, concerns the loss functions: can we evaluate the loss resulting from a wrong decision, and, more fundamentally, can this loss necessarily be expressed as a figure at all? No general answer can be given; obviously the loss must often be very difficult to assess. Sometimes it may be sufficient to know the shape of the loss curve without knowing exact values for it. Occasionally, in gambling problems for example, the loss function is well defined. These difficulties in Wald's theory are very real, but it would be a rash person who would say that decision functions will never play a part in the experimenter's technique. Even when loss functions and prior probability distributions are not known, the general background of the theory may give us a guide to the significance levels we should use. The 5 per cent level of significance is rapidly becoming hallowed by tradition for no very obvious reason, yet it is not very difficult to find a situation when it seems inappropriate. Consider the situation of a plant-breeder testing a large number of new strains against an established one in the course of developing a new variety. An improvement of 10 per cent in yield may be very desirable,

depth.... The conditions were ideal for the study of the sand concert, and the first item was sufficiently prolonged – it lasted perhaps about four minutes – for me to recover from my surprise and take in every detail. The men working at the well started a rival and less musical concert of ribaldry directed at the Jinns (desert spirits) who were supposed to be responsible for the occurrence.... I realized that the key to the situation was Sa'dan, seated on the top of the slope. It was evident that the music was being engendered by the sand sliding down the steep slope from under him.

Philby followed Sa'dan's example and found that he, too, was able to produce the same sound by setting masses of sand in motion down the side. The noise commenced with a grating sound and increased gradually to a musical booming, which just as gradually decreased and died away. He experimented by pushing a bottle into the singing sand, and as he withdrew it there followed a wail like that of a trombone. At another time he plunged into the moving mass of sand half-way down the slope, and it appeared to throb beneath him like a great organ.

These singing sands of southern Arabia have become known to science only during the present century. But the phenomenon was known to the Chinese at least a thousand years ago. One of their writers left an account of an area in the province of Kansu where it had been noted in the ninth century. He described the 'Hill of Sounding Sand', which was 500 ft high in places and possessed strange qualities: 'Its peaks taper up to a point, and between them there is a mysterious hole which the sand has not been able to cover up.' The writer said that in the height of summer this hill of sand gave out sounds of itself, but if trodden by men or horses, the notes could be heard for long distances. And he described a custom which was followed to induce the singing:

It is customary on the tuanwa day [the Dragon Festival on the fifth of the fifth moon] for men and women from the city to clamber up to some of the highest points and rush down again in a body, which causes the sand to give forth a rumbling noise like thunder. Yet when you come to look at it the next morning the hill is found to be just as steep as before. The ancients called this the Hill of Sounding Sand; they defied the sand and worshipped there.

Miss F. French and Miss M. Cable, missionaries in inland China, also recorded their observations of the phenomenon in Chinese Turkestan. The City of Sands (Tunwang) takes its name from the ranges of sand dunes that lie to the south, stretching out into the great desert of Lob. These sand hills possess the property of 'singing' when the sand is moved. Before a desert gale blows, a sound like the rattle of drums is heard, but at any time the hills can be made to 'sing' by climbing to the knife-like edge of the highest point and sliding down. The great vibration then produced seemed to spring from the very centre of the dune.

Tschiffely, the 'iron Swiss' who rode on horseback from Buenos Aires to Washington, records an experience on the Peruvian coast. He fell asleep one night on a sand hill, but was awakened several times by a noise like the beating of drums or like a motor launch travelling on a river. As he could see nothing, he went to sleep again. The next morning he noticed that he had been sleeping near a 'gentilar', as the ancient Indian burial grounds are called. He was asked by the natives if he had heard the 'manchang'. This sounded rather like Chinese to Tschiffely, and he asked them what it meant. They explained that the sand hill was haunted and that every night the dead Indians of the 'gentilar' danced to the beating of drums. In fact, they told him so many blood-curdling stories about the hill that he began to consider himself lucky to be alive. He heard later that both Baron von Humboldt and Raimondi had expressed the opinion that the sounds heard so often during the night from this hill were due to underground waters that moved as the temperature changed. Another theory put forward is that sea breezes blowing from a certain direction hit the sandy ripples on the slopes of the hill to produce this strange sound.

Although it is to the deserts that one must turn to hear the finest exhibitions produced by singing sands, a somewhat similar phenomenon has been reported from beach sands. An example, unique along the west coast of Scotland, is afforded by the singing sands of the Bay of Laig on the little island of Eigg in the Hebrides. The first such discovery in England appears to have

been made by C. Carus-Wilson, who about 60 years ago found singing sands at Studland Bay on the coast of Dorset. They have been recorded also on the coast of North Wales; and in the United States two observers have reported them at seventy-four places on the Atlantic Coast alone.

Beach sands 'sing' differently from desert dunes. The beach sands are best described as 'whistling' or 'squeaking', according to R. A. Bagnold, who summarizes existing knowledge in the final chapter of his book *The Physics of Blown Sand and Desert Dunes*. The squeak or whistle is produced, he explains, by any rapid disturbance of the dry top layer, especially just above the high water level when the sand has recently dried out after a shower. It is produced when the palm of the hand is swept across it quickly or when the sand is given a light stab with the end of a pencil. When the sand is removed from the beach, it does not long retain its sound-producing quality. Grains of singing beach sand examined were rounded, but not markedly so, and were fairly uniform in size.

In contrast with the whistling of the beach sands, 'the great sound which in some places startles the silence of the desert' is quite a different noise, according to Bagnold:

> I have heard it in south-western Egypt 300 miles from the nearest habitation. On two occasions it happened on a still night, suddenly – a vibrant booming so loud that I had to shout to be heard by my companion. Soon other sources, set going by the disturbance, joined their music to the first, with so close a note that a slow beat was clearly recognized. This weird chorus went on for more than five minutes continuously.... Native tales have woven it into fantasy; sometimes it is the song of sirens who lure travellers to a waterless doom; sometimes it is said to come upwards from bells still tolling underground in a sand-engulfed monastery....

The sounds produced by desert dunes certainly vary; travellers have compared them to a ship's siren, a throbbing organ, the beat of drums, a trombone, and the twanging of a monster harp. In some instances, it seems that the softer tones are missing; others say that standing on the sand when it is singing is like resting on a huge stringed instrument while a bow is being drawn

slowly across it. The note emitted by the desert sand is much lower than that of the beach sand and at a distance of 600 yards has been likened to the rumble of thunder.

Writing from Egypt some few years ago, Lieutenant-Colonel de Lancey Forth spoke of the following experience in the great sand-dune country to the south of Siwa:

> I found, after a strong westerly wind had blown throughout the day and had banked the fine drift sand high up on the knife-edged tops of the dunes, that sometimes in the evening, when the wind had died away, leaving a deep stillness in the air, this fine drift sand slid down in streaks over the coarse big-grained red sand which forms the steep slopes of the solid part of the dunes, and the friction of the one rolling over the other gave out a noise like distant rumbling thunder with a deep musical note as that of a 'cello in it.

Forth's reference to the evening is paralleled in Philby's account. When he was listening to the singing in the afternoon, one of his men (referring to the desert spirits) said, 'You wait, just wait till the evening and you will hear them letting off their big guns.'

Bertram Thomas, too, noticed the noise late in the afternoon, when the heat of the day was fading. Apparently, another factor also favours the close of day. During the day the wind blows the fine drift sand to the tops of the dunes, and toward sunset, when the wind usually dies down, it begins to roll down the slopes. Dryness seems essential, for the ancient Chinese manuscript states that the Hill of Sounding Sand gave out noises only at the height of summer; and Philby likewise reported that early in the morning, when the air was cool and the sand somewhat moist, he failed to elicit any response from it. And a few weeks later when there had been a little rain, there was no music in the sands.

Examination of the sand has not revealed any peculiarity linking the whistling sands of the beach with the booming sands of the desert. Samples from the dunes do not reveal any distinguishing features. The grains are no more uniform in size than those of many silent sands; and though clean sand sometimes seems to sing best, Bagnold heard it in a desert region where the

sand was dirtier than usual and was wetted appreciably only once or twice in a decade.

A. D. Lewis found that when singing sand was taken from the Kalahari Desert to Pretoria, it lost its 'voice' unless kept in air-tight containers. But the quality could be restored by heating it to about 200° C.

Physicists are generally agreed that the sounds are caused by the rubbing of grains against each other, but as yet there is no real explanation of the mechanism by which they are produced. When further critical studies are made, the answers may be forthcoming. Meanwhile, when you are in the desert, keep an ear open for one of the strangest concerts ever to come from nature's versatile music box.

RESEARCH REPORT

A. W. HASLETT

PLANT AND ANIMAL VIRUSES

ADVANCES in research on plant and animal viruses have been reported lately from the Virus Laboratory of the University of California. The first, by H. Fraenkel-Conrat and Professor Robley Williams, is the reconstitution of a plant virus from components – protein and nucleic acid – previously separated from it. The virus was that of tobacco mosaic. It causes a mottling of the leaves in infected plants. A similar mottling was produced in leaves inoculated with the reconstituted material, but not by either component separately. The activity found was small, between 0.1 and 1 per cent of that of the natural virus. But plant viruses are notoriously sensitive to chemical treatment, and to take one to pieces and put it together again is something which has not been done before. The knowledge that even one nucleoprotein can be so treated will open up a whole range of new experiments which may go far beyond work on viruses.

The advance in research on animal viruses has been the preparation of one of them, the virus of poliomyelitis, in the form of crystals observable in an ordinary light microscope. This has been achieved by C. E. Schwerdt and F. L. Schaffer. The effect is to bridge the gap between the plant viruses, which appear to be substances, and the larger of the animal viruses which are more like organisms.

Tobacco mosaic virus was introduced to chemical study in 1935, when W. M. Stanley, now director of the California laboratory, prepared it in the form of two-dimensional crystals. (It does not form true three-dimensional crystals, although other plant viruses were soon found to do so.) Two years later, F. C. Bawden and N. W. Pirie, in England, showed that liquid crystal-

line substances isolated from several strains of the virus were chemically nucleoproteins. More recent evidence, obtained by a variety of methods, has suggested a more or less definite structural arrangement. Quoting from Fraenkel-Conrat and Robley Williams: 'It appears that about 2,800 protein sub-units of molecular weight near 18,000 are arranged in a helical manner to form a rod with a hollow core. The nucleic acid is believed to occur as strands in the core.' Compared with the complete virus, which has a molecular weight of about 50 million, these protein sub-units are relatively small; they have been obtained only as breakdown products, which neither show activity as a virus nor have been re-formed into larger units. Larger protein fragments have, however, been obtained. These are of molecular weight about 400,000, and are shaped like doughnuts (Inset 1). Either these particles or fragments of intermediate size, of molecular weight about 100,000, may be units in the reconstituted virus.

Electron micrographs, taken by R. Hart in the same laboratory a year earlier, showed the partial splitting of nucleic acid and protein. This was brought about by warming the virus in detergent* for about a minute. In this way part of the protein was removed, and the nucleic acid exposed. In micrographs of preparations so obtained, strands (taken to be nucleic acid) were seen to pass through the centre of a rod or coat of protein – as it has been said, like the lead through a pencil. In later work by Fraenkel-Conrat, the same method of using a detergent to remove the protein was used to separate the nucleic acid component completely and without evident damage.

Protein, also without evident damage, was separated from the complete virus by longer-established methods. Different preparations of the virus were used from those from which the nucleic acid component was separated.

The virus was reconstituted essentially by mixing. Ten parts of protein solution were mixed with one part of nucleic acid solution (both 1%), and the mixture made slightly acid and kept at a temperature of 3°C for at least 24 hours. This alone restored

*Sodium dedecyl sulphate in mildly alkaline solution.

activity as a virus, although in some preparations further procedures were carried out. Tobacco leaves were then inoculated with amounts ranging from 1 part in 100,000 to 1 part in 10,000 of these combined preparations, and developed mottling in from 2 to 30 places per half leaf. The mottling was indistinguishable in appearance from that caused by the natural virus. On the other hand, neither the protein nor the nucleic acid preparations separately, nor the combined preparation without standing, produced mottling when inoculated. The protein preparation was tested at concentrations up to 180 times as great as the lowest concentration of the reconstituted preparation which showed activity, and the nucleic acid preparation at concentrations up to 5 times as great. That might seem conclusive. But viruses, like other substances of biological activity, may be inhibited by other materials which are present with them. Control experiments were therefore done in which small quantities of intact virus were added to the protein and nucleic acid preparations, and were shown to be still active in their presence. The conclusion was thus supported that the activity found in the main experiments had been produced by combination of the previously separated components.

The remaining question is to account for the smaller activity shown by the reconstituted, compared with the natural, virus. Electron micrographs (Inset 2) show that the reconstituted particles differ in appearance from those of the natural virus only in one respect, that the proportion of short particles is greater. Statistically, only one in ten of the reconstituted particles were as long as 0.3 microns, the smallest length which seems to be necessary if a particle is to be infective. That leaves a substantial gap to be accounted for. The simplest conclusion is that there is a failure at both stages. Not enough particles are long enough. And of those that are long enough, only a minority – probably about 3 per cent – have been 'reconstituted with the structural faithfulness necessary for infectivity'.

The crystallization of the poliomyelitis virus calls for less comment. The individual crystals (Inset 3) are about a thousandth of an inch long and contain nearly a thousand million

virus particles. They were obtained by concentrating a sample of the virus contained in the same tissue-culture fluid (tissue from monkey kidneys) that has been used in the production of poliomyelitis vaccine. In the final stage of crystallization a preparation of virus particles was covered with a weak acid-salt solution and placed in a cold room for 24 hours. About 40 per cent of the initial sample had by then crystallized.

The size of the virus particles is relevant to the fact of crystallization. The poliomyelitis viruses are usually described as spherical particles of 8 to 12 millimicrons diameter. They are among the smallest of the animal viruses, that of foot-and-mouth disease being about the same size. For comparison, the particles of tomato bushy stunt virus – the first virus to be isolated as three-dimensional crystals – are about 26 millimicrons in diameter. It is thus a reasonable expectation that, if the plant viruses are essentially self-replicating substances, devoid of further organization, so also may be the smaller of the animal viruses. This is not so in the case of some that are larger. The dimensions of particles of influenza A virus, for example, are about ten times greater. Characteristics derived from different strains of this virus have been found by Professor F. M. Burnet and his colleagues to recombine,* much as do genetically inherited characters. The particles of vaccinia virus are about twice as big again, and electron micrographs (Inset 4) published by I. M. Dawson and A. S. McFarlane have given clear evidence of local structure. Although some of the smaller animal viruses may well be as much substances as are the plant viruses, this is not true of animal viruses in general. A more speculative conclusion is that the methods of chemical reconstitution which have been applied to tobacco mosaic virus may be applicable, not only to other plant viruses, but also to the smaller animal viruses which can be crystallized.

Fraenkel-Conrat, H., and Williams, Robley C. *Proceedings of the National Academy of Sciences of the United States of America*, **41**, 690 (1955).

Schwerdt, C. E., and Schaffer, F. L. *Ibid.*, in the press.

*See, for example, *Science News 21*, 114 (L951).

THE ANTI-PROTON

The expectation that for every type of charged elementary particle there should exist another, identical in mass, but opposite in charge, has been well fulfilled. It originated in the theory of the electron which was put forward by P. A. M. Dirac in 1928 and confirmed four years later by the discovery of the positive electron. It has been further strengthened by successive discoveries of charged meson particles, of which both positive and negative particles have been found whenever sufficient evidence has been available. The anti-proton, or negative proton, has remained elusive none the less, in spite of tentative reports by cosmic ray physicists. The reason is the relatively large amount of energy required for the creation of negative and positive protons in pairs, the small probability of observing the results of suitable collisions by chance in the earth's atmosphere, and the difficulty of establishing the nature of individual nuclear particles when the energy of the originating particle is not known. The creation of anti-protons under controlled conditions in the laboratory has now been confirmed by Dr E. O. Lawrence and his colleagues of the University of California Radiation Laboratory. Their success fulfils one of the main hopes behind the design of the laboratory's 'bevatron' accelerator, the proton synchroton whose successful operation was recorded in *Science News 32*.* At that time pulses of protons had been accelerated to an energy of 4,700 million electron-volts (MeV). In the experiments now reported, the 'bevatron' was operated at 6,200 MeV – practically its full design energy.

Under these conditions, negative and positive protons are formed in pairs, in the proportion of one pair to every collision leading to meson particles, when the proton beam falls on a copper target. A total of 40 anti-protons had been found when first reported. Their negative charge was confirmed by the direction of their deflection in a magnetic field. Their mass was found to within 5 per cent by measurements of the momentum and

*See also the article by L. L. Green on 'The Acceleration of Charged Particles to High Energies' in *Science News 22*.

velocity of the particles. Momentum was obtained from the curvature of their paths; velocity by two methods, one of them a time-of-flight measurement between counters 40 feet apart. Within the limits of accuracy stated, the mass of the anti-proton was confirmed to be the same as that of the proton. They are annihilated, as expected, in collisions with matter (i.e., with ordinary protons). But whereas the energy released in the annihilation of positive and negative electrons is in the form of radiation, the collision of an anti-proton and proton leads to the formation of positive and negative mesons. The identification of these particles – not yet further specified – will be awaited with interest; although the anti-proton had been predicted, the manner of its annihilation had not been.

INTERSTELLAR GAS

The fact that radio waves of about 21 centimetres wavelength are emitted by the hydrogen atoms present in interstellar gas was mentioned by F. D. Kahn in his article in *Science News 34*. He recorded also some of the conclusions that have been drawn from measurements made with radio-telescopes working on this wavelength. In this way, for example, it has been shown that the interstellar gas has a temperature of about $-148°$ C; that it moves, in general, round the centre of the Galaxy; and that its average density is of the order of 10^{-24} gramme per cubic centimetre. Theory shows that a radio spectral line should be associated also with atoms of heavy hydrogen, or deuterium. But atoms of deuterium are much more rare than are atoms of ordinary hydrogen, and the deuterium radiation is also more difficult to detect because of its longer wavelength – about 91.6 centimetres. However, in a paper which has been published lately in the *Doklady* (or *Comptes rendus*) of the Academy of Sciences of the U.S.S.R.,* G. Getmantsev, K. Stankevich, and V. Troitskii have announced the detection of monochromatic radio-emission from unionized atoms of deuterium in the direction of the centre of the Galaxy. Preliminary calculations had

*In the number of 11 August 1955, received in Britain November 1955.

shown that the intensity of this radio-emission must be well below the threshold for direct detection by even the most sensitive of ultra-high-frequency receivers. But it was believed that the deuterium radiation might be detected indirectly, by the mathematical analysis of recordings of signals picked up. The receiver used was of the kind which is tuned at a constant rate through the frequency of the signal expected, and in which signals separated by a constant difference of frequency (40 kilocycles per second in this case) are repeatedly compared. It was used in conjunction with a radio-telescope of 13-feet diameter. When 13 such recordings were combined, evidence of a radio spectral line was obtained within a few kilocycles per second of the expected frequency. The discrepancy was within the estimated limits of error. As had been predicted, this deuterium line was in absorption. The width of the line was about 30 kilocycles per second – which, again, was roughly as expected. Finally, the deuterium line was found when the radio telescope was directed towards the centre of the Galaxy, where there is most material, but not in the direction of the Galactic Pole, where the amount of interstellar gas is much less. The strength of the deuterium absorption line leads to the conclusion that the ratio of deuterium atoms to hydrogen atoms in the region of the Galactic Centre is of the order 1 : 300. This is a much greater abundance of deuterium than that found on the earth (where the ratio is about 1 : 7,000) or in meteoric dust.

OXYGEN AND ATHLETICS

Although speed in sprinting seems to be limited mainly by the speed at which muscles can contract, the performance of an athlete running a mile is limited by the rate at which oxygen can be got to his muscles. The reason for the distinction is that muscles can operate for a short time on credit, that is without immediate supply of oxygen, but in doing so produce lactic acid, which accumulates in the blood. There is therefore an interest in a series of measurements of rate of oxygen consumption which have been made lately in Sweden on a number of world-class milers and skiers. They suggest that the limit of perform-

ance in running a mile may still be quite a long way from having been reached.

The measurements were made by Dr P.-O. Åstrand of the Department of Physiology, the Royal Central Gymnastic Institute, Stockholm. His subjects included three runners (John Landy, who at the time of writing holds the world record for the mile, and, at the time of testing, held also the world record for 1,500 metres; and two Swedish runners, whose best performances over 1,500 metres were within 2 to 4 seconds of Landy's). The skiers were six members of the Swedish national ski team, five of them men and the sixth a woman. Measurements were also made on 48 normal but well-trained men students, and on 44 normal but well-trained women students. Although there have been earlier measurements with the object of comparing athletes and others, the present series appears to be the first which enables reasonable comparisons to be made between athletes. It was also remarkable for the number of new world records – in terms of oxygen consumption – which were established.

The measurements were made with a Douglas bag, which implies having a clip over the nostrils and breathing through a valve so connected that the normal surrounding air is breathed in while the air breathed out is collected. It should perhaps therefore be recorded at once that both the three runners and the first three members of the ski-team were placed in each case in the same order as that of their best athletic performances. Measurements on the runners were made during the last minute of a 6-minute run at a speed of 20 kilometres an hour on a treadmill set at an angle of 1°. The skiers were given a preliminary warming up, skied for 3 minutes at almost maximum speed on the level, stopped to change over from natural breathing to breathing through the valve, and were then measured during the second of two further minutes while they were ski-ing uphill.

The previous 'record' was established in 1937 by D. R. Lash, an American two-miler who at the time held the world record for that distance. He was measured while running at a slightly faster rate (21.6 kilometres per hour) but without gradient, and

attained an oxygen intake of 5.35 litres per minute, or of 81.5 millilitres per kilogramme of body-weight per minute. In terms of crude rate of oxygen consumption, each of the first three skiers had higher figures, the highest being 5.88 litres per minute; in terms of oxygen consumption per kilogramme weight, the first was slightly above and the second slightly below the figures recorded for Lash. But the most remarkable fact about the measurements was that Landy's rate of oxygen consumption, although higher than that of the Swedish runners, was well below the rates either of the earlier American runner or any of the five Swedish male skiers. The conclusion is thus suggested that Landy's performances as a runner, compared with those of earlier generations, owe more to methods of training and running a mile than to inherent physical advantage. Compared with either Lash or the best skier, the difference on a body-weight basis was about 6 per cent.

The average on a body-weight basis for all the men athletes was higher by some 30 per cent than the average of the men students. The rate attained by the woman skier was 7 per cent less than the average of the men athletes, but 17 per cent more than that of the men students and 40 per cent more than that of the women students. Other measurements were made of vital capacity, maximum heart-rate, and other variables, but rate of oxygen consumption showed by far the best correlation with athletic performance.

Åstrand, P.-O. *Nature*, **176**, 922 (1955).

ATOMIC ENERGY

Some problems of the second stage of development of atomic energy were discussed by Sir John Cockcroft, Director of the Atomic Energy Research Establishment at the inaugural meeting of the British Nuclear Energy Conference, a professional organization formed by five bodies representative of engineers and scientists. He said that, whereas in the United States it was practical to build each of the more promising types of reactors, the present feeling was that only one type of stage-two reactor

could be built in Great Britain. He indicated that the possibilities of two types in particular were at present being considered.

One of these is the pressurized water reactor, in which the water which removes the heat is retained in the liquid state in the reactor. Representatives of the Central Electricity Authority and of two industrial groups are now helping at Harwell with a preliminary design study of this type. As an indication of what this meant, Sir John stated that a design study required a team of 15 engineers and draughtsmen, a reactor physics group to build sub-critical or zero-energy assemblies, and chemists and metallurgists to tackle the chemical and fuel-element problems which arose. In a pressurized water reactor, the uranium of the fuel elements would probably be in the form of uranium oxide for the sake of the resistance which it offered to water. But if ordinary or light water was used to moderate, or slow up, the neutrons released in fission, this uranium-oxide fuel would need to be 'spiked' with plutonium produced by stage-one reactors (those substantially of the Calder Hall type), and it was possible that plutonium, too, might be used satisfactorily in the form of its oxide. The second type of stage-two reactor, mentioned specifically by Sir John, was the sodium-cooled, graphite-moderated reactor. Of this type, he said that uranium metal could be used, since it was compatible with liquid sodium, but that prolonged testing of the fuel elements would be necessary. For such purposes, the experimental reactors 'Dido' at Harwell and 'Pluto' at Dounreay would be important. In relation to the third stage of development, he said that the homogeneous reactor – in which the nuclear fuel is circulated in solution – was being studied with the help of chemical industry 'since this reactor will be a highly specialized chemical plant with built-in facilities for the continuous chemical processing of fuel'. In all, 42 representatives of industrial organizations are taking part in preliminary studies at Harwell of different reactors.

A further type of research which affected industry was that on possible uses of radiation, for example in the production of plastics. Some 30 investigations were being done at Harwell on behalf of industry with help from a technological irradiation group which provided intense sources of radiation.

HYDRODYNAMICS RESEARCH

An account has been issued of new equipment for hydrodynamics research which has been installed at the Admiralty Research Laboratory, Teddington. A comparison is drawn between the position in aerodynamics, where for twenty years programmes of basic research in aid of aircraft have been intensively pursued, and that in hydrodynamics, where there has been little basic research in aid of the design of bodies travelling under water. The new laboratories house entirely novel equipment which it is suggested will assist materially 'in investigating the possibilities of making major advances in a comparatively unexplored field'.

The most spectacular item is a 60-ton rotating beam and circular channel, 34 feet wide and 15 feet deep. The beam has a mean working diameter of 100 feet, and models up to 20 feet long are tested at a depth of 5 feet of water. At the mean radius of 50 feet, speeds of up to 150 feet per second (90 knots) can be given to the model. Experiments can be observed and photographed from two underwater observation chambers fitted with large windows of thick glass. The water is specially treated and filtered to assist high-speed photography, and objects at up to 100 feet distance can be clearly seen.

The equivalent of the wind tunnels used in aerodynamics research is the water tunnel. Two of these have been built. The first has a working diameter of 12 inches, and was built largely for experience. The second and larger tunnel has a jet diameter of 30 inches, and speeds of up to 35 knots can be attained in the working section of the tunnel. Powered models of up to 10 inches diameter and 10 feet long, carrying their own recording instruments, can be tested in it. The relatively big diameter of the models has been made possible by using a slotted wall for the working section, which in effect is partially open and partially closed. This takes the form of a cage of 'Perspex' bars which encloses the jet of water within an observation chamber.

In both tunnels refrigeration plant is needed to control the temperature of the water when working near full power. Detailed schemes and layout were worked out by the Royal Naval Scientific Service in conjunction with the Ministry of Works.

Research using the new facilities should make a big contribution to knowledge of hydrodynamics, and the applications of this knowledge should extend beyond the purposes which presumably are mainly in view. It may be hoped, therefore, that room will be found for experiments which can be freely published, and that in planning the use of the laboratories the requirements of hydrodynamics as a science will be borne fully in mind.

SOME BOOKS RECEIVED

THE POLAR AURORA BY CARL STÖRMER (Oxford University Press, International Monographs on Radio, 1955), pp. 403, 55s.

Professor Störmer's book can be regarded either as his own memorial to fifty years' work on aurorae or as an introduction relevant to much research to be undertaken during the International Geophysical Year. Apart from the great amount of observation and theory which is here summarized, he gives clear indications also of gaps in knowledge. There are 34 pages of unusually well reproduced photographs. Connexions with other parts of geophysics are suggested rather than developed.

ELECTRONICS by A. W. Keen (London: Ward, Lock & Co., Ltd., 1955), pp. 256, 25s.

Mr Keen combines a university background with experience of research, development, and teaching as an electronic engineer. In spite of self-confessed difficulties in the ordering of his material, his book is probably the best introduction of its kind to the whole range of electronics from valves, transistors, and photoelectric cells to computors and particle accelerators. His treatment both of theory and devices is descriptive, and the manner such as might help a biologist to cross the gap from elementary electricity to accounts of equipment which might concern him. The addition of four pages of advertisements seems doubtfully worth while.

PRINCIPLES AND APPLICATIONS OF PHYSICS by Otto Blüh in collaboration with J. D. Elder (Edinburgh: Oliver and Boyd, 1955), pp. 866, 45s.

This book is of a kind which could have been written and first published only in Germany or North America; its purpose is to give a broad but lengthy account of physics, including its outgrowths and relations with other disciplines, for university students who may be either non-physicists or physicists in training. The abandonment of traditional sub-divisions is useful. What is substituted for them is

a distinction between the results of the phenomenological approach, and of another which demands '*visualizable* concepts' (author's italic), on which it lays 'particular value' and to them 'ascribes full reality'. To like pictures is human; to base a course or a book on the further qualities claimed for them is to confuse science with a philosophy of science which at least is not generally held.

SYMPOSIUM OF THE SOCIETY FOR EXPERIMENTAL BIOLOGY NO. IX: FIBROUS PROTEINS AND THEIR BIOLOGICAL SIGNIFICANCE (Cambridge University Press, 1955), pp. 370, 50s.

So much work is being done on the fibrous proteins both of connective tissue and, for example, of muscle, flagella, and dividing cell nuclei that coherence is hardly yet to be expected. The general biologist or teacher listening in to this symposium, held in 1954 in Leeds, may hope only to find bewilderment pierced sometimes by excitement. Even the specialist might have liked one of those assessments after the meeting that J. B. S. Haldane, in his own field, has often so usefully given.

SOME ASPECTS OF LIFE IN FRESH WATER by E. J. Popham (London: Heinemann, 1955), pp. 123, 6s.

Published in the scholarship series in biology, this clearly written introduction to the principles of freshwater biology is based about equally on habitat and the sources of organisms; food chains and ecology are illustrated from a Lancashire pond in the hope that further field work will be encouraged.

A HISTORY OF THE WARFARE OF SCIENCE WITH THEOLOGY IN CHRISTENDOM by Andrew D. White (London: Arco Publishers, Ltd., 1955), pp. 474, 35s.

Dr White's book was completed in 1895. He had been the first president of Cornell University, and had represented his country abroad. He wrote within ten years of Gladstone's intervention in the Huxley-Wilberforce controversy. For White, the issue, still live, was between science and 'dogmatic theology'. Now his own book, published sixty years later in England, appears in turn primarily as an historical document.

ABOUT OUR CONTRIBUTORS

G. H. A. COLE graduated in physics at University College London in 1949, and in 1952 was awarded a Ph.D. in theoretical physics for work on the kinetic theory of liquids. After two years at the Admiralty Research Laboratory, Teddington, he returned to University College in 1954 with an I.C.I. Fellowship, and is now doing research on magnetohydrodynamics with applications to the upper atmospheres of the earth and sun. He acts also as consultant to the Admiralty on theoretical problems.

C. H. L. GOODMAN was born in 1927 and educated at Harrow and Oxford. After taking a degree in chemistry, he did research on the influence of impurities on the recrystallization of alumina at high temperatures. From Oxford, he went to the Research Laboratories of The General Electric Company Limited, Wembley, where, for the past six years he has been searching for and studying new semiconducting compounds.

IAN M. L. HUNTER, born 1927, entered psychology through an interest in animal behaviour and physiology, and, in 1950, graduated from Edinburgh with a first-class honours degree in science. At Oxford, he worked on visual discrimination in the rat, but soon found himself investigating similar behaviour in young children. Since then his interests have embraced the study of progressively more complex and characteristically human activity. On obtaining his D.Phil. in 1953, he returned to Edinburgh University as Lecturer in Psychology. His researches centre round experiments on perceiving, learning, and thinking processes as they occur in normal and abnormal humans, in adults and children, and in a few animal species. Reports of his investigations have been published in Britain and the United States.

BERNARD MAYO was educated at King Edward VII School, Sheffield, and Magdalen College, Oxford, where he graduated in Literae Humaniores. During the war served in Royal Signals. Aged 35, he has been a Lecturer in Philosophy in Birmingham University since 1947. He is the author of papers on various philosophical subjects in *Proceedings of the Aristotelian Society* and elsewhere, and has published a book, *Logic of Personality*.

J. A. NELDER read mathematics at Cambridge, afterwards obtaining a diploma in Mathematical Statistics. Aged 31, he is now statis-

tician to the National Vegetable Research Station at Wellesbourne, Warwick. He wrote in *Science News 24* on 'Taking a Sample'.

P. REASBECK is a Research Fellow in the Physics Department, University of Birmingham. After war service as an R.A.F. pilot, he returned to Durham University to take B.Sc. and Ph.D. degrees in chemistry. Did research in the Londonderry Laboratory for Radio-chemistry on the age of meteorites, and on the chemical composition of the upper atmosphere in conjunction with the U.S. Army rocket programme. Is now engaged on radio-chemical studies of nuclear disintegrations produced in the Birmingham synchrotron and on the cyclotron-production of radio-isotopes for medical and physical research.

E. R. YARHAM was educated at the City of Norwich School and at Borough Road College, London. Fellow of the Royal Geographical Society. Schoolmaster by profession; headmaster of a school in Norfolk. After the war, at the invitation of the American Council for Educational Research, went to the United States for an extensive tour of that country for educational and geographical research. Has broadcast and written on geographical subjects for leading periodicals in Britain and overseas.

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