The Scientist Speculates

AN ANTHOLOGY OF PARTLY-BAKED IDEAS

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Preface

The intention of this anthology is to raise more questions than it answers.

I. J. Good

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Abbreviations

pbi: partly-baked idea, or *p*-baked idea, where *p* is the 'bakedness'

G.Ed.: General Editor

A.Ed.: Associate Editor

B.Ed.: Biology Editor

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Ideas About Ideas

'The formulation of a problem is often more essential than its solution, which may be merely a matter of mathematical or experimental skill. To raise new questions, new possibilities, to regard old problems from a new angle, requires creative imagination and marks real advance in science.'

EINSTEIN AND INFELD, *The Evolution of Physics.*

1 pbis

by I. J. GOOD

A partly-baked idea or pbi is either a speculation, a question of some novelty, a suggestion for a novel experiment, a stimulating analogy, or (rarely) a classification. It has a bakedness p that is less than unity, or even negative. The bakedness of an idea should be judged by its potential value, the chance that it can be completely baked, its originality, interest, stimulation, conciseness, lucidity and liveliness. It is often better to be stimulating and wrong than boring and right.

A very rough guide to the maximum length that a pbi should have is given by the formula

$$10^{9px/2} \text{ words}$$

where x, the importance of the topic, is between 0 and 1. For example, the maximum length for a negatively-baked idea is less than one word. An idea can compensate in importance what it lacks in bakedness, and conversely. The formula is applicable to each sentence and to each paragraph, as well as to the whole of a contribution. For the non-specialist, the formula makes sense even when $px = 1$, but in this anthology px rarely exceeds $7/9$.

A possible justification for the exponential or antilogarithmic form is that if an idea is developed to a certain depth d, then the size of the expository tree increases roughly exponentially with d, if the multifurcation of the tree is the same at every level.

One may conjecture a similar formula for the distribution of ideas among people. This distribution is certainly very skew, perhaps something like that of the Pareto income distribution. Furthermore, very speculatively, perhaps a proportion

$$10^{-12q}$$

of ideas have $px > q$; and a proportion

$$10^{-10q}$$

of people inaugurate them.

A suggestion for a periodical *Half-Baked Ideas* was published in 1958.* As a consequence Mr Alan J. Mayne offered his services as an Associate Editor. Professor Marvin L. Minsky, in a discussion with Mr Mayne, suggested that a book would be easier to organise in the first place. It was not until late 1960 that we found a publisher with enough initiative to agree to a contract, and during 1961 all the papers were received, refereed, and edited.

Some of the papers contain ideas in more than one domain of knowledge, so that a strictly logical sequence for the papers was not possible, and the use of the index is recommended. Some of the most technical papers are collected at the end of the book, irrespective of topic. The months of receipt are given in an Appendix.

Two objections have been raised against the publication of

* *IBM J. Res. Dev.*: volume 2, pages 282–8, 1958.

partly-baked ideas, both concerned with the murky matter of credit assignment:

(i) The man who publishes might get all the credit at the expense of the more diligent later worker who develops the idea, perhaps even in ignorance of the earlier publication.

(ii) The man who develops the idea might get all the credit at the expense of the more imaginative man who altruistically gave the idea away by publishing it.

I shall not take refuge in the glib answer that credit does not matter. Since we are not all saints, *our own* credit does matter to most of us, like our own money and our own freedom. I would rather reply that mistakes in the assignment of credit can be made for many different reasons, one of them being that other people's credit might seem unimportant. The obvious thing to do is to award joint credit when joint credit is due. Fewer mistakes would be made if more attention were paid to the theory of credit-assignment, *kudology* (from the Greek κῦδος, meaning kudos). This subject could be regarded as a branch of economics, and many books could be written about it. It would depend on the theory of probabilistic causality.*

When there is nowhere to publish an idea it is liable to get lost unless its saintly or drunken originator gives it away free in conversation. Thus the lack of a medium of publication acts as a disincentive even to the verbal dissemination of good ideas.

2 A Self-referring Idea

ANON.

Half-baked ideas of people are better than ideas of half-baked people.

* 'A causal calculus', *Brit. J. Phil. Sc.*: volume 11, pages 305–18, 1961; volume 12, pages 43–51, 1961; erratum, 1962.

3 Comment on this Anthology
by J. E. LITTLEWOOD

I expect that what I am about to express is felt by many contributors. I welcome the idea of the anthology; I look forward to reading the contributions of others; and I shall never be intolerant of them. But I felt doubts about committing myself to my own, and it took a certain strength of mind to overrule them.

4 'Ignoratica'
by FÉLIX SERRATOSA

On June 1st, 19.., in the Waldorf Astoria of New York City. Mr Gog received the very funny visit of Dr Horeb Naim, who asked for some financial support in order to create a somewhat peculiar professorship. Dr H. Naim claimed to be the founder and to assume the mastery of a new discipline, 'Ignoratica' – the Science of *unknowns* –, at the University of New Mexico.

The reasons displayed by Dr H. Naim were so convincing, the originality and importance so evident that Mr Gog had no inconvenient to give the necessary financial support required.

Many years have passed over since then, and we feel somewhat frustrated to know nothing else about Dr H. Naim and his promising new Science.

Actually, Mr Gog and Dr Horeb Naim have never existed. They were the fruit of the imagination of Giovanni Papini,* the famous Florentine writer. Neither has 'Ignoratica' existed as a Science, *but it really exists as idea.* Idea – unique fruit of Giovanni Papini's explosive imagination – that unfortunely has not been scientifically collected by anybody up to now.

However, Dr I. J. Good's attempt to publish 'An Anthology

* PAPINI, G.: *Il libro nero:* Spanish edition, Mundo Moderno, Buenos Aires.

of Partly-Baked Ideas' just looks like a *partly-baked idea* about what the new Science of 'Ignoratica' could be. The idea of Dr Good and his associates, if excelent in principle, could seem scientifically poorly structurated to somebody and, therefore a lack of some important contributions from first-line scientists will have, perhaps, to be regreted.*

Our pretension is to give a further step in this *half-baked idea* and make it scientifically wider and catholic; it is necessary to focus the matter inversely: first, to plan the problems, difficulties, deficiencies and *ignorances*; matters and ways to be followed in order that new ideas and original suggestions and solutions come out. We suggest the foundation of 'Ignoratica' as a chapter of every known branch of Science, not as a professorship, but as a systematised and specialised doctrinal body, and to collect it in periodical publications (every three or four years will presently be sufficient). The task is not easy, it is necessary the concourse of prominent scientists from all over the world, with great experience in their own fields of activity; the task must be done slowly and it is suspected to be inexhaustible. The more the man pushes forward in the field of Science, the more the man will realise how far he is from the goal.

The paramount importance of 'Ignoratica', and even its moral consequences, are brilliantly displayed by Papini's fiction character, and to him we refer. However, it is convenient to justify and make a brief sketch of the IDEA, and for that we will choose our own field of activity – Organic Chemistry.

Our limited experience in research work, whether at the M.I.T. or at the University of Barcelona as a Research Associate, has taught us the many difficulties encountered by the young generations of postdoctorated chemists – with obvious vocation to fundamental research work – to find fields of activity sufficiently interesting, promising and relatively virgin, to enable them to do some original contributions. Such difficulties are not born by insufficient basic preparation – in the classical

* We think that many of the items in this book are 'ignoraticae'. *Eds.*

and conservative sense – or due to a lack of knowledge. We would say that it happens very often all the contrary. Those young chemists have learned Chemistry in such a well systematised form, so perfectly organised and so dogmatically taught; so much emphasis has been put down by the teachers about the brilliant and recent successes in the few last years, that – besides to feel very proud to belong to that privileged class of *Antropoid*, named man – they feel like having been born some years too late. Some years of experience are required to realise which are the important problems and the subjects to be explored with some probability of success. Once the chemist is *settled down*, he has, generally speaking, no lack of ideas; then the problem is how to find the means and right atmosphere to develop them properly. Science, like earth, is very generous and always gives profits, but in the meantime some years of youthful enthusiasm and fertile creativity have been lost.

For the welfare of Science and Mankind, for these young research chemists – and also for those with more experience, with great experimental and mental skillness, but less imaginatively gifted – the systematisation of the ever pending problems in organic chemistry about reaction mechanisms, new natural products, efficient and elegant preparative methods . . . would open a new horizon for their conquests and creative capacity. Evidently, there would not be a lack of prominent scientists ready for that systematisation of a part of 'Chemical Ignoratica', and the *unknown* would become more and more evident, many problems would be solved, and always a further step would be possible.

We must pay attention to some previous essays more or less related to that purpose. As an example, we will mention Sir Alexander Todd, editor of 'Perspectives in Organic Chemistry',* who states in the preface: '. . . it is to be regarded as a collection of essays covering subjects with which each author is intimately

* Ed. TODD, SIR A.: *Perspectives in Organic Chemistry*: Interscience Publishers, New York, London, 1956.

associated; the essays do not seek to present subjects in full detail, but they contain something of the writers' own reflexions and speculations'. The title – *perspectives* – is quite significative. R. B. Woodward – the man of total syntheses – in the chapter 'Synthesis' makes really an sketch about what 'Chemical Ignoratica', limited to the organic field, should be. Woodward not only tells us of the great syntheses accomplished successfully in the history of Chemistry, but he points out which the next goals must be – some of them, as the total synthesis of penicillin and chlorophyll, brilliantly and recently reached. Furthermore, Woodward emphasises the great opportunity of 'the synthesis of molecules or systems which possess the capacities of the enzymes for direction, facilitation, and control of organic reactions'; the importance of transannular reactions; the usefulness of biogenetic considerations; the advantages of intramolecular reactions and 'the consequences of simple fixation of intermolecular geometrical relationship' in organic reactions.

In our opinion, an sketch of the 'Chemical Ignoratica' (to be published in some sort of '*Ignoratica Chemical Acta*') should include:

1\. '*Unknowns*' about limitations and possibilities of the chemical symbolism; nature of the chemical bond; stereochemistry, optical rotation and absolute configuration; theoretically possible structures; possibilities of analogic computers to solve fundamentals problems; elegant preparative methods to introduce certain groupings; reactions in phisiological conditions; biogenetic pathways; reaction mechanisms; rational use of catalysers, with special emphasis to the possibilities of the biological ones.

2\. What remains to be done in the field of steroids, alkaloids, antibiotics and other natural substances.

3\. Reviewing of classical and routinary industrial synthesis and proccesses to find, if necessary, more efficient and better ones......and so on, *being necessary to concrete sufficiently in every topic.*

But the 'Ignoratica' should be extended to every field of Science. As Dr Horeb Naim states, besides making the inventory of unknown things, it should be possible to classify them in two main groups: one covering subjects with great probability to be reached in the near future, and other with more utopian topics, whether for its nature or improperly focussing. A thirth mission remains yet: 'investigate by means of the history of Sciences how and which methods were used in the past to discover new things that were even unknown by first-minded men. This investigation, historical and analytical in character, will not be less fundamental that the two other ones'.

However, where 'Ignoratica' would be extremely useful and could help to resolve problems of '*red-hot*' actuality, is in the field of astronautics and other 'space Sciences'. *It is absolutely necessary* to have an 'Space Ignoratica'. The conquest of space by the man must, logically, open an endless series of new problems, new *unknowns* and questions easily known or predictable by the specialised scientists who – right now – are working on it. Problems of chemical, engineering, medical . . . nature that, perhaps, if they were known by prominent scientists from other fields of activity, could have rational and even genial solutions. It is well known the fact that going into a new field, a first-mind, free of every prejudice, can contribute with very original ideas. The conquest of space is a fact: we must facilitate it and gain time making known which are the *unknowns* and problems, in the present and in the future, to arrive quickly to the goal.

Summarising, we expect of the generosity, disinterestedness, and wide sight of prominent scientists, with great experience in their own fields, to joint their efforts and colaborate to make known – mainly to the young generations – the *unknowns*, problems, subjects and topics waiting for a solution, in form of specialised international periodical publications. We also ask the support and colaboration of the Universities, Industries, Official Organisms, and, finally, the Publishers.

To be able to attend to the birth of 'Ignoratica' as a branch of every known Science, is our wish and our IDEA.

The author wish to acknowledge his indebtedness to Dr José Castells for helpful suggestions.

5 Dutch pbis

by MERVYN E. WISE

Many partly-baked ideas, as well as fully-baked ones, are in print in lists of 'stellingen'. These are theses (propositions) which are submitted with every doctor's thesis (dissertation) in the Netherlands. It is advisable that their number at least equals the number of examiners when the dissertation is defended in public. The last one is often less serious than the others; sometimes this is the thirteenth. They are sharp, and critical or controversial. The thesis is often in English, but the stellingen are in Dutch. I was told that this is *not* because they often take the form 'The works by *A*, *B*, and *C* on the problem *P* all rest on a fallacy'. This type is avoided in the list below, and a few words have been added to (try to) justify most of them, since I shall not have to defend them in a University hall. The opinions are my own; the controversies are of course not new ones. The stellingen are, I hope, new, as such, but I should like to apologise to anyone if I have reproduced one of his by chance.

1. If observations are fitted to an empirical law, and the law fits a model or theory, one should always distinguish clearly between the model and the law. If this is not done both may be discredited, though often the law is right and the model wrong.

2. Regularities in the earth's major relief were pointed out by Wegener and explained in terms of his concept of continental drift. This was ridiculed because he proposed a physically impossible mechanism to explain it. Yet he was probably right in his basic idea.

3. In order to make long-range weather forecasts, empirical

and statistical laws of persistence of types of weather must be better understood. This can best be achieved by studying lifetimes of anticyclones, which tend to stay in one area.

4. The recent excess of long spells of fine, warm weather (in and near southern England) in odd compared with even-numbered years merits a careful statistical analysis.

5. When studying weather fluctuations of temperature and rainfall in relation to the incidence of a disease, the seasons must be studied separately. (In Western Europe, temperature is, in winter, positively correlated with rainfall and, in summer, negatively.)

6. There are explanations for the increase in lung cancer other than the increase in cigarette smoking; they fit the facts for one country but at once fall down on going abroad. Any epidemiological study is best carried out in several countries.

7. Quantal dose-response analyses are overworked; often the response is really a continuous variable and one should try to measure it.

8. When growth or learning curves are plotted as a function of time, there are irregular deviations from a smooth curve that are not generally independent. The biological processes will be better understood when the random variation is brought into the model in the right way.

9. Asymptotic laws are over-stressed by mathematical statisticians; we often want to know what happens when n is not infinite, but 20 or 5 or 3.

10. At present electronic computers hinder biological research. But further improvements to desk calculating machines would help immensely.

11. Large-scale multivariate analyses, such as with 20 or 50 linear equations, open up a useful field of biological research: its object should be to convert the results into the most meaningful form scientifically, and to find out what analysis should have been done instead.

12. Traffic, transport and accident problems ought to be

studied in an independent foundation for pure and for operational research. It would need to be well enough supported to protect its scientists from the consequences of any good work they do that will, *ipso facto*, infuriate some or many people.

13. A scientific journal of standing should invite quotations and newspaper cuttings for a column (like 'This England' in the *New Statesman*) of examples of the use and (especially) the misuse of statistics.

6 The Place of Speculation in Modern Technology and Science

by J. D. BERNAL

In these days the production of an article on untested ideas is almost an impossible, certainly a very unusual, task for a scientist. For we have been trained over the years not to say anything on which we have no evidence, not to put out work which has not been thoroughly checked, not to bring in any personal elements. And yet a discussion on speculative and unfinished ideas is exactly the opposite of this. Here we cannot be sure of anything: we can only imagine and hope, usually in an intensely personal way.

It is odd, really, that no one had thought of collecting such ideas before – at least not for a long time. For, actually, at the birth of modern science, it was one of the commonest of practices, as witness the *Century of Inventions* of the Marquis of Worcester. Even the great Newton filled the greater part of the end of his *Opticks* by queries which were really speculations. Interestingly enough, it was precisely Newton's queries that were to have the greatest effect on the development of physics in the eighteenth and early nineteenth centuries. It was not what he could prove, for this had a sterilising effect, but it was what he did not know that stimulated other people to work. In that sense the method of query may still have its uses.

I think one of the reasons why people are more averse to it now than in the past is that the enormous body of scientific publication would discourage any publication of speculation even if there were a channel for it.

Nevertheless, speculation disguised as legitimate hypotheses, even if subsequently proved false, still has its use in science; much of science actually grows in this way.

Now this is all by way of excuse for going forward with speculation and even publishing it. Nevertheless, actually to do so is naturally apt to reflect unfavourably on the ability of the scientist and so it is lucky for me, at any rate, that I have been so long in the field that I cannot really do much damage to my reputation by going on producing crazy ideas. I can recollect the answer given to me two years ago by my son when I suggested he might add his name to a paper of mine in which he had helped considerably. He said: 'It is all right for you but I have my reputation to consider.'

I imagine most scientists, in fact most people, carry on a kind of internal monologue which goes on for years and years about the things which interest them. However much we want to act on our intuitions we find that in most cases we are in no position to do so, because they are either too numerous or too vague or too impracticable. Most may limit their imagination to immediate day-to-day concerns but some people go beyond that. They go into daydreams which may or may not have a continuity or be episodic like those of the celebrated Walter Mitty. This is the stuff of which imaginative writing comes, based essentially on experience but twisted to suit our desires or to make a better pattern.

Now the scientist, and particularly the experimental scientist, has daydreams rather different from the writer's: they tend to be about things rather than people. Although started by fancy, they tend to be controlled by experience and knowledge. I know myself, as I have kept a certain number of notes of these over the years, that although there is a tendency to lack continuity, I have developed only a relatively small number of ideas but have gone on working on them on and off for a long time.

This speculative working is quite different from the directed working in scientific research where at every stage you try to check the reliability of what you have done by an experiment or to find how things work out in practice. Purely speculative work goes on entirely interiorly, coming out occasionally in a note or a drawing but never being realised in practice. I say 'never', I should say perhaps 'hardly ever' because, in fact, most of the practical inventions have come from such speculations, but there must have been millions of speculations which had never got there.

The pleasure of it to the scientist is to see how far he can go in making a self-consistent picture, though one continually changing, of what might work. Now the question may well be put – 'Why, if you can speculate about what might work, why, if you can check it so far as possible against your scientific knowledge, why do you not go further with it and actually test it in practice and make it work?' The answer is that you cannot do everything and, especially when you are older, you realise what an enormous effort it would be to get the thing done, an effort which could only be at the expense of other occupations and of other ideas which, if not so grandiose, at any rate are more immediately realisable. There is additional caution which applies to material projects, and that is, that it is impossible to go on with such developments in practice without involving a number of other people with different capacities who have to be sold on the idea and also get it the necessary administrative or money backing. Both of these are jobs in themselves which you may not be wishing or able to carry out, especially if it means dropping most other work.

Nevertheless, there may be some use in unloading some of this hypothetical and unfinished business, not so much because it might be taken up and realised, but because in examining it and criticising it something new might be found out – at least, that is my hope.

What is set down in this article is necessarily only an arbitrarily selected sample, but to make it representative I have chosen two speculations covering as wide a range as possible, one from the field of engineering and the other from pure science. Later on it will appear that these two have some logical connexion but I was not consciously aware of it when I chose them.

Actually, the first is the much easier, at any rate to me, and the one in which I spend the most idle time; it does not require such an effort because the forms that it deals with are immediately visualisable. I can build the models of my ideas in my head, I can see how they go, I can do an occasional calculation to see that it is not nonsense, and I can always rely on the fact that I am under no obligation to do anything practical which might spoil them.

In pure science such an obligation does exist for a scientist like myself. Ideas ought to be checked by experiment and calculation. Speculation is therefore only permissible in regions far away or long ago where one man's guess is as good as another's. That is why I have chosen mine in the distant field of the origin of life.

A practical speculation, that is, devising something that would actually work, can come about in two quite different ways: one is to consider what is needed and the other to consider what is possible. One results from a survey of the field of industry – of requirements – and the other from the properties of matter, that is, coming from pure science into application.

In practice, from the history of science, one finds that these two approaches are usually followed by different kinds of people but I have, myself, a good deal of sympathy with both and have even tried them both. When the requirement is immediate and not very far removed from actual practice, the problem is not a very difficult one and it usually happens that if you are lucky enough to get the answer you get it at once and it works.

I can still remember during a discussion early in the war on coping with the fuses of unexploded bombs, that I hit in a few minutes on a solution derived from schoolboy egg-blowing. It consisted simply of emptying the explosives out of the bomb with steam from a safe distance and thus making it harmless regardless of the nature of the fuse. Under wartime conditions, such an idea could be developed within two months, the necessary equipment produced in another two and the whole scheme become operational within half a year.

The big requirements are harder to visualise, and when they are visualised the way to meet them usually involves radical changes in knowledge and thought. I could instance, in our day, the need for the direct generation of power from the thermal sources without moving parts, which is only just attracting serious attention, and that of the transmission of electrical power through space as we already transmit signals. The solution to both of these problems by thermoelements and masers may be just under the horizon. But, as Bacon pointed out in relation to gunpowder as a means of shooting missiles, the solution may not be found by seeking to meet the requirement by improving existing methods but by exploiting some newly discovered property of matter – a semi-conductor or a resonance phenomenon. For these we must depend for some time longer on a still undefinable flair for the new and for its implications and applications.

Failure to speculate on or follow up observations represents an immeasurable waste of opportunity. There must be lying around today, in front of our noses, hundreds of these cases many of which might also have revolutionary effects in practice. I recall the remark of Szent-Györgyi: 'Discovery consists of seeing what everybody has seen and thinking what nobody has thought'.

But discovery in itself is not enough, what is needed as well is to find the way of linking the possibilities released by the discovery with the real requirements. This is the true task of invention, once carried out by the ingenious and persistent individual and now probably less effectively if less painfully by large research departments.

It is against this background, which should reduce it to its proper proportions, that I am putting forward one of my own speculations. It is one about which I have been thinking on and off for the best part of thirty years without ever trying seriously to put it into practice. It is a relatively easy one because it involves nothing more elaborate than ordinary elementary physics but it ties a trivial observation in physics to a great human need. The need here is the universal need for water – all of the time in the desert regions and for some of the time almost everywhere. Taking the world as a whole, the major limitation to the growth of crops – and it is an *enormous* limitation, of the order of a hundred- to a thousandfold – is that most regions with good sunshine are chronically short of water. One has only to think how extremely fertile are the banks of the Nile compared to the deserts on either side of it. If we could irrigate all the desert to the same extent that the Nile irrigates the plain of Egypt, think what an enormous increase there would be in the food available for mankind. And that is not the whole story, as you will see.

Now, there are many, many ways of getting water for crops, ranging from the most primitive devices for pulling it out of the river in buckets, to atomic energy schemes for desalting sea water and pumping it up to irrigate high levels of land. My thoughts on this subject really antedate the atomic era, for what I wanted to do was something extremely simple and almost automatic. I suppose I was inspired by that significant but forgotten work *The Great God Waste* by J. L. Hodgson. At any rate, ever since I read it the need to irrigate the desert regions has been at the back of my mind.

What gave me the new clue as to how to do it easily was something apparently completely trivial. I was visiting a research station on Lake Windermere and talking to a scientist who was studying the behaviour of newts, particularly their habit of crawling under stones in the day time and coming out at night, which he quite rightly associated with the dangers of loss of water vapour through their thin skins. He had actually carried out an experiment which he thought should bear on this. He put plugs of cotton wool at each end of a long wide tube, damping one piece of cotton wool and not the other; in each plug he put a thermometer. He found to his surprise that when the damp cotton wool was at the bottom, the bottom thermometer showed cooling and when at the top it did not. He could not understand this as he thought it ought to be the other way round, that as water was H_2O, it was heavier than oxygen and the water vapour ought to go down and, consequently, should be most affected when the wet piece was at the top. His error was a rather childish one; it was that he thought of oxygen as O instead of O_2. This brought to my mind the obvious fact, which I must have known since my first few chemistry lessons, that water vapour is just half as heavy as ordinary air, and therefore will go up of itself. It was only then that I realised that I had made the same mistake in my childhood. I had believed that the reason why water vapour went up from the sea was that it was heated, whereas that has nothing to do with it: it goes up because it is light.

It is on the basis of this principle that the rest of the fantasy develops. Now here we have a gas half as heavy as air and very easily made; why should we bother to lift water up when it will lift itself in the form of vapour? All that we need is the appropriate apparatus, effectively the same that man has used for many, many years – the chimney – which takes up light air automatically and maintains a draught.

What is wanted is a chimney which is to be filled with steam instead of smoke. I proceeded to construct in my mind and on paper, with a few calculations, the kind of arrangement that might work (*see* Fig. 1). You must imagine to yourself a desert coastline with a cliff or bluff rising to a hill plateau perhaps three or four thousand feet high, such as occurs on many of the desert coasts of the world, in Arabia, in North Africa and in Australia.

In front of it lies a beach or saltmarsh coastal plain and behind there is a desert. All that is needed here is to build an arched tunnel for the steam all the way up the side of the hill. Water from the sea, superheated by the sun, is allowed to flow into pits where it meets a stream of preheated dry air and evaporates,

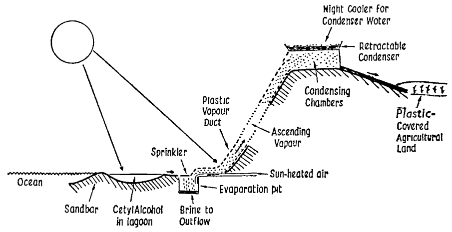

FIG. 1

leaving the salt behind and turning into vapour which can be further heated by the sun. The mere existence of the chimney will ensure that a steady stream of water vapour will flow right up to the top.

There, all that is necessary is to reverse the process and condense the vapour to water again. This, also, is an operation which we know very well and which even primitive man has used in the dewponds which we find at the top of the downs. In other words, the whole process is really very similar to the rain which forms when wind carries sea-evaporated water and is precipitated as rain on to high coastal mountains, only here we direct the water vapour and do not just let it go loose. As it rose it would cool to near the saturation point by expansion. At the top it would flow into a series of chambers, which would be kept cool by water cooled by evaporation arranged to occur only during clear nights. The whole process would be completely automatic, would require no power and no attention. The sea-water would just run in at the bottom and the fresh water would run out at the top.

Now, when speculating on this, one comes across a large number of difficulties which have to be overcome one by one and so can be overcome simply in the mind and by bookwork. One main weakness of the whole scheme, for instance, is that it depends on having water as hot as possible to start with at the bottom and to get water as cold as possible at the top. Such problems have in fact been solved recently and remarkably cheaply by the process of covering over pools of water with a very, very thin film of some self-spreading substance like cetyl alcohol, enabling the temperature to be raised to something getting near to 40°C, quite enough – as French engineers have shown – to work the low-pressure end of a steam-turbine used in this case with a condenser cooled by deep sea-water.

The fundamental snag, I am convinced, is not on this technical level at all. I think such a scheme could be made to work, but the question is really whether it can be made to pay. Here two other factors come in. First of all the process does not require power, in fact it produces power but it needs a considerable static equipment. Somehow the channels for the vapour have to be built big, strong and cheap. This question of scale is, indeed, the great difficulty of every system which has to work at low pressures, pressures in this case slightly less than atmospheric pressure.

I believe now it would be possible, by a method which has in fact far wider application and which combined with this might work very well, that is, by using thin plastic sheet which could be built up on a light framework, providing a series of channels dozens of yards across, suitably engineered not to be blown away by the wind but, of course, inevitably vulnerable to attack by animals or ill-intentioned people. If the capital cost could be kept down it would still be a very expensive way compared to the way in which nature just takes the vapour up and drops it down as rain, but if nature did this everywhere there would be no shortage of water. Like many other enterprises a scheme such as this would work in the first place where it paid most to get water, that is, in desert regions near the sea where there was a local demand not so much for subsistence crops as for vegetables and other valuable produce.

It would have to compete with existing methods of desalting by electricity or distillation using oil fuel, often domestic and cheap. Yet in the long run I believe that the natural method will find its place especially if the method of vapour-trapping could be stretched, not only to cover the production of the water and lifting it into place, but also its utilisation when it was there. This idea, which has in fact already been practised to a limited degree in Israel and one or two other places, is that of totally enclosed agriculture. Now, enclosed agriculture of a sort has been practised for hundreds of years but the function of the greenhouse made for northern countries is not one of preserving water vapour so much as preserving heat. In the drier and hotter parts of the world water-preservation is much more important.

The whole economy of a living plant is to take up a fairly dilute salt solution from the ground and evaporate it out of its leaves. This is not only, and indeed not mainly, for the purpose of providing enough water and salts for the growth of the cells in the leaves and the stalk. It is really necessary in order to keep the water-flow system which carries the nutrition to all parts of the plant in action, a kind of suction or pressure-driven water-circulation. The practical result of this is that most plants, other than those specially drought-adapted like the cacti, pass through, by transpiration, many times the amount of water they need to maintain growth. In a reasonably damp climate this does not matter; the plant has all the water it needs, in fact the transpiration is most useful in keeping down the water-logging of the soil. However, in dry countries, this is certainly not the case. Where every drop of water counts it seems such a pity to lose nine hundred and ninety-nine drops out of a thousand by transpiration from the leaves which goes straight up as vapour into the upper atmosphere. In the end, of course, it must come down but probably somewhere in the middle of the Pacific Ocean.

Man cannot indefinitely afford this circulation of water through and out of growing plants in an essentially dry atmospheric planet. But until recently there was no possibility of preventing water vapour going up into the upper atmosphere. Now there is such a method just coming in which undoubtedly will become more and more practicable as time goes on. The thinner you make the transparent plastic sheet which is proof against water vapour and at the same time against deterioration from the sun's rays, the nearer you get to being able to cover in whole areas by such sheets maintained essentially by a slight excess of air-pressure from below and, in that way, arrange for the complete re-circulation of condensed water vapour inside the enclosed area. Even without separate condensers the vapour will tend to condense as drops of water at night and run back into the soil. In this way it should be possible to increase by very large factors, of the order of a hundred, the utilisation of water in tropical and sub-tropical regions.

All that would be needed then, from the sea, is a kind of topping up, and the same process, the same materials which are already indicated for lifting water vapour by its own lightness, would suffice here to provide water for large areas and might even convert it from the range of a luxury for producing vegetables for a local population to producing cash-crops like sugarcane or food-crops such as rice. We shall certainly need all we can get in the next few decades.*

* *Note added in proof.* The danger of any carefully followed-up engineering solution to a problem is that it might be completely undercut by another based on the application of some overlooked physical principle. The need to produce salt-free water has led to the equivalent processes of boiling, electrolysis and freezing, all with relatively high operative costs, and to a radically new solution capable of being used on a very large scale. This consists in using varieties of expanded ice with melting points a few degrees above zero and stabilised by volatile hydrocarbons like propane (see *New Scientist*, volume 12, page 756, 1961). It should now be clear that the large-scale production of fresh from salt water, or possibly from water vapour in the air, is one that can only be solved in all its aspects, economic and engineering as well as

Now there is a sample of my speculative invention for what it is worth. I see it as lying along the line of development but still rather far from any immediate application. Its long-term prospect is because it lies in the direction of economy, of doing things which do not use, as most do now, the maximum but the minimum amount of power by making more use of engineering and less of natural processes.

At the present time the efficiency of industrial processes is increasing in a rather peculiar way. The productivity per man-hour is increasing rapidly, it may double within four or five years, but the productivity per kilowatt-hour is actually going down almost as rapidly. When we talk of each man being able to use so much power, it really means he is going to burn up this amount of energy per minute and get what is often a very trivial return out of it. The actual power *used* in engineering processes, even admitting that the process is necessary, is probably at an efficiency of between one and two per cent. And most of the things that are done in engineering are not necessary anyhow. All the machining, the pressing, the forging and so forth that goes into the fabrication of metal components are really admissions that we do not know how to persuade the ore or the metal or the corresponding plastic to get itself into the shapes we really need without this great expenditure of energy and waste of materials.

A rationalised industry would attempt to get the ultimately desired effects tending to human welfare by modifying as little as possible the play of natural forces.

From these essentially simple engineering problems I would now like to pass somewhat abruptly to the field of fundamental science. The speculations I have chosen as illustrating this kind of thought are drawn from the almost infinitely more complicated fields of biology. Here, as this is not formally my own field, I may take the licence of going beyond the reach of scientific confirmation.

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scientific, by international co-operation working with considerable research and development resources.

In the problems of the origin and early history of life, with which I have occupied myself fitfully for a number of years, there is little or no chance of making decisive and unambiguous observations or experiments. But there is all the more scope for imagination controlled by a rigorous logic, making use of indirect evidence to reconstruct a sequence of more and more plausible images of the past.

It may not matter much if these pictures are wrong. Sometimes this may even help the process of the discovery of truth. A really crashing mistake stimulates other people to prove that it is wrong whereas before they would not have thought it worth while looking into the particular question at all. Controversies have served in their time to point the interests of scientists and have led to the devising of experimental tests which, even if they invalidate the theories of all the protagonists, always lead to new truth.

The classical case in the nineteenth century was the great Pasteur-Pouchet controversy on the possibilities of spontaneous generation of living forms. At first sight Pouchet was right: it was quite obvious that life arose spontaneously in the various bowls of broth which he prepared. But Pasteur very carefully showed that it was not so in reality. The new life could always be traced to germs coming from the outside. But an inference, drawn illogically from his flawless experiments, held back for many years the solution of the deeper problem; that is, how did life get to this earth in the first place without the germs? Arrhenius's speculation that it came in spores from outer space only pushed the problem further back.*

This particular problem of the origin of life is still very much with us and as we can see now by hindsight its solution has been held back for a good fifty to a hundred years owing to the caution of the scientists against wandering into a field so full of

* *Note added in proof.* If B. Nagy and G. Claus's observations of primitive organisms in carbonaceous meteorites (see *Nature*, volume 192, page 594, 1961) are confirmed, then we will be inevitably pushed to looking further back beyond the earth and even outside our solar system.

emotionality and religious preconceptions. It was also one little adapted to the historical-palaeontological method of study so fashionable in the late nineteenth century because the material evidence was destroyed. Nor did the experimental approach seem any more applicable for, since the sixteenth century, no one has thought that the creation of life would be an easy laboratory exercise.

The sceptical attitude which the older scientists took up to the question of the origin of life might be considered as another demonstration of a general thesis of the *provisional* character of all scientific knowledge. Indeed, this, owing to the rapid march of science, is more obvious now than it has ever been and seems about to become, paradoxically, a major limiting factor to scientific advance. Techniques are developing so rapidly, different branches of science are interacting so intricately, that it becomes more and more probable that what is said today will be proved wrong in a relatively short time – two years, a year. So, why bother? Why not wait until the thing is found out? But, of course, such an attitude, very safe indeed for any particular scientist to take, would, if adopted by all, lead to a complete stop of science itself. Nevertheless, what might be called the philosophy of *provisionalism* may become a very real and even positive factor in scientific method of the near future. Indeed, it is already a very good corrective to over-confidence and is a training for people to be able to take leaps of imagination without requiring at any time to believe that they had discovered not merely the ultimate truth, but even a fundamental improvement on the truth held at the particular time. It may be an improvement, it may not; but it is worth making even if it is not.

Speculations on the origin of life have been going on for more than a century, culminating in the Oparin-Haldane hypothesis of 1924–28. This postulated an original earth's atmosphere without oxygen in which the chemical molecules which were to form life were formed under the action of the sun's ultraviolet light. From that time on, this basic theory has been elaborated by more and more workers and supported by actual synthesis in the laboratory. The subject has become almost a respectable one, as witness the Biochemical Union Symposium in Moscow in 1958 on 'The Origin of Life on the Earth'.*

My own contribution has been more purely speculative and systematic. Beginning with my Guthrie lecture of 1947, I have tried to work out the kind of conditions, the character of stages, that may have occurred in the early evolution of life. I have tried to set out in order the different problems that have to be solved. But I have done no more than provide tentative solutions to one or two of these. For the rest, all I have done is to list a whole set of queries which must be put and to which some kind of answer must be found, ultimately by experimental methods.

In the meantime, this provides a kind of scaffolding on which we can build a picture of an evolving universe. How insecure such a scaffolding can be has been shown by the recent switch-over in searching for origins from what may be expected on a purely inorganic earth, from very, very simple compounds of the type of ammonia and carbon dioxide, towards a realisation that some of the work of synthesis has been done first and rather unexpectedly in outer space, through the synthetic action of cosmic rays. More and more evidence is coming in which points to an origin in meteorites or comets of quite complicated carbon compounds. These are arriving on the earth and presumably have arrived on it since its foundation four thousand odd million years ago.†

If this view is true or at least acquires some temporary plausibility, it would provide a literally far-fetched explanation of how the complexity that characterises life here or on other planets may have arisen in the first place. Living processes are characterised by a degradation of free energy. Life is, in a sense, a phenomenon of decay highly localised in an inorganic universe.

* *The Origin of Life on the Earth*, a symposium: Pergamon Press, London, 1959. OPARIN, A. I.: *Life: its Nature, Origin and Development*: Oliver and Boyd, Edinburgh, 1961.

† See footnote, page 23.

The new idea is that an accumulation of free energy in complex molecules was first brought about by the chance impact of the most energetic elementary particles in cosmic rays. These in their almost total disorder would have contributed first steps to the essential and growing order that was to manifest itself in life.

These new discoveries and ideas give us a chance to write a new first chapter or preface to the history of life in its modern form, but it need not alter the subsequent chapters. There remain problems of a basic nature as to how the early molecules that were formed by more or less straight chemical methods ever managed to get together sufficiently to produce a dense enough medium. I have suggested that it was done by absorption on clay: there may be other methods. Then, as soon as there were fairly complicated polymer molecules, another question arises as to their aggregation and of the formation of proto-organisms, organelles and cells. Now the electron microscope is revealing more and more that the cell is really a very complicated world of its own, with its City in the nucleus and its fields in the cytoplasm, each containing a large number of other interior bodies of a recognisable subsistent kind, the microsomes, the mitochondria, the basal nuclei of cilia and centrosomes. Such ordered agglomerations must have a constitutional history of their own, maybe as complicated as that of the first City states.

The two unchecked speculations that I have set out here, those on means of raising water from the sea and thus providing for the food needs of an impoverished and growing population, and those of steps in the chemical origins of life, may seem quite unconnected except for their occurrence in the same brain. It is also obvious that they reflect current economic and scientific preoccupations. It is only very recently, however, that I have seen that they possessed an internal logical connexion which may have guided me unconsciously to them, though far less effectively than if I had made even an elementary use of Marxist philosophy at the outset.*

* Bakedness (of the reference to Marxism) = 1/4? *G. Ed.*

For both have one essentially material aspect in common. Both deal with the production of food or, more precisely, with that of simple nitrogenous and carbohydrate compounds. Now the argument could have run like this: if life originated spontaneously on a hitherto inorganic earth, the first steps, which could not have needed complicated enzyme and co-enzyme systems, must have been very simple. Therefore, they should be quite easy to carry out and that not only in the laboratory but as industrial processes. The way to food production can be found through a study of the chemistry of the origins of life.

I did not in fact argue this way. Instead, I was given the answer first and found the question afterwards. For it is precisely the attempts at reproducing simple organic precursors of life in the laboratory, first by Miller, Terenin and others by using external energy sources, and now by Oró,* using simple free-energy containing molecules like hydrocyanic acid, that point the way to an effective and purely chemical food industry.

Oró's method of passing hydrocyanic acid into ammonia along the lines first described as far back as 1876 by Wipperman, and dormant ever since, produces not only a number of amino-acids but also purines, the basis of both proteins and nucleic acids. Rapidly developed it should serve to solve the most pressing problem of tropical nutrition: the shortage of proteins.

Now, it should be evident, especially from such demonstrated interconnexions, that in these kinds of speculation the main value lies in having a sufficiently wide appreciation both of needs and of possibilities. For this, all aspects need to be kept in focus at the same time yet, in view of the diversity of specialisation of science, this is becoming in these days increasingly difficult. If we are not to fail, as a consequence of our success, the idea of the solitary speculator will have to be replaced by what might be called collective speculation; in other words, we must go back to one of the oldest and most valuable forms of scientific thought, the dialectic, the conversation-discussion-dialogue, what you

* *Nature,* volume 191, page 1193, 1961.

will, which the Greeks indulged in, as have in early modern times such bodies as the Accademia de Lincei or the Royal Society. Here the object was not so much to prove one is right and the other is wrong, as to form a collective impression, to get what the Quakers call a sense of the meeting.

Such dialogues have occurred over and over again in science but are now becoming an absolute necessity. The small groups representing the different sciences are tending to come together. But what has been a purely casual, and often social, aspect will now have some further degree of organisation. This is becoming all the more necessary because all the earlier efforts were very limited in aim. In what I have been discussing, I have stressed only the qualitative aspects. However, if we bring in the quantitative aspects as well, and they are becoming more and more important, we will have to rely increasingly on the radically new factor in thought processes, the utilisation of electronic machines.

We are just entering one of the most exciting transformations in the history of science, indeed in the history of all thought because, by going into partnership, so to speak, with new machines, we should be able to enlarge and transform human thought and its range of application. To start with this will lie in the direction of speed, and next, probably, in that of what we may call comprehension, being able to take in at one time far more data – meteorological or health data, for example – than the human mind has ever been able to take in before. The machine can cope directly with such multiplicities but for the moment has to boil them down to something the human mind, which works very much slower but in a far more versatile way, can cope with. The speculations of the future may then be the speculations, not of one man or of many men, but of all humanity and their machines.

7 Practical Climate Control*

by O. G. SELFRIDGE

The climate of the Los Angeles basin in southern California, long one of its main attractions, has in fact in recent years been deteriorating so that it now is a serious menace to health. (Note that there is no weather in Los Angeles, only climate.) The problem is smog: an offensive mixture of atmospheric pollutants from industry, incinerators and automobiles, that makes eyes to weep, throats to cough and itch, and lungs, presumably, to irritate and grow susceptible to other ills.

The Los Angeles basin is a west-facing basin at about sea level surrounded by mountains up to eleven thousand feet high. Especially in summer, the cool upwelling of the Pacific Ocean (the sea temperature at this point with latitude 34° N. is about 55°F.) chills the air to form a temperature inversion. Consequently the pollutants are not dispersed, but build up, trapped in the basin.

Some of the frantic proposals discussed shamefacedly in California (they don't really like to admit that there's any problem at all) have included large fans to destroy the inversion, or tunnels through the mountains to let the smog distribute itself over the surrounding deserts on the other side of the mountains. The trouble with the first is that the energy required to blow the inversion away is the energy required to destroy the inversion by heating (at least); with the second, that there is a large pass on the eastern end of the basin which leads to the town of Palm Springs. This pass, though thousands of feet deep and several miles across, seems to have negligible effect on the smog.

The suggestion I make here is to get at the cause (scientists' ploy number one), which is the low temperature of the upwelling waters of the eastern North-Pacific Ocean. Large ocean-basins

* This contribution is placed in this chapter because it is closely connected with a part of J. D. Bernal's contribution. *G. Ed.*

do tend to have upwelling (hence cold) waters on their eastern edges. But the driving head cannot be very great, surely, of the order of millimeters or fractions of a millimeter.

Let us, then, place a shield, at a depth of a hundred feet or so, over a 200-mile stretch of the sea, outwards perhaps a hundred miles or so. The sun would rapidly heat the surface water, and the atmosphere above it, the increased evaporation would aid convection through thunderstorms, and the smog would be washed and convected out of the Los Angeles basin.

As a matter of fact, a similar phenomenon occurs off the coast of Ecuador and Chile. Normally, the cold water there causes a vast coastal desert and occasionally a warm equatorial current, a few dozens of fathoms deep, called El Nino, penetrates and temporarily collapses the *status quo*. Thunderstorms provide the desert with many years of average rainfall in a few hours; the desert blooms; incidentally, the fish in the ocean die because of the cut-off of nutrients from the cold waters; so do the birds that live off them.

I suggest that an easy way to make the shield is to use very thin sheets of polyethylene, say one mil thick, which should be weighted by anchors and buoyed up by air bubbles if the up-welling water is not enough. The total weight of the sheet at 100 lb. per cubic foot is of the order of two megatons suggesting that a large part of the total cost will be in emplacing and anchoring it.

If it is decided (by those reliable best-interests-of-the-people authorities who do decide) that the increased rainfall is too catastrophic (it would be, and many of the houses built on the sides of hills in the Los Angeles area would slide down as they do in rainy winters even now) they could replace the subsurface sheet with a surface sheet painted *black*, to increase convection without increasing the moisture supply. In either case the temperature inversion would be broken.

8 A Splendid National Investment

by O. G. SELFRIDGE

It would be a splendid national investment for a country to select at random a half-baked idea (weighted in favour of those from the better scientists, i.e. those like us) and put it into practice as energetically as possible. We claim that this would in the long run, and only in the long run, be enormously profitable.

One would set up a national half-baked commission, whose members would choose between the proposals, not much on the basis of obvious merit – for those would be picked up by private enterprise or other organs of the government – but more on the basis of fancy. An ordinary running budget of a million pounds a year would cover its costs. Naturally, as soon as any idea showed any signs of *working*, it would be discarded. Not more than 10 per cent. of the budget would, at first, be allowed for psychic games of any kind.

2

Information About Information

9 Non-communicating Discourse

by R. H. THOULESS

In using language for the purpose of communicating (as distinct from using it for the purpose of thinking), we may be carrying out any one of a number of different language activities. Wittgenstein called these 'language games'; I think we shall be more generally understood if we call them different 'modes of communication'. The person using language for communication may, for example, be doing any of the following things:

1. Stating that so-and-so is the case, e.g. 'Kangaroos are found in Australia'.

2. Stating a linguistic usage, e.g. 'Marsupials are mammals whose females have a pouch for carrying their young'. Often (but not always) similar in grammatical form to statements in mode No. 1 but distinguished by the fact that the point of the statement is to say how a word is used.

3. Asking what is the case, e.g. 'What do kangaroos eat?'

4. Asking about a linguistic usage, e.g. 'What is an ungulate?'

It would be simpler and more convenient to refer to these modes of communication as respectively: 'giving factual information', 'giving linguistic information', 'asking for factual information', and 'asking for linguistic information'. There is a sense, however, in which every mode of communication can give information about the communicator. Even if the communicator talks nonsense, this may convey the information that he is a foolish, prating knave, although certainly this was not the intention of the communication. These four modes might alternatively be characterised as types of communication in which there is the intention of giving or asking for information.

The above four modes of communication are among the commonest met with in scientific writing and talking. There are, however, many other modes of communication. For example:

5. Giving an explanation. This covers a variety of communicatory techniques directed towards resolving a state of cognitive uncertainty or puzzlement.
6. Asking for an explanation.
7. Making an evaluation.
8. Asking for an evaluation.
9. Giving an order.
10. Asking for instructions as to what to do.

And many others: giving a greeting, making a joke, making poetry (in a wide sense to include the creative use of prose as in oratory), paying a compliment, provoking a quarrel, resolving a quarrel, etc. All are real modes of communication used in various contexts; all serve genuine communicational needs.

There is some imprecision in the lines dividing these different modes of communication, but not to such an extent that some such classification cannot be used for the purpose of content analysis of speech or writing. There is an obvious difficulty, for example, in making the very important distinction between sentences conveying factual and those conveying linguistic information. The same sentence (e.g. 'Whales are large marine mammals.') may convey factual and linguistic information in varying degrees, depending on whether the receiver of the communication is uncertain about the habits of whales or about the use of the name 'whale'. The lover's statement 'Your eyes are beautiful' may be not only using the mode of communication of paying a compliment, but also mode No. 7, making an evaluation.

There are other forms of compliment involving no evaluation, and evaluations involving no compliment. This indefiniteness of boundary between classes is a complication in the classification but not, I think, a bar to its practical usefulness.

Bearing fully in mind this source of uncertainty, and remembering the many widely-different purposes which may be served legitimately in communication, we are nevertheless forced to the consideration that much of what we hear or read comes under none of these classes for it serves no communicational purpose though it may serve other purposes in giving satisfaction to its originator or even to its recipient. It is to these ways of using language that I propose to apply the name 'non-communicating discourse', which may be conveniently shortened to 'N.C.D.'.

What is meant by non-communicating discourse may be illustrated by an invented example, which I hope may be recognised as merely a concentration and not a distortion of reality. Let us suppose a public speaker is addressing a large audience:

'Ladies and Gentlemen,

'We have gathered together in this great hall. We have come from far and near. A year has passed since our last coming together, a year of hopes and disappointments, of joys and sorrows. Now again we lay aside our daily cares, and once again join in affirming our conviction that we must do what is right, and that we must identify ourselves once again with the dynamic stream of living realities . . .' and so on.

This speech recognisably starts with the kind of N.C.D. which the critical man in the street is inclined to specify by the phrase 'he talked but didn't say anything'. Grammatically the sentences look as if they belonged to No. 1 mode of communication: stating what is the case. But they do not communicate because they state nothing that could conceivably be unknown to any of the audience. Let us call this mode of N.C.D. that of 'vacuous affirmation'. It is, I think, very much the commonest type of

N.C.D. In some contexts, audiences are so used to it that they see nothing odd in it.

The speaker himself might imagine that he was engaging in the mode of communication to which we have given the general title of 'making poetry'. Superficially what he says appears to have the form of oratory, but is distinguished from this by its non-creative and banal character. A mere stream of spoken language is not oratory.

Vacuous affirmation is the easiest mode of N.C.D.; it is not the only one. There are also the various ways of talking nonsense. The imaginary speaker I have taken for illustration starts with three sentences of vacuous affirmation, then he goes on to make two nonsensical statements. The first is the tautology 'we must do what is right' which cannot significantly be denied. The second is a reference to 'the dynamic stream of living realities' which is so vague that it is difficult to see how any statement about it could be significantly asserted or denied.

Generally, nonsense is found in a different context from that of vacuous affirmation. A large amount of vacuous affirmation seems to be tolerated (perhaps even expected) from the political platform and the pulpit. It is also used by after-dinner speakers when they are not exercising their proper function of using the complimentary mode of communication. Something subtler is, however, needed by the deliverer of a more sophisticated type of address (such as a lecture) if the speaker is to cover up the fact that he has little or nothing to communicate. For such a purpose the various modes of nonsense are required, and, if skilfully used with a sufficiently erudite vocabulary, they may achieve their object with better success than they deserve.

Not in speech only but also in writing we may find N.C.D. weakening, by dilution, the primary intention of a book to communicate something. There is a book on gardening in which each chapter starts with a paragraph of N.C.D. The chapter on Roses, for example, begins with the vacuous evaluation: 'The rose is the sweetest flower that blows' and continues in the same strain to the end of that paragraph. It is only in the second paragraph that the real business of communication begins. This opens with the forceful informational sentence: 'The rose is a gross feeder and should be well dunged.' Fine! But how much better it would have been if this were the opening sentence of the chapter and the author had put the vacuous first paragraph into the waste-paper basket.

That writers of books on gardening may waste their readers' time by N.C.D. is not perhaps a very serious matter. A more serious question is to what extent the same thing happens in books of a more academic character. To get a complete quantitative answer to this question would require arduous and rather tedious research which has not yet been carried out. I have only a general impression based on my knowledge of the literature of psychology, and my impression is that it is more prevalent than one might hope. I noticed it first in my own writings; successive revisions reduced its amount. I was afterwards surprised to find that some other writers on psychology seemed to tolerate it to a much larger extent.

Much N.C.D. is to be found in writings which would be generally recognised to be not in the academic tradition. There are books of popular psychology written by those without academic training in psychology, and without knowledge of the results of psychological research. They are commonly aimed at uplift or self-improvement (both admirable targets). Much of their material, however, is found to be N.C.D. of various types. There are such vacuous affirmations as: 'The young child learns to use the word "Mama" to refer to his mother', 'The student whose mind is wholly engrossed in a mathematical problem is not aware of what is happening around him', or 'A stone is not conscious of its surroundings'. One must not, of course, condemn these affirmations as vacuous merely because they look like statements of mode No. 1 but do not make any real communication as to what is the case; they might be serving some other communicatory purpose. One can only decide they are vacuous when an examination of their context shows that they are not serving any communicatory purpose at all.

What strikes one at first sight as vacuous affirmation may, for example, turn out to be serving a communicatory function in demonstrating a new or old linguistic usage. Early in Stout's *Manual of Psychology,* one finds a statement that 'so far as man is wholly absorbed in attending to the movement and sound of the waves, he is not attending to himself or to his own acts of watching and listening'. At this point the reader may condemn it as N.C.D. Certainly the statement by itself is tautological and non-communicating. But two sentences further on, its communicatory purpose is found to be that of demonstrating a linguistic usage: it illustrates how the author proposes to use the term 'objective point of view'.

In psychology, as in other branches of science, the application of experimental method has led to a change of emphasis. From being primarily concerned with communicating the usage of a linguistic system, its texts have shifted to being primarily concerned with communicating information in the narrower sense of what is the case. Still, they use a technical vocabulary partly common to all writers on the subject and partly peculiar to the author. It is still necessary also to make some communications in mode No. 3 conveying linguistic usages, and it is necessary to understand when statements have this function so that they may not be rashly condemned as non-communicating.

The same considerations apply to the seventh mode of communication, that of giving an explanation. One widely-used method is to make assertions on matters already familiar to the hearer in the hope that he will see an analogy between these and the unfamiliar situation that is being explained, and so that his puzzlement may disappear. Thus, the physics master, explaining the relation between the resistance of a wire and the area of its cross-section, may say: 'If you had water running through a narrow channel, more water would flow if you dug the sides of the channel and made it wider.' This presumably is a fact already familiar to his hearers but the statement of it is saved from being a vacuous affirmation by its communicatory function in the mode of explanation.

In the less popular types of scientific communication, N.C.D. is relatively uncommon. One would not expect to find it in an article written for a technical journal; one would hope not to find it in a scientific textbook. Sometimes this hope will be disappointed; to maintain a continuously high level of communicatory efficiency demands vigilance on the part of the author and this vigilance is sometimes relaxed.

I will illustrate this possibility by reference to a single modern work which is generally recognised as representative of a rigidly scientific school of psychology. This opens with the sentence: 'Men act upon the world, and change it, and are changed in turn by the consequences of their action. . . .' This is more sophisticated N.C.D. than 'We are gathered together in this great hall' and needs] perhaps a second reading before it is realised how vacuous it is. It is not only the opening sentence; the paragraph goes on saying nothing in a highly sophisticated vocabulary: 'Certain processes which the human organism shares with other species alter behaviour so that it achieves a safer and more useful interchange with a particular environment.' It is not until the fourth paragraph that communication begins with a linguistic statement about how the author proposes to use the term 'verbal behaviour'. The preceding three paragraphs do not tell any previously unknown facts; they do not introduce a new way of talking. Nor do they seem to be in the fifth communicatory mode (i.e. explaining); the linguistic statement at the beginiing of the fourth paragraph would have been just as clear without them. Unless these paragraphs serve some communicatory purpose that I have failed to detect, it must be supposed that their distinguished author has failed to notice that he has succumbed to the temptation to dilution by N.C.D.

If one defines the communicatory efficiency of speech or writing as the percentage of it that is not N.C.D., there is an obvious research problem of what communicatory efficiency different authors achieve. I suppose it would rank from near zero in the popular orator or writer to near 100 per cent. in most technical writing. I have not done that research but merely scratched here and there to convince myself that there is a problem. I hope someone else will take it up more systematically. I should not recommend it as a topic for a PH.D. since distinguished corns might be trodden on. More suitable perhaps for one who has retired; the results might conveniently be published after his funeral.

It is a defect in the programme of research here suggested that we have at present only subjective and disputable criteria for the identification of N.C.D. I do not see any immediate prospect of making the rules for its identification so definite that they could be programmed on to a machine which could then carry out the process of identifying N.C.D. automatically. When the difficulties of such programming have been overcome, we shall have not only a research instrument of value but also a practical process of great usefulness. We can imagine, in the future, a statesman about to make an election tour. Before he leaves, his secretary says: 'We have just run your fifty-minute speech through the N.C.D. eliminator. You will be happy to know, sir, that it is now altogether communicatory and that you will be able to deliver it in three-and-a-half minutes.'

The practical purpose of the study of N.C.D. is that knowledge of its nature may help to eliminate it, at any rate to eliminate it where it is inappropriate. The rationally-oriented individual need not be fanatical about this; it is not his purpose to get rid of non-communicating social interchanges, such as small talk and remarks about the weather. No one is liable to mistake these for communicatory social interchanges and they are not interfering with the proper use of language for communication. Where he has a right to be intolerant is when he finds himself sitting through speeches at political meetings, banquets and in church which are at the same communicatory level as remarks about the weather. He is not opposed to political speeches, after-dinner speeches or sermons. About politics and religion there is obviously much to be communicated; it is very desirable that those speaking on these topics should be trained to communicate and to avoid using language in a way that does not communicate. After dinner, the communicatory function is likely to be that of greeting or compliment; it is reasonable to ask that the use of language should be restricted to serving those functions and that, when it has served them, the speaker should sit down and not feel under compulsion to go on talking. Also, the rationally-oriented individual will welcome creative oratory in any of these contexts, but he will not tolerate N.C.D. as a substitute for this however well it may be delivered.

If it is agreed that the reduction to a minimum of N.C.D. is a worthwhile practical aim, it may be suggested that the place to start is the school, probably in the sixth form. A topic called 'The Use of English' is becoming of increasing importance; its aim is to increase skill in the using of language as a means of communication. Exercise in the analysis of the modes of communication used in a particular piece of writing (on some such lines as those suggested earlier) might be found to have a value in sharpening the pupil's awareness of what is being communicated and of when nothing is being communicated. So it may be hoped he would learn not to tolerate non-communicating discourse in his own writing and speech, and to be somewhat intolerant of N.C.D. in writing and speech of which he is the consumer.

It must be agreed that the most moderate programme of increasing communicatory efficiency by suppression of N.C.D. would not be approved by everybody. The prevalence of N.C.D. in some situations must mean that some people like it. In such situations they may prefer to be lulled into a somnolent condition by the expected non-communicating word-sequence, and might resent being told anything with which they were previously unfamiliar, whether it was a fact, a duty, or an evaluation. I hope, however, that a sufficient number of people would agree with me that a world in which a speaker sat down when he found he had nothing more to say would be a better world than one in which he went on talking.

10 A Classification of Rules for Writing Informative English*

by I. J. GOOD

If we wish to mechanise a process usually performed by human beings it is advisable to begin by classifying the human process into small units, even if, in the end, the machine uses very different methods from the human ones. Such classification is rather conveniently set out in the form of a decimal classification, which is logically equivalent to a 'tree'. This form has the two advantages of ease of cross-reference and of ease of detection of omissions. Any such classification is of theoretical interest to cyberneticists, even if the tree is not subdivided down to its atomic components. In the present paper the process selected for classification is that of writing informative (technical) English. Nearly everything in the paper applies equally to any other language.

When we aim to mechanise a human process we may begin to understand the process better and may find that the main fruits of our labours are to enable human beings to carry out the process more efficiently. This will probably be one of the effects of the attempts to mechanise translation, for example. Likewise the main value at present of the classification given below will be for educational purposes. To take the classification down to its

* This paper is essentially a reprint of a paper in *Methodos*, volume 7, pages 193–200, 1955; and is largely based on KAPP, R. O.: *The Presentation of Technical Information:* Constable. It is reproduced here with the permission of the publishers of *Methodos*. A similar classification has been made of fallacious arguments (*Methodos*, volume 11, pages 147–59, 1959; reprinted in *Technometrics*, 1962).

atomic components would be a far too ambitious task in the present state of knowledge. In fact it would be much more difficult than the corresponding problem for the process of translation. The tree for mechanical translation would however have many twigs in common with that for writing informational language.

The classification may be of some value as a check list for teachers of English composition. It is possible that similar classifications, more or less simplified, could be used by students. Many students may derive some interest from finding out where their own mistakes fit into the classification, especially if the format is made attractive.

A heading to which this classification itself inevitably cannot completely conform is number 222.261.

1. *Auxiliary aids.*
    11. Reference works.
    12. Stationery (e.g. loose-leaf books and cards, including punched cards; no rules will be given here for the use of punched cards).
        121. Check lists.
    13. Shorthand. (Here and elsewhere the advantage should be weighed against the cost in terms of time or money.)
    14. Machines.
        141. Typewriters.
        142. Tape recorders.
    15. Secretaries and wives.
    16. Friends (preferably critical).
    17. Printers.
    18. Publishers.

2. *Classification of the writing of a single work, roughly in time order.*
    21. Planning.

211. Planning large units.

211.1 Classification (taking into account the probable readers).

211.11 Determination of correct sequence. (*See* Notes A and B.)

211.12 Continuity (transition).

211.13 Qualifications.

211.131 Avoidance of qualifications.

211.132 Warning of qualifications to come.

211.133 Separation of a passage from its qualification.

211.2 Making notes, possibly loose-leaf.

211.3 Use of classes 1 and 3.

211.4 Sense of proportion.

211.41 Inclusion of important points.

211.42 Not elaborating the obvious

211.43 Not emphasising unimportant details, especially if wishing to influence a decision. But unimportant details may be included for the purpose of preserving a record.

211.5 Simplicity.

211.6 Conciseness: sheer bulk can deter the reader, but see class 222.261. (When in doubt leave it out.)

211.7 Summaries. (*See* Note C.)

211.71 Pre-summaries.

211.711 Preface. (*See also* class 34.)

211.712 Introduction.

211.713 Contents.
211.714 Successive expansion. (*See* Note D.)
211.72 Post-summaries.
211.721 Conclusions.

212. Planning small units.
212.1 Sub-classification.
212.11 Determination of correct sequence: in particular, the avoidance of irrelevance.
212.12 Continuity and the avoidance of brackets. (Footnotes are best introduced at the ends of sentences.)
212.13 Qualifications.
212.131 Avoidance of qualifications.
211.132 Warning of qualifications to come.
212.133 Separation of a statement from its qualification.
212.2 Making notes. (*See* Note E.)
212.3 Use of classes 1 and 3.
212.4 Sense of proportion.
212.41 Inclusion of important points.
212.42 Not elaborating the obvious.
212.43 Not emphasising unimportant details, especially if wishing to influence a decision. But unimportant details may be included to protect the record.

212.5 Simplicity. (For example, in a logical argument each step would be nearly obvious. This does not contradict 212.42. One can afford to be very complete provided that 222.27 is obeyed.)

212.6 Conciseness. (But *see* 222.261.)

212.7 Explanation of the unfamiliar in terms of the familiar: otherwise can it be called explanation? (*See* 238.)

212.71 Metaphors. (These should be apt, fresh, and very easy to understand. The best ones are apt even when expanded. It should be made clear that a metaphor is really intended to be one.)

212.72 Explanation of significance. (This includes alternative, less rigorous, explanations of a logical argument, and physical comment in a mathematical argument.)

212.8 Summaries and generalisations. (*See* Note C.)

212.81 Pre-summaries, e.g. chapter or paragraph preambles.

212.811 Definition of the circumstances.

212.812 Definition of the theme.

212.813 Reminder of knowledge. («It will be recalled that . . .»)

212.814 Subsidiary reminder of knowledge.

212.815 Designation of method (e.g. denial of intention: *see* Note F.)

212.816 Generalisations. (*See* Note G.)

212.82 Post-summaries. (These are often memorable generalisations, possibly half-truths: «Only a half-truth can be expressed in a nutshell.»)

22. Execution of the body of the work in detail.

221. Linguistic problems.

221.1 Verbal, i.e. problems concerning individual words. (Use Roget's *Thesaurus* or similar work.)

221.11 Circumlocutions. (*See* Note H.)

221.12 Jargon.

221.13 Definitions.

221.14 Glossary, perhaps incorporated in a Glossary-Index.

221.2 Grammatical. (Details omitted from this classification.)

221.21 Verbal grammar.

221.22 Punctuation.

221.23 Paragraphing.

221.3 Style (*See* 222.29.)

222. Psychological (emotional) problems. (These also occur under heading 21.)

222.1 The writer's psychology.

222.11 «Resistance». (For example, on reading through this classification one may tend to regard as unimportant the points in which one is least expert.)

222.12 Complacency (to be countered by self-criticism, without necessarily becoming a Marxist).

222.13 Impatience. (The writer, if impatient, may start to write before the planning is sufficiently complete. He may rationalise his impatience by thinking he will save time.)

222.14 Procrastination. (The opposite of 222.13.)

222.15 Carelessness. (Countered to some extent by class 24.)

222.16 Wishful statement and exaggeration.

222.2 The reader's psychology.

222.21 Taking into account the probable readers. (This is important over a wider field, e.g. in 211.1.)

222.22 «Resistance» arising from professional pride and from intellectual and other vested interests.

222.23 Associations. (Cf. 212.7 and 212.813.)

222.24 Understanding, i.e. associations supplied by the reader. (This class is hardly required, since it is what most of the classification is about.)

222.25 Memorising. (Preparatory phrases: *see* Note J; judicious repetition, perhaps containing a little that is new; memorable phrasing, such as mnemonics, epigrams and slogans; remarks awaited by the reader, e.g. if he is first asked a question. This class resembles 222.24, but is easier for the writer to overlook.)

222.26 Proper pace. (Not too much in one sentence, especially important in lectures; moments of quiescence.)

222.261 Justifiable padding. (*See* Note K.)

222.27 Be stimulating, perhaps even at the expense of breaking some of the other rules occasionally.

222.28 Comment words. (It is possible to be too objective.)

222.29 Some slight pandering to snobbery, though this may lead to conflict with simplicity and clarity. Avoidance of condescension.

223. Same as 212.

23. Trimmings for ease of reference.

231. Contents.
232. Index.
233. Bibliography.
} (Mentioned elsewhere.)

234. Headings.

235. Section numbers at the tops of pages.

236. Possibly the use of the Universal Decimal Classification, or something similar.

237. Numbering of the equations, theorems, etc. in mathematical work. Perhaps repeating equations instead of merely referring back to them by number.

238. Pictures, including graphs. (Not too much detail, cf. class. 212.5: this is especially true of lectures.)

24. Revision. Leave a good time-interval. Complete rewriting may be advisable.

3. *Processes that continue all the time when writing a given work.*

31. Running index (on cards, say).
32. Running bibliography (on cards, say).
33. List of things to do: very important.
34. Writing and rewriting of Preface, corresponding to changes in intentions.

4. *Methods of learning how to write.*

41. Practising.
42. Being self-critical.
43. Reading some good books on writing.
44. Getting critical friends to read your work. (Cf. 16.)
45. Rereading the present classification, mentally expanding it.

Notes.

A. Can be done with the help of cards.

B. For example, when describing apparatus think whether to describe its purpose first.

C. A certain American mathematics instructor says, 'First I tell 'em what I'm going to tell 'em, then I tell 'em, then I tell 'em what I told 'em.'

D. I.e. covering the whole ground in successively greater detail.

E. Including notes made while writing, e.g. marginal squiggles to indicate passages known to be, or suspected of being, badly written at the time of writing. The use of marginal squiggles enables the writer to avoid too early a pre-occupation with details.

F. By 'denial of intention' is meant a warning that a method of interpretation that may very well occur to the reader, reasonably or by mere association of ideas, is not intended.

G. Generalisations should usually be supported by means of examples. An example should be a true example, and not an analogy, should be free from extraneous associations, and should deal with matters that are thoroughly familiar to the reader. Consider whether to put the generalisation at the beginning or the end of a string of examples. (Cf. 211.11 and 212.11.) A generalisation should perhaps be put at the end of a string of examples if it requires thought in order to be accepted.

H. For example, 'in the case of', 'from so-and-so's point of view', 'involves'. (It is often possible to delete all 'involveds' involved.) Circumlocutions are attempts at dodging (*a*) saying something more neatly; (*b*) searching for a correct noun; (*c*) searching for a correct preposition; (*d*) recasting a sentence that has begun badly.

J. Use of phrases such as 'to sum up', 'in fact', 'thus', 'in short', 'let me repeat'. These are beacons marking the entrance to still water. Other examples of beacons are the colon, the appearance of the end of a paragraph, a short sentence followed by a long one.

K. By 'justifiable padding' is meant something inserted in order to keep the reader's mind on the point previously made, the insert being something that the reader could have thought of for himself if he had paused. (Without the insert he may not have paused.)

Summary. A preliminary to any attempt to mechanise a human process should be a classification of the components of the process, even if in the end the machine uses unhuman methods. Hence, classification of the components of any human process is of interest to cyberneticists. The present paper is an attempt to classify the rules for writing informational language, but the classification is taken only far enough to be of probable educational value, i.e. for improving human beings rather than for replacing them.

The lay-out is in the form of a decimal classification, which is logically equivalent to a 'tree'. But cross-referencing introduces loops into the tree.

11 Deliberate Misplints

by I. J. GOOD

The numerical analyst, L. J. Comrie, used to discourage the infringement of copyright of mathematical tables by introducing deliberate errors in the least significant digits of some of the entries in his tables. I wish to suggest another application of deliberate errors.

The accuracy of proof-reading varies greatly from one author to another, also from one time to another for each author. It would be possible for an editor to keep a check on the accuracy by having one or two deliberate errors per page introduced in the galley proofs. Each error could be a single wrong letter, so that it could be easily corrected by the printers, but other types of error may be found to be more useful. Both the printers and the editor would have an index of the errors. It might not be tactful to tell the authors of the plan, although if they were told they might be encouraged to be careful. If the plan became fashionable the authors would mostly know about it anyway. Each page on which the deliberate errors had been missed by the authors could be checked extra carefully by the editor, or possibly could be returned to the author for a recheck.

12 Technical Glossary*

'It has long been known that . . .'	I haven't bothered to look up the original reference.
'While it has not been possible to provide definite answers to these questions . . .'	The experiments didn't work out, but I figured I could at least get publication out of it.
'High purity . . .' 'Very high purity . . .' 'Extremely high purity . . .' 'Super purity . . .'	Composition unknown except for the exaggerated claim of the suppliers.
'. . . accidentally strained during mounting.'	. . . dropped on the floor.
'. . . handled with extreme care throughout the experiments.'	. . . not dropped on the floor.
'It is clear that much additional work will be required before a complete understanding . . .'	I don't understand it.
'Unfortunately, a quantitative theory to account for these effects has not been formulated. . . .'	Neither has anybody else.
'It is hoped that this work will stimulate further work in the field.'	This paper isn't very good, but neither is any of the others on this miserable subject.
'The agreement with the predicted curve is excellent.'	Fair.
'. . . good.'	Poor.
'. . . satisfactory.'	Doubtful.
'. . . fair.'	Imaginary.

* The first eight 'definitions' are due to McClimont, William, and first appeared in *Shipbuilding and Shipping Record*, volume 91, page 301, 1958; and the next twelve to Graham jun., C. D., and first appeared in *Metal Progress*, volume 71, pages 75–6, 1957. The last two are new. I am indebted to the two authors and two publishers for permission to re-use the material. *G. Ed.*

'As good as could be expected considering the approximations made in the analysis.'	Non-existent.
'Of great theoretical and practical importance.'	Interesting to me.
'Three of the samples were chosen for detailed study.'	The results on the others didn't make sense and were ignored.
'These results will be reported at a later date.'	I might possibly get round to this sometime.
'Typical results are shown.'	The best results are shown.
'Although some detail has been lost in reproduction, it is clear from the original micrograph that . . .'	It is impossible to tell from the micrograph.
'It is suggested . . .' 'It may be believed . . .' 'It may be that . . .'	I think.
'The most reliable values are those of Jones.'	He was a student of mine.
'It is generally believed that . . .'	A couple of other guys think so too.
'It might be argued that . . .'	I have such a good answer to this objection that I shall now raise it.
'Correct within an order of magnitude.'	Wrong.
'Well known.'	(i) I happen to know it; (ii) well known to some of us.
'Obvious', 'of course.'	(i) I was not the first to think of it; (ii) I also thought of it independently, I think.

13 The Future of International Languages

by ALAN J. MAYNE

Van der Burg[1] has classified languages as natural, artificial, and synthetic. In the last part of his book, Bodmer[2] has given an excellent detailed exposition of the principles on which an artificial language should be based. The most important of these are simple grammar, clear and easy syntax, and as wide as possible a use of words already used internationally, mostly having Latin or Greek roots. Although this problem has not yet been solved satisfactorily, Peano's *Interlingua* and Hogben's *Interglossa*[3] are reasonable first approximations to it, the latter being somewhat better. The devising of a first-class artificial language would need a considerable co-operative effort among a variety of scientists and other experts but would not be as difficult as the preparation of a first-rate synthetic language, nor would it allow such accurate communication. Nevertheless, it seems to be an important project for the near future. If it succeeded, it would probably rapidly become popular among scientists because of its ease of learning and its use of many words already familiar to them; a formal introduction of it might well be unnecessary, as it might 'catch on' just as a result of a few scientific publishers deciding to start using it. Its spread among other professional groups and traders might also be rapid.

As van der Burg points out, the preparation of an 'ideal' synthetic language will be elaborate and difficult. It might take several decades to devise in a fairly full form; but there is a prospect of faster advance because synthetic 'languages' are already being widely and internationally used for limited purposes. The most notable examples are mathematical, chemical, and musical notations, and biological nomenclature. Unfortunately, communication cannot occur at present in these subjects without also using a considerable amount of natural language. The most urgent task for developers of synthetic languages is to remedy this defect; an extremely useful first project would be the development of a set of international signs to cover all the common words at present used by mathematicians, both essential connecting words and words for basic concepts, thus making much mathematical literature readily understandable without learning any foreign natural language.

It is noteworthy that the most successful synthetic languages so far devised do not have a universal spoken representation; in this respect they resemble written Chinese. Bliss[4] has tried to apply this principle to devise an international language for ordinary people as well as for the intelligentsia. His basic principle is to use a few fundamental signs, representing really important and widely-used concepts, together with supplementary signs giving very simple but suggestive pictorial representations of everyday objects and living organisms. Pictorial signs of this sort could well become a very useful international medium of discourses for many simple purposes such as travelling, shopping and feeding. But it would be extremely hard to convey the more abstract concepts.

In the fairly near future then, either English or a well-devised artificial language might become a universal medium of scientific and perhaps other intellectual communication, while a simple sign system will quite probably be used round the world to cover the most elementary conversational needs of ordinary people. During the rest of this century, synthetic written languages (and machine languages used by electronic computers) may become comprehensive as well as universal in specific scientific and other technical disciplines, perhaps eventually to merge in a synthetic language system with much wider scope. A spoken synthetic language will be achieved much later, if at all!

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14 Anglo-Russian Loglan

by I. J. GOOD

The main feature of the artificial language Loglan* is that its vocabulary is made as 'learnable' as possible to speakers of the eight most frequently used languages, each language being given, in a certain sense, a weight proportional to its frequency of use. Although Loglan is not intended to be an international language, but only an aid for investigating the relationships between language and thought, this feature of learnability is obviously highly desirable in any international language. But, in the present state of the world, only Russian and American English should be taken into account, and a modified form of Loglan should be designed with this in mind. It would then obviously be much easier for Russians and Americans to learn than the unmodified Loglan is, and it could be used as an international language.

Although Anglo-Russian Loglan would not explicitly cater for, say, the Chinese, even for them it would have a saving grace. For, after learning Anglo-Russian Loglan, it would be easier than it was originally to learn Russian and English.

The language should be expressible in both the Roman and Cyrillic alphabets reduced in size. Then, for example, an American would learn the language in the Roman alphabet. After he knew the language he could use his knowledge in order to become familiar with the Cyrillic alphabet. He would then be in a favourable position for learning Russian if he wished.

* *See,* for example, BROWN, JAMES COOKE: 'Loglan': *Scientific American,* volume 202, pages 53–63, 1960.

15 A Synthetic Language*
by A. R. VAN DER BURG

Types of Language

Most languages are *natural languages*, which have grown in the course of history. They are quite irregular and unsystematic, and they must be learned word by word. Besides natural languages, there are *artificial languages*, such as Esperanto, which are based on natural languages. In a third group are the *synthetic languages*, completely built up from first principles.

Conditions for an Ideal Synthetic Language

An ideal language, which can be used for all types of communication, oral, written, and with the aid of a machine, should satisfy the following conditions:

1. The language must be comprehensible on the basis of its system and not on the basis of learning its words. This means that it must be possible for everybody who knows its construction and the structure of the relevant concepts and relations to express himself in it without having seen or heard the needed words before.
2. The words must be pronounceable for all people in the world. This implies that certain sounds which are difficult to pronounce for certain people cannot be used.
3. The words must be intelligible to all people, that is, the sounds must be clearly different and the meaning of a word must not be sensitive to small deviations in pronunciation.
4. In the same way, the spoken language must be intelligible to a machine.
5. The message must reflect the meanings of the sender in a

* Some of the ideas in this contribution, or very similar ones, were also submitted by José Serrano Camarasa. *Ed.*

simple, unambiguous and exact way to any receiver who understands the language.

6. The spoken and written words must be as short as possible, consistently with the other conditions.

7. Each written sign must correspond with one sound or combination of sounds. In certain cases, one sound may be represented by a combination of signs.

8. The number of signs should be made an economic optimum.

9. The signs must be simple and clearly distinguishable by the human eye and by a machine, even when there are small deviations from standard.

10. If possible, signs used for machine-coding should be the same as signs used for reading purposes.

11. The language should have a purely functional grammar. Few, if any, irregularities are permitted in it.

12. Mechanical translation from signs into sounds must be possible.

Phases of Action

A new language of this sort would have to be prepared in successive stages, the most important of which are as follows:

1. A systematic survey of all human concepts. The number of concepts is of the order of one million.

2. Similarly, all existent relations between concepts should be systematically arranged as far as possible, although this ideal can never be completely attained.

3. Cataloguing of all technical aids, varying from a pen to the most complicated computer. Some idea must also be obtained of future technical developments in this field. For each aid, a list should be made of the conditions that it imposes on the language.

4. Design of optimal sounds and signs, taking into account conditions 1 to 4.

5. Representation of all concepts and relations in sounds and signs according to a general principle.
6. Elaboration of the system for all fields of application.
7. Testing and correction.
8. Designing a programme for introducing the language.
9. Introduction of the language.

Man-power and Cost Needed

The construction of such an ideal synthetic language will demand a vast amount of intelligence and scientific knowledge all over the world. Among the specialists needed for this purpose, there must be: linguists, experts on semantics, psychologists, logicians, philosophers, operational researchers, organisation experts, management scientists, statisticians, computer experts, and other scientists and engineers.

The cost of such a project will amount to several millions of pounds at least, perhaps even a few tens of millions. Introduction of the language all over the world will cost about ten times as much. To make it really international, it would be sensible to have this investigation done under the auspices of the United Nations or Unesco.

Conclusions

Such a synthetic language will never be a substitute for one's own mother tongue, but it can improve communications between all types of people who speak different languages. In particular, it can simplify international scientific contact, not only because it is international but because it is so cheap. It will be cheap, first because it can be learned easily, with little loss of time due to learning many words, complicated grammar, and irregularities; secondly, because it will be compressed both in its spoken and in its written form (no shorthand needed any more!). Printing-costs will be much lower in this new language than in any existing language.

Its logical structure will help to make people's thoughts clearer and more exact. This will lead to less confusion and misunderstanding in the scientific world as well as in everyday life. Thus, this language will make an important contribution to human progress.

This new language has not yet been named, and I am not able to name it, because its name will be an automatic consequence of its own linguistic system!

16 Tunish: the Language for Orderly Man*

by D. S. BLACKLOCK

'For the ear trieth words, as the mouth tasteth meat; let us choose . . . what is good.'

(Job. xxxiv. 3, 4)

There is an urgent need for a simple but flexible language that takes full account of Man's capabilities *and deficiencies* as a receiver, processer, and transmitter of information: a language that can readily be used for storing information electronically for subsequent retrieval. For these requirements I have designed a modified form of English called *Tunish*, and I have tested it both for ease of reading and for phonetic appeal.

Tunish has an alphabet of sixty-three symbols, some corresponding to single phonemes, some to pairs, and a few to the most common syllables. The logical design of these symbols makes them easy to memorise and interpret, and they can be typed conveniently on a keyboard consisting of only eight keys and a space-bar. A pair of corresponding fingers on the two hands, such as the two forefingers, are never both depressed at once, and the

* We have condensed, with the help of Mr Brian N. Lewis, a much longer paper, which dealt with many other aspects. There are hundreds of artificial languages, and we cannot judge the merits of Tunish as compared with the others, but at any rate the author has approached the subject in a scientific spirit. *Eds.*

thumbs are free to operate the space-bar. For ease of learning, the alphabet is listed in a sequence that enables sixty of the symbols to be paired; within such pairs the phonetic link is usually close.

The optimising of the keyboard interacted with the spelling of Tunish, and was solved as a problem in linear programming.

Tunish words tend to be only half the length of words in natural English, mainly because several symbols convey syllabic values, and, for example, because a single symbol can represent very similar sounds.

Many Tunish words are formed in such a way that their spelling has a bearing on their meaning. For example, the word for 'insect' is FL-AN-T, in three symbols: this word would be easily learned by relating it to FL-IE and AN-T, two symbols each.

The vocabulary now contains about 4,000 words, and covers all the main fields of human endeavour.

Speeds of typing of 180 words per minute should be within the reach of people trained with the help of simple electronic apparatus that would regulate the selection of test words, the speed of dictation, and the supply of clues direct to the fingers.

Note that the automatic conversion of typescript to an eight-unit telegraphic tape would be very simple, and a six-unit code would also be possible. The human reading of the eight-unit tape would be easy because there is so much order in the system. The spoken, printed and tape patterns would be concordant.

There is a more intensive use of suffixes than in English, and this greatly simplifies the learning of the language in its spoken and written forms.

The language is not highly formalised: for instance, there is no question of trying to assign a unique meaning to each word. On the contrary, it is recognised that the multiple connotations of ordinary words provide a rich source of new ideas. The new language is essentially practical, with scope to expand. It is simplified, but not impoverished.

Although Tunish has a ternary-code base as distinct from the binary code that is so widely used in electronic computers, it would nevertheless encourage the introduction of computation in radix 16 instead of 10. This would make communication with electronic computers easier.* Less obvious is the possibility of constructing a much more powerful slide-rule in radix 16. It is the wrong time in history for Britain to decide at last to go over to the metric system in her currency!

17 A Metaphonetic Conjecture

by I. J. GOOD

Good's consonant, spelt *p͡t*, is pronounced by curling your top lip over the top row of teeth, putting your tongue against the lip, and making an explosive sound. It is halfway between a *p* and a *t*; for example, the syllable *p͡tip͡t* sounds equally like *pip*, *pit*, *tip*, and *tit*; and *p͡tap͡t*, *p͡top͡t*, and *p͡tup͡t* also each have four interpretations. I conjecture that the initial *pt* of words like 'Ptolemy' and 'Ptah' may at one time have been pronounced by using this consonant, the consonant being at first reserved exclusively for the names of kings and gods. I feel more respectful to Ptah when I pronounce his name this way, but all the same I make the conjecture with my tongue in my cheek.

It is probable that the above conjecture will never be either refuted nor confirmed, since the ancient pronunciation of 'Ptah' is unknown, and it is quite possible that the two initial letters were separated or preceded by a vowel.†

* Radix 8, the octal system, would be preferred by many people, since the learning of the multiplication table is much less strain on the memory. *G. Ed.*

† Information received from the Department of Egyptian Antiquities, British Museum.

18 A Royal Road for Learning Alphabets

by I. J. GOOD

This suggestion is not for serious linguists, but it may be of value to poor scientists who wish to struggle with an occasional Russian technical paper, with a dictionary in one hand; also for Jewish boys preparing for their Bar mitzvahs.

A short book should be published in the English language, but with all the letters transliterated into Cyrillic (or into the Hebrew alphabet). The printing could easily be mechanised, working from the original linotype tape.

It may be that an even more royal road would be the use of a well-designed teaching machine. It is a matter for experiment.

19 The Information Content of a Work of Art

by NELSON M. BLACHMAN

The idea of determining the information content of a work of art – literary, musical, or visual – is an intriguing one. In fact, in his 'Mathematical Theory of Communication' (*Bell System Tech. J.* volume 27, page 379, 1958), Claude Shannon ascribed to *Finnigan's Wake* a high information content: 'Joyce . . . enlarges the vocabulary and is alleged to achieve a compression of semantic content.' (Page 399.) The calculation of the information content is based in this case and in others (*see*, for example, MOLES, ABRAHAM: *Théorie de l'Information et Perception Esthétique:* Flammarion, Paris, 1958) upon the unlikelihood of its being generated by a random process of appropriate statistics. Underlying this calculation is the notion that the work of art is designed to inform and that its novelty is important.

However, the fact that people want to listen to music that they have heard before and know intimately shows that the lasting value of the music does not lie in its unlikelihood or informativeness. Rather, it lies in the feelings evoked in the listener. The same is true of other art forms, though it may be harder to separate the informative from the artistic aspects in some cases. An orchestra is, thus, not part of a communication channel but of a control system. This is particularly easy to see in the case of a jazz orchestra, which may be influenced by feedback from the controlled element – the audience. The object of the system is to induce enjoyment and to evoke pleasurable associations.

Psychologists have been able to implant electrodes in the 'pleasure centre' of the brain of a rat and have found that the animal will sometimes repeatedly press a button that electrically stimulates this centre in preference to any other activity, even to eating when hungry. The human counterpart of this phenomenon would seem to be the repeated insertion of coins into a juke box in order to hear 'rock and roll', which must have a similar effect on the brains of certain adolescents; this 'art form' has a negligible information content, but some hold it in high regard.

A complete description of the feeling engendered by a work of art would contain as much information as the feeling itself, but this amount of information, again, is an unsuitable measure, for people want to experience the work even though (or because) they know exactly how it will affect them; the description is no substitute for the sensation.

Measuring the information content of a work of art would be like determining the channel capacity of a lighting circuit – how fast can the light switch transmit information to the lamp? The value of the circuit bears no relation to its informational capacity; its purpose is simply to provide light by which to see. Its capacity, like the information content of a work of art, is uninteresting. Information theory can be applied properly only to communication channels carrying messages of undetermined utility; works of art *inter alia* must be judged by other standards. [If there were a measure of *gestalt* information it would be relevant to art, but there is no such measure at present. *G. Ed.*]

20 Immortal Art

by I. J. GOOD

All 'analogue' (= continuous) records gradually deteriorate. The only form of storage of unlimited life is discrete (e.g. digital): when it begins to deteriorate it can be regenerated in a mint copy.

I suggest that the greatest works of art should be stored discretely in order that they should have a chance of literal immortality. This suggestion would already be practicable for all musical performances (by means of pulse code modulation). It will become practicable for films and painting in due course, and ultimately even for the legitimate theatre.

3

Minds, Meanings and Cybernetics

21 The Dimensions of Consciousness

by DENNIS GABOR

I want to propose an investigation by certain scientific principles into the non-communicable qualities of the conscious mind, in the hope that such an investigation might assist philosophers, psychologists, brain physiologists and perhaps also those engaged in producing 'mechanical intelligences'.

In one extreme philosophical view consciousness is everything; in another extreme view it is nothing. 'Nobody believes in solipsism and few people even profess to believe in it' (Eddington). Most of us are quite satisfied with a formulation such as this: 'I have a consciousness, which receives sensory data from an outer, real, physical world, and images, concepts and urges from my unconscious mind.' I would not write this if I were not convinced that the outer world contains consciousnesses with whom I can communicate.

These communications are of two types. One is by messages which are entirely in terms of the public world, without any reference to subjective qualities; they could be addressed to a robot as well as to a human being. The messages of the other type contain only hints instead of descriptions, and explicitly assume the existence of consciousness similar to mine. If I write of 'that *frisson* which runs down the spine in intense musical enjoyment' or of 'that paradisical feeling just before waking on a sunny summer morning', I feel sure that at least some of my readers will be able to identify these undescribable feelings. But this sort of reference, so powerful in poetry, is not suitable for a scientific discussion, which must be entirely in terms of the first type of message. The pathway of these messages must be *via* the physical world. We cannot exclude the possibility that after a century of frauds and failures parapsychology might yet establish new ways of communication. As an inveterate rationalist I believe that if such new ways were found, they could be rationalised in terms of physical agencies, perhaps other than those now known.

Nobody denies that *his* consciousness exists, opinions differ only on the point how important or urgent it is to introduce it into scientific discussions. Physical science owes its great success just to the fact that it has concentrated on 'objective' events, and has built out of them a consistent physical world. This does not mean that consciousness has been completely excluded, because we cannot discuss 'objectively' anything which is not contained in at least two consciousnesses. It means only that a discussion is possible without bringing in the *sui generis* qualities of consciousness, such as colour or pain. Physical science was signally unsuccessful so long as it asked questions such as 'the magnet loves the loadstone, but does the loadstone also love the magnet or is it attracted to it against its will?' (An Arab physicist of the twelfth century.) On the other hand, psychoanalysis was highly successful in accounting for the unconscious mind in terms of the conscious mind, such as feeling, willing, love and hatred.

It is the emergence of 'intelligent machines' which has made it so important to have a new look at the old problem of consciousness, and has brought up such teasing questions as 'Can an android feel pain?' (I. J. Good). It is now urged on us from many sides that science will be forever incomplete so long as it does not recognise consciousness in its own right. I have left this sentence intentionally vague, because I can only vaguely apprehend what is in the mind of such distinguished scientists as Hinshelwood and Polanyi when they express this view. What I want to propose is a programme for approaching this aim in the only way which I can conceive as scientific: by making a *logical map* of consciousness. I define consciousness as the ensemble of non-communicable subjective qualities. By a logical map I mean a structure in which certain subjective qualities, the basic, irreducible ones, remain undefined, and are connected with others, composite or derived qualities, by operations which can be entirely expressed by messages of the first type. I want to go a step further and assume that many if not all of the basic qualities can be arranged in ordinal sequences; these then form the 'dimensions' of consciousness.

What I have sketched out is a maximum rather than a minimum programme. In a minimum programme the operations themselves would be left undefined, and characterised only by their abstract group structure. (Cf. Eddington, *New Pathways of Science.*) But this would be far too difficult for a start, and should be undertaken only if the first programme, unexpectedly, broke down.

As a first rough division our consciousness contains sensory data, images, and concepts derived from these, values and drives. Let us start from that small corner of the map which is already completed and can serve as a model for the rest: from the three-dimensional manifold of light and colour sensations.

Colour is a paradigma of a subjective sensation *sui generis*. It is impossible to convey to a colour-blind person the separate qualities of red and green, but we can find out by objective experiments whether he can order his colour sensations in two dimensions or in one. The problem has found its objective solution in the Maxwell-König colour triangle, and even more completely in the three-dimensional non-Euclidean geometry of Schrödinger. We can consider it as established that the normal person can completely order his light sensations in three dimensions. (Supernormals with more than three have never been found.)

The other sensory data have not been so successfully analysed.

We know that smell and taste have four dimensions, but we cannot as yet allot a definite dimensionality to sound sensations. There is good evidence that speech is analysed by the ear and brain in six dimensions (plus time, which is a common dimension with other senses), but we cannot as yet set a limit to the dimensionality of musical sensations. Tactile sensations are ordered in three dimensions, and these form the 'primary space' which is brought to conformity with the space of light and sound sensations in a way which is far from simple.

Though the map of the sense data is not yet complete (it will be a long time before we have a satisfactory theory of visual *gestalten*), we can consider it as complete for the purpose of the present investigation. We can also pass quickly over the major part of the 'population' of the conscious mind: images and concepts. These can be derived by 'public' operations from the sense data. When in doubt we must ask ourselves 'Can a machine do it?' If the answer is in the affirmative, the image or concept is considered as a derived one, otherwise we have to look further, to the qualities which I have roughly classified as 'values or drives'. A square is obviously a derived concept, a 'beautiful face' is not.

In order not to miss anything, I would recommend the investigator or his assistant to take a dictionary and make a list of words with a subjective component in their meaning. He will find a surprisingly small number of nouns, but a rather large number of adjectives.

We come now to the most important, but also most difficult, part of the work, to the analysis of values, emotions and drives. If I write down a sentence such as 'Pain is the most horrible reality, yet physical science ignores it altogether' I have used two subjective terms, 'pain' which is a drive, and 'horrible' which is a value, while the rest belongs to the public language.

The aim of the analysis is to reduce the number of undefinables to a minimum. I do not want to interfere with those who might undertake the work of giving a full baking to my half-baked idea, but it is a fair guess that all values such as 'horrible' will turn out to be derivable from emotional drives such as 'pain' or 'anger'.

Pain is used in two senses, for physical pain and for the mental pain which is felt in bereavement or in frustration. Both have displeasure as their common component. (In German *Unlust.*) Are these two, basic undefinables, or are both derived from one elementary dimension, the axis pleasure-displeasure, by adding the qualifications 'located in the body' and 'located in the mind'? I must leave the decision to the 'baker' but at this point I wish that he were not only a linguist, a logician and a master of introspection, but also a learned zoologist, because it will help him to know that there are low organisms which obviously follow urges, without any evidence of nerves of pain.

Assuming that the investigator can clear this hurdle, he can then contemplate the drive of *fear*. This leads to an interesting dilemma. Fear can be the conscious anticipation of a painful future, and as such it is logically derivable from pain or displeasure, because we can construct machines which anticipate. But fear can be an anxiety without a conscious cause, and not only in neurotics. Does this mean that we must give anxiety an extra dimension, or do we prefer to explain it as unconscious anticipation? My preference is definitely for the second choice. The unconscious mind is a tremendous fact which we cannot dodge; it is like a large office with a great number of clerks in it, each with his 'own time', while the conscious is like the Director's office, in which only one clerk can report at a time, to avoid confusion. If we take a step beyond the behaviourists who treat the individual as a 'black box' and admit introspection and subjective qualities, we are stopped at the threshold of the unconscious which we cannot treat otherwise than as a black box. This, of course, brings a somewhat unsatisfactory metaphysical element into any such investigation, but we cannot help it; the conscious mind is not a complete system.

The investigator can then go on with the investigation of other emotional terms such as hope, desire, willing, hatred, etc., showing up their interconnexions, if any, or recognising them as undefinables if necessary. There is one more bit of advice I can give him, and this is to study what little we know of drugs which have specific effects on the mind, in particular on specific emotions. He may then try to map the effects of the drugs on top of the map of emotions; any coincidence might be of interest. Great caution is of course necessary in sifting the evidence, which is as contradictory as possible, and new results more often annul previous ones than not. (For instance it is now established that adrenalin *need not* make people angry.)

Good luck to the 'baker' whoever he or she may be!

22 Clues to an Understanding of Mind and Body

by MICHAEL POLANYI

I have written on various occasions about the stratification of the universe, and have analysed both the logical structure of our knowledge concerning consecutive levels and the way in which the workings of two such levels are related. By way of introduction I shall sum up the results of this inquiry in one paragraph, hoping that what follows later will make its meaning clearer.

Each level of existence is a comprehensive entity rooted in the level below it. When we focus our attention on a higher level we rely on our awareness of its particulars (forming a lower level) on which we are not focussing at the time. The higher level relies for its workings on the laws governing the particulars of the lower level and these laws also limit the range of the operations on the higher level and account for its failures. But the operations of the higher level cannot be accounted for in terms of the laws which govern the lower level, hence the examination of the particulars of the lower level does not reveal the laws of the upper level. The higher level is unaccountable in terms of its particulars.

The extent to which the particulars of a lower level are specifiable, and to which their connexion forming a comprehensive entity can be explicitly described, varies from one type of system to another. A machine can be taken to pieces and each of its parts be examined in itself. This will not tell us how they combine to make the machine work, but the working relation of the parts can also be explicitly stated, in terms of engineering.

Specifiability is much more limited in other cases. When we recognise the physiognomy of a person and can say that it expresses fear, anger, boredom, or alertness, or when we make a difficult diagnosis, or exercise expert knowledge in identifying an unusual specimen of some kind, we are relying on our capacity to recognise a comprehensive appearance without being able to say what exactly are its particulars and how they combine to a comprehensive entity. This is true also for comprehensive entities of a practical kind, achieved by the exercise of a skill. We may swim, ride a bicycle, play the piano or the violin, without being able to identify some of the most important elements which constitute these actions, nor be able to tell what rules we are following in effectively combining these elements. And diagnosing and testing go together; skilful knowing and doing are always combined.

Some of the particulars of a lower level, and in certain cases all of them, may be observed in themselves, but thus observed, they convey no comprehension of the upper level. On the other hand, when the particulars on the lower level are noticed as clues to the comprehensive entity on the upper level they are not observed in themselves. These two kinds of awareness are mutually exclusive: we can either observe a particular uncomprehendingly, or read it as a clue to a higher entity which it signifies to us.

The higher animals form a particular class of comprehensive entities: they are distinct individuals governed by active centres. Embryological development is controlled by centres of growth; the vegetative functions on which life depends are controlled by the autonomous nervous system; the central nervous system has motoric and sensory centres and contains, less localisably, centres of intelligent behaviour. Finally, on the highest level, we recognise the human person as the centre of responsible judgement.

Thus, the particulars of the centrally-controlled individual are the workings of its centres. This makes for a two-way relationship between these particulars and the comprehensive entities formed by them. The animal appears constituted by the integration of particulars our awareness of which conveys to us our understanding of the animal as a centrally-controlled individual. We observe a person's mind by reading the workings of his mind.

I have said that we can never be sure of identifying the particulars of a physiognomy. I may add now that they could never be identified at all except by previously attending to the physiognomy as a whole. Since the workings of the mind form such a physiognomy, the behaviourist programme of attending to the particulars controlled by the mind, instead of to the mind, is impossible and indeed absurd. Ryle's conception of the mind, which identifies it with its workings, is equally unacceptable, since it fails to distinguish between the observation of these workings and the reading of them, though these two are mutually exclusive.

Growth and physiological functions are totally or predominantly unconscious, but we are usually aware of our own motoric and intellectual efforts. Yet we do not normally observe our body or our mind while we are using them. Instead, we are aware of our own exertions of mind and body in terms of the purposes we are pursuing or the objects to which we are attending. Thus, our awareness of ourselves in action is related to our objectives, in the same way as our awareness of the parts of a comprehensive entity is related to our attention fixed on that entity. Our awareness of our own limbs in action is of the same subsidiary kind as our awareness of another animal's limbs in action; so that, as we live in our limbs in using them, so we live in another animal's limbs in observing his behaviour. This is also how we observe another person's mind. We read the acts of his mind by relying on our awareness of them in the same way as we are aware of our own mental efforts in pursuing our own objectives. All observation of life and mind is convivial.

Two major classes of human artefacts are also known by a manner of indwelling. We observe the principle of a machine by viewing its parts as organs – which is also the way its inventor first imagined the machine. We know a language by attending not to its sounds, but to its words. Indeed the stratification of speech goes further; we know sentences by attending subsidiarily to the words composing them, and we grasp the meaning of a discourse by attending subsidiarily to its sentences. This is how we know a work of art; its purpose is merely to be dwelt in, as in a vivid and intelligible extension of our being.

The fact that we know another mind by indwelling, means that we understand it as the agent of the same kind of understanding by which *we* understand it. The sight of a man's alert eyes and face instantly conveys to us the presence of a conscious, sane and intelligent mind, having the same faculties that we ourselves exercise as conscious, sane and intelligent beings. Principal among these faculties is the capacity to comprehend particulars in terms of coherent entities. Such intelligent combination of two kinds of awareness is an essentially conscious act. The effort to understand something and the subsequent achievement of seeing an aggregate of particulars as parts of a comprehensive entity can be ascribed only to a sentient being; no insentient automaton, however closely it may reproduce the signs of such a performance, can be said to strain its attention and to see certain things one way rather than another. The representation of man as an insentient automaton is indeed as contrary to our observation of other minds as it is to our experience of our own mind.

Remember now Laplace's vision of universal knowledge. He said that if at any moment we knew the positions and velocities of all particles of matter, and the forces acting between them, we could compute the positions and velocities of the same particles at any future or past moment, and thus all things to come and all things gone by would be revealed to us. This mechanical conception of the universe would have to be transposed today into quantum mechanical terms, but it would still recognise only one single level of existence, acknowledging no comprehensive entities nor the ensuing stratification of existence. This raises questions which I shall exemplify by the case of machines. The principles according to which a machine works, cannot be accounted for in terms of physics and chemistry. Yet the machine is an inanimate body. How can it be then that physics and chemistry should fail to describe it fully? And if there do exist superior principles which control its comprehensive actions, how can these fail to interfere with the laws of physics and chemistry which apply to the parts? How can the machine actually rely on the laws of physics and chemistry for performing its functions as a machine?

The answer is that the laws of physics and chemistry do not determine the configuration of positions and velocities in which they start to operate. Laplace himself says that the initial conditions have to be given before the physicist can make any predictions. So any mechanical system can be shaped initially according to principles which are not accounted for by physics and chemistry, and it may then continue to function in accordance with these same principles while relying on the laws of physics and chemistry. A machine comprises two levels of existence because its initial parameters are controlled by the laws of technology which cannot be accounted for by the laws of physics and chemistry.

In so far as the living body functions as a machine, these conclusions can be readily applied to it too. Physiology consists of operational principles relying on the laws of physics and chemistry which control the parts in which these principles are embodied. Physiology cannot therefore be accounted for by the laws of physics and chemistry, any more than the operational principles of a machine can be. The operational principles of living beings are embodied in the parameters left undetermined by physics and chemistry – in the same way as in machines.

It is customary to identify the mechanistic explanation of living beings with an explanation in terms of physics and chemistry. We see now that this is mistaken. We should recognise that a living being, even when represented as a machine, comprises two levels of existence, of which the higher relies on the lower, without interfering with the laws governing the latter.

The only way, therefore, to reconcile a mechanistic conception of the universe with the fact that it has given rise to the evolution of living beings and, eventually, to the emergence of machines, is to assume that these comprehensive entities were preformed by a suitable pattern of parameters within the mass of primordial incandescent gases. Instead of rushing about at random, its particles must have been ordered by such a pattern of positions and velocities as would manifest itself, as the gas cooled down, by producing living beings and the whole evolutionary development, including man and all the works of humanity. Nothing would then be new; in the atomic structure of the primordial gases, the works of Shakespeare could have been legible to a Laplacean mind, provided it understood English.

But the assumption of such an infinitely sophisticated original gas would save the comprehensiveness of mechanics only by abandoning the randomness of thermal motions on which thermodynamics is based. And even so it would be useless. It could explain machines and living beings working as machines, but no ordered pattern of primeval gases could account for the sentience of living beings, since physics and chemistry know nothing of sentience in matter.

Being thus forced to abandon mechanical preformation, we must look out for some acceptable conception of cosmic epigenesis. We find some clues of this in the fact that living beings cannot be represented altogether as machines. This seems true even on the vegetative level; embryological development can repair early mutilations by using material of the embryo resourcefully for the achievement of normal shapes and organs. Next, mutilated animals can immediately adapt themselves by producing suitable patterns of behaviour. These feats have been likened by gestalt psychologists to the way animals and men mentally reorganise the field of experience towards a new purpose. And such primitive integrations contain already the germs of radical intellectual innovations achieved by human genius.

Attempts have not been lacking to represent these innovative functions by machines, but these would at best lead back to a mechanical predeterminism that would ascribe to the primeval incandescent gases of the world a pattern so intricate that in it would be foretold the evolution of all living beings, and all the works of human genius, to the end of time – while it would also represent human beings once more as insentient automata.

The absurdities of mechanical predetermination appear so linked to the absurdity of representing human beings as robots that the two must be eliminated simultaneously. Consciousness must be recognised as endowed with the capacity of acting as a first cause. At every stage of organic evolution at which there emerges a higher level of existence that cannot be logically accounted for by the laws governing the level below it, we must postulate the presence of an essential innovation initiated by a first cause, which, for the sake of continuity, we should endow with some degree of sentience.

Great intellectual innovations are more rapid than those taking place at the lower levels of evolution and they also have the greatest intensity of conscious effort. We may adjoin therefore to the hierarchy of existential levels an increasing intensity of innovations occurring between levels, and a growing consciousness of these innovations.

We observe the centre of a responsible judgement by entering into the thoughts of a person delivering it; we gain a glimpse of great minds by immersing ourselves in their masterpieces. The first causes observed here illustrate the fact that even at the highest level the scope of such causes is restricted. It is conditioned by parochial circumstances of time and place, and its tasks are limited to the exploitation of hidden possibilities of innovations. We may assume then – by continuity – that the task of innovating causes at lower levels is likewise restricted by the range of possibilities. Judging by analogy, we may conceive indeed that these causes are evoked by the proximity of yet-unrealised potential levels of higher integration, just as our awareness of hidden knowledge presents our mind with problems and evokes our efforts to solve them.

One part or other of these conclusions has been anticipated by Butler, Lloyd Morgan, Bergson or Whitehead. Yet I feel that the logical distinction between existential levels lends a new coherence and compulsive force to the argument.

23 Mind and Consciousness

by SIR CYRIL BURT

Every psychologist and neurologist is at times tempted to speculate on the relation between a man's body or brain and what we call his consciousness or mind. Nearly all would agree that science at present has no explanation to offer; a few would maintain that from the nature of the case there never can be an explanation. The suggestion that I want to put forward is a modified version of the old dualistic heresy. To many therefore it will seem just a pointless revival of that 'bifurcation of nature' which is nowadays accounted one of the most unfortunate metaphysical legacies bequeathed to us by that overrated thinker and scientist, Descartes. In its basic form the issue was raised, and some would say settled, sixty years ago by the greatest of American psychologists, William James, when he published his famous essay entitled 'Does Consciousness Exist?' His answer, echoed by a host of followers from that day to this, was in one word, 'No!' The further protests, launched in this country by Whitehead and Russell, in America by the New Realists and Behaviourists and by the 'logical positivists' from Vienna, have by now, we shall be told, exorcised 'the ghost in the machine' for ever.

Certainly if, after the manner of the older empiricists from Locke to Mill, and the later nineteenth-century psychologists who adopted the same standpoint (including James in his earlier writings), consciousness is regarded as a kind of mental stuff – a peculiar and somewhat unsubstantial kind of substance capable of assuming a number of different 'states' – I should heartily agree. But the vast majority of writers appear to have regarded it as the name not of a substance, but of an attribute.

There is an old conception of the *scala naturae* which finds its clearest exposition in Linnaeus's subdivision of nature into three progressive kingdoms: first, merely material bodies, lifeless and mindless (the stars, the planets, 'the whole choir of heaven and furniture of earth'); secondly, material bodies which have the property of life, but not of sensibility or movement (the plants); thirdly, those material bodies which are endowed, not only with life, but also with sensation and the power of voluntary movement (the animals and ourselves). The last class possesses the distinctive attribute of consciousness; the other two do not.

If, however, we stop at this point, we seem to land in a tangle of difficulties which are even more confusing than those we tried to escape. First of all, we are still left with an unresolved dualism: the aim of science, it will be said, is not to leave one unexplained quality side by side with others, but to seek unity beneath diversity: 'Occam's razor is and must be the basic axiom of all scientific theorising'. Secondly, the consciousness of men and of animals is not a property of the creature as such, but only of a special part. So far from pervading the entire body, as the Jewish rabbis taught, it is associated solely with particular portions of grey nerve-tissues, situated, in the case of man, within and on the surface of the brain. It thus seems to characterise certain forms of matter only.

Accordingly, pinning their faith on these monistic principles, the physicists of the classical period tried to secure unity by explaining the universe in terms of a single substance – matter – endowed with a single property – motion (i.e. change of position in space during an interval of time) – obeying one universal law, the law of strict deterministic causation. Already by 1780 La Mettrie's doctrine of *L'homme machine* had become the orthodox creed of the French encyclopaedists; Büchner's *Kraft und Stoff* (1855), which by 1890 had run to eighteen editions and had been translated into thirteen languages, gave the doctrine its later scientific shape. Its simple mechanistic postulates supplied a philosophical foundation for the doctrines of the German materialists – Moleschott, Marx, Haeckel, with their numerous disciples – and so eventually for the reflexologists, T. H. Huxley, Bekhterev and Pavlov. Its early successes seemed to guarantee its truth. 'Anyone acquainted with the history of science,' says Huxley, 'will admit that its progress has meant, in all ages and now more than ever, the extension of the province of *matter* and *causation*, and the gradual banishment from human thought of what we call spirit and spontaneity.'[1] And his words are still echoed by the behaviourists of today.

Now, so long as the brain was regarded as a mere machine, consciousness remained an obtrusive anomaly. There seemed to be only three ways of disposing of it:

1. The older materialists, who still thought of it as a stuff, claimed that it was a mere by-product generated by the physico-chemical processes in the nervous tissue: 'The brain,' said Vogt, 'is simply an organ which excretes feeling as the kidneys excrete urine.'

2. The later reflexologists rightly rejected this spurious analogy. No one, it was argued, can pour John Brown's consciousness into a test-tube for chemical analysis; no electron microscope can detect his visual sensations; no stethoscope

applied to his skull can hear his inner soliloquies: all we can observe is John Brown's behaviour. And behaviour is nothing but a mode of motion, which can be described and discussed in in the language of physical science. 'The time has come,' Professor Watson said, 'when psychology must discard all reference to this intangible something called consciousness.'

3. But there is a third alternative in which the more eclectic type of materialist has taken refuge. Accepting consciousness not as a substance, but as an attribute, he re-interprets it as the inner, private or subjective aspect of what, outwardly, publicly, and objectively, appears as a material process taking place in the individual's nervous system. This, for instance, is the solution advocated by R. J. Hirst in his recent book on *The Problems of Perception*. It is in effect a return to the so-called 'identity hypothesis', which F. H. Bradley, the champion of idealism, once scornfully ridiculed by saying that 'apparently what one observes when one smells an unpleasant odour is just the stinking state of one's own nervous system'. Nevertheless, the theory certainly avoids the cruder fallacies of the older materialists and the behaviourists, and at the same time overcomes a dualism of the Cartesian type – the hypothesis of an immaterial unextended substance called mind which is somehow coupled to a material substance extended in space and called a body or brain. Body, brain, and the physical environment that stimulates them are assumed to be genuine realities; so that this type of monism still postulates a universe of matter, though certain portions, which possess a peculiar 'complexity', somehow manage to be conscious.

However, whether we suppose that the material structures concerned generate consciousness or whether we suppose that they themselves are conscious, it is clear that on either hypothesis such structures can no longer be *purely* material in the accepted sense of that term, i.e. structures composed of the atoms and molecules studied by the physicist and chemist. It is true that Dr J. N. D. Sutherland has recently declared that there is nothing in the life or behaviour of man which 'cannot ultimately be explained in terms of the chemistry of the nervous system'. But this is manifestly false. When an electric impulse stimulates the nerve-cells at the back of the brain, I see colours; when an impulse of precisely the same type stimulates the nerve-cells of my temporal lobes, I hear musical sounds. And these conscious experiences and their differences cannot themselves be described in terms of the chemical changes in any kind of matter. The phenomena and the laws of chemistry have nothing to do with seeing green or red, or hearing a symphony concert. If the brain was really responsible for such characteristics or could generate such experiences, then it would be much more like what is popularly termed a mind, and quite different from the pinkish putty-like mass dissected by the anatomist or stimulated by the surgeon's electrode on the operating table.

How then do all these fallacies arise? In my view they spring from the fact that each of the foregoing theories rests on the tacit assumption that the phenomena in question are to be described in terms of substances and their attributes, that is, in terms of the traditional logic of subject and predicate. Modern logic, however, insists on the supreme importance of a third category, namely, the category of relation; and modern science is built up by arguments which consist of relational propositions rather than of predicative propositions.

Now I hold that the experience which we ordinarily term consciousness – in the sense of being conscious *of* or aware *of* – is neither a substance nor an attribute, but a relation. There is a red chair to the right of my desk; and just as I can sit *on* the chair, so I can be aware *of* the chair (whether the 'I' is the same in both cases is another problem). Further, I actually am aware of this awareness. Just as I directly perceive the spatial relation between the chair and the desk, so I directly perceive the cognitive relation between myself and the chair. In all these cases the relations are unique and irreducible. They cannot be explained in terms of anything else. We can only point them out, much as we point to red or green when we explain the meanings of these words to a tiny child.

Introspective analysis reveals different species of cognitive relation – perceiving is different from sensing, and both are different from remembering and imagining. Moreover, I can be pleased or displeased with what I sense or perceive; and I can want the thing that I call up in memory or imagination. The capacity for standing in these peculiar relations to certain objects seems to be characteristic only of certain types of entity or event; and it is entities or events of this kind that we call mental. Further, what I am *most* directly aware of – red patches, a green expanse, a musical tone, a pleasant feeling – are qualities that are not themselves properties of material things as studied by science. Technically they are designated 'sensa' or 'sense-data'; and if they are not physical characteristics, then it is tempting to call them mental also. Indeed, the older type of introspective psychology, with its theories of colour-vision and of hearing, made a close and systematic study of these peculiar phenomena under the name of 'sensations'.

Bertrand Russell put the whole argument in a nutshell when he pointed out that a blind man could know the whole of physical science, but a man with normal sight can know a great many things which a man, blind from birth, can neither perceive nor conceive. Similarly, a man born deaf could know the whole science of acoustics and learn all there is to learn about the theory of harmony, melody, and musical form, but could not experience music as such. Thus experience is at once the basis of science and much wider than science. The champion of empirical science therefore should be the last to deny that consciousness exists, for he is constantly assuring us that his conclusions are founded on observation, and there could be no observation without a conscious observer.

But this, it will be said, means reverting to a frankly dualistic theory. Even though we have rejected the old dualities of substances or attributes, we are still insisting on an irreducible dualism between relations.

And to this I want to answer – why not? After all, the most obvious thing about the universe is not its unity, but its variety. In the past the demand for monism has led science more often into error than into truth. We are confronted with both *A* and *B*: one monist declares that *A* is merely an unrecognised form of *B*, a rival monist retorts that *B* is really an alternative form of *A*; neither is content to wait until someone has discovered an underlying *X*, a *tertium quid*, which will comprehend both, without excluding either. The history of science and philosophy, from Thales and Pythagoras onwards, is littered with wrecks of discarded attempts to explain the universe in terms of just one single principle. In much the same spirit the nineteenth-century monists, faced by the glaring contrast between mind and matter, spent much of their time battling for the supremacy of *either* one *or* the other. In the older seats of learning, at least in this country, where Kant and Hegel and their idealist disciples still dominated philosophy, mind was held to be the sole reality; in non-academic circles, matter was elevated to the same solitary throne, and mind was banished to outer limbo.

Now I venture to maintain that scientific progress has never yet resulted from simply dismissing incongruous phenomena as negligible illusions just because they refuse to conform. Again and again, as the story of scientific discovery testifies, stray exceptions to orthodox theories which have been ignored or rejected by one generation have become cornerstones in the new theories built by the next. No doubt it is a sound working maxim to concentrate on one set of variables and treat the others as irrelevant; but it is equally important to change about and examine the latter, while keeping the first set constant in their turn. The safest policy is to assume that the universe is more – not less – diversified than appearances suggest, and silently hope that an underlying synthesis will reveal itself when the time is ripe.[2] Occam's razor is a two-edged blade; but even that only says, '*Entia non sunt multiplicanda praeter necessitatem*,' and what I am proposing to multiply are not entities, but merely relations, or, to use a more fashionable term, 'interactions'.

Accordingly the suggestion that I am venturing to put forward is briefly this: I want to propound the notion of a system of psychical interactions, in addition to the system of physical interactions that constitutes the material universe, with the two systems meeting or intersecting at certain common points. We have already acquired some slight knowledge of the psychic system. That we do not possess a fuller knowledge springs simply from the fact that no comprehensive investigation by the rigorous methods of experimental science was undertaken until a century ago. The science of psychology today is roughly in much the same inchoate stage as the science of electromagnetism two centuries ago.[3] The curious property of amber or electron, and the still more astonishing way '*lapis hic ut ferrum ducere possit quem Magneta vocant Grai*',[4] were known long before the days of Aristotle;[5] yet had you described to Aristotle's own disciples the practical possibilities of electricity or hinted at an electrical theory of matter, they would have treated both with the same incredulity that Mr Hansel now professes in regard to psychical research.

Nevertheless, behind the universe of material atoms, pictured so vividly by Lucretius, there lurked another universe of electromagnetic forces. It would indeed be quite easy for the modern physicist to conceive of two completely separate universes existing simultaneously – one composed of elementary particles which would have mass and therefore gravitational interaction, but, like most of the man-sized objects familiar to us, no electric charge and therefore no electrical interaction, and the other composed of elementary particles which are subject to electrical interaction, but, like the photon, have no mass and are therefore incapable of gravitational interaction. The two systems might interpenetrate; and yet an observer belonging to the electrical system would be unaware of the existence of the other system, while a second observer, whose body was subject solely to gravity, would remain blissfully ignorant of the electrical system. In the actual universe as we know it, however, certain entities – electrons and protons, for instance – prove to be subject to both kinds of interaction. Hence, the two systems, for reasons we cannot even guess, are in some measure linked. More recently still, nuclear physics has detected two other types of interaction, previously quite unsuspected – the 'weak', which is yet vastly stronger than gravitational interactions, and the 'strong', which is even stronger than the electromagnetic. Each type of interaction obeys its own laws, and so far at any rate has defeated all attempts to reduce it to any other type.

This being so, there can be no antecedent improbability which forbids us postulating yet another system and yet another type of interaction, awaiting more intensive investigation – a psychic universe consisting of events or entities linked by psychic interactions, obeying laws of their own and interpenetrating the physical universe and partly overlapping, much as the various interactions already discovered and recognised overlap each other.

On this assumption the function of the brain would be not to 'produce' conscious phenomena, but to detect them selectively: its neuronal network would act like a network of receiving aerials, not generating psychic activity, but amplifying, transforming, and directing it, so that it can perform a useful function in a physical world. Until we have expanded both our knowledge and our vocabulary, I suggest that the simplest way to conceive of such a psychic system would be on the analogy of a 'field'.[6] But it would be a field not of force, but of 'information'. Nor would it occupy space and time, though, if information is to be communicated to a physiological organism, the field must be capable of exercising some kind of spatio-temporal influence. Professor Eccles, our greatest living authority on the working of the brain, has assured us that, in spite of the demurrers of behaviourists like Watson and philosophers like Ryle, the actual structure of the brain suggests 'that it *is* the sort of machine a "ghost" could operate, if by "ghost" we mean an agent whose action escapes detection even by the most delicate physical instruments'. Indeed, he has outlined in some detail a 'neuro-physiological hypothesis of will', based partly on the uncertainty principle and partly on the fact that particular parts of the brain (presumably the 'motor areas') are so constructed that they could act as detectors and amplifiers of minute extraneous influences exerted by the 'willing agent'.[7] Similarly, I would suggest, the sensory areas of the brain (together with their adjuncts in the rest of the neuro-sensory system) could operate as selective detectors of perceptual information.

The brain, or rather the neuro-sensory system, has to function selectively because it is an instrument for controlling a material body whose dimensions and radius of action are measured in feet and inches, and whose maximum velocity is 100 yards in 9 sec. It would have been quite easy for Nature to evolve a brain with telepathic and clairvoyant powers, or to have endowed it with microscopic or telescopic eyes and an electrical sense-organ, just as it equipped the myriapods with forty or more feet, instead of two. But for primitive life on this particular planet such an *embarras de richesses* would have made for extinction, not survival: any such prodigy would have quickly found itself 'lying bewildered in a ditch, wondering which things came after which'.

The account which both physicists and physiologists commonly give of the process of perception seems to me to be psychologically misleading. They take the world of physics to be the real world, and the world of consciousness is assumed to be merely an appearance of this reality – an incomplete, fragmentary, and distorted selection of the grosser aspects of a self-existent physical universe. The 'sensory inputs', originally received by our eyes, ears, skin, and muscles, have to be combined (so we are told) 'to give some kind of coherent synthesis'. And even so, the results only supply us with 'a kind of map or model . . . symbolic of events which they are quite unlike'. And 'the *real* physical world' has to be 'inferred from the symbolic data'.[8]

This treats 'the conscious world of common sense' as primarily a world of sense-data, whereas it is from the very outset a world of things perceived. What the 'plain man' – and indeed the plain baby and the plain animal – are conscious of, are not simple sensations from which complex physical objects are inferred and constructed, and then projected into external space, but three-dimensional objects, stereoscopically perceived, out there in space from the start. Indeed, even when the quantum physicist himself undertakes an experiment to verify his theories, he still describes the apparatus and the results he observes in terms of this everyday perceptual world. Fact, as Heisenberg[9] observes, is that which can be described in the language of common sense. The theories are inferred. Contrary therefore to the physiologist's description, it is the world of consciousness which, so far as our meagre knowledge can take us, brings us nearest to the 'real world'. The world of physics merely offers a highly abstract selection, sifted and sorted out from the conscious world, and then reconstructed with the aid of numerous inter- and extrapolations. Hence it would be more correct to regard the physical world as the 'model or map' – filled in tentatively in certain places, much as the older atlases used to colour in Central Africa or regions round the Poles.

The ultimate truth can only be that *both* worlds as we know them – the physical and the psychical – are but working models of a deeper and fuller reality, at present unknown, and possibly unknowable. It is tempting to suppose that it must be in some way psychophysical, and therefore 'neutral' in the sense that it is neither *purely* mental nor *purely* material. In that case, it might be said, we are after all opting for a kind of 'neutral monism'. But it would be a transcendent not an immanent monism. At this point, however, it is no longer the scientist, but the metaphysician who begins to speculate. And that, as Uncle Toby used to say, means quite another story.

REFERENCES AND NOTES

1. HUXLEY, T. H.: *Collected Essays:* volume I, page 159.
2. In fact most of the pioneers of materialism have commonly left themselves a small loophole in some corner of their writings. One device is to ascribe to the substance called matter 'the potentiality for mind': Haeckel even rechristened his basic matter 'psychoplasm'. Another is to introduce a subsidiary principle, like 'emergence', which allows entirely new attributes to 'emerge' under certain specific conditions. Perhaps the most prudent course was that which Huxley christened 'agnosticism'. But all these expedients prove to be little more than verbal concessions: once made, they play no further part in the writer's main argument.
3. Cf. PRIESTLEY, J.: *History and Present State of Electricity:* 1767. Gauss, fifty years later, managed to incorporate what little was then known of magnetic and electrical forces, together with the Newton law of gravitation, in a single mathematical treatment of the inverse square law – a plausible, but deceptive unification.
4. LUCRETIUS: *De Rerum Natura:* book VI, lines 908–9: ('how the stone which the Greeks call Magnesian is able to attract iron').
5. ARISTOTLE (*De Anima*, A, v, 411, and ii, 405) states that the properties of the lodestone (and Diogenes adds those of amber also) were known to Thales – the first man of science of whom we have any precise knowledge (c. 585 B.C.).
6. Cf. BURT, C.: 'Field Theories and Statistical Psychology': *Brit. J. Statist. Psychol.:* volume XII, pages 153–62, 1959, and refs.; GOOD, I. J.: 'The Mind-Body Problem, or Could an Android Feel Pain' (in the press).
7. ECCLES, J. C.: *The Neurophysiological Basis of Mind:* page 272f, 1953. A somewhat similar hypothesis was discussed by A. Eddington, *The Philosophy of Physical Science*, 1930.
8. ECCLES, J. C.: op. cit.: pages 280–1.
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24 Puzzle-learning versus Game-learning in Studies of Behaviour

by DONALD MICHIE

This paper is concerned with an elementary distinction which, it will be argued, is of crucial importance in the study of animal and human learning. The distinction is that between a one-person game or 'puzzle', and a two-person game (*see* Davies, 1950).

In a puzzle, as the term is here used, the player makes a succession of moves, each of which results in a changed state of the apparatus. After each move the rules tell the player whether the puzzle has now been solved or not. They may tell him that there are no more legal moves available to him, even though the puzzle has still not been solved, in which case he must restore the apparatus to its initial state and start again. In this event the value assigned to the outcome of his attempt can be conventionally described as a minus quantity, say -1. The outcome of a successful attempt may be given the value $+1$. One could imagine a more complex case where different valid solutions of the puzzle were graded (for example according to economy in the number of moves) on a quantitative scale, having values $+1$, $+2$, $+3$, etc. and that unsuccessful outcomes might be assigned similar gradations on a scale of negative numbers. We shall, however, take the simpler two-valued case for ease of exposition.

The distinguishing feature of a puzzle, as opposed to a game, is that the change effected in the apparatus by a given move is fully determined. In a game, on the other hand, the player has to take into account not one single necessary consequence of his move, but a range of alternative possible consequences, a selection from which will be made by his opponent's move or moves before it is again his turn to move.

A game, it will be contended, summarises the essential features of most of the situations with which animals are confronted in real life, whereas most of the problems given to animals in conventional studies of learning are 'puzzles'. I shall attempt to show that mechanisms of learning which give high efficiency for games are ill-suited to puzzles, and vice versa. Consequently it is possible to arrive at misleading conclusions if one presents puzzles, as is the custom of experimental psychologists, to central nervous systems which have been adapted by natural selection to cope with games.*

Animal-environment interaction as a two-person game

In the language of games the two 'persons' are the animal and its environment. Moves are made alternately. The environment's moves are called 'stimuli' and the animal's moves are called 'responses'. The outcome of the game is assessed after some number of moves. The object, from the animal's point of view, is to secure an outcome of the highest positive value, outcomes being valued according as they contribute to the satisfaction of the animal's needs, or, on the negative side of the balance sheet, to the causation of discomfort, pain, etc. It is not, of course, supposed in any literal sense that the *environment's* play is guided by any object, although in special circumstances it may be, as when the effective environment consists of a human being or another animal. On the other hand, it will necessarily be subject, as are the animal's responses, to the 'laws of nature', which correspond to the rules of the game. They determine what alternative moves are possible (legal) in a given situation. Additional restrictions on the animal's moves are imposed by the limited range of responses to a given stimulus allowed by its innate behaviour patterns. Purring is not a possible move for a cat to make on receipt of a painful blow.

* The author's interpretation of a game is perfectly reasonable but it should not be confused with that of Borel, von Neumann, and Morgenstern, where there is always a conscious opponent. *G. Ed.*

The essence of trial-and-error learning consists in whittling down the range of innate potential responses to a small number which become habitual. The process of selection in the light of experience can be likened to that which transforms a beginner at chess, who may (as Black) make any one of the twenty legal replies to 'White 1 . P – K4 . . .', into an expert who would not seriously consider more than at most eight, and who probably uses only three or four of these habitually. This analogy should not be pressed further than its simple illustrative purpose demands, since human learning of a game like chess makes heavy use of 'insight learning' in addition to 'trial-and-error'; this article is concerned only with the latter category.

Before examining the main thesis: that optimal mechanisms of trial-and-error learning are fundamentally different according to whether the task constitutes a puzzle or a game, it remains briefly to substantiate the claim that most real-life situations are games rather than puzzles. This can be seen to be true as soon as we recognise that at any point in time an animal is responding *not* to the total actual situation (which it is in no position fully to assess) *but to the total stimulus-situation.* Thus, the sight at noon on Monday of a pine-tree from a westerly point at five metres distance constitutes the same stimulus-situation as the sight of the same tree from the same vantage-point at noon on Tuesday, assuming reasonable constancy of climatic and other conditions. This is so even though every pine-needle of the tree has meanwhile moved. But the actual situation underlying the same stimulus-situation on the two occasions may be very different. A mountain lion may be in the tree on Tuesday which was not there on Monday, with the consequence that a response which leads to a favourable outcome on one occasion (e.g. using the tree for shade) may have a disastrous outcome on another occasion. This multiplicity of actual situations underlying a single stimulus-situation, and the resulting multiplicity of consequences attendant upon a given response, is a sufficient criterion of game-like rather than puzzle-like structure. The animal's ignorance of the actual situation which underlies the current stimulus-situation corresponds to the predicament of the chess-player, who is inevitably ignorant of his opponent's strategy although fully aware of the current position on the board to which that strategy has contributed.

To summarise the ideas outlined above, we present two diagrams. Fig. 2 depicts a sequence of choices made by an animal

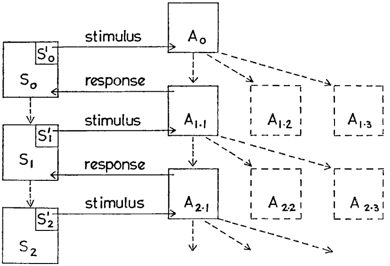

Fig. 2. The interaction between animal and environment represented in terms of a puzzle. S_0, S_1, S_2 denote successive states of the environment or 'actual situation' with the corresponding 'stimulus situation' denoted by S'_0, S'_1 and S'_2. The sequence of states of the animal A_0, $A_{1\cdot1}$, $A_{2\cdot1}$, shows the path actually taken, with alternative choices leading to potential states $A_{1\cdot2}$, $A_{1\cdot3}$ and $A_{2\cdot2}$, $A_{2\cdot3}$, etc.

confronted with a puzzle. In this case (unlike that of a game) the stimulus-situation contains all relevant features of the actual situation. In Fig. 3 the diagram shown in Fig. 2 is reproduced with a new feature added, which converts the puzzle into a game. The new feature is the existence of alternative *potential* transitions of the environment of which the animal must take account. These transitions are entirely compatible with the stimulus-situation, although not with the actual situation of which the animal is necessarily unaware.

We shall now consider the kinds of learning behaviour which

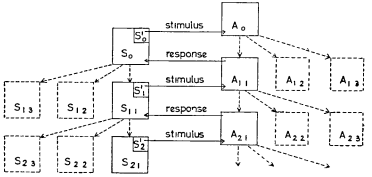

Fig. 3. The interaction between animal and environment represented in terms of a game. The new feature as compared with Fig. 2, is that a given response, say that arising from $A_{1\cdot1}$, may trigger off any one of *several* alternative changes in the environment, for example those leading to states $S_{1\cdot1}$, $S_{1\cdot2}$, or $S_{1\cdot3}$. These paths $S_0 \rightarrow S_{1\cdot1} \rightarrow S_{2\cdot1}$, and $A_0 \rightarrow A_{1\cdot1} \rightarrow A_{1\cdot2}$ represent those taken by environment and animal respectively on a given occasion.

would be appropriate to these two very different kinds of problem.

Learning to play a game versus learning to solve a puzzle

Within the strict context of trial-and-error learning there is no alternative, when faced with a new game or a new puzzle, but to embark on a series of randomly-chosen moves. Sooner or later the series of moves will terminate in an outcome, favourable or unfavourable; this is where learning begins. It is obvious that the probability of the terminal move, the next time that the same position is encountered, must be modified in some way. More specifically, if the outcome-value was negative the probability must be reduced, and if it was positive, it must be increased. But by how much? We here come upon the first important contrast between puzzle-learning and game-learning. It can be seen at once that, in a puzzle, the probability change, or 're-inforcement', should be maximal. That is to say, if the move has immediately led to a solution of the puzzle (outcome-value $= +1$), then the probability of repeating the successful move next time the opportunity arises should be adjusted to unity. If the move has led to immediate failure (outcome-value $= -1$), then the probability of repetition should be adjusted to zero.

This is only true of a *game* where the outcome immediately follows the last move, without an intervening move by the opponent. When a delay, occupied by the opponent's move, precedes the outcome the state of affairs is entirely different. Consider the following example (Fig. 4) from the game of Noughts and Crosses (otherwise known as Tic-tac-toe):

O	O	X
O	X	

Fig. 4. Noughts and Crosses.

On the first occasion when this position arises Cross places his move in the lower right-hand corner, and is defeated when his opponent promptly completes the left-hand column of noughts. In a world of puzzles this would be sufficient for Cross to 'learn his lesson' and never again repeat the fatal move. But in a world of games this move *may* be the one which in the long run gives the best results. One unfavourable result is not sufficient to exclude the remote possibility that Nought has a 'blind spot' which causes him on most, but not all, occasions to reply to the centre square and thus to allow Cross to win. Evidently Cross should be chary of repeating the losing move, but should not discard it completely until further evidence on its actuarial risk has accumulated: the probability of this particular response *should* be reduced, but not to zero. This principle does not hold (an artificial case from the real-life point of view) when the opponent's play is guided by 'best strategy'.

In real-life situations the outcome even of a terminal move is frequently indeterminate, as in the case of Cross's move in the above example. All that can be attached to it in the light of the animal's accumulating experience is an increasingly well-charted frequency-distribution estimating the relative probabilities of the various possible outcomes. Yet this is *not* true of the laboratory conditions under which learning behaviour is commonly tested. In the typical and simplest case, the animal is rewarded if it turns left and punished if it turns right, and this rigid connexion between move and outcome-value is held invariant throughout the experiment. The animal, however, is not to know this. If, therefore, it requires a substantial number of trials before settling decisively for the left rather than the right turn, its sluggishness should not be imputed to imperfect learning powers: it may merely indicate that the animal has a better grasp than has the experimenter on the realities of its own daily life.*

The second major contrast between game-learning and puzzle-learning concerns the relation between temporal sequence and the strength of reinforcement. In formulating his classical Law of Effect, Thorndike (1911) drew attention to 'the effect of increasing the interval between the response and the satisfaction or discomfort', namely a dimunition of the amount by which the probability of response is modified in the light of the outcome.

In terms of game-learning we interpret 'interval' as meaning the number of further moves intervening before an outcome is reached. An efficient game-learning mechanism should modify the probability not only of the move immediately preceding the outcome, but, in diminishing degree, also that of the penultimate move, the ante-penultimate move, and so on. This principle has been utilised in constructing a simple machine which 'learns' to

* This point is relevant to the teaching of mathematics to young humans whose previous training makes it difficult for them to accept the comparative lack of ambiguity of mathematical statements. *G. Ed.*

play Noughts and Crosses (Michie, 1961). Even though the desirability of applying non-zero reinforcement to pre-terminal moves may seem obvious, we have to ask ourselves what is its *precise* justification. As a first approach we can frame our answer in rather loose language: the *rationale* of discouraging earlier moves which have led to one particular unfavourable final outcome (or encouraging those which have led to a particular favourable outcome) is that we take the outcome-value as evidence that a given earlier move was bad (or good) in some more general sense than that it happened to lead to this particular result in this particular case. More rigorously, we must consider the formal representation of a game or puzzle as a branching tree. The principle of 'guilt by association' to which we have given a loose expression above can be expressed by saying that twigs of the same branch tend to bear similar fruit: that is to say, the total variation in outcome values, instead of being distributed at random over the terminal spots, shows a trend towards homogeneity within families of spots, and heterogeneity between families.

The principle is well known to game-players, and is a commonplace of real life. But is it also true of puzzles? Doubtless it is true of many puzzles, but it can easily be seen that it need not be true of any given puzzle and that there is no reason at all why it should be true of the particular puzzles which experimental psychologists devise for their animals.

In the first place, there may be no opportunity for the principle to operate owing to insufficient variation of outcome values. This is so in a maze in which only one terminal spot contains a reward, the remainder carrying punishments. A simple maze which does not allow re-tracing is shown in Fig. 5. The terminal spot containing the reward is boxed in the diagram.

The three pre-terminal spots represent choice-points, of which the first can be termed 'primary' and the other two 'secondary'. Since this is a puzzle and not a game, the most efficient learning procedure will, as we have seen in an earlier section, discard immediately and irrevocably any secondary choice which has once been followed by a negative outcome. A real animal, as we have also seen earlier, will not do this because, in our submission, it is adapted to game-learning rather than puzzle-learning. It will

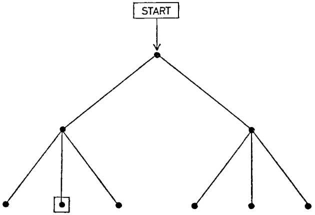

Fig. 5. A simple maze without re-tracing, drawn and labelled in such a way as to exhibit the formal structure of a puzzle.

also display another behavioural feature irrelevant to puzzle-learning, namely a modification of *primary* choice consequent upon a negative outcome. If its first run of the maze took it to terminal spot (*a*) where it received a punishment, it will tend on the next occasion to make a left rather than a right turn at the primary choice-point. This spread of reinforcement to a pre-terminal move is an adaptive mechanism in game-learning, owing to family-likeness of terminal spots. Yet in the puzzle under consideration the average number of trials needed for solution of this puzzle (given maximal reinforcement of secondary choices) is exactly $3\frac{1}{2}$, and this expectation is completely unaffected by any spread of reinforcement to the primary choice.

A different version of the same maze might contain two reward-boxes, and these could be disposed in two essentially different ways, as shown in Fig. 6. Here it is less obvious that spread of reinforcement is ineffective in contributing to learning-speed. Everything, in fact, depends on whether the puzzle belongs to type *A* or type *B*. For type *B*, which exemplifies the 'family-likeness' of outcomes characteristic of games, a negative outcome should indeed result in a negative reinforcement of the primary

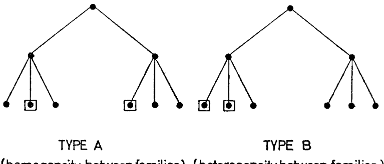

Fig. 6. Simplified representation of two contrasting types of puzzle, one in which twigs of the same branch tend to bear similar fruit (Type *B*) and one in which fruits are dispersed evenly over branches. For more general application of this distinction we would have to consider sub-branches, sub-sub-branches, etc., before arriving at the twigs, or 'terminal spots'.

choice: if twigs of the same branch tend to bear similar fruit, it is better, after a disappointment, to try another branch! But for type *A*, a negative outcome should result in a *positive* reinforcement if maximum efficiency is required. This is because having eliminated one negative outcome in the family, we expect a correspondingly higher proportion of positives in the surviving members of the same family. In such a case a fundamental feature of learning which forms a normal and necessary part of animal behaviour would not only be useless to the animal, but would be actively harmful and serve only to lead it into trouble.

The main ideas that have been advanced can be summarised as follows:

1. Real life has the structure of a game rather than of a puzzle.

2. Efficient game-learning by trial-and-error requires two fundamental features in the reinforcement system: (*a*) partial

rather than absolute reinforcement of the terminal move, and (*b*) spread of reinforcement to pre-terminal moves.

3. Both these features are exemplified by trial-and-error learning in animals.

4. By testing experimental animals with puzzles rather than games these features can be nullified and may even unwittingly be turned to the discredit of the animal's estimated powers of learning.

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25 A Theory of Cruelty

by W. H. CAZALY

The essential element in both cruelty and kindness lies in *pleasurable awareness* of distress *or* pleasure respectively in the recipients of cruelty or kindness *on the part of the donor.* Cruelty or kindness imposed on inanimate objects, such as dead creatures or images, partakes much more of the nature of purely symbolic action and can hardly be regarded as true cruelty or kindness. Or, of course, they may be the result of acute mental abberration which renders a person incapable of distinguishing between a sentient creature and one without feelings.

Before coming to the theory to be put forward, the significance may be pointed out of the lack of evidence of true cruelty or kindness, as above defined, in non-human living creatures. The 'cruelty' or 'kindness' apparently shown by animals is almost certainly an invalid anthropomorphic interpretation of behaviour. The kitten does not play with the mouse for the sake of the pleasure the kitten feels through awareness of the mouse's agony, but because this kind of play is practice for adult hunting. So indifferent is the kitten towards what the mouse feels, that it will play with any small object roughly resembling and moving like a mouse. The creature protecting and feeding its young is doing so not out of pleasure it feels in the pleasure of the young, but because it is driven by instinct – which does not cause it to be 'kind' to adults.

Coming to the aetiology of cruelty and kindness, one finds in such studies as have been made little or no consideration of the evolutionary aspects of either. Kindness, true, seems to be regarded as a development of 'family feelings' alleged to exist in non-humans and humanoid ancestors, which have biological advantages. Herd or pack loyalty in non-humans, too, is said to underly the origins of human kindness or at any rate of human altruistic behaviour. These approaches, however, lead to theories which fail to account for large areas of both human and non-human 'kind' behaviour; they also seem liable to be tainted with unjustified anthropomorphic interpretations of non-human behaviour. For there is very little acceptable evidence that it is the awareness of pleasure, on the parts of non-human donors, in the pleasure – or distress – experienced in recipients of treatment from donors which causes the donors to behave as they do; their behaviour can be satisfactorily accounted for by unsentimental motives.

Whence, then, are human cruelty and kindness derived, if not from rudimentary mechanisms in non-humans? Are they peculiar to humanity alone? If so, why?

The theory put forward here is that a special mechanism does exist, and that it is peculiar to humans. Its function is to provide pleasure from awareness of distress/pleasure in others. Distress/pleasure in others makes itself manifest by more or less subtle behaviour, perception of which is interpreted as distress/

pleasure; when this interpretation is conveyed to the mechanism mentioned, that mechanism promotes pleasurable emotions.

For convenience, the mechanism is here called PPO – standing for pleasure-from-awareness-of-pain/pleasure-in-others.

Non-humans, it is suggested, can get along without PPO; it is not necessary for their biological survival in natural conditions, since their instincts suffice for that. Man, however, is remarkably poorly equipped for survival without his exceptional intelligence: he lacks many survivally-useful instincts and physical characteristics possessed by non-humans and has to rely on his intelligence for individual and genetic survival.

It is possible that PPO has its origins in the necessity for group discipline. Man early found biological advantage in group-living without strong group instincts. He was loyal to the group, however, only so long as that loyalty served his own personal appetites and welfare; when it failed to do so, he tended to act in group-disadvantageous ways. Since this reduced the biological efficiency of the group, it was necessary to force him to be loyal against even his own wishes and welfare. Killing or expelling him for disloyalty was disadvantageous since it weakened the group numerically. Resort, therefore, was had to the expedient of making loyalty to the group agreeable and disloyalty to it disagreeable.

For these measures to be effectively applied, it was necessary for humans living in groups to become pleasurably aware themselves of distress and pain in others; that would make them willing to reward or punish. Their intelligence would enable them to differentiate between loyal and disloyal behaviour and PPO would urge them to reward or punish as measures devised by their intelligence. If PPO did not exist and, therefore, operate, it is difficult to see how their primitive intelligence alone would have enabled them either to realise that others felt pleasure or pain or to have taken the further step, if they had realised it, of rationally utilising pleasure/pain in others to promote group loyalty.

Whether or not the foregoing is eventually found to be a valid theory of the evolutionary origin of PPO, the postulate that PPO exists seems to lead to plausible explanations of a number of phenomena. Its nature can, of course, only be quite speculative. It may be a compact mechanism with a definite location in the cerebral system or it may be a diffuse entity. It seems probable, if the evolutionary theory of origin outlined above is valid, that it is likely to be located at least partially in the forebrain, since it is imagined as peculiar to humanity, which is a late product of the evolutionary process and the human forebrain the latest acquisition of that product. It may be a monovalent mechanism producing pleasurable emotion regardless of whether pleasure of pain in others is presented to it; or it may be bivalent, in two parts as it were, one reacting to pleasure and the other to distress in others.

The assumption that it is bivalent suggests that normally the two parts are balanced, so that pleasure and pain in others equally give rise to pleasure; and when imbalance exists, the individual is on the whole cruel or kind, depending on which side is the more weighty in terms of sensitivity or reaction efficiency. Balance may be rare or only of short duration and easily upset, which would account for the rarity of serene, just personalities. Balance may be upset by hormone influences (adrenal secretions, for instance, may stimulate the cruelty part) or by dietary changes or by disease mental or physical – thus there is still room for functional psychological theories of cruelty and kindness. The chief difference between the human devil and human angel may be a matter simply of PPO imbalance.

That PPO is still of biological advantage, indeed a necessary part of human make-up, may be shown by two considerations: one, of what would happen if there were no such thing as PPO and the other of what its action may be in human lives today, in social life especially.

Considering first the results of absence of PPO – it is hard to imagine the kind of human society produced by beings completely indifferent to each other's distresses and pleasures, likes and dislikes. They would be motivated only by their own personal distresses and pleasures, likes and dislikes. For any kind of co-operative group activity to occur with such creatures, there would have to be some super- or sub-human universal factor determining their behaviour and determining it purposefully and overriding their personal preferences. Something of the kind may actually occur in, for instance, ant or bee life. Free will, morality, conscience, cruelty and kindness themselves would be impossible under such governance; the group would have to consist of robots unquestioningly obeying either the super-will or instincts, each, possibly, seething with loves, hates, longings, fears and angers but unable to perceive them, save by highly logical intellectual inference, in others. Without the super-will or the instincts, no group activity would be possible at all.

Considering the second, it is hard to see whence police, soldiers, judges, disciplinary systems of any kind, could be obtained if there were no pleasure to be obtained by inflicting distresses on others. There may be a few persons who can inflict distresses without any pleasure, but they are in fact very rare and seldom become policemen, soldiers, and so on. It is significant that so much pleasure is undoubtedly roused by the distressing treatment of criminals – in other words, to 'disloyal' members of the group. There is more to it than virtuous moral notions. Nor, unless pleasure was felt in pleasure in others, would saints, reformers, kindly, altruistic persons feel moved to act as they do. Purely intellectual considerations do not motivate the great humanitarians or appeal to their followers. It is impossible to doubt, as a matter of common observation, that pleasure in pleasure and pain in others operates powerfully and universally in everyday domestic life and in means of livelihood, as well as in public affairs.

If this PPO exists, to discover its nature and location, thence control of it, would seem to be probably the most urgently desirable research. The results of that research might well bring about fantastic and beneficial changes in human conditions by their proper application.

26 A Hypothesis about 'Projection'

by T. R. MILES

Many users of projective tests[1] assert that every verbal response by the subject contains a meaning. This notion links up with the claim, originally made by Freud and taken over in present-day psychiatric social work, that even apparently 'free' associations are really tied in the sense of having a personal significance of which the subject may be unaware.[2]

Experimentally-minded psychologists have tended to view this 'projective' approach with suspicion on the grounds that it does not allow for falsifiability. The following is a 'half-baked' plan for statistical validation,[3] with possible implications for further study of personality. Subjects are presented with the letters of the alphabet in random order, and are required to say whether a particular letter is 'masculine' or 'feminine', writing down either M or F on the printed form according to 'what first came into their heads'.[4]

In a preliminary survey this test was given to 155 schoolchildren aged 10–12, and the hypothesis was tested that the sex given to the first letter of their Christian name[5] would coincide with their actual sex, e.g. that a boy whose name was John would put J as masculine and that a girl whose name was Hilda would put H as feminine. Of 96 boys 62 were 'homonymic' in this way and 34 'heteronymic'; of 59 girls 42 were homonymic and 17 heteronymic. On the hypothesis of random association there is no reason for any departure from an even split of homonymic and heteronymic responses ($p = 0{\cdot}5$, $q = 0{\cdot}5$). Our observed departure (104–51) lies outside 4σ from the expected result

(77·5) and is therefore highly significant![6] The association is therefore not random, but appears to have something to do with the letter which starts the subject's Christian name. This 'half-baked' idea might be of value if homonymics and heteronymics were found to differ in other recognisable personality traits.

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1. Psychological tests in which the subject is presented with a stimulus – often of an indeterminate kind such as an ink-blot – and is asked to report what he sees or to tell a story about what he sees.
2. *See*, for instance, FREUD, S.: *The Psychopathology of Everyday Life*: esp. page 282, 'There is nothing arbitrary or undetermined in the psychic life': tr. A. A. Brill, London, 1914.
3. Compare *Math. Rev.*, volume 21, page 1172, 1960. *G. Ed.*
4. Similar exploration could be made into fantasies about the *colour* of letters. Compare RIMBAUD, A.: 'Sonnet des Voyelles'.
5. They were required to state at the top of their form the name by which they were usually known at home.
6. σ stands here for the standard error of a binomial distribution and is given by the formula $\sigma = \sqrt{(npq)}$, where (in this case) $n = 155$ and p and q both equal 0·5. A result lying outside 4σ in the specified direction would occur by chance less than once in 30,000 trials.

27 Nylon Uppertights

by JOHN T. PHILLIFENT

Why should the inventive skill, design and industry needed to produce sheer, fully-fashioned nylon be applied solely to the female leg?

Why, in fact, should sheer nylon be regarded as glamorising and attractive, on legs, but repulsive, apparently, on arms, shoulders, neck, bosom, and so on?

28 The Polarity of Emotions

by ARTHUR KOESTLER

When the experimental psychologist or neurophysiologist speaks of 'emotion' he nearly always refers to rage, fear, sex, hunger, pain, i.e. to emotive behaviour associated with the sympathico-adrenal system which tends to beget overt motor activity. There exists, however, a different class of emotions – sympathy, identification, non-sexual love, wonder, worship, artistic experience – which do not tend towards overt activity but towards quiescence and catharsis. The former may be called the aggressive-defensive, or self-assertive, emotions (S.A.), the latter the participatory, or self-transcending, emotions (S.Tr.). The latter are the stepchildren of contemporary psychology and usually treated as a suspect category of pseudo-emotions unworthy of attention in the experimental laboratory. This may be a hangover of the great ideological currents of the nineteenth century, stressing the biological struggle for existence, the survival of the fittest, the acquisitive and competitive aspects of social behaviour.* Victorian religion, patriotism and 'pure' love were blended with so much crude hypocrisy that the experimental psychologist could hardly be expected to put them on a par with the sex and hunger drives. Yet the participatory emotions play an essential part in any form of cathartic psychotherapy and are equally essential for the understanding of certain higher mental processes. Typical situations which elicit them are, e.g. listening to Mozart, looking at a majestic landscape, being in love or engaged in religious meditation. Each of these

* The *ambiance* of this 'Darwinistic psychology' is reflected in passages like the following, from Dr Crile's *The Origin and Nature of the Emotions*, published 1915: 'When the business man is conducting a struggle for existence against his rivals, and when the contest is at its height, he may clench his fists, pound the table, perhaps show his teeth, and exhibit every expression of physical combat. Fixing the jaw and showing the teeth in anger merely emphasize the remarkable tenacity of philogeny. . . .'

activities – or shall one say passivities – may cause a welling up of emotion and a discharge of the lachrymal glands, while the body is becalmed and its tensions are drained. Oddly enough we apply the term 'moving' to those emotions which do not 'move' us to overt activity but lead to the draining or inhibition of sympathico-adrenal excitations. The common denominator of these experiences is a feeling of participation, or identification, or belonging; the self is experienced as part of a whole which may be Nature, Mankind, a personal bond, or the Anima Mundi; and the need is felt to behave as part of some such real or imagined entity which transcends the boundaries of the individual self, whereas in the hunger-rage-fear type of emotions, the ego is experienced as a self-contained unit and ultimate value.

The characteristic discharge-reflex associated with this class of emotions is weeping. Its counterpart, laughter, has long been recognised as a discharge-reflex of aggressive (malicious) excitation which has become redundant by a sudden switch of thought (Spencer, 1911; Freud, 1905). Laughter tends to produce exaggerated gestures and heightened muscle tone, including the reflex contractions of about fifteen facial muscles, due to sympathico-adrenal excitation; in weeping, muscle tone is lowered, the body slumps, the face sags. Sobbing, with its short, deep, gasping inspirations, followed by long, sighing expirations, is the opposite of the respiratory action in laughter. The lachrymal glands are under dual control; weeping is due to parasympathetic innervation, while shedding tears in laughter seems to be due to sympathetic innervation (as in the defensive reflex of washing away a foreign body). In short, the type of emotion discharged in weeping is devoid of the galvanising quality of the hunger-rage-fear class; instead of begetting muscular activity, it tends towards quiescence and catharsis, lowers respiration and pulse-rate; it neutralises adrenal excitation and, in extreme cases, produces trance-like or comatose states. The contrast between the two reflexes corresponds to the traditional duality of the tragic and the comic, which in its turn seems to be derived from the polarity of the self-assertive (aggressive-defensive) and the self-transcending (participatory) aspects of human behaviour.

Extremes apart (e.g. rage tantrums or religious trance), emotive behaviour is a compound in which both types of emotion participate. In love – sexual or maternal – the aggressive or possessive S. A. element and the identificatory S.Tr. element may reinforce each other. In restrained animosity they inhibit each other. In the explorer and scientist, ambition is nicely balanced with devotion. In outbreaks of fanaticism, devotion to a creed serves as a catalyst for destructive mass behaviour. Even the elementary activity of feeding has an S.Tr. component – expressed in the mystic belief of Primitives in sharing the virtues of the devoured animal or enemy; in the communion with the slain god (Zagreus, Orpheus, the Eucharist) and in the rituals of conviviality. All this reminds one of the various modes of interaction between the two branches of the Autonomous Nervous System – antagonistic, synergic, catalytic, over-compensatory, according to circumstances (Gellhorn, 1943, 1957); and the term ambivalence appears as an oversimplification.

The S.Tr. emotions are, of course, not always pleasurable. Pleasure and unpleasure (as distinct from pain) seem to form an independent scale of affective values superimposed on both types of emotion (as brightness values are superimposed on colour), indicating, in a special code, satisfaction or frustration of the emotive drive (which seems to resolve von Neumann's paradox: 'A sadist is a person who is kind to a masochist'). Crying in bereavement, for instance, may be said to express the frustration of an S.Tr. bond.

In the present confused state of the semantics of emotion it seems that the criteria to be applied to a new classificatory hypothesis are its heuristic value, and its compatibility with neurophysiological data. About the former I have written elsewhere (1949); regarding the latter, the correlation between the S.A. emotions and the Sympathetic Nervous System seems to be firmly established. To assume a symmetrical correlation of the S.Tr. emotions with the Parasympathetic Nervous System would be tempting, but the subject is full of pitfalls. More than thirty years ago, Cannon (1929) wrote: 'The sympathetic is like the loud and soft pedals, modulating all the notes together; the cranial and sacral innervations are like the separate keys.' Since then, the significance for psychology of the anatomical and physiological contrast between the two branches of the Autonomous Nervous System has become more evident, to the extent that 'rage is called the most adrenergic, and love the most cholinergic reaction' (Cobb, 1950). A further correspondence between patterns of emotive behaviour and modes of interaction between the two branches of the Autonomous Nervous System emerged when it was shown that the vago-insulin system may act, in different circumstances, as an inhibitory or a catalytic agent in the glucose-utilisation process, and may also produce over-compensatory after-effects (Gellhorn, 1943, 1957, 1960). Hebb (1949) suggested that a distinction should be made between two categories of emotions, 'those in which the tendency is to maintain or increase the original stimulating conditions (pleasurable or integrative emotions)' and 'those in which the tendency is to abolish or decrease the stimulus (rage, fear, disgust)'. A few years later, Olds (1959, 1960) and others demonstrated the existence of 'positive' and 'negative' emotive systems by electrical stimulation, and further showed that they were activated respectively by the Parasympathetic Nervous System and Sympathetic Nervous System centres in the hypothalamus. These hints all seem to point in the same general direction, but they are too vague to allow us at this stage to co-ordinate the participatory emotions with the Parasympathetic Nervous System – except as a half-baked suggestion for further experimental investigation. Olds's self-stimulating rat, for instance, cannot be said to display any participatory type of emotion – but then it is only a rat, and we can hardly expect to find a platonic component in its sex drive.

Experiments of the following type may help to shed some light on the question: give one half of a class of music students a parasympathetico-mimetic drug, the other half a placebo, and let them report on their emotional reactions to suitable records. Try the same, if permission can be obtained, during meditation hour in a monastery. Use the methods indicated by Gellhorn (1943, 1957, and 1960) to measure the chemical balance between vago-insulin secretion and adrenal secretion before and after prayer, meditation, listening to appropriate music.

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29 On Listening to Oneself

by ARTHUR KOESTLER

People who listen for the first time to their own voice played back on a tape recorder usually get a shock. The explanation usually given is based on the difference in pitch and timbre between hearing one's voice mediated by intracranial processes and listening to it mediated by acoustic waves emanating from a mechanical device; but there seems to be more to it. To offer myself as an example: I am of Hungarian origin, and although my foreign accent retains the specific density of pea-soup, I was virtually unaware of this till I first listened to my voice on a recorded broadcast. I have a good ear for other people's accents, yet perceive my own voice as if it were free from it. An obvious analogue are people singing grossly out of tune, yet unaware of the fact until accompaniment provides corrective control – of which class I am also a member.

This seems to suggest that in the perception of one's own voice, the actual acoustic production plays a subordinate part and that the main component of the percept is the auditory image of the intended production in the Central Nervous System.* This implies that the image is directly projected on to the receptor areas, and thus reaches awareness by a short-cut. The discrepancy between image and performance would be caused by faulty auditory-vocal co-ordination, but this distortion would be masked by the process referred to above. Similar considerations might apply to gesture and movement, where defective kinaesthetic-motor co-ordination may interfere with performance; but once more, the clumsy gesture is screened off from awareness by the direct impact of the image of the intended graceful movement on perception.

* Cf. CHERRY, COLIN and SAYERS, B. M.: *J. Psychosomatic Res.*: volume 1, pages 233–46, 1956. *G. Ed.*

30 Reverberation

by ALEXANDER FORBES

The concept of self-exciting circuits of neurons in the central nervous system has been largely developed and tested experimentally by Lorente de Nó, published in the *Archives of Neurology and Psychiatry*, 1933, Vol. 30, pp. 245–91. Although he had mentioned the concept in a lecture in Strasbourg in February 1930, it was, I believe, first fully developed and tested with a series of experiments, in an extensive paper published in 1933.[1]

The idea of self-exciting circuits in the nervous system has been much discussed in the years since 1930 and has played an important part in the many efforts to understand what goes on in the nervous system. It has even been proposed as a basis for memory, but clearly it cannot be the basis of more than very transient memories, for on recovery from anaesthesia so deep as to suspend nearly all activity in the cerebrum, memories of the past return.

It may be of historical interest to note that the first time I ever heard the word 'reverberation' used in connexion with prolonged after-discharge in the nerve centres was in a conversation with Sherrington in the parlour of the house in Cambridge, England, where I was living in the summer of 1921. In September of that year we were discussing prolonged after-discharge, revealed by sustained contraction of a muscle after powerful reflex stimulation. Sherrington remarked to the effect that there might be a sort of reverberation in the nerve centres. The idea was appealing, but its full significance did not register with me for several years.

I had already published a paper[2] in which I suggested that prolonged after-discharge might depend on extensive chains of neurons with many synapses which required a long time to traverse, and I called them 'delay paths'. Again, in 1922,[3] I presented the same idea, delay paths, as the basis for prolonged after-discharge. In 1926, Fulton[4] pointed out that delay paths would not suffice to explain after-discharge lasting many seconds, for no pathways in the nervous system accessible to afferent impulses are extensive enough to require several seconds for impulses to traverse them. I then invoked Sherrington's suggestion of reverberation, repetitive circulation of impulses in closed circuits. So far as I am aware the first appearance of the word 'reverberation' in this connexion in print was in 1929.[5] In 1930 it was repeated and attention was drawn to the fact that reverberation answered Fulton's objection to the delay-path explanation of prolonged after-discharge.[6] This exchange of views is well described by Lorente de Nó on page 280 of his above-mentioned paper.[1]

How well substantiated is the theory of repeated circulation of nerve impulses in closed circuits I do not know. Anatomical evidence supports the theory, and it offers the most plausible explanation of prolonged after-discharge. The theory has played a large role in the literature of the last thirty years. I believe that the idea originated in the remark Sherrington made to me in 1921. If it has merit, the credit should go to Sherrington, the originator of many of the most important concepts in neurophysiology.

It should be further noted that in the decade referred to, I was trying to implement Keith Lucas's suggestion that we should inquire whether events in the nerve centres can be explained on the basis of conduction as revealed in peripheral nerve. Since 1930 researches with the micro-electrode and the cathode-ray oscilloscope have revealed clearly that different types of excitation occur in the synapses, dendrites and nerve cell bodies. My quest was germane in the 'twenties, but obsolete today.

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31 Periodicity and Rounding-off in the Thinking Process

by JORDAN M. SCHER

It is common experience that thoughts shift. Built into everyday speech are pauses at words, phrases, clauses, sentences, etc. Little thought has been given, however, to the process of the pause, interruption, or shift as such. Some time ago, in the course of studying the abnormalities of communication in the psychiatric patient, I began to wonder about and observe the pausing process itself.

It occurred to me that there was a certain natural periodicity in the human being at rest and untroubled as he shifts from one manifest thought-area to another. In some preliminary efforts to determine the existence of this natural periodicity, or minor phasing shifts, I seemed to find that I experienced the perception of such thought-shifts about every six seconds. I tested several colleagues in a similar fashion to determine the occurrence of a perceived thought-shift in them at rest and under two different stimulus situations: (1) 'Now think about someone who makes you angry.' And (2) 'Now think about a situation that arouses you sexually.' The precise meaning of what I was looking for was unknown to them and they were instructed merely to indicate to me when their thoughts shifted by pressing a key. A similar approach was used with patients of diverse diagnoses. What seemed to come out of these efforts was the observation that it would seem that the normal period of perceived thought-shift was approximately six to ten seconds in length whereas the period of stimulated or directed thought change was about ten to fifteen seconds. Lysergic acid, as might be expected, considerably protracted and made aberrant the reporting of perceived thought-shifts.

Individuals suffering from disorders, such as obsessive thinking, impulsive behaviour, and schizophrenic thinking, seem to have errors in the thought-shift process, both in the manner already described ('minor phasing') and at the level of 'major phasing' which I now describe. By 'major phasing', I mean the rounding off of larger groups of ideas customary in turning our attention from one situation, activity, or thought-area to another. Normally, the healthy individual is able to round off, change the subject, or turn his attention. The pathological individual seems to be wired in such a fashion that when he reaches a point of normal thought-break, or ideational shift, he fails to round off and seems to reverberate interterminably, as in an echo-chamber. Such an event may lead the so-called impulsively-oriented individual to introduce an external act in an effort to produce the necessary rounding off.* Thus, impulsive behaviour would not be the product of an urge one cannot stop so much as the effort to stop a reverberating thought by an external act. The act would then be an effort to end an otherwise interminable idea. For certain schizophrenic patients and for the obsessive individual, normal rounding off seems to present merely a pivot on which to revert back to the same deeply-grooved rut. Rather than a moving forward in time and space, there is a bending back of time and space.[1] The postulation of the existence of such pausing mechanisms in the human organism suggest an underlying rhythmical structure to thinking,[2] perhaps relating it to the well-known electrical rhythmicity. The rhythmical nature of

* Scher, J. M.: *The Law of the Finite Act: a View of the Impulsive Act:* (unpublished).

thinking thus presents a take-off point for understanding some aspects of normality and pathology in the human. Taking the rhythmical structure of man as the foundation on which to build a psychology of man, a new view of psychiatric pathology and therapy may be evolved. Much of my current work is based on an elaboration of this view. Perhaps this idea will lead us to very fruitful explorations of the thinking process.

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32 A Hypothesis of Recognition[1]

by VICTOR SEREBRIAKOFF

My ideas concerning the organisation of the cerebral cortex are based on an analogy with an industrial communication network, which is always polyhierarchical, that is, it consists of several strata.[2,3]. More precisely, it consists of two polyhierarchical structures placed vertex to vertex: *see* Fig. 7.

One of these structures leads from the customers to the managers, and the other from the workers to the managers. The customers correspond to the sense organs and the workers to the motor effectors.

Recognition is a process of reducing a large number of decisions to one decision, and is closely related to the meaning of 'definition'. The part of Fig. 7 leading from the sensorium to the summits is strongly analogous to the definition of a definition

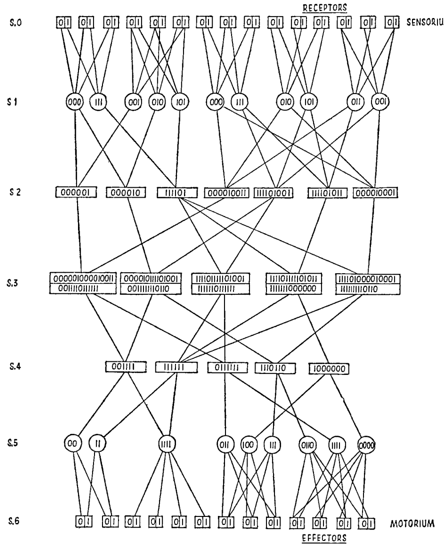

Fig. 7. A polyhierarchic network of five 'summits'. A transducer capable of recognising five configurations of fifteen variables and responding accordingly.

given by Good.[4] McDougall too suggested a hierarchical structure for the intellect.[5]

In this model, recognition is identified with the stimulation of a summit, but many summits may correspond to a single concept. The amount of specification increases with stratum height, and the number of nodes representing a single concept decreases. An important feature of such a structure is that it explains how recognition is possible for an exceedingly large variety of possible inputs. (For example, the 'same' input at different points of the sensorium can be recognised as the same.) Moreover this recognition will be possible even if the individual components of the network are unreliable. (Cf. von Neumann.[6]) It will often occur even if the structure receives serious damage.

We now suppose, following all recent authors, that learning is probabilistic, that is, it depends on variations in the probabilities of transmission of pulses from one element of the structure to another. These probabilities are varied by means of some reinforcement mechanism, possibly depending on the circumambient fluid. (*See*, for example, Good.[7])

The motor hierarchies depend on divergence instead of convergence.[8] One would expect to find cells in the motor structure with fewer afferent and more efferent connexions. The pyramidal cells which are found in the motor area may serve this function.

In order to explain attention, 'set', or expectancy, we simply assume that each *need* corresponds to some internal afferent signal.

It is tempting to identify the ego as a point which has maximum dynamic connectivity with the entire network. The analogy is that of a factory manager moving around the factory.

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8. Cf. HAYEK, F. A.: *The Sensory Order:* page 93, London, 1952.

33 Botryological Speculations[1]

by I. J. GOOD

In my opinion, 'Clumpology', or better 'Botryology', the science of defining and finding 'logical clumps' in a set of objects, is destined to be of great importance in the future and will have applications to philosophy, semantics, mechanical translation, psychology, artificial intelligence, information retrieval, determination of species, scientific classification, general systems[2], architectural planning[3] and the theory of art generally. It will also tend to encourage cross-fertilisation between these fields of study. The justification for this prediction is that the search for clumps is an essential part of the art of classification, itself fundamental to all human thought.

I use the term 'botryology' rather than the 'theory of clumps' because its development is likely to be as much experimental as theoretical.

Botryology differs from the theory of finding clusters in ordinary space, such as clusters of galaxies, in being, on the whole, concerned with logical and qualitative relationships rather than with ordinary distance (or with a metric satisfying the triangle inequality). In short, the definition of a clump depends on measures of *relevance*.[4] Measures of relevance could also be defined in terms of clumps, so that a comprehensive search for clumps may need to be iterative.

In any information-retrieval system the notion of 'degrees of relevance' is vital, whether or not it is given a reasonably sharp numerical definition. It is basic to any scoring system to be used for deciding whether to deliver a document to a customer, and for putting the documents into a priority order for him.

Imagine an 'oriented linear graph' (nodes connected by branches) in which a node can represent a field of knowledge, document, proposition, phrase, word, concept, library request, or library customer. Each pair of nodes, A and B, is joined by a branch with an arrow on it, and an associated measure of relevance of B to A, say R_{AB}. If $R_{AB} = 0$, then there is no need to draw the branch. The relevance of B to A is not necessarily equal to that of A to B, so we need two branches, one in each direction.

If the information represented by the graph corresponds to academic knowledge, and if the thickness of a branch is proportional to R_{AB}, then, from a distance, the graph should resemble the structure diagram of a university.

Several distinct definitions of R_{AB} can be given, based, for example, on:

1. Distance apart in a classification of the structure of a tree or of a Birkhoff lattice;

2. Conditional probability of use, applicable both to documents and words;

3. Association factor, $P(A \& B)/P(A)P(B)$, or its logarithm, the amount of mutal information (P here means 'probability of');

4. Reference and citation: two documents are at distance 1 if one refers to the other, and n apart if connected by n such links. Two documents may be connected by several paths. Imagine an electrical network with a resistance of one ohm in each link.

Put A at voltage 1 and B at voltage 0. Then R_{AB} could be defined as the current flow from A to B.

5. Interest relevance. Let A be a customer and B a document dealing with a subject in which A is interested. Then B is relevant to A by 'interest'. This, of course, could be quantified.

Since there are various kinds of relevance, the total measure of relevance might be represented by a vector, but we suppose for the sake of simplicity that the components are linearly combined into a scalar.

We should now like to have some definition of *clumps* or *ganglia* in the linear graph, with a view to an objectivistic or semi-objectivistic classification of the material. We could define a set, C, of nodes as a *clump* if it has large *clumpiness*, where clumpiness might, for example, be defined by

$$\min_{A} \sum_{B} R_{AB}{}^{\alpha} \Big/ N^{\beta}$$

where α and β are positive constants, N is the number of nodes in C, the summation is over all B in C, and the minimum is over all A in C. A clump is *complete* if the addition of a single node would bring the clumpiness below some threshold. (There is also the possibility of replacing the matrix $R = (R_{AB})$ by some polynomial in R before applying the above definition. A polynomial in R is a linear combination of its powers, and the rth power of R can be interpreted as r-apart relevance. When all the elements of R are either 0 or 1, the powers of R have an especially simple interpretation.)[5]

If a *term* used for information retrieval has much justification, it will always correspond to a clump; also the existence of a term will tend to encourage the formation of a clump. But there will be clumps not corresponding to pre-existing terms, and these will suggest the need for new terms.

As science develops new clumps form and others dwindle in importance or even cease to be clumps. This will most easily happen if more weight is given to current measures of relevance than to past ones.

After defining a set of clumps according to any procedure and thus arriving at a set of indexing 'terms', there is still more work to do. Each clump can be examined to see if, with some other values of the parameters, it breaks up into a structure of smaller clumps. This process is analogous to the analysis of an idea once we have got the general hang of it. It is an important facility for an artificial intelligence, analogous to biological 'differentiation'. On the other hand, we can consider a new graph whose nodes are the clumps of the original graph; we can redefine 'relevance' between two nodes of the new graph by averaging the relevances that connected the two clumps originally, and we can then search for clumps in the new graph. These will be clumps of clumps (or clumps of type II) in the original graph. By continuing this procedure, we should be able to determine the general organisation of clumps, which will be partly, but not entirely, hierarchical.

If the nodes of the original graph were propositions, then the result of the analysis should be a graphical representation of the structure of the field of knowledge concerned. There would be clumps corresponding to words, things, concepts, properties, and branches and twigs of knowledge, and the relationships between them should be quantitatively exhibited. The result should shed light on all human activities and should be of great intrinsic interest.

The discussion, so far, is relevant to semantics, but does not by itself contain a definition of 'definition'. This is my next topic.

Much of modern philosophy consists of linguistic analysis. It may be asked why not consult a dictionary if you want to know the meaning of a word? The answer is that language is almost as complicated as life and cannot be captured within the covers of even a ten-volume dictionary. Linguistic analysis consists largely in defining words which you know in advance will not be satisfactorily defined in any existing dictionary. If we knew how the information was organised in our brains the knowledge would shed light on philosophical problems. There would be a neurophysiology of philosophy.

If we had a different word for every different concept, it would take more than a lifetime to learn the language. Hence, some confusion is necessary, and this keeps philosophers in a job. It pays to have the confusions, and it also pays to know the sorts of error that can arise as a consequence of the confusions. People who prefer to wallow in confusion are liable to commit errors which lead to social catastrophe. The avoidance of such errors is the main function of philosophy. So much for the philosophy of philosophy.

In lectures, the philosopher John Wisdom (not to be confused with his cousin J. O. Wisdom) has emphasised that we call an object a cow if it has enough of the properties of a cow, although no one of these properties is essential. If, from the first flying saucer that arrived from Mars, a being emerged that looked like a cow, we might very well call it a Mars-cow. In the extremely unlikely event that, apart from its origin, it was indistinguishable from an earth-cow, even to the extent of being able to interbreed with it, then I think it would be hardly misleading to call it simply a 'cow'. The proposition 'This really *is* a cow' means, I think, 'It cannot be misleading to call this a cow'. That is what 'is' is, and a concept is a generalised cow.

This definition of a definition can be improved. An object is said to belong to class C (such as the class of cows) if some function $f(p_1, p_2, \ldots, p_n)$ is positive, where the p's are the credibilities (logical probabilities) that the object has qualities $Q_1, Q_2, \ldots, Q_n$. These probabilities depend on further functions related to other qualities, on the whole more elementary, and so on. *A certain amount of circularity is typical.* For example, a connected brown patch on the retina is more likely to be caused by the presence of a cow if it has four protuberances that look like biological legs than if it has six; but each protuberance is more likely to be a biological leg if it is connected to something that resembles a cow rather than a table.

Consequently, in order to compute the probability, with respect to the evidence, that we are looking at a cow, we need in principle to solve a large set of non-linear equations, especially if the log-odds (odds $= p/(1-p)$ where p is a probability) are required to much accuracy. (A probability can often be judged to high accuracy when it is close to 0 or 1, although the log-odds cannot be.) The calculation would need to be performed iteratively if done on any existing electronic computer.

In view of the circularity in this definition of 'definition', the stratification in the structure of the cerebral cortex can be only a first approximation to the truth.[6] (Likewise, the philosophical thesis that scientific theories are hierarchically organised is only approximately true.)

The functions, f, vary to some extent from person to person, and even in the same person, from time to time. A definition, like a clump, can become of only historical interest. After a word has had a great deal of use, the relevant functions become too complicated to be readily formulated in detail, except sometimes in mathematics. Hence the difficulties of semantics.

So much for the meaning of 'meaning'.

Estrin[7] made a suggestion for the retrieval of scientific information which, although he did not say so, was a direct translation of Wisdom's cow. It is also possible to have information retrieval systems analogous to the above more general definition of 'definition'. For example, a customer could say that he wants a document that certainly contains reference to either Relativity or to the Lorentz transformation and probably refers to electrons or protons, or, if it refers to Mercury will probably refer to perihelions, etc., and the retrieval system would return a list of documents with the probabilities or relevance attached. This list would constitute an ordered bibliography.[8] It is interesting, although it should be a truism, that philosophy can have practical implications. Philosophy, like mathematics, can be pure or applied.

Logical library classification has the well-known feature of having approximately, but only approximately, a hierarchical structure. This is illustrated in Fig. 8.[9] It may be hoped that

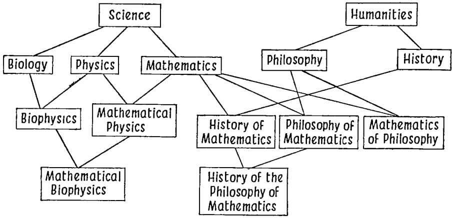

Fig. 8 THE APPROXIMATE HIERARCHICAL STRUCTURE OF KNOWLEDGE (Detail)

clumpology could lead automatically to such diagrams, and thus be of aid in library classification.[10] If the branches of knowledge are broken down very far, the tree-like structure becomes obscured, and the familiar method of decimal (or literal) classification is largely defeated.

Among other things, a brain is an information retrieval system.[11] In consequence, every suggestion concerning information retrieval can be put forward as a theory of memory. It is not surprising then that there are strong resemblances between the diagrams of the previous pbi and this one, and one on memory submitted by Christopher Scott of Addis Ababa. Personally I feel confident that recognition and recall operate by a mechanism strongly reminiscent of the definition of 'definition' and not primarily, for example, by linear hunting (like a computer running through a magnetic tape), although the latter theory has been seriously proposed.[12]

The hierarchical organisation of the central nervous system has been proposed on many occasions, for example by Mc-

Dougall[13] and Hayek[14]. For example, Hayek refers to the notion of successive acts of classification at various levels, in which the classification at one level becomes the subject of classification at the next level. In the speculations of Hebb[15], which are similar to those of Hayek, the most basic kind of clump is called a 'cell assembly'. Each cell assembly consists of a great number of neurons, all of which can reverberate simultaneously for, say, a quarter of a second, and usually gives rise to a distinctive conscious thought. When an assembly is active it tends to inhibit the remainder of the cerebral cortex, but soon it suffers from 'fatigue' and another assembly and another thought takes over.[16]

It may be conjectured that clumps much smaller than full assemblies can also reverberate, but cannot attain dominance. These sub-assemblies might be involved in the operation of the unconscious mind. In fact, many of them might reverberate simultaneously and nearly independently, especially during dreamless sleep, when no full assembly is reverberating. Even during wakefulness, it is possible that, all the time, numerous sub-assemblies, possibly hierarchically organised, simultaneously reverberate and help to determine what is the next full assembly to reverberate. The new assembly is the one that is most stimulated by the assemblies and sub-assemblies that have been recently active. Since it is largely determined by the recent full assemblies, it must have been receiving stimulating and inhibiting impulses from them simultaneously. We must assume that the inhibiting impulses, when full on (i.e. before decay set in) are more effective than stimulating impulses, but decay faster. This point should be allowed for in experiments on artificial neural networks.

The sub-assemblies could well correspond to the sub-demons de-exorcised by Selfridge,[17] and the nodes in Serebriakoff's diagram could represent sub-assemblies and assemblies. It is an interesting metaphysical speculation that the sub-demons might have some psychic existence.

Strictly, in order that a cell assembly should be classed as a clump, we should need to allow negative relevancies (I hesitate to call them 'irrelevancies'), in order to cope with inhibition. In the particular formula given above for 'clumpiness', we could take $a = 1$, and make negative relevancies numerically very large, perhaps even equal to minus infinity.

Cell assemblies correspond to named or unnamed concepts, and the logic of their functioning is the syntax of concepts. Mysterious though this syntax is, it is clear that it must involve both the idea of a clump and that of a definition.

When allowance is made for the quantising (all-or-none) effect of a neuron, the cell-assembly theory explains how an *approximation* to things we have seen before can be sharply recognised. The need for the explanation is made especially clear by the probable fact that every optical impression received throughout our waking lives is different from every other.

Even if the mechanism of the brain were deterministic, which would surprise me (in some interpretations), the assemblies would be capable of performing probabilistic somewhat-hierarchical information retrieval. The use of artificial neural networks may ultimately solve the problem of the retrieval of published scientific information. The analogy between assemblies and random superimposed coding (Zatocoding) should be noted.[18]

Although I believe that recognition and recall are performed by processes parallelling the definition of 'definition', I do not suppose that the iterative calculation mentioned before is ever performed serially in the brain. If we regard the neurons and synapses as its basic elements, a brain is a parallel computer. In a quarter of a second it performs an exceedingly large number of elementary operations, perhaps as many as 10^{14}, and this is enough, even with a great deal of duplication, to cope with the equivalent of a very complicated iterative process. In comparison with the brain all 'parallel' electronic computers built so far must be regarded as serial machines. In ten years time maybe we will have super-parallel computers capable of performing 10^{14}

binary operations per second.

What applies to information retrieval also often applies to mechanical translation.[19] The input language is at the sensorium, the output language at the motorium. Each word in the output language can be regarded as an object whose 'presence' is judged probabilistically from the text of the input language. Some words could be translated without any appreciable thought or mediating activity. If we ever construct an electronic network of the doubly hierarchical variety for mechanical translation it would be economical to have connexions between strata that were by no means adjacent, even sometimes straight from the input to the output. Of course the avoidance of mediating activity has its dangers, as in the well-known example of translation from English to Chinese and back, in which 'Out of sight, out of mind' emerged as 'The invisible are insane'.

Botryology is being applied experimentally to the 'thesaurus' approach to mechanical translation, for the automatic determination of a thesaurus.[20] A danger is that the two thesauruses found for two languages may turn out not to be nearly enough isomorphic. A proposal has been made for *encouraging* the isomorphism, by putting both the vocabularies into a single weighted oriented linear graph.[21] There would be 'relevancies' within each language, also between them. The latter would be defined by means of a statistical analysis of human translations, and perhaps also with the aid of a, not yet compiled, probabilistic dictionary. One would hope, by means of botryology, to get the two vocabularies to fall into a hierarchy of bilingual clumps.

Translation exemplifies the distinction between inwards and outwards connectivity. Most of the assemblies used in translation are concerned with both the input and the output languages. But there are assemblies that are concerned almost entirely with contemplation, as when listening to some kinds of music. In dreams, too, it seems that mainly contemplative assemblies are activated, or better 'near-assemblies' since they are not 'high' enough to affect the motorium much. Perhaps we need more mental energy to do something than just to lie there.

McDougall regarded it as an established fact that the higher levels of the nervous system exert inhibitory influences on the lower levels.[22] When our ego is high up we are unaware of the goings on in our minds that are revealed to us in dreams. The higher and bigger assemblies win out in the inhibitory game.

But if a group of sub-assemblies gang together by being cyclically activated they may be able to maintain control for some time. Such a cycle can be produced in a relaxed subject by means of a monotonous input having a rough period of several seconds. The result is known as a hypnotic trance. This theory of a chain-gang may be regarded as a slight extension of the theory of local reverberatory circuits of neurons.[23] The gang takes orders from the local external stimulus that brought it about, and puts them through to the motorium without appeal to the higher summits. These paths of unconscious communication are already present as part of the economical organisation of the cortex. Similarly it is possible for vital social phenomena to come about by means of propaganda, without official control, and such phenomena are perhaps as rare as the hypnotism of a man. I think it would be fruitful to work out some quantitative theory along these lines, in order to increase the bakedness.

One difference between ordinary sleep and a hypnotic trance is that in sleep the subdemons enjoy freedom uninhibited even by a gang of other little demons. This seems to be good for the id.

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1. This article is based in part on my papers: (i) *Brit. J. Phil. Sc.*, volume 9, page 254, 1958; (ii) 'Speculations concerning information retrieval': IBM Res. Center, Yorktown Heights, New York, 10 Dec. 1958; (iii) *International Conference on Scientific Information*, 1958 (Washington, D.C., 1959), page 1404; (iv) 'The mind-body problem, or could an android feel pain?': (March 1960), in

Theories of the Mind (Ed. J. M. Scher, Glencoe Free Press, forthcoming); (v) 'How much science can you have at your fingertips?': *IBM J. Res. Dev.*, volume 1, pages 282–8, 1958, wherein lines 27–8 of page 283, col. ii should be deleted; (vi) Contribution to the discussion of K. Spärk-Jones's paper at the International Conference on Machine Translation of Languages and Applied Language Analysis, Sep. 1961, to be published for the National Physical Laboratory; (vii) 'Speculations on Perceptrons and other automata': *IBM Res. Lecture*, RC-115, issued June 1959. Other relevant papers, not mentioned later, include TANIMOTO, T. T.: 'An elementary mathematical theory of classification and prediction': *IBM*, 17 Nov. 1958; RESCIGNO, A. and MACCACARO, G. A.: in *Information Theory: Fourth London Symposium* (London, 1961, ed. by Colin Cherry); and SNEATH, P. H. A.: *Systematic Zoology*, volume 10, pages 118–39, 1961. The name 'botryology' derives from the combining form 'botry-'.

2. *See* the Yearbooks of the Society for General Systems Research, Ann Arbor, Michigan.
3. I understand that Christopher Alexander has been thinking about applications of clumpology to architecture, but I have not seen his work.
4. In addition to Refs. (ii) and (v) in Note 1, other work of relevance to 'relevance' includes FANO, R. M.: in *Documentation in Action*, pages 238–44, New York and London, 1956; MARON, M. E. and KUHNS, J. L.: 'On relevance, probabilistic indexing and information retrieval': *J. Assn. Computing Machinery*, volume 7, pages 216–44, 1960; BAR-HILLEL, Y.: 'Some theoretical aspects of the mechanization of literature searching': Jerusalem, *Hebrew Univ., Tech. Rep. No.* 3, 74 pp., April 1960, and BAR-HILLEL, ICSI (*see* Note 1), page 1405; and (noticed after this paper was written) WATANABE, S.: *Information and Control*, volume 4, pages 291–6, 1961.
5. For other definitions, *see* PARKER-RHODES, A. F.: 'Notes for a Prodromus to the Theory of Clumps', LRU. 911.2, Cambridge Language Research Unit, January 1959; and later papers issued by the CLRU by the same author and NEEDHAM, R. M. Needham points out that in the actual search for clumps it is convenient to use definitions not involving 'min'. But having found an approximation to a clump, a definition involving 'min' may very well be used for cleaning it up. In Ref. (ii) of Note 1, a suggestion is made for finding nodes that are good candidates

for inaugurating clumps, namely those corresponding to the large components of the main eigenvector of R, or to large row sums of R^2 or R^3, etc.

6. HAYEK, F. A.: *The Sensory Order:* page 70, London, 1952.
7. ESTRIN, G.: ICSI (*see* Note 1), page 1392.
8. Quoted from Ref. (ii) of Note 1. *See also* the paper by MARON and KUHNS (Note 4).
9. Similar to a diagram in Ref. (ii) of Note 1.
10. Experiments are being organised at the CLRU (*see* Note 5).
11. The elaboration of this fact was one of the themes of Ref. (v) of Note 1, and of a paper by FAIRTHORNE, R. A., at the Fourth London Symp. on Information Theory (*see* Note 1).
12. For references and discussion, *see Nature*, volume 179, page 595, 1957.
13. McDOUGALL, W.: *An Introduction to Social Psychology*, Methuen, London, 1908; page 433, 25th edn. 1943.
14. *See* Note 6.
15. HEBB, D. O.: *The Organization of Behaviour:* Wiley, 1949.
16. The need for inhibition, to make the cell assembly theory plausible, was pointed out by MILNER, P. M.: *Psych. Rev.*, volume 64, pages 242–52, 1957. The *A.Ed.* suggests that sometimes the assemblies do not dissolve readily, and that this could lead both to the single-mindedness of many great men, also to obsessional insanity and to many mental disorders. My own feeling is that these qualities and faults would sometimes be a consequence of the logical organisation of the assemblies, such as the existence of closed cycles, acquired in nurture; but that there is often a genetic or endocrinal cause for the assemblies to dissolve only slowly. These remarks should be compared with pbi 31.
17. SELFRIDGE, O. G.: 'Pandemonium: A paradigm for learning': in *Mechanization of Thought Processes*, pages 511–26, HMSO, London, 1959.
18. *See* Ref. (vii) of Note 1.
19. MASTERMAN, M., NEEDHAM, R. M. and SPÄRK-JONES, K.: ICSI, pages 917–35 (*see* Note 1).
20. By the CLRU.
21. *See* Ref. (vi) in Note 1.
22. *See* Note 13, page 441.
23. *See* pbi No. 30, by Alexander Forbes.

34 De-armchairisation

by W. MAYS

One of the persistent myths in the folklore of modern philosophy is that philosophical questions are immune from experimental study. As Ayer once put it, 'Philosophical theories are not tested by observation. They are neutral with respect to matters of fact.'* But he now tells us, 'It appears that philosophy does after all intrude upon questions of empirical fact.'†

In recent years there have been a number of attempts to study philosophical questions empirically. Jean Piaget, the Swiss psychologist, has, for example, approached epistemological questions by making a detailed empirical study of the genetic development of such scientific concepts as number, space, time, etc., reconstructing their development as a system of successive levels in the child's thought. What is really important about Piaget's approach is that it gives us, for the first time, a method of testing experimentally many of the concepts and principles which philosophers and logicians have been discussing on a normative level for centuries. It must be remembered that these concepts and principles have in the past been regarded as sacrosanct and closed to empirical investigations. Nevertheless, normative rules cannot be found without first studying actual usage.

The Norwegian philosopher Arne Naess has been another pioneer in this field. He used the questionnaire method to investigate the philosophical opinions of the ordinary man regarding the notion of 'truth'. Naess found that the more sophisticated person tended to accept a relative theory of truth, while the ordinary man tended to accept an absolute theory.

Naess believes that the techniques of the social sciences, in the

* *The Problem of Knowledge:* page 7, Pelican.

† *Philosophy and Language:* page 20, O.U.P. Compare Max Black, *Language and Philosophy*, page 165, Ithaca, 1949.

widest sense, are relevant to some philosophical discussions. He bewails the paucity of interest in the application of such techniques to philosophical questions. He says, if we take up a volume of modern philosophy, we will find talk about other people's behaviour, the usage of terms, the intended meanings, the habits, attitudes, etc. of human beings. 'Hundreds of sayings,' he goes on, 'about the ordinary usage of "true" or the common-sense view of truth or absolute certainty are incorporated in philosophical theses, but in spite of this very little is done to elucidate the problems by means of research.'*

Although among philosophers Arne Naess has been a pioneer in this field, many more such research studies, using objective experimental procedures, are necessary. It is not asserted that this sort of study will give us a justification of philosophical concepts. All that is claimed is that it may be a useful auxiliary to philosophical studies, and add another dimension to them. We have indeed been moving towards such an experimental approach from the time of Ernst Mach onwards, with logical constructions and operational definitions of concepts. In place of the rational reconstructions of the logical positivists, we now look for an experimental reconstruction.

Piaget, for example, has, in his comparative study of concept-formation in the child and adult, used the clinical method of question and answer. In recent years he has supplemented this by means of laboratory experiments. Naess, on the other hand, influenced by the methods of the social sciences, prefers to use the questionnaire method. But this method does not enable us to probe below the surface of the responses.

The kinds of experimentation should depend upon the type of philosophical question we wish to study. Now there has been a shift from the older types of epistemology, enshrined, for example, in the work of Kant, with its interest in the nature of time and space, causality, etc., to somewhat different

* *Proc. of the XIIth Int. Cong. of Philosophy:* 'Philosophers and Research in the Soft Sciences', pages 256–7.

types of question, such as synonymy, analytic-synthetic, extension-intension, semantic truth, etc. The whole tendency of modern epistemology has been to shift its interest from perceptual and physical problems, to ones concerned with problems in language and meaning. One of the tasks, for example, of philosophical semantics is to make the meaning of the words we use more precise.

We need to note that there is a difference between the concepts used by science and those used by philosophy. To the scientist it is of little moment if his concepts contradict those of the man in the street. An excellent example of this is to be seen in the heliocentric hypothesis. With philosophical ideas things are a little different. They ought to be discoverable in the thought of the ordinary man, as well as in the sophisticated theories of the philosopher. Philosophical questions cannot be entirely divorced from psychological ones.

35 Musicolour

by GORDON PASK

The construction of an automaton that will produce an aesthetically valuable output autonomously is an exceedingly complex undertaking. It is, however, possible to envisage relatively simply systems capable of *co-operating* with a human operator in the production of aesthetic material. The device described below illustrates some principles on which such systems could be based. It was built by Robert McKinnon Wood and me in 1954 and 1955, and is called 'Musicolour'.

The device associates changing patterns of shape and colour with music produced by any musical instrument under the control of the operator. It is, thus, different in principle from Disney's *Fantasia*, but is related to the colour-music system developed by Professor Lehrner at the Moscow Academy of

Sciences and exhibited at the recent Soviet Exhibition in London. It differs from this in that the association between a particular sequence of sounds, say $M(t)$, and of visual patterns, say $V(t)$, is not fixed but can be varied automatically by the system in such a way as to improve the value of the association for a particular operator. At any moment, then, the device is characterised by a transformation λ,

$$\lambda \left\{ M(t) \right\} = V(t),$$

which depends on a set of parameters x_i. These parameters are varied according to two kinds of rule so chosen that the device operates as a 'learning machine' and as a 'teaching machine'.

The learning aspect of the device is based on a feed-back from the operator indicating his approval or disapproval of the current output. In this respect it operates exactly like an industrial optimiser. The teaching aspect, clearly related to the learning aspect, is directed towards enabling the operator to modify his behaviour, i.e. the input $M(t)$, in such a way as to obtain the best possible performance from the device. This is done by making the machine vary its parameters in directions that will cause an increase in the average variation of the output $V(t)$, thus providing the operator with as much information as possible.

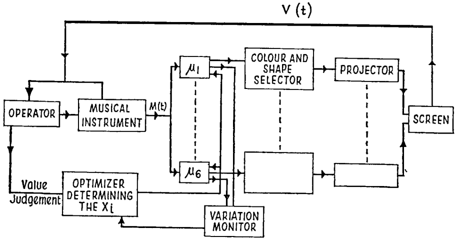

Fig. 9. Musicolour.

The flow diagram (Fig. 9) shows some details of a system we have actually constructed and investigated. The input $M(t)$ is passed through a set of six variable filters $\mu_1 \ldots \mu_6$ whose characteristics depend on the x_i. The output of each of these filters controls the selection of colours and shapes projected on to a screen by a projector associated with that filter, the selection depending on the x_i. The output $V(t)$ is the pattern produced by all six projectors operating simultaneously. Some measure of the average total variation of the outputs of the μ_j over a suitable period of time (between $\frac{1}{2}$ and 2 min.) is monitored and an optimising system varies the parameters so as to increase this quantity just as in any 'hill-climbing' optimiser. The operator's feed-back is used in the same way.

In judging the performance of the device the operator and machine must be regarded as a single system, as is shown in the diagram. Experiments with over fifty performers indicate not only that the operator eventually obtains a better performance than he did initially (which would, in itself, simply mean that he has learned to operate the device as one learns to play any musical instrument) but also that the parameters reach a stationary state, i.e. that the device learns in addition to the operator.

The apparatus consisted of one rack of electronic equipment plus some rather sizeable servomechanisms for the mechanical control.

36 Audiovisual Reading

by I. J. GOOD

When we read we waste the aural input. Is there some way of taking information in faster by making use of the eyes and ears simultaneously? A machine that could follow the eye movements and read aloud to us synchronously would not be helpful: we read much too fast for that. Granted that a machine could follow our eye movements, the question is: *what noises ought the machine to produce?*

For example, should the machine pronounce only the more informative words, i.e. on the whole, the rarer words, isolated italicised words, and 'not'? For a cheap machine the rare words could be defined as those not in its store. An expensive machine might use a complicated procedure depending on the probabilities, or even on the conditional probabilities of the words.

Another possibility is that the machine could become noisy if the eye movements indicated that the reader was falling asleep, or otherwise reading inefficiently.

37 Winking at Computers

by MARVIN L. MINSKY

In coupling a computer to a man, for use as a thinking aid, it might be very profitable to use the oculomotor signals as the computer input. This is a channel of high information rate but, perhaps more important, it might have a special psychological character. For unlike other motor actions, signals to move the eyes have a character somewhat akin to internal signals requesting information from memory. The effect of operating the computer in this manner might then have more of the quality of using a structure already within the mind as contrasted with using an artificial accessory.*

* Perhaps the machine should be fitted with mobile glass eyes: if it could interpret angry, bashful, and glazed eyes, perhaps it could simulate them too. Compare pbi No. 36. *G. Ed.*

38 Where is Our Microtechnology?

by MARVIN L. MINSKY

Clearly it is possible to have complex machines the size of a flea; probably one can have them the size of bacterial cells. Consider the amount of effort that has been spent on miniaturisation of electronic components and circuits; the expenses must total thousands of millions. But all of this has been spent on special jobs. None of the immense effort has gone into developing general-purpose small fabrication equipment. That line of attack has been neglected.

Consider contemporary efforts towards constructing small fast computers. The main line of attack is concentrated on 'printing' or evaporation through masks. This is surely attractive; in one operation one can print thousands of elements. But an alternative, equally attractive, has been ignored. Imagine small machines fabricating small elements at kilocycle rates. (The speed of small mechanical devices is extremely high.) Again, one can hope to make thousands of elements per second. But the generality of the mechanical approach is much greater since there are many structures that do not lend themselves easily to laminar mask construction.

This idea is in the air, but no one seems about to expend a few millions to start it off. Once we have a few computer-controlled microfabrication machines the project could advance very rapidly.

39 Remote-control Dentistry

by I. J. GOOD

This contribution is not so much a proposal for remote-control dentistry, as a suggestion of a problem, the problem being to guess what the cost would be in, say, fifteen years from now.

If a dentist could operate by remote control he would be able to work sitting down in comfort at a control panel, and he would become less tired and make fewer mistakes, provided of course that the remote-control apparatus was good enough. His accuracy could be improved still further if the (stereoscopic) image of the mouth could be larger than the real mouth. In fact the size could even be variable, going right up to the size of a room. I hesitate to suggest as large a magnification as this, except that it is amusing to imagine the dentist operating with a pneumatic drill from the inside of the mouth.

It should be held in mind that the apparatus, if feasible at all, could be produced in tens of thousands, so that the development costs would not be very important, especially as the techniques developed would have many other applications. If the price could be got down to £10,000 per machine, then the scheme would in my opinion be well worth while. The patients would be prepared to pay more for shorter sessions than at present.

Note that any electronic computation required could be done centrally, and shared between several remote-control machines.

40 Self-organising Pumps and Barges

by GORDON PASK

Contractile polymers exist whose contraction is triggered by a change in the concentration of a specific ion in a surrounding aqueous solution. There are also chemical 'computing systems' wherein metallic threads acting as the primitive components are built up by electrodeposition from an aqueous solution when current is passed between electrodes. When suitably energised the thread structures are self-organising systems behaving at any moment as a network of 'artificial neurones', for the metallic threads can perform the necessary functions of amplification and connexion, signal-wise, between different points. Further, the development of specific structures can be reinforced, either by varying the total current density or the total concentration of metallic ions. Hence, an observer watching the behaviour of a developing network is able to thwart or encourage particular lines of development, using one method of reinforcement or the other. As demonstrated so far, a signal, in the system, has been a sharp and localised change of current. Whenever such a change occurs the local ionic environment is modified.

It would be possible to develop a network on the surface of a sheet of contractile polymer, lying in a suitable solution. The ionic changes corresponding with signals in the network would stimulate local ionic activity and the effect of this would, in turn, lead to further stimulation. In this application, the amplifying characteristics of the thread structures are unnecessary, for the sudden triggered contractions of the material will deform the connective network and constitute a functional amplifying mechanism. Given uniform reinforcement and a radial current distribution (between a central point electrode and an outer ring), I predict development of a network which would organise circularly symmetrical contractions of the sheet (having radial and circular components). Reinforcement of particular modes of activity would lead to the development of a network structure in which these modes become more probable and dominant. In principle we have a sort of organisation in which the details of the controlling network will have been adapted in the process of evaluation to satisfy the overall criteria for reinforcement.

One practical suggestion along these lines is a distributed pump acting, like the intestine, by peristalsis. Consider a double-walled tube, the space between the walls filled by the solution we have discussed. The tube is made of strong plastic material, but attached to the plastic there are longitudinal and transverse strips of the contractile polymer exposed to the solution. Current is passed (in the simplest case, lengthwise down the cavity) and some kind of contraction will occur. Let articles (which may be as irregular as those that the intestine* will handle) be placed in one end of the tube and let the system be reinforced if and only if, as a result of the contractions, they emerge from the other end. In practice, of course, I recommend a stage-by-stage reinforcement, and it must be possible to renew the solution. Such a distributed pump would optimise its organisation to contend with variation in the articles to be conveyed as well as with changes in the external environment.

One other possibility is a sea creature. Take something like a 'Dracona' barge – a towed 'bag' used for transporting oil. Provide it with the network-forming and contractile systems embedded in its structure. In addition there is a radio receiver and servomechanism such that the system is reinforced when the sea creature is heading in a desired direction. The creature will 'learn' to propel itself. Since it must carry its own power source, the creature is practicable only in so far as it also learns to make use of the energy of water movement – but after superficial consideration I believe it could do this. In conclusion, though I am only half serious about using this particular kind of self-organising system, it certainly illustrates a principle ('peristalsis') which can be profitably realised by a more convenient technology.

* Of a camel? *G. Ed.*

4

PSI

Editorial

Pity the poor parascientist. When his results reach the one per cent. level of significance they are not believed, and when they are much more significant than that he is suspected of fraud or incompetence. His results will be generally accepted if they become readily repeatable by other experimenters. Since this has not yet happened I am including this chapter with some trepidation. But if there is anything at all in parascience it is very important to science and to men. *G. Ed.*

41 Parascientific Glossary

by ALAN J. MAYNE

Parascientific phenomena (often called *paranormal phenomena*) are phenomena which cannot be explained by contemporary science, even in principle, and which are thus not yet included in any of the existing generally-accepted scientific frameworks. They may be classified according to their degree of similarity with known phenomena, so that some of them are *paraphysical*, because they can be viewed as an extension of physical phenomena, others are *parapsychological*, because they can be viewed as an extension of psychological phenomena, and others, which are well beyond the present frontiers of scientific knowledge, can be called *transcendental*. In practice of course some parascientific events might consist of combinations of these types of phenomena.

A *sensitive* is a person who possesses markedly at least one parascientific faculty.

The term *psi* is often used to cover parascientific processes and faculties.

The most widely-investigated group of alleged parascientific phenomena is *ESP* ('extrasensory perception', which would be better named 'extrasensory communication'), where information is conveyed to a mind by parascientific means. Its main subdivisions are: *telepathy* (where this information is communicated from another mind), *clairvoyance* (where it is communicated from a material system other than a brain), *precognition* (where it is communicated from a future event). Another alleged parascientific phenomenon is *PK* ('psychokinesis') where a material system (usually with random behaviour as with a set of dice) is influenced by parascientific means.

Most people who claim to be sensitives believe that they can apply their parascientific faculties to various practical purposes. For example, *dowsers* search for underground water or minerals by paranormal means, *radiesthetists* attempt medical and other diagnoses by such methods, and *healers* attempt parascientific medical treatments.

The best known form of parascientific investigation is the *ESP experiment*, where tests are made for the parascientific transfer of information to a *percipient* or *subject* from a person (*agent*) or a material system. In the simplest cases, the percipient *guesses* in turn each of a series of assigned symbols (*targets*), which are also viewed by the agent if one is present. Any correspondence between a guess and its target is a *hit*.

42 Psychosomatokinetics

ANON

The science that deals with such phenomena as that, during an interview, your collar comes up over your ears and your shoe-laces come undone. (More purely: (i) the production of nervous tics, (ii) the power of bodily motion by an act of volition.)

43 ASP on ESP

by A. S. PARKES

What's to be done about extrasensory perception, a problem that lurks in a most tantalising fashion on the fringes of science? I have four suggestions to make. The first is to abolish the name because it begs the question of the nature of the supposed phenomena. Words such as telepathy (the perception by another person of a thought or an image in somebody else's mind), clairvoyance (the perception of an object or an event which is not known to anyone else) and precognition (the perception of some event or image which has not yet happened or appeared) are more explanatory and less committal.

The second suggestion is to interest professional biologists who are more likely than others to investigate such matters objectively and unemotionally. For those of us who regard man as essentially biological material, telepathy, clairvoyance or precognition, if they exist, arc properties of biological material and therefore come within the proper interest of the experimental biologist. Certainly, if they exist they are natural phenomena and any tendency to regard them as supernatural is to be deprecated.

Considered as a purely scientific matter, the problem is quite easy to formulate. It is: do telepathy, clairvoyance, or pre-cognition exist in any experimentally demonstrable form and, if so, how do they work? The word *experimental* should be stressed. Almost everyone at some time feels that he or she has been able to read someone else's mind or to perceive future or distant events. As a result a vast mass of anecdotal material has accumulated, some of which is very striking indeed, but when allowance is made for retrospective imaginings, for coincidence and for similarity of mental habits it is difficult to believe that this material is of scientific value. Another difficulty is that, for obvious reasons, the subject has been bedevilled with trickery. The combined intrusion of the supernatural, on the one hand, and of trickery, on the other, has proved most damaging, and with a few notable exceptions the field has been shunned by reputable scientists. This is a great pity, because there has now accumulated a body of evidence which it is impossible to dismiss abruptly as nonsense.

During the last thirty years or so determined efforts have been made in several centres to carry out in this difficult field experiments of a kind which would pass muster in ordinary laboratory work. On the whole the protagonists of the scientific approach, notably Dr J. B. Rhine of Duke University, U.S.A. and Dr S. G. Soal in London, have succeeded in evolving tests susceptible to statistical analysis, though whether the tests themselves have always been carried out under rigidly-controlled conditions is open to doubt. Of the various symbols or objects which have been chosen to facilitate analysis of the results of experiments in telepathy and clairvoyance, the most commonly used of all have been cards bearing symbols or pictures of animals, the number of different ones being kept down to five to facilitate statistical work. The arithmetic is thus basically of the simplest kind. If a card is turned up by one person (the agent) the probability of a second person (the caller) guessing it correctly is one in five, and if the experiment is carried out enough times one should get almost exactly 20 per cent. correct calls. Conversely, any clear excess above probability maintained in a long series of calls would be highly suggestive. If the agent looks at the card, a positive result might indicate telepathy or clairvoyance. If he does not, clairvoyance on the part of the caller would be indicated. To demonstrate pure telepathy, it would be necessary for the agent merely to visualise a card.

A vast number of tests of this kind have been carried out, and it is quite clear-cut that most people have no power to guess the nature of a symbol or picture on an unseen card above probability or sufficiently above probability to suggest that something more than chance is at work. Moreover, no one has been discovered who can guess cards with a high degree of accuracy, such, for instance, as four correct calls out of five over a long run. Nevertheless, a few people have been reported to call very significantly in excess of probability, some of them over long periods, some under conditions of clairvoyance as well as telepathy, some with agent and caller separated by long distances, and one or two apparently even showing precognition in calling significantly not on the current card but on the one to be turned up next.

What are we to make of such results? Are the usual statistical methods inapplicable to such data? Have the experiments been carried out in such a sloppy fashion that the results are meaningless, or do individuals in fact exist who show some slight powers of telepathy, clairvoyance or precognition in tests of this kind? At present, I do not think it is possible to be positive either way, but my own view is that when due allowance is made for the obvious contingencies, there still remains something to be explained. Certainly, the third thing to do about E.S.P. is to keep an open but critical mind. The fourth necessity is to find some new line of approach, because with the extreme scarcity and inconsisteney of subjects giving positive results it is not easy to see how much further progress can be made along present lines. However, one aspect of the matter has received little attention and could provide a further clue. Almost all experiments have been designed to test the powers of the caller in receiving a mental image, and thousands of people have been screened for this capacity. On the other hand, there have been few tests of the relative powers of different agents to project mental images, it being tacitly assumed that the recipient caller is the essential element in the partnership. Is it out of the question that the primary factor is the agent or that both are involved? What appears to be needed is a mammoth trial in which, say, a hundred people are each tested in each capacity with every other participant. Unfortunately, such a labour would be a very formidable undertaking and the weary routine involved might well discourage all concerned.

Considering the supposed phenomenon as a biological characteristic, the observed distribution seems distinctly odd. Can it be that the faculty is dying out, not appearing, in the course of human evolution? In other words, is the faculty vestigial rather than rudimentary? On this line of thought it would be logical to look for it in primitive rather than in highly-civilised people, and one cannot help recalling the extraordinary stories of the powers of African and other primitive tribes in becoming aware of events happening at a distance or in the future. Proper investigation of the so-called bush telegraph, if it could be carried out, might be of extreme interest. And further along this line of thought, could meaningful experiments on apes or other animals be designed and, if so, what would they show?

Whatever the future brings in this field of research I feel certain that the explanation, if one becomes necessary, will lie in the realms of science and not in that of metaphysics: it will lie in the existence of some hitherto undiscovered biological faculty in man. In support of this view can be cited the fact that new and very remarkable perceptions are constantly being discovered in lower animals in explanation of faculties hitherto appearing inexplicable. We may consider, for instance, that some moths can smell members of the other sex a mile away, that many birds navigate over vast distances by means of unknown methods and that honey-bees have perceptions of the most extraordinary nature. Among mammals, the near-blind bat avoids obstacles and locates insects by a most delicate echo-sounding system. Any human being who suddenly acquired such powers would certainly be written off as bogus. Can it be that having accepted radar and sonar and other near miracles we reject too readily the idea of psychar?

44 Are Anomalies Normal?

by CHRISTOPHER S. O'D. SCOTT

Most of us realise at the back of our minds, but few of us face the fact, that impossible things are constantly happening. Take, for example, the disappearance of an everyday object. We put down our pen on the desk and two minutes later it is not there. No matter how hard we search we can't find it. Probably the wife finds it the next day on the mantelpiece. We know we never went near the mantelpiece, but what can we say? Some sort of mistake must have happened, we tell ourselves, and forget the matter as quickly as possible.

We must all have experienced the same thing in the laboratory. From time to time experiments do go wrong. When it happens we say the materials were impure, or make some equally *ad hoc* excuse. We then repeat the experiment until it goes right; we certainly don't take the anomalous result seriously as a new natural phenomenon.

Maybe we ought to pay more attention to this kind of thing. A few people have done so, but limiting their attention always to a special field. The early psychical researchers, for example, looked for cases of telepathy and clairvoyance and found them. Dr Rhine then extended the search to cover a hypothetical psychokinetic effect. Again he found it. Spencer Brown went even further and looked for a generalised non-randomness effect; he too found what he was looking for. If the laws of nature are constantly being broken, we should *expect* each person to find the particular anomaly he is looking for. I suggest a new law of nature: natural anomalies of all kinds occur with perceptible frequency.

If the above is half-baked, what follows is one-hundredth baked.

Not *all* kinds of anomalies occur. Machinery, for example, can be made consistently reliable. Skyscrapers never fall down. Judging by psychical research, the anomalies have certain consistent characteristics. For example, they are trivial, they tend to be associated with particular individuals, they tend to occur better in American universities than British. Nevertheless they are quite unpredictable and, within these limitations, seemingly random.

To the statistician, this combination of random occurrence within humanly meaningful categories immediately suggests one thing: *stratified random sampling*. A scientifically-trained anthropologist (there aren't any in our world) wishing to obtain maximum information about a society with minimum interference would use this method. Briefly, it involves selecting purposively certain categories of event or object for study and distributing observations at random among the constituents of each category.

May we then be the victims of some kind of extraterrestrial anthropologist, temporarily abstracting our pencil for study as a representative human artifact (twentieth century), following Professor Rhine around as a representative of some key human type, and (perhaps accidentally) reacting on his personal cause-effect sequence with every random observation of the professor's work?

If this is true, maybe this article will show the intruder that he has gone too far, that the secret is out; and maybe this will put a stop to his activities.

Or will he protect himself by preventing its publication?

My own guess is that he is too intelligent to fall for either of these policies. He has learned enough about human nature by now to know that life on Earth will go on exactly as before even after the publication of this article. [Probably his sample did not include this article. *G.Ed.* Probably his sample *does* include myself, judging by my unusual experiences! *A.Ed.*]

45 Speculations Concerning Precognition

by I. J. GOOD

1. In 1945, the famous British mathematician, J. H. C. Whitehead, quite informally made the following suggestion to me in conversation: 'Perhaps we each have a guardian angel out in space, to whom we send out thought waves faster than light, and who reflects them back. According to the special theory of relativity the signal would return before it was sent, and this might explain precognition.'

It is of course one of the usual tenets of the special theory of relativity that a signal cannot be sent faster than light. But this is a tenet partly because the opposite assumption would lead to causal anomalies.[1]

If B moves relative to A with velocity, v, where $v < c$, his clock is slowed down, in A's frame of reference, by a factor $\sqrt{(1 - v^2/c^2)}$, where c is the velocity of light. If $v > c$, the slowing down factor is formally not negative but imaginary. Hence, velocities greater than that of light are formally inconsistent with the special theory of relativity,[2] and if we assume that such velocities are possible we can only guess what the effect would be. A simple hypothesis is that, if radiation travels much faster than light, from a point P to a point Q and back, then it arrives back after a delay equal to *minus* the time that light would have taken to perform the same journey.

The question arises whether there is any evidence for this hypothesis. Curiously enough, there is. For the time light takes to get to the moon and back is

$$2 \times 240{,}000/186{,}000 = 2.6 \text{ seconds},$$

and this is precisely the average time of the precognitive effect in Soal's experiments with Shackleton![3] It may be asked whether the moon was above the horizon when significant results were obtained. Mr Stephen Abrams has kindly inspected the data and concluded that there was no demonstrable relation between the position of the moon and Shackleton's rate of success or between the earth-moon distance and his guessing time. So the hypothesis seems to be moonshine.

2. My reply to Professor Whitehead was that we could dispense with the guardian angel, if we assumed a spherical universe. For the signal might then go the whole way round the spherical universe and be focused back to its original position, as in a whispering-gallery. Some distortion of the path could be expected, so that the focusing would not be sharp.[4] The return signal would be dispersed over a volume perhaps many times that of the earth. Our chance of picking up the signal transmitted by someone else would then appear to be independent of distance in terrestrial experiments. Variable time intervals could be associated with variable speeds of transmission, and with more than one circuit of the universe.

3. In an early draft of a paper on causality[5] I mentioned another theory of precognition, but deleted it in later versions because it was not completely relevant to the main theme of that paper. The idea was that, in addition to our universe, there is another moving backward in time, so that the two universes can be visualised as two trains moving in opposite directions. These two universes would not usually interact very much.[6] (Whether they are called two universes, or two halves of the same universe, is of course a purely linguistic matter.) Signals from our own future might sometimes get across to a passenger in the other universe, who might then retransmit this information to us, possibly garbled, when he reaches our present. This speculation requires that the whole of space-time can be regarded as already existing, in some sense, and seems to necessitate a second time dimension, as in the quarter-baked speculations of Dunne.[7] There are indeed some indications in physics that time can run backwards.[8] For example, Feynman suggests that a positron may be regarded as an electron momentarily moving backwards in time. An apparent objection to Feynman's theory is that it

seems to break the symmetry of the universe, since the backward motions occur only rarely. This symmetry may be restored, together with the symmetry between positive and negative charges, by assuming that there are two halves of the universe moving in opposite time directions, as in the above speculation. This speculation is in principle testable, with great difficulty, by locating a planet belonging to the other half and making observations on it at fairly close quarters. (Mr Abrams asks whether the 'handedness' of the two halves [*see* pbi No. 84] would be opposites).

4. In 1946 or 1947, I made the following remark to Professor Rosenfeld: 'The psi function [Schrödinger's wave function, not to be confused with 'psi' meaning psychic effects] in quantum mechanics is mysterious enough to provoke the conjecture that it may in some sense explain features of the mind. The inter-phenomena, in the sense of Hans Reichenbach, may, in some circumstances, be *phenomena* for the mind.' He replied that it often happens in science that a concept introduced for one purpose was found to have a quite different application. The apparent impossibility of classical descriptions of atomic phenomena and of subjective experience may have a common source.

A further feature of the wave function is that it suggests a field theory of mind. A field theory does seem to be natural in order to understand how the activities of numerous neurons in a brain somehow summate.[9] Perhaps psi depends on ψ.

One of the features of quantum mechanics is that probability densities are obtained as the product $\psi\psi^*$, where ψ^* is the complex conjugate of ψ. It is tempting, though quarter-baked, to suggest that ψ^* is the psi function of a conjugate universe. Then, if we are permitted to use complex probabilities, the product $\psi\psi^*$ would represent the probability of an interaction between the two universes. Since the time co-ordinate in special relativity can be formally regarded as a space direction multiplied by $\sqrt{(-1)}$, it is formally natural to replace ψ by ψ^* in the conjugate universe if the direction of time is reversed.

Unfortunately, this form of the two-worlds theory (which I thought of in 1947, together with the possibility of thereby restoring symmetry between electrons and positrons) is very nearly self-contradictory, since it assumes that the complex probabilities in the two worlds represent statistically independent events, for otherwise why multiply them? And in this case why should the number of particles be preserved? The preservation of the number of particles is closely connected with the so-called 'reduction of the wave packet', for which no one has yet offered a satisfactory philosophical 'explanation'.

5. As another speculation, consider the fantastic sensitivity of the human sensory organs and nervous system. The ear can detect atmospheric vibrations of amplitude one hundred millionth of a centimetre, and, under suitable conditions, the eye can detect the presence of only a few quanta of light.[10] It may well be that the Second Law of Thermodynamics, which is statistical, is not entirely applicable in these circumstances, nor in the operation of the brain at very low energy levels. But, according to Eddington, the Second Law of Thermodynamics is the only law of physics that describes the direction of time. So, when thinking with the minimum of energy, one's thoughts may be influenced by the future as well as by the past. (Note also the uncertainty principle in the form $\Delta t \, \Delta E > h/4\pi$.)

When I drew Mr Whateley Carington's attention to this speculation in 1944, he referred me to a section of one of his own papers,[11] with the remark: 'I am not at all sure that it is even approximately on the right lines; or rather, perhaps, that if it is, then it will come to much the same thing as yours in the end.'

6. The most familiar paradox of time-travel and precognition is that if you can see the future you can change it, so what did you see? A possible resolution of this paradox is in terms of the theory of the 'branching universe'. This theory occurred.

(i) in science fiction in a quarter-baked form;

(ii) in 1954, in a 0·4-baked form, when I used it in a discussion from the floor at a congress of philosophers at

Oxford, in order to point out where the probabilities in quantum mechanics might come from, and also in order to prove that it was non-tautological to say that a cause precedes its effect (*see also* pbi No. 104);

(iii) in 1957, in a serious scientific paper by Everett[12] as a metaphysic for quantum mechanics.

In a nutshell this theory states that 'in reality' the universe branches out at each micro-micro-instant into countless myriads of universes having no communication with one another by any known method. The theory seems intuitively to require that space-time has at least one extra dimension. The extra dimension is not necessarily either spacial or temporal.[13]

The psychophysical twist to the branching-universe theory is that you can, after all, get a glimpse into a future. But this is not a future of *all* the individuals into which you split at each moment. In other words: along a second time dimension countless futures exist *now*, but each will seem unique when it arrives along the ordinary time dimension. The simplest hypothesis is that the probability of precognising a possible future is proportional to its physical probability. We all have innumerable identical twins with whom we very seldom communicate.

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10. HECHT, S. *et al.: Science*: volume 93, pages 585–7, 1941.
11. WHATELEY CARINGTON: 'Experiments on the Paranormal Cognition of Drawings, IV': *Proc. Soc. Psych. Res.*, volume 47, pages 155–228, esp. section 3, page 221, 1944.
12. EVERETT, HUGH: III, *Rev. Mod. Physics*, volume 29, pages. 454–62; followed by an enthusiastic assessment by John A. Wheeler.
13. Five-dimensional theories have been proposed for relativity and quantum mechanics. In one of these theories, the radius of curvature, along the fifth dimension, is only 10^{-30} cm! For several references, *see* WHITTAKER, SIR EDMUND: *History of the Theories of Aether and Electricity*, 1900–1926, page 191, 1953. BAILEY, V. A.: *Nature*, volume 184, page 537, 1959, and volume 186, pages 508–10, 1960; EDDINGTON, SIR ARTHUR: *Fundamental Theory:* Chapter III, 1946; HASKEY, H. W.: *Proc. Edin. Math. Soc.:* (2), volume 7, pages 174–82, 1946; CORBEN, H. C.:

Rend. Sem. Mat. Fis. Milano: volume 23, pages 152–63, 1952; and STROMBERG, G.: *J. Franklin Inst.:* volume 272, pages 134–44, 1961, who refers to BENNETT, J. G., and others. A speculation that space has four dimensions was made as early as 1873 by Zöllner, in order to explain alleged disappearance and appearance of objects. (*See* KLEIN, FELIX: *Elementary Mathematics from an Advanced Standpoint:* volume II, page 62, Engl. trans. 1939.) It can now serve for the modern theories of continual creation and annihilation (Bondi, Gold, Hoyle, Kapp). Doog truly said that the metaphysics of today is the physics of tomorrow. It would be interesting to know whether Einstein was in the least influenced by Zöllner's half-baked idea. *See also* FECHNER, G. T.: 'Der Raum hat vier Dimensionen' (1846) in *Kleine Schriften von 'Dr Mises'*, pages 254–76, Leipzig, Breitkopf und Härtels, 1875. A doctoral dissertation has been written on five-dimensional field theories by TUPPER, B. O. J., 1958 (London University), unpublished.

46 The 'Fitting-in' of Parapsychological Experiments

by GERTRUDE R. SCHMEIDLER

Biologists, psychologists, and social scientists could incorporate small parapsychological investigations into their own experiments. Surprisingly often, they could fit them into unoccupied niches of experimental time, and accumulate information with a minimum expenditure of research effort.

For example, ESP tests incorporated in studies of drug effects, localised brain injuries, or electroencephalography might identify the brain processes or brain areas that facilitate or inhibit ESP, along with those that facilitate or inhibit learning and perception. Patterns of ESP success could be investigated in conjunction with research on social interaction, group activity and leadership. In fact, parapsychological experiments could usefully be included in almost any type of investigation of a whole functioning organism, since ESP scores may depend on many physiological, psychological and social variables. Some of these correlations are already being studied by parapsychologists, for example those depending on personality and attitude.

47 Technological Routes to Immortality

by JAMES S. HAYES

An experimental biologist specialising in the study of human behaviour, and more particularly one who employs, as I do, the observational techniques invented by Freud, is perhaps particularly likely to find himself speculating on the possibility of technological routes to immortality. Freudian psychoanalysis itself exists fairly close to the murky borderline between respectable technological science and religion. Medicine shades into psychiatry; psychiatry into psychoanalysis; and the latter merges by almost imperceptible gradations into philosophy and religion – in the pejorative sense of those words.

If a religion is defined as a socially transmitted system of beliefs, by means of which people attempt to order their behaviour, then science itself is a religion; and in certain areas of behaviour (those pertaining to food production and the care of the body and of machinery) it is the dominant religion of a large part of our species. In other areas, for instance the regulation of interpersonal and sexual relationships, it is much less successful, and in regard to the strong desire of the individual to go on living forever it has as yet nothing concrete to offer at all.

These are precisely the areas in which the analyst is attempting to work, and is in direct competition with the purveyors of the traditional, pre-scientific religions. The latter offer their clients an unsupported assertion that they will survive death: in the West as immaterial spirits; in the East by way of reincarnation without any memory of their previous lives. Neither assertion lends itself to observational test. Classical psychoanalysis has countered with the equally ingenious and untestable assertion of a 'death instinct'. The patient is told that he is mistaken in supposing that he would like to go on living: really he has a deep instinctive urge to die, although unfortunately (unlike his sexual urge) the desire is inherently incapable of being introspected.

I am convinced that this doctrine owes its place in psychoanalytic dogma to its consolatory function. The death *wish*, a reality attested to by the act of suicide, arises as a reaction-formation, much like impulses to excessive cleanliness or sexual abstinence.

Now, an essential part of exosomatic[1] evolution, i.e. culture and especially science and technology, is its function of first formulating widespread wishes in the form of wish-fulfilling myths and then finding ways to gratify them – albeit often in ways quite different from those adumbrated in the myths. There is a clear line of descent from the myth of Daedalus, through the fatally optimistic courage of Otto Lillienthal to the modern jet-lincr. It is possible that technology may in the near future find means to gratify the desire for immortality, although in a manner no more like that envisaged by the religious than the airliner is like the wings of Daedalus. This is quite likely, because exosomatic evolution, like organic, advances at an exponentially increasing rate. The predictions of scientists as to the likely date of a given technological achievement are characteristically wrong by about two orders of magnitude. If we think of the distance to a given technological break-through in scientist-man-hours rather than in chronological time, personal immortality is probably no farther away than controlled hydrogen fusion.

Let us first take a brief look at the possibilities of what I will call the medical route to immortality. We can certainly expect a very worthwhile extension of the effectively useful life-span, first from improvements in surgical technology and later from the un-ravelling of the fundamental nature of the ageing process.[2]

However, even if medical advances make it possible to keep one's body in good working order indefinitely, this is by no means equivalent to meeting the demand for immortality – for two quite fundamental reasons. It is immediately obvious that, if there is a positive probability of meeting one's death by accident in the course of a normal life-span, then the probability approaches unity indefinitely closely as the life-span approaches infinity. It is unlikely that means can be found to render accidental death completely impossible. But suppose, *per impossibile*, that this be done, simple arithmetic at once confronts us with the conclusion that, were any births to occur, however infrequently, in a truly immortal population, a date must occur at which even a solid sphere of human bodies expanding radially at the velocity of light would not be adequate to contain us all. And no biologist could feel happy about the ultimate prospects in this uncertain universe of a species which had altogether ceased to reproduce.

If we really intend to find a way to immortality, therefore, we must look in another direction.

Several lines of thought converge to suggest what that direction may be. Some arise out of the psychoanalytical experience. Many analysts, from Freud onwards, have documented what appeared to be instances of telepathy between themselves and their patients. I have frequently made observations of this kind myself, and have also found the hypothesis of telepathy between a patient and his pathogenic mother or father, however unwelcome, less implausible in accounting for strikingly bizarre behaviour than any alternative I could devise. Many people dislike the idea that telepathy can occur, for a variety of reasons of which the most respectable, hence most frequently conscious, is that it does not fit smoothly into our current physical theories.[3] This objection relies on a misunderstanding of the status of physical theories. They are not the Truth; they are a collection of pragmatically useful models. If the whole of physical theory could be mapped on to a single self-consistent axiomatic system the objection would have force, but this is not the case, and I believe it to be a rationalisation. I have long felt that the conventional questions asked about telepathy ('Does it occur, and if so, how?') are less likely to be fruitful than the question: 'If telepathy occurs at all, what prevents it from occurring all the time? How does the mind (or the brain) insulate itself from the potential influx of other people's experiences?'

I want to suggest that this protective mechanism may be our old psychoanalytic friend, the Freudian 'censor'. We don't know what is going on in other people's heads for exactly the same reason that we don't know most of what is going on in our own: because we don't want to know.

This has been shown to be true for *inferential* knowledge of the activities of other minds and we may hope that psychotropic drugs like LSD 25, which seem to be capable of allaying the pain associated with the acquisition of unwelcome knowledge,[4] may open the way to the large-scale use of telepathy. And if, *by this or any other means*, a really high-capacity channel for the transmission of information from one brain to another can be developed, the problem of achieving personal immortality is in principle solved.

Consider the following situation.

Your body, by now perhaps 130 years old, and consisting for the most part of prosthetic devices, is lying partly in a hospital bed and partly on the surrounding shelves and tables. It will soon become impracticable to maintain the necessary oxidative reactions in your brain. Someone younger is wheeled in, his own mental activities are temporarily suspended and the process of transferring to his brain the whole of the information content of your own begins. As it nears completion, you begin to be aware of sensations deriving from his exteroceptors and proprioceptors as well as your own. You see with his eyes, hear with his ears. A sympathetic technician asks what was formerly the other fellow's body how you are feeling, and on receiving a satisfactory reply turns off what was, a few minutes ago, your heart. After a short rest period, the temporary block on access to the experiences acquired by your new body on its own account is removed, and you begin to remember having been him as well as yourself. But this in no way diminishes you, nor decreases your certain knowledge of your own existence.

The process must clearly be symmetrical: to your host he is still himself with an extra lifetime of experience to draw upon. For '. . . [in] immediate experience . . . consciousness is a singular of which the plural is unknown.'[5]

Since personal identity, like life itself, clearly consists of an ordered structure of information, a 'message', and since messages can be replicated indefinitely many times, there is no reason why any one personal identity could not be simultaneously made available to several brains and be said to inhabit simultaneously many bodies. There is a clear advantage in such multiple replication. For if an identity using one body has a probability, say $p = 0{\cdot}5$, of being accidentally destroyed in unit time, then if it is using two unrelated bodies this probability reduces to $p = 0{\cdot}25$, and so on; by the use of many bodies the probability of the accidental extinction of a personal identity can be made as small as we please. A time may come when almost every person one meets will have at least some part of his identity in common with oneself. Under these conditions the death of a body would not be a reason for grief, as now death is a reason for grief.

Quite other lines of thought also suggest that such developments as these must be expected. From a broad biological point of view, our subjective wishes for personal immortality are almost irrelevant, perhaps no more, if no less, relevant than our subjective enjoyment of copulation is to the biological advantages of sexual reproduction.

Since the formulation of information theory we need no longer be uneasy about speaking of evolutionary 'progress' or 'advance'. A 'higher' species means one which stores more information, a property which logically entails a lower probability of extinction by random environmental change[6]. From this point of view the essential continuity of exosomatic and organic evolution is self-evident, and the developments I am envisaging are merely an extension and improvement of what we already do imperfectly by means of libraries, letters, telephone systems and the like. If identity resides in ordered information, the distinction between the metaphorical immortality of a man in his work and his literal personal immortality is one of quantity rather than (or as the Marxists would say, transforming into) one of quality. It is merely a matter of a difference of a few orders of magnitude in the amount of information transmitted.

As the ratio between the amount of useful information available to our species as a whole, and that accessible to any one individual, becomes larger at an (as usual) exponentially increasing rate, we can less and less afford the inefficiency involved in personal death. Every scientist struggling hopelessly to catch (we no longer say keep) up with the literature will understand exactly what I mean.

Nonetheless, however completely the relevant technological problems are solved there are certain limitations which are not technological – limitations of principle. Even though we reduce the problem of the survival of the human individual to the problem of the storage of a quantity of information, information can only be stored in physical systems and the quantum properties of matter set an immovable upper bound to the quantity of information which can be stored in a physical system of any given size. We can calculate an upper bound for the information storage capacity of a human brain from quantum-mechanical considerations, hence infer an impassable limit to the number of 'life-experience units' which could be made available in their entirety to any single human body.

It is of course perfectly possible that men may prefer to perpetuate their life experiences in the memory storage banks of giant computing engines rather than, or as well as, in the brains of their descendants. But even so, the same considerations must apply. If each of us insists on the preservation of the whole of our life experiences, then the technological developments we have been considering will merely delay the date at which even a solid sphere of organised matter expanding at the velocity of light will be inadequate to contain us all. The conclusion is inescapable, therefore, that if we find a complete technological means to individual immortality, we will be constrained by the most fundamental properties of the universe we inhabit to renounce, voluntarily, a part of our achievement and reinstate an equivalent of death: namely, the jettisoning of those parts of our memory systems which our expanded consciousness discern to be least essential, least valuable, at any given time. The grave, therefore, must always claim a partial victory, albeit death will have lost its personal sting.

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48 Explosive Telepathic Fields

by I. J. GOOD

Owing to the shortage of good telepathic percipients, the following experiment has not yet been tried, although I proposed it in 1950 in a private communication to Dr S. G. Soal. Suppose that, say six, very good percipients were simultaneously put to work at card-guessing. Perhaps their 'mental fields' would somehow interact and support one another so that their combined guess would nearly always be correct. It would be like a collection of Uranium 235 atoms above the critical size, and not simply a matter of independent 'voting'.* (If averagely bad percipients were also present they might very well tend to ruin the results.) If there is a critical size for a group of outstanding percipients, we do not know what that size is. But if the effect were once demonstrated there is no obvious limit to its effectiveness. Apart from the potential value to such things as military intelligence, it might also lead to an understanding of the nature of ESP. Then we may manage to discover an ESP drug, 'espalin' let us say. When we all take espalin we may all go into a state of joint consciousness, like the Martian particles in Olaf Stapleton's *Last and First Men.* (Most 'psionicists' would say that such a 'group mind' would not occur unless the individuals were in a reasonable state of rapport or mutual harmony. Dr R. H. Thouless suggests that this may be achieved by some form of joint activity, such as community singing or a Quaker silence with everyone holding hands with his neighbours.) Instead of a consciousness depending on 10^{10} neurons we may look forward to one depending on 10^{20} neurons. Perhaps this is what the collective unconscious is waiting for.

* The complementary suggestion of getting one good percipient to make several guesses at what is (unknown to him) a single target, as a method of 'putting psi to work', is made by THOULESS, R. H.: *Internl. J. Parapsychology*: volume 2, pages 21–36, 1960.

49 A Method for Encouraging Clairvoyance in Rats by Electrical Stimulation of Cerebral 'Pleasure Centres'

by STEPHEN I. ABRAMS

Nothing is known at present about the phylogenesis of psi phenomena, but there has been a certain amount of argument about whether they are vestigial. There has even been speculation by Sir Alister Hardy on the possible role of telepathy in evolution.[1] But folk tales about strange abilities of animals and a limited amount of research with horses, cats, butterflies, dogs, pigeons and paramecium have provided some support for 'an-psi'.[2] If animals should have ESP and PK, they would be useful subjects in parapsychological research, because legal, moral and practical difficulties restrict the scope of research with humans.

The evidence of human parapsychology suggests that the laws of psi perception are unlikely to be much different from the laws of normal perception. In particular, one's chances of success in an experiment depend in considerable part on one's motivation.[2,3] Therefore, in comparative parapsychology motivation should be a variable of the first importance. A simple experimental device where animal motivation may be studied is the Skinner box. Here a rat or a pigeon is placed in a box with a lever in it. When the lever is depressed, the animal is rewarded with food pellets or with water. The animals quickly catch on to the trick and can eventually be persuaded to play ping-pong and to pilot guided missiles.

A more powerful incentive than food or water was discovered several years ago by James Olds, working in Hebb's laboratory at McGill. He found that electrical stimulation of certain areas of the rat brain, especially the septum, appeared to produce pleasant sensations. When the animals were placed in Skinner boxes and reinforced with a couple of volts of alternating current transmitted to chronically implanted electrodes, they stimulated themselves as often as 7,500 times in a twelve-hour period.[4]

This technique might be adapted to an ESP situation. The rats would be placed in a Skinner box with two levers in it. After the first acquisition session, a rat would obtain cerebral reinforcement only when he pushed the right lever. Which lever was the 'right' one would depend on the action of a randomising device. A rat which stimulated itself significantly more than 50 per cent. of the time would be a clairvoyant rat.

However, the alternation behaviour of rats is such as to suggest that they would, very sensibly, take up a position at one of the levers and keep pressing it, knowing that they would be stimulated half the time. To correct for this, the rats could be given mild external shocks, transmitted by a wire grid on the floor of the box, every time they pressed the wrong lever. But this would possibly mean that the rats would become neurotic rather than clairvoyant. Thus, as a further refinement, the experimenter might be allowed to vary the intensity of both negative and positive reinforcement. This would be permissible so long as the experimenter was ignorant of the order of the random series. But unless the experimenter decided in advance on a procedure for varying the intensity of reinforcement according to the animal's pattern of response, it would be possible to argue that the experimenter and not the rat was clairvoyant, or that both of them were. An additional refinement might be to use a p value greater than 1/2, one high enough not to encourage neurosis and low enough to ensure roughly equal response frequencies at the two levers.

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50 EEG and ESP

by I. J. GOOD

The following suggestion was made by A. J. Good in correspondence with the well-known psychic researcher, Whately Carington, in 1946. Carington expressed approval, but unfortunately died the next year.

A man is placed in a dark room, in which a light is flashed at random moments of time, the interval between flashes being randomly determined, such as by a radioactive source. The man's EEG (electroencephalogram) is recorded on one track of a magnetic tape, and the flashes of light on another. The tape is then automatically analysed statistically to see if the EEG shows any tendency to forecast the flashes of light. The experiment would have five advantages over card-guessing experiments:

(i) It would be easier to be sure that recording errors had been eliminated.
(ii) A precognitive interval might be revealed that would be too small to be noticed by means of ordinary card-guessing experiments.
(iii) The evidence might be acquired at a great rate, before the percipient became bored.
(iv) It would be possible to detect unconscious precognition by the percipient.
(v) The experiment strikes out in a new direction, whereas several card-guessing experiments have already been done. For all we know, the experiment might be success-

ful with a large proportion of percipients, even if they had not been specially selected, in which case the experiment would be easily repeatable. Ease of repeatability is an extremely important desideratum for all kinds of psychic experiment, and it has not yet been attained.

I discussed this idea in 1951 with Grey Walter and J. A. V. Bates, at meetings of the informal Ratio Club, and at that time they felt that their equipment was not adequate for the experiment, but they both tell me that the situation is now much improved.

This contribution is based on a letter in the *J. of Parapsychology*, volume 25, pages 57–58, 1961.

51 Self-training in Parapsychology

by R. H. THOULESS

It is desirable that we should learn how to specify as objectively as possible reliable conditions for high scoring. Various possibilities seem to be open: induction of favourable attitudes by repetition of words or word sequences, the use of methods of breath control (*pranayama*), sensory deprivation, the use of drugs. The last of these has been used without conspicuous success; the others remain to be tried. Any such training system would probably have to be prolonged and disagreeable. To begin with, therefore, it must be worked out on the experimenter himself.

52 Indirect Experiments in Psychical Research

by ALAN J. MAYNE

There is good reason to believe that psychic faculties operate at a subconscious level of the mind, if they operate at all, and that they are easily inhibited by unfavourable states of the conscious mind. One way of minimising the risk of such inhibition, during experiments in psychical research, is to carry out these experiments in such a way that the subjects participating in them *are not aware that their psychic faculties are being tested.* I shall now outline several proposals for such experiments, whose possibility seems only to have been hinted at[1,2] or mentioned briefly elsewhere[3,4,5]:

(*a*) Existing types of PK and ESP experiments in game-like situations could be taken one step further by asking people to play suitably-designed games, without telling them that they were at the same time taking part in a parapsychological experiment.[6] These games would offer scope for ESP and for PK. The outcomes of the trials would all be recorded, either by a score-keeper for the game or by hidden witnesses watching the game or, preferably if possible, by automatic recording instruments which belong to the usual machinery of the game. These outcomes would be analysed to see whether their scores depart significantly from the level to be expected in the absence of ESP and PK.

(*b*) Similarly, the experimental subject could be asked to undertake a contest with a machine. A prize would be awarded to him for good enough performance. Unknown to him, the behaviour of this machine would be partly determined by a randomiser. The experiment would aim to find out how far the randomiser is disturbed by PK.

(*c*) In ESP experiments, the percipients may subconsciously 'aim' to score at an average level which coincides with their conception of what the score should be in the absence of ESP.

In many situations, most people make noticeably or even seriously incorrect estimates of mean chance expectation. If such situations were used in ESP experiments, the percipients would be motivated to score *away from* the true 'chance level' and would thus be more likely to show ESP.[7]

(*d*) ESP tests could be incorporated secretly into ordinary psychological experiments. For example, the percipients could be asked to memorise lists of symbols. Unknown to them, a run of ESP targets would be chosen as follows: for each item in the list, the target would be chosen at random from those symbols which *differ* from the correct symbol.

(*e*) The effect of possible psychic influences on a wide variety of activities could be investigated by dividing the participants in an experiment into two groups. The first group would be assigned to the chosen activity. The second group, *whose existence would be unknown to the first group*, would be urged to encourage or to inhibit, by psychic means, the peformance of the first group. Control trials would also have to be made in which the second group does not operate.

(*f*) Related to the idea of indirect psychic experiments is the idea of investigating how far psychic faculties contribute to such activities as creative work and intuitive decision making, or to more 'ordinary' tasks such as intellectual work, searching for errors in calculations, and tracing faults in machines.[8] Relevant experiments could be devised.

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7. A similar suggestion was made by J. S. Hayes in a lecture to the British Society of Psychical Research about 1948. *G. Ed.*
8. Several people have suggested that machine faults might sometimes be due to PK. [Pauli used to claim that machines would not work when he was present and he described this as 'Pauli's exclusion principle'! *G. Ed.*]

53 A Suggestion for a New ESP Experiment

by MICHAEL S. SANDERS

Experiments* have shown that, after an image has been presented to a person subliminally, for example by exhibiting a picture, for so little time that it is not consciously perceived, it may influence the results of word-association tests given to that person shortly afterwards. In addition, material presented subliminally may influence the person's dreams on the following night.

It is possible that this process may depend to some extent on ESP. In order to test this hypothesis the experiments could be done with some of the images hidden instead of being just visible.

54 Can Wishing Affect Weather?

by WILLIAM E. COX

Can the combined wishes, however casual, of a population of people tend to produce fine weather? We can obtain evidence by

* FISHER, C.: *J. Amer. Psychoanal. Assoc.*: volume 4, 1956.

examining climatic records and seeing, for example, whether there is more sunshine or less rainfall on days when fine weather is especially desired, such as on Sundays, than on other days. It may be objected that Sundays might tend to have finer weather than week-days owing to lower industrial air pollution, but the meteorologists to whom I mentioned this possibility thought it unlikely. But even if this objection is valid, it would not apply to working days, where there are, nevertheless, special events like festivals or sports, when many people hope for fine weather.

According to newspaper records, there were 211 days when the sun did not appear by afternoon press-time at St Petersburg, Florida, from 1910 through 1957. Only eleven of these days were Sundays.

[In the absence of psi, there are odds of about 20,000 to 1 against, at most, eleven cloudy Sundays occurring on these 211 days, as a result of coincidence. I have unfortunately been unable to verify the raw data from the original source. *A.Ed.*]

[Could it be that afternoon press-time was on the average later on Sundays than on week-days? In any case the evidence for psi can, as it were, be no greater than the odds that the mentioned meteorologists were right. *G.Ed.*]

55 Can Thinking Make It So?

by GORDON PASK

Build a self-adapting system, preferably chemical,* think hard at it, and see what happens. Statistical methods of analysis should of course be used.†

* PASK, G.: in *Mechanisation of Thought Processes*: National Physical Laboratory Symposium, volume 11, pages 917–22, H.M.S.O., London, 1959.

† The Associate Editor believes that several people have independently thought of this idea, including himself! *G. Ed.*

56 The Generation of Psychic Phenomena by 'Intelligent Networks'

by MICHAEL WATSON

Recent work on 'artificial intelligence' has suggested that it may be possible to design suitable configurations of non-living matter which display many 'mind-like' qualities. Such configurations might include not only specially-designed machines but also various types of 'random' or 'partly random' network.[1]

It is interesting to speculate that similar 'non-biological intelligent networks' may sometimes arise spontaneously during the natural evolution of arrangements of physical matter and force-fields.[2] These configurations are probably so subtle that they would be very hard to detect with existing instruments; indeed, if they occur, they seem to operate largely by paraphysical means, as yet unknown to physicists. If such 'intelligent networks' are present, they would show 'purposes' of their own or have purposes impressed on them paraphysically by local 'psychic factors'; for example conscious or sub-conscious aspects of the minds of living organisms or of disembodied 'spirits'.

One aim of such a system might be to modify the behaviour of the natural phenomena occurring in its neighbourhood, in accordance with one of its characteristic patterns; thus new sets of phenomena would be generated with laws partly built to its specifications.

In extreme cases, the system would perform so well that witnesses of its manifestations would think that they were observing a new set of 'universal' parascientific phenomena, describable by objective laws: whereas, in fact, these phenomena would be *local* and very emphatically *not repeatable* all over the universe though perhaps *partly repeated* in a limited number of places. There is some reason to think that such phenomena have actually occurred, possibly in connexion with: the 'odic' or 'odylic' phenomena of Baron von Reichenbach (the discoverer of paraffin); the 'radionic' or 'psionic' phenomena;[3] Blondlot's 'N Rays'; Miller's repeated 'positive' results for the Michelson-Morley experiment; 'flying saucers'. Many strange events seem to be associated with such incidents[4] while they last, but they usually 'fizzle out' after a few years, before scientists are able to verify them!

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1. *See*, for example, GOOD, I. J.: 'Could a machine make probability judgements?': *Computers and Automation*, volume 8, pages 14–16, 24–6, 1959; 'Speculations on Perceptrons and other Automata': *IBM Res. Rep. RC* 115, page 19, June 1959.
2. If this happens, it is more likely to occur in very olden objects such as jewels. *See also* pbi No. 85 by Maude. *G. Ed.*
3. *See*, for example, MAYNE, A. J.: *Brit. Soc. Dowsers J.*, volume 15, pages 77–87, June 1959, for a general discussion of some of these phenomena and further references to their literature.
4. GARDNER, M.: *Fads and Fallacies in the Name of Science*, 2nd Edn., Dover Publications, 1957, mentions further possible instances, though he has a highly sceptical attitude to reports about them.

57 On the Threshold of a Transcendental Science

by A. K. TALBOT

After many years during which I have studied and observed many strange, apparently parascientific, phenomena, I have come to the shocking conclusion that the psychical researcher, *while carrying out his investigation, cannot afford the luxury of a neutral* *and unbiased attitude* if he wishes to advance beyond the vicious circle of endless repetition of half-satisfactory experiments which are the usual reward of the half-convinced experimenter. He should 'identify himself with' and 'become at one with' the psychical situation by adopting, *in the way of a working hypothesis as it were*, a wholehearted acceptance of the phenomena at their face value, regardless of how much this deliberate act of acceptance may outrage his intellectual convictions. My advice is this: 'Swallow them wholesale, all the claims made by spiritualists, occultists, and mystics, and set out to prove them, bearing in mind that the phenomena in question are elusive and have to be tricked into an admission of their reality by dint of much ingenuity. While thus *acting as a wholehearted believer, keep a corner of your mind alert, watchful, and unemotional, avoiding all partisanship.* A pretty little piece of mental acrobatics; if you don't feel up to it, seek the assistance of an analytical psychologist!'*

In order to make reasonable progress, the psychical researcher needs also a suitable philosophical and scientific basis for his hypotheses and theories about parascientific phenomena. We need a model of the universe into which all these strange happenings, which our remote ancestors took so much for granted, fit easily as natural manifestations of a reality beyond the reach of our ordinary senses.

The available knowledge about these phenomena is still much too sketchy to allow scientists as yet to go much beyond preliminary speculations about this model. A breakthrough in this vital research is possible only if we learn to think along radically new lines.

* This seems shockingly unscientific at first sight, but it fits in well with Rhea White's suggestion ('Depth Perspectives in Experimental Parapsychology': *Int. J. of Parapsychology*, volume 2, pages 5–21, 1960) that active co-operation between conscious and subconscious minds is a prerequisite for success in ESP experiments. *A. Ed.*

58 A Possible Application of Extrasensory Perception in the Determination of Crystal Structure

by S. C. WALLWORK

One of the best established techniques for the determination of crystal structures by X-ray diffraction is that of 'Fourier refinement'. For a crystal with a centre of symmetry this refinement process becomes one of obtaining a more and more correct set of (plus and minus) signs to apply to the observed 'structure factors'. At each stage, the signs and the structure factors are combined in a 'Fourier synthesis' which results in an electron density map indicating the positions of atoms in the structure. From these positions a new improved set of signs can be calculated, so once a sufficient number of the more important structure factors have been allotted correct signs, the Fourier map can be interpreted, and the structure usually refines automatically.

Several methods are available for obtaining a first set of signs, but this is one of the few experimental situations where ESP might be of practical value.* If the signs were guessed, and a weak ESP effect were operating, it is possible that a sufficient number of signs of the more important structure factors would be guessed correctly to allow an interpretable Fourier map to be obtained. The situation is ideal for the application of ESP since it is possible for only a small excess of correct guesses above the chance score to lead to a point from which refinement could proceed automatically. In contrast with experimental tests for ESP, any form of 'cheating' is admissible. That is to say, any

* Search problems occur more often in non-experimental fields, and in information retrieval, in which fields ESP may be a component of 'serendipity'. Even the above problem may be regarded as theoretical in that it is a question of finding the correct one of 2^n hypotheses for n signs to explain the result of an experiment already made. *G. Ed.*

information derivable by normal means which may help in the determination of signs could be taken into account.

This method has been partly tested by the author. The signs were guessed by a colleague at the beginning of the structure

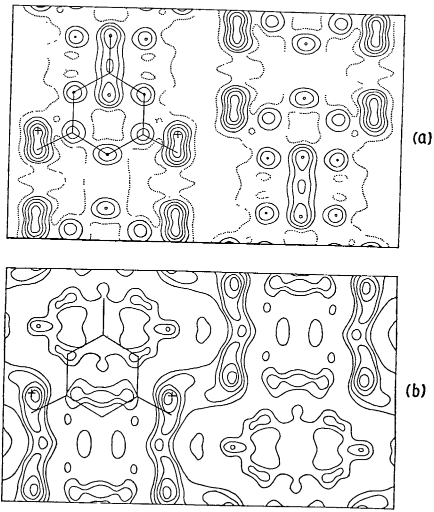

Fig. 10. Comparison between (*a*) the final electron density map of a projection of the crystal structure of phloroglucinol dihydrate, and (*b*) the corresponding map obtained by using guessed signs in the Fourier synthesis.

determination but were not used to calculate a Fourier electron density map until after the solution of the structure by the normal methods. Although only 31 signs had been guessed correctly out of 51, the Fourier map was recognisable in comparison with the map based on the correct signs (as shown in Fig. 10) and would probably have refined to the correct structure.*

If ESP is operating here, it must presumably be by precognitive perception of the signs finally obtained after refinement. Since it is usually difficult to distinguish experimentally between precognitive and non-precognitive ESP, the type of experiment suggested here may be of further interest as a method of studying precognitive ESP.

59 Precognition and Reversed Causality

by ALAN J. MAYNE

1. *Introduction. The Possible Causes of Precognition*

When it is not explainable in terms of normal causes alone, a 'precognitive event' can, in principle, result from at least one of the following phenomena:

(*a*) *Reversed causality*, where an 'effect' precedes a 'cause'.

(*b*) *Paranormal inference*, where a combination of ESP and normal inference is used to deduce future events.

(*c*) *Paranormal influence*, where a combination of PK and normal actions is used to influence future events.

(*d*) *'Field-prediction'*,[1] where 'normal' events are produced by a system of fields *A*. These fields are isomorphically copied on to a system of 'psi-fields' *B*, which can give an advance representation of *A*. Thus, the system of *B* fields acts as a predictor of the system of *A* fields,, because the transitions between different states of *B* are faster than the corresponding transitions between the states of *A*.

* It would be interesting to know whether this was typical for so small a deviation from 25·5. *G. Ed.*

(*e*) *Pattern effects*, where biological-psychological-psychic patterns influence both the state of mind of a person and the course of certain events, in a correlated way and such that the influence on the person precedes the occurrence of the events.

(*f*) *The actions of 'outside intelligences'*[2], who influence the mind of a person and/or a subsequent event, so as to set up a correspondence between them. This phenomenon can be considered as a special case of a pattern effect, but it is worth considering separately. 'Outside intelligences' could, for example, be groups of extremely advanced scientists living on other planets, collective human 'superminds', or god-like beings.

One of the chief problems of research into 'precognition' is to assess the various parts played by these different phenomena and to determine their relative importance in different types of 'precognitive' effect, if such effects occur.

2. *A Suggested Experimental Test for Reversed Causality*

Most of the types of ESP experiment which have been used to test 'precognition' are not strict enough to test for reversed causality, because they fail to exclude rigorously all the alternative causes of 'precognition'.

The essential requirements for a decisive test of reversed causality are that the processes for determining the target symbols should not be set into operation until after the guesses have been completed, that the choice of targets should not be deducible by 'normal' inference and that it should thus be partly random, and that the choice of targets should not be susceptible to influence by PK, 'field-prediction', pattern effects, or 'outside intelligences'.

The nearest approach to this ideal seems to be obtained by choosing targets as follows[3]. For each choice of target, calculate a symbol number n, by means of an explicit mathematical formula of the form $n = f(a_1, \ldots, a_r, b_1, \ldots, b_s)$, where a_1 to a_r are the outputs of r separate randomisers, at least some of which should be of a type believed to be hard to influence by PK[4], and b_1 to b_s are s results of a very intricate calculation, performed by an electronic computer for the first time; r and s are positive integers, and r should preferably be greater than 1. Corresponding to each symbol number, thus calculated, there is a unique *predetermined* choice of target symbol. Any effect of PK on the computer could effectively be ruled out by incorporating elaborate checks on the accuracy of its calculation, and by rejecting its output unless all these checks are satisfied. 'Normal' methods of inferring the results of this calculation could be excluded by making it so complicated as to be fantastically beyond the unaided capacity of even the best mathematicians, and sufficiently different from previous problems for its answers not to be deducible from theirs.

If such an ESP experiment yielded positive scores, with such high statistical significance that the possibility of their occurrence by 'coincidence' could be ignored, then only two conclusions could reasonably be drawn. Either some 'outside intelligence', with brilliant scientific ability and remarkable powers of control over terrestrial phenomena, would have to be operating, or genuine reversed causality would be occurring. The latter alternative seems to me to be much more likely.

3. *Is Reversed Causality a Plausible Concept?*

Reversed causality can be defined mathematically as a situation where the equations describing the situation under consideration at the moment of 'precognition' must contain explicit functional dependence on future states of that process.

Good[5,6] has pointed out that it is not tautological to say that a cause always precedes an effect, so that denial of this could have genuine scientific significance. He has also[6] suggested some possible ways in which reversed causality might operate.

It is often attacked because it is thought to lead to a paradoxical conflict with 'free will', but such a conflict rarely, if ever, seems to arise in actual instances of alleged precognition. For example, in the collection of typical cases of this sort discussed by Mrs Rhine[7], some of the incidents may not be due to true precognition, while the others probably involve reversed causality in combination with at least some of the following effects: part of the information in the 'precognition' is spurious but indistinguishable from the genuine information present; the information contained in the reversed causal influence from the predicted event is incomplete or does not all reach the conscious mind of the percipient; the reversed causal influence comes from an event preceding the predicted event but from which the predicted event could be inferred; the percipient does not have enough control over the situation to intervene effectively enough to avert the predicted event; the percipient decides not to intervene or is unable to do so. In the rare cases of apparently genuine precognition where none of the above explanations is possible and where there thus seems to be a conflict with 'free will', the intervention of 'outside intelligences' would seem to be the only possible explanation.

4. *Miscellaneous Speculations on Reversed Causality*

In the phenomena normally studied by scientists, it seems evident that whatever reversed causal influences are present would very quickly be absorbed, presumably far more easily and rapidly than the analogous forward causal influences, so that their effects would not stretch back for more than a very small fraction of a second; thus they would be noticeable only at a 'quantum' or 'sub-quantum' level. However, ESP phenomena may well act by extremely high resonance[1,8] of the mind of the percipient with some other mind or with some physical event, so that reversed causal influences of a type important in ESP might flow back for a considerable time before meeting a suitable 'resonator'. In extreme cases, this time might be several years, as is suggested by some of the actual cases claimed.

Contemporary physics postulates that all 'force-field' influences move with velocity c, the velocity of light in a vacuum. But perhaps a very small proportion of such influences move with different velocities, in some cases even in a reversed time direction. It is interesting to investigate whether such a phenomenon would give rise to small but still detectable deviations from the predicted orbits of atomic electrons, sputniks and planets. Reversed causality might also lead to small deviations from its predicted position of an electron beam in a cathode-ray tube, just before the field in one of its plates is switched off or allowed to decay with a certain time constant.[9]

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2. This suggestion is not original, but I have not been able to trace its origin, though F. W. H. Myers seems to have been one of its earliest proposers.
3. This procedure is a refined and more rigorous version of a method proposed and used by MANGAN, G. L.: 'Evidence of Displacement in a Precognition Test': *J. of Parapsychology*, volume 19, pages 35–44, 1955.
4. For example, such a randomiser could be based on accurate measurements of the passage of vehicles past a given point on a road, or, as J. B. Rhine and his colleagues have suggested, they could be based on accurate meteorological measurements.
5. GOOD, I. J., at the Oxford Philosophical Congress in 1954.
6. GOOD, I. J.: 'Speculations Concerning Precognition', 'Computers, Causality and the Direction of Time', 'Two-Way Determinism', pbis 45, 104 and 100.
7. RHINE, L. E.: 'Precognition and Intervention': *J. of Parapsychology*, volume 19, pages 1–34, 1955.
8. MAYNE, A. J.: 'Dowsing, Radiesthesia, and Modern Physics': *Brit. Soc. Dowsers J.*, volume 15, pages 77–87, 1959.
9. WALLWORK, S. C., in a personal communication to the author.

60 A Theory which is Impossible to Believe if True

by I. J. GOOD

Here is a theory about the purpose of our life on earth which I have been putting around verbally during the last thirteen or fourteen years, even though I don't believe it. In fact, if it is true, it is impossible to believe it.* The theory is that we are on earth as a punishment for crimes committed in heaven. We each have to run through a deterministic punishment, more or less severe, according to the magnitude of our heavenly offence. Even if we are happy here, it is a poor sort of happiness in comparison to what we shall have when we return. But part of the punishment is that we cannot believe the theory. For if we did, the punishment would not be effective.

* *See* 'The mind-body problem, or could an android feel pain?' in *Theories of the Mind* (Ed. J. M. Scher, Urbana), 1962.

5

Sociology, Economics, Operational Research and Games

61 A Hypothesis Concerning Road Accidents

by COLIN CHERRY

I have often heard it argued that if ten motorists a year could be hanged for dangerous driving, thousands of lives would be saved. But I do not believe it, rather holding an opposite view, that the motorist's behaviour would improve if fewer constraints were imposed upon him, not more; and that, of all the restricting conditions which bear upon him, one is intolerable, namely, the fact that he cannot speak to his fellows. This, I submit, accounts for his attitudes, for his morals (while driving) and for his conduct.

I am of the school which holds that the one basic faculty which distinguishes us from the animals is that of speech. More generally, the use of all forms of sign and symbol; but most of all, speech. All our other human expression rests upon this; all our conceptual framework and communicable thoughts; our civilised attitudes to one another. Without communication there can be no human society.

If you want to knock the humanity out of a man, hold him *incommunicado*. Shut him away, put him in prison, or on the barrack square, send him to Coventry. Cut off from his fellows, encased in a world of his own, he is an outcast and hate grows in him. With conversation, free and unrestricted between us, we are, to one another, human beings.

A motor-car draws up and stops, and the driver asks you the way somewhere. He seems to you a gentle, courteous fellow. But once departed, *and moving*, he is red in tooth and claw; not to those with whom he is chatting, inside his own cosy little world, but to all the others outside, similarly enclosed in their isolated cells. No word, no gesture of sympathy, nor understanding can pass between them. Each knows nothing of the others' emotions or attitudes to themselves; and what you do not know in such loneliness, you invent. And it is only too easy and natural, as a motorist, to attribute the worst attitudes and motives to other motorists, with whom no human intercourse is possible. God help me, I'm one myself.

It might be objected at this point that communication between motorists exists by virtue of the rules of the road – the road signs, the hand signals, traffic lights and flashing indicators and all the other devices and rules explained in the Highway Code. But this is not the case. We must sharply distinguish between two classes of communication which use 'language' and 'sign-systems'. By a 'sign-system' is meant, in Semiotics*, any system of *invented* signs and rules, which are laid down and centrally imposed. The rules are 'rules of obedience'. Into this class fall the signs and rules of road conduct – laid down in the Highway Code. Similarly, mathematics is not a language, as commonly stated, but a 'sign-system'. A mathematician cannot break the rules, if he is to remain a mathematician. There are endless other examples: all involve rigid, unchanging rules.

These systems are centrally imposed and inviolate and with them only a definite, fixed pattern of communication is possible. Into this class too we must put the signs used by the animals – their various reflexes, their cries, their postures and patches of colour; these are centrally 'imposed' by Nature and are fixed

* The Theory of Signs.

and automatic. On no account can they be called 'language'.

In contrast, human language is an open system, freely evolved by our mutual contacts and exchanges. There are no rules which cannot be broken. These rules are 'rules of conformity'. Anyone who departs far is soon brought back into the fold. There is continual variation and change and, with language, it is possible to give expression to almost any thought.

While driving you are held *incommunicado* by the very mechanics of the process. This constraint upon you is the most intolerable of all. The authorities and the public who profess a certain concern at the road-slaughter are for ever suggesting more constraints. It is not more that we are wanting but fewer.

As you drive along the road you see in your mirror another car closing up behind you, jockeying for position and settling down two feet behind your rear-lights. It takes every ounce of self-control to accept this situation, and to feel well-disposed, as you sense on your neck the hot breath of this monstrous hate-object. How this feeling might be dispelled by a few courteous words! Or when the situation is reversed and you find your path blocked by some slow-coach; the same sense of irritation and frustration which you feel rising within you and which you ache to suppress. If only you could explain to him! It is just the same sense of momentary anger as when you are cut off on the telephone – beaten, frustrated – a sense which disappears the moment conversation is restored.

In the same way, cut off in your motor-car the struggle within you is the struggle to communicate. As it is, to effect any change you can do only two things; you can honk at the other man or you can hit him. You can express no finer shades of emotion; no other modes are open to you. Let us admit it – cars are used as coshes and the roads are filled with hate.

I therefore suggest to the Minister of Transport, in all seriousness, that cars should be fitted with miniature radio transmitter-receivers, with a range of about one hundred yards; this, I am convinced, would not only reduce collisions between motorists, but would virtually eliminate them.

[In my opinion, the only way to obtain a really large reduction in road accidents is computer control of the movements of all motor vehicles on all roads except those with light traffic. The driver could then no longer be a menace, and would do little else besides telling the computer his desired route, then wait! *A. Ed.* Computer control of traffic has also been suggested by others, for example, J. D. Bernal. *See also Life in the Twenty-first Century*, page 165, Penguin, 1961. But Colin Cherry's suggestion could be taken up much sooner, if it is practicable. *G. Ed.*]

62 When I Hear the Word 'Gun' I Reach for My Culture

by I. J. GOOD

If the rulers of nations could be encouraged to form a friendly clique there would be some prospect that World Government would come about peacefully. Such a clique might be formed if all the rulers lived together semi-permanently in, say, Geneva, and if conditions were made pleasant enough for them there. It would be necessary to keep them happy by pandering to all their cultural, biological, and psychological needs, so that a permanent team of psychologists would need to be in attendance. A common language would be spoken, agreed by UNO, and its knowledge would be a necessary qualification for entering the clique.

In order that the delegates should maintain control in their own countries, it would be necessary for them to attend their national assemblies by proxy, with the aid of television. For example, the picture of the Prime Minister would appear on a large screen in the Houses of Parliament, and would be at least as imposing as he would be in person. When an Assembly became too noisy the picture would grow in size, and its voice would become louder. The degree of impressiveness of the ruler's image could be determined by means of an automatic control system, depending on the short-term average noise level in the Assembly.

The incentive to learn the international language would be considerable, and soon it would be taught in schools throughout the world.

63 Oceans for UNO

by ERIC A. WALKER

Let's give the oceans and the seas of the world to the United Nations.

One of the chief difficulties in forging the United Nations into a truly significant force for international law and order lies in the fact that it has primary legal jurisdiction over nothing. So far as I know, every inch of land in the world is owned by one or another government. And none of these governments has shown the slightest interest in turning over some of its sovereignty to the world government. In fact, it would now seem that governments own – or believe they own, which is perhaps the same thing – the air above their ground.

But no one claims to own the oceans. It shouldn't, therefore, be too difficult a matter for the various nations to agree to United Nations' jurisdiction for them. No single country would have to give up a thing. We already refer to 'international waters'. What agency has a better right to exercise control over these waters than the only existing international government?

I think we shouldn't underestimate the potential of such an action. International rules for the use of the oceans are greatly needed, for safety, if for nothing else. Control of the shipping lanes is not a small force in itself. Piracy of the Portuguese liner occurred on the high seas, giving no nation clear-cut jurisdiction in the matter. With United Nations' control, an embarrassing situation could have been a routine police matter.

In addition to commerce, however, the oceans are used by the navies of all nations. With jurisdiction, the United Nations could work towards a system in which naval movements would have to be filed – and perhaps eventually, approved – with an international body. And if, as most countries seem to believe, the skies above land belong to the country that owns the land, surely any international tribunal would rule that the skies above the international waters belong to the constituted international governing body. If this were established, the United Nations would not only have sovereignty over *something* but would also have a very important segment of the earth's surface.

Further, such control might easily give the United Nations an independent source of income. Fishing rights could be sold or leased – and the revenue could be used to outfit the fleet that would be needed to police the use of these leases. This would, or could, be a beginning of an international police force. Eventually, the United Nations could commission commercial ships that would sail under its flag. Even more important, the mineral content of the oceans now looks as though it will become a very important resource, perhaps especially in connexion with nuclear energy. If it does, and if these resources are owned by the United Nations, the world government has not only a source of income but also a practical measure of control over the abuses of the world's supply of such materials. Of course, some believe the oceans will, in the future, provide much of the world's food supply.

Control of the oceans would give the United Nations jurisdiction over some 70 per cent. of the surface of the earth. No one now claims ownership of this surface. Yet this surface can command control over important national actions. And most certainly these bodies of water contain valuable minerals. No one nation can hope to win control over them. Let's give them to the United Nations.

[While this book was in the press, a detailed suggestion along similar lines was submitted to the UN secretariat by Dr Eugene

Staley of the Stanford Research Institute, California. See *New Scientist*, volume 13, page 121, 1962, *G.Ed.*]

64 An hbi Submitted Humbly to the Scientific Authorities

by OLIVER G. SELFRIDGE

Over three quarters of our globe lies under the authority of no country. This is chiefly because no country considers itself in a position to exercise authority over it. I am talking, of course, of the bottom of the ocean.

It seems to me that an international group of scientists, for the good of the world, ought to lay claim to the bottom of the ocean, and they themselves should exercise authority over it; a benevolent despotism if you wish, but the awkward ethical considerations are much relieved by the very small population there. Instead of romantically seeking lost treasure, the easiest way to profit would be to charge the cable companies rent. It would probably not even be necessary to make specific the threat to disturb the cable by electronic interference or by more drastic means. It is true that if control were adequate, trespass by sinking ships ought to be penalised by, say, the worth of the ships and cargo. It is an interesting question whether it is not too ambitious to attempt to exercise authority even up to the atmosphere and charge ships rent, or perhaps merely charge submarines.

It is clearly feasible now to set up deep-sea stations of the kind needed to exercise such control. At some point it will occur to the national authorities. I merely suggest that we get there first; we might help our side by refusing to aid merely national attempts.

65 The Social Implications of Artificial Intelligence

by I. J. GOOD

When the first really flexible electronic computers were still under construction, Turing[1] wrote an article on whether a computer could 'think'. He was not concerned with metaphysical questions since he *defined* 'thinking', for the purposes of his paper, as the ability to answer questions in a manner resembling a human being, though on a teleprinter. His discussion disposed of many philosophical objections and was probably influential in provoking some of the programming effort on learning machines and problem-solving machines.[2] At the time, the majority of scientists, tending to rely on primitive scientific induction, 'biological induction' as it were, denied the feasibility of intelligent machinery, and a frequently heard cliché at symposia on computing machines was that 'a computer will do only what it is programmed to do, except when it goes wrong'. They used to go wrong very frequently at that time, but even when they did they did not do anything intelligent. The cliché served a purpose in explaining the nature of current computers to the uninitiated, but it has begun to wear thin now that computers have been programmed to play draughts (checkers) and to prove elementary mathematical theorems.[3] Some of the proofs have been better than those that the programmers themselves would have thought of. A good example is the proof that the base angles of an isosceles triangle, AOB, are equal; namely that the triangles AOB and BOA are congruent since $OA = OB$, $OB = OA$, and the angles AOB and BOA are equal!

It is true that the programs so far have not produced much really original 'thought', but the work is being greatly accelerated both by improvements in computers, and in progamming techniques, especially the latter. The elementary instructions in these programs are being built up into larger and more intuitively appealing units, and they enable the human to communicate with the machine with greater and greater flexibility. Programs can be quickly modified, in minutes rather than weeks, and consequently the work on artificial intelligence can be expected to expand exponentially during say the next eight years. The variety of applications will likewise increase rapidly and it is not easy to see where the saturation point will be.

A very important question when trying to judge where this point will be is whether a computer could be effectively programmed by means of ordinary typed language. This will, I think, become possible if and only if nearly perfect mechanical translation becomes possible, for I agree with Miss Masterman[4] that the latter will require a deep analysis of semantic problems. These problems involve the logic of analogy whereas existing programming languages for computers barely do so.[5]

It is possible that programming techniques alone will not be sufficient for this goal to be reached, and it may be necessary to build machines having something of a biological structure resembling that of the brain. That is, it may be necessary to incorporate random-looking, if not actually partly random, networks in the machine. Methods of reinforcement, already used in programs,[6] may be much more effective when applied to a functionally more natural structure containing a great deal of 'parallel working'.[7] The training of such a machine would resemble the training of a baby, the primary methods being *demonstration* together with positive and negative reinforcement, followed by *talking* together with reinforcement. A machine that combined these 'bionic' features with those of an ordinary computer might well be the solution.[8]

A baby is a very complicated 'device', a product of billions of years of evolution, but only a million of those years were spent in human form. Consequently our main problem is perhaps to program or build a machine with the latent intelligence of a small lizard, totally unable to play draughts. A small lizard is handicapped by having a small brain. If we could build a machine with the latent intelligence of a small lizard, then, at many times the expense, we could probably build one with that of a baby. With a further small percentage increase in cost we could reach the level of the baby Newton and better.[9] We could then educate it and teach it its own construction and ask it to design a far more economical and larger machine.

At this stage there would unquestionably be an explosive development in science,[10] and it would be possible to let the machines tackle all the most difficult problems of science. Many of the most pressing problems, such as those of medicine and of information retrieval, would make giant strides every month, and human scientists might have to take a back seat.

Such machines, properly 'motivated', could even make useful political and economic suggestions; and would *need* to do so in order to compensate for the problems created by their own existence. There would be problems of overpopulation, owing to the elimination of disease, and of unemployment, owing to the efficiency of low-grade robots that the main machines had designed. (These robots would take their orders from people, but their 'brains' would probably be in the main machines which would communicate with their 'bodies' continually.)

Provided that the suggestions made by the machines were sensible and provided that they were clothed in impressive status-acquiring boxes, we should at last have an effective *deus ex machina*.

It may be argued that machines cannot be properly motivated because they do not feel pleasure or pain. The answer is twofold: (i) homing missiles, for example, behave *as if* they were motivated; (ii) perhaps machines *could* feel pleasure and pain after all.[11]

It would be economical for the machines to be able to communicate with one another, so as to be able to act as a single machine. Here they would have an advantage over human committees who have to communicate via the slow medium of speech. We do not need to work out how the machines would communicate with one another, since the method would itself be suggested by them. Presumably the organisation would be hierarchical. If the communication were tight enough it would be a matter of definition whether we had one machine or several.

If a machine were selfish it might not wish to replace itself by designing a much better one. But we could overcome this difficulty by permitting the machine to improve itself, for example, by becoming larger.

In fact it is desirable that, sooner or later, there should be only one of these futuristic oracles, under the control of the United Nations. Otherwise there would be a danger that the machines would come into conflict.

If the first two machines are built in America and Russia they do not need to come into conflict. For if they meet at a summit conference, possibly via satellite communication, they will presumably decide to connect themselves together into a single machine. This is a possible route to world government. Oracles of the world unite!

For what it is worth, my guess of when all this will come to pass is 1978, and the cost $\$10^{8.7 \pm 1.0}$. It would be cheap at the price.

My reason for estimating so high a cost is that the human cerebral cortex has some 10,000,000,000 neurons, each of which has perhaps a hundred dendrites. But since the electronic components of the future will operate some million times as fast as a neuron, I may have overestimated the cost.[11]

I need hardly say that my expectations may be wrong, and that we may have to be satisfied with machines that are *merely fantastically useful* for information retrieval and for other activities that require only a modicum of intelligence combined with an immense store ('memory'). Such machines will almost certainly transform both scientific research and trading methods. They will also be able to translate languages and write music and poetry, and will do these things at great speed, but most of this work will have the appearance of being uninspired by gifted human standards. If there is such a ceiling to the intelligence of machines, then the main hope of an 'explosive' development in all sciences and arts will come from specially bred men and women. (The possibility of breeding men of greater genius than has ever been known before is especially well discussed by R. A. McConnell[13] in an article that should have been submitted to this volume.) But I regard it as 'odds on' that the 'ceiling' does not exist. It is possible that the ceiling for machines will exist until the artificially-bred human geniuses attack the problem!

[Some problems in science might still be so intractable that even a very high-quality artificial intelligence would take a very long time to make appreciable progress on them. This may well be true of the really hard problems faced by psychical research; in such problems, no decisive progress has yet been made, in spite of the efforts made by many of the best scientists for many decades. Furthermore, in some of these very difficult fields of investigation, human scientists might have an advantage over an artificial intelligence, by being able to obtain clues of the right sort from experiences that are not translatable into machine language. *A. Ed.*]

REFERENCES AND NOTES

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2. A suggestion that a problem-*putting* machine may be more economical has been made by Mieczyslaw Choynowski, 'Zalezenia cybernetyki a zagadnienia biologii ('Assumptions of cybernetics and problems of biology'), Państwowy Zaklad Wydawnictw Lekarskich, Warsaw, 1957.
3. For an excellent survey of such work *see* MINSKY, M. L.: 'Steps

towards artificial intelligence': *Proc. I.R.E.*, volume 49, pages 8–30, 1961.

4. Miss Margaret Masterman in a Symposium, 1961, published by the Aristotelian Society, 169–216.
5. 'Analogue' computers are stupidly named: they should be called 'continuous computers'.
6. *See*, for example, FRIEDBERG, R. M.: *IBM J. Res. Dev.*, volume 2, pages 2–13, 1958; volume 3, pages 282–7, 1959; SAMUEL, A. L.: *IBM J. Res. Dev.*, volume 3, pages 210–29, 1959; MICHIE, D.: in *Penguin Science Survey*, No. 2, 1961. GEORGE, F. H., has also done some unpublished work.
7. *See*, for example, Frank Rosenblatt's papers on the Perceptron, put out by the Cornell Aeronautical Laboratory, 1958–60; and my paper 'Speculations on Perceptrons and other Automata', *IBM Res. Rep.*, *RC* 115, June 1959.
8. 'Could a machine make probability judgments?': *Computers and Automation*, volume 8, pages 14–16 and 24–6, 1959. One of the points in this paper was that we should tend to say that a machine had made a judgement when it had made a decision whose logic was not fully understood by the designers and programmers. Such judgements are liable to occur, in suitable machines or programs, as a consequence of *reinforcement*. An important question is under what circumstances a machine should reinforce itself. One criterion, when it is trying to classify objects, is whether the objects put in each class are correlated with each other in properties *not already used* in the process. The process should then be reinforced, but also the new properties should be incorporated, at first with small weights. Other criteria occur in the 'theory of clumps'. *See* pbi No. 33. For a further note on reinforcement, *see Information Theory: Fourth London Symp.*, pages 248–9 (ed. by E. C. Cherry), London, 1961.
9. Perhaps a real or artificial brain containing more than a certain number of neurons would involve insuperable problems of organisation, and might be 'mentally unbalanced', just as business organisations and countries can be too large. This would explain why genius is allied to madness, and why people as a whole are not much more intelligent. But both these things are susceptible of more acceptable explanations. For example, geniuses and madmen are often maltreated in their youth. Also the human cerebral neuron may be as small as it physically can be without too much communicational 'noise', and the head may be about

as large as it can be without the human becoming top-heavy, and the human body may be about as tall as it can be without undue strain on the bones.

10. *J. Roy. Stat. Soc. A*, volume 114, page 107, 1951; and the IBM paper in Note 7 above. By 'explosive' I mean far faster than has ever occurred before in any science.
11. *See* 'The mind-body problem, or could an android feel pain?': in *Theories of the Mind* (ed. by J. M. Scher, Glencoe Free Press, Urbana) forthcoming. In this paper I overlooked Michael Scriven's, 'The compleat robot: a prolegomena to androidology', in *Dimensions of Mind* (ed. by S. Hook) 1960.
12. It has been emphasised by R. A. Fairthorne in a private communication that experiments on the stimulation of organisations, such as artificial neural nets, may be very misleading when the number of components is not large enough, and that there may be an analogy with the 'scale effect' in mechanical engineering. He refers to *Computer J.*, volume 1, 1958, No. 1, and to R. M. Fano, in *Proc. Int. Conf. on Sci. Information*, Washington, D.C., volume 2, page 1407, 1958.
13. McConnell, R. A.: 'The absolute weapon': *Amer. Inst. of Biol. Sc. Bull.*, volume 11, pages 14–16, 1961.

66 What to do about Automation

by OLIVER G. SELFRIDGE

One of the pressing problems of this century is Automation, and its social implications, especially unemployment. We suggest that being unemployed is a harder job than most and should be rewarded accordingly. If the pay were high enough, people would willingly give up half their pay in order to have a job.

67 A Problem for the Hedonist*

by I. J. GOOD

Suppose that it were discovered that a state of pleasure is always associated with a particular kind of space-time pattern of electromagnetic field, or other physical system, and that we were capable of producing such patterns in the laboratory.† Presumably this would be useful in the construction of androids,

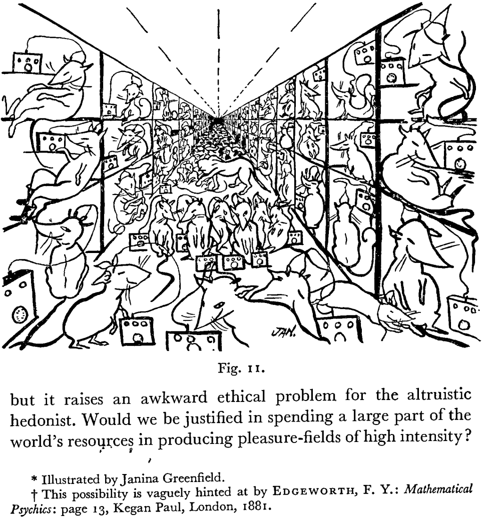

Fig. 11.

but it raises an awkward ethical problem for the altruistic hedonist. Would we be justified in spending a large part of the world's resources in producing pleasure-fields of high intensity?

* Illustrated by Janina Greenfield.

† This possibility is vaguely hinted at by EDGEWORTH, F. Y.: *Mathematical Psychics*: page 13, Kegan Paul, London, 1881.

The above notion of inanimate pleasure-fields may seem far-fetched to many readers, but a similar ethical problem can be framed in terms of the pleasure centres of the rat's brain, as discovered by Olds and Milner.* Should we breed billions of rats and supply them each with a pleasure-producing machine? (Fig. 11.) These machines could be automatically switched off at meal-times in order that the rats should not starve.

It could be argued that pleasure is not the same thing as happiness, but presumably the rats would be happy.

The absurdity of such a scheme, either for rats or for people, seems to constitute a common-sense refutation of altruistic hedonism. Perhaps the basic principle of social ethics should be that we should maximise the chance that the human race should be immortal?

68 Will the Discovery of a *d* Lead us to ethicophysics?

by STEFAN THEMERSON

Let us imagine a kind of Henri Poincaré Island in which a Tellurian's only food is a kind of Hieronymus Bosch Skunk. No other means of satisfying his hunger are given to him; if he does not want to die, he must hunt for a skunk, whose nature it is to surround himself, when attacked, with a scent which is not to the Tellurian's liking. The Tellurian's profound dislike of this smell is something he was born with, and he has to be really hungry to make himself overcome it.

This is an abominable situation. Abominable, but comparatively simple. There is no mystery in it, so far. The Tellurian feels

* *See*, for example, HEBB, D. O.: *A Textbook of Psychology*: page 143, 1958.

his hunger, and he feels the smell, there is a straight fight between the two feelings, both equally clear: hunger and smell.

Now, however, let us try to imagine that there exists a skunk which produces a new and peculiar kind of smell. A smell-less smell. It still evokes in the Tellurian the same pattern of behaviour as before, and yet he does not feel it in his nostrils. He does not feel it at all. And when he observes himself, as he sometimes does, and notices that he is behaving as if the skunk stank, and it doesn't, it does fill the Tellurian with Great Wonderment. And he invents some Tremendous Rigmarole to explain this state of affairs, because he cannot know that his body has been entered by a molecule of what was no longer a smell but was still keylike enough to open the door of that particular kind of behaviour.

At this stage of our deliberations, we were fortunate to open a copy of *Nature* (3 January 1959) – (whenever we realise that we are not sure any more whether black swans really exist, we lay aside our copy of *Mind* and open a copy of *Nature*) – at a page where the discovery of a completely new word was reported. It has always been our opinion that the discoveries of new words are the most important events in the history of scientific thought. The discovery of a new fact, or a new gadget, just adds new data to the equipment you are burdened with in your mortal life; it is when you hear of the discovery of a new word that you feel the history of mankind making a step forward. The word we have in mind this time was discovered jointly, on 12 November 1958, by Mr P. Karlson of the Max-Planck-Institute for Biochemistry, Munich, and Mr M. Lüscher of the Zoological Institute, University of Bern, and it is spelt: *pheromones*. Their report runs as follows:

> During the past few decades, investigations have been made into various active substances which, though they resemble hormones in some respects, cannot be included among them. For example, the sexual attractants of butterflies are, like

hormones, produced and secreted by special glands; minute amounts cause a specific reaction in the receptor organ (the antenna of the male), which eventually leads to a state of copulative readiness. Unlike hormones, however, the substance is not secreted into the blood but outside the body, it does not serve humoral correlation within the organism but communication between individuals. . . . We propose (therefore), the designation 'pheromone' for this group of active substances. The name is derived from the Greek *pherein*, to transfer; *hormon*, to excite. Pheromones are defined as substances which are secreted to the outside by an individual and received by a second individual of the same species, in which they release a specific reaction, for example, a definite behaviour or a developmental process. . . .

For the purpose of our hypothesis, let us assume the existence of pheromone *d* and of hormone *n*.

Pheromone *d* is produced and expelled into the surroundings by an animal whenever he finds himself in distress, fear, or in *articulo mortis*. Assimilated by an animal belonging to the same or a not very far removed species, it evokes in him a pattern of behaviour ($d =$ 'dislike') which may be described as 'moving away from the source of stimulation'.

Hormone *n* is produced by an animal when he is in need, e.g. of food, and it is injected into his own bloodstream, thus evoking in him a pattern of behaviour ($n =$ 'necessities', 'needs') which may be described as, e.g. 'Killing for food'.

There is a constant keeping of balance between hormones *n* and pheromones *d*, and it assures the survival of the species. If there had ever appeared a species of carnivorous creatures completely lacking in hormone *n*, it must have starved itself to death in the first generation. If there had ever appeared a species completely lacking in pheromone *d*, it must have become extinct, as its members, not restrained by the action of *d*, would have eaten the offshoots of their own family before these had time to mature to give rise to a new generation.

And thus, the tragic *factor T**, or the conflict that results from Needs + Dislike of what is needed, when it is vitally necessary to do what one vitally dislikes, *can* be reduced to the interaction that takes place between pheromone *d* and hormone *n*.

Now, here is a hypothetical case-history:

1. There is a prey in front of a Tellurian. The prey is frightened. But the Tellurian is not hungry. $d > n$. The Tellurian does not kill.

2. After a time the situation changes: the Tellurian becomes hungry. But the prey is gone. $n > 0$; $d = 0$. The Tellurian asks himself: Why did he not kill when the prey was there, conveniently near? The question puzzles him. If the prey were a *stinking* skunk, the Tellurian would have remembered its odour, and the fact that the skunk stank would serve him for an explanation. But it was *not* a skunk. There was no sensation of smell there at all. It was a pheromone. Strong enough to release a specific reaction, but too weak to be detected by nose.† And thus, all that our Tellurian's Memory can tell him is that there was something, not seen, not heard, not smellable, which (mysteriously?!) stopped him from killing the prey. An *X*!

3. So now he, the Tellurian, again lies in wait for the prey to come. He is hungry. And the prey comes. $n > d$. He kills. And yet, even if he is a tiger, he still makes noises and faces, and displays other signs indicating that it isn't such a simple business, that he has to overcome some resistance when in the act of killing. In this case it is a physiological resistance: his hormone *n* has to overcome the pheromone *d*.

4. However, he has now killed and eaten. He is hungry no

* THEMERSON, S.: 'factor T': Gaberbocchus, London, 1956.

† A similar problem is actually being studied in connexion with our sensitivity to temperature changes. Prof. G. M. Wyburn says: 'There is evidence that the central threshold for cold "sensation" is higher than the actual threshold value required to produce an increased discharge of the cold thermo-receptors; which implies that there is an input for our thermal "automation" and control mechanism below the level of consciousness.' (*New Scientist*, 9 Feb. 1961.)

more. Yet he remembers the time when he was hungry. And if the next opportunity to kill now arises, he finds himself under a number of stresses. Physiologically (or, let us say something like: on the sensori-motor cortex level), he is not hungry now, $n < d$, and he should refrain from killing. Psychologically, however (or, let us say, on the frontal cortex level), the *M*emory of the time when he suffered hunger may be strong, so strong, $Mn > d$, that it may overcome *d*, and he may find himself in the process of killing and storing food for the future.

5. Thus we have two manners of killing and two manners of not killing. The unconditioned and the conditioned manner of killing (based on *n* and *Mn*); and the unconditioned and the conditioned manner of not-killing (based on *d* and *Md*).

6. If our Tellurian happens to be what zoologists like to call a 'thinking animal', he has the power to observe his own behaviour and he tries to explain it. He understands the Necessity of Killing. Both on the physiological (sensori-motor? unconditioned?) and on the psychological (frontal cortex? conditioned?) levels. He is also capable of inventing some explanation of *not*-killing on the psychological level of brain behaviour. But *not*-killing on the physiological level defeats his comprehension. Why shouldn't he! What is it that makes him not kill?! He doesn't know anything about the existence of that smell-less smell, pheromone *d*, and he says: there is an *X*! And he says: when we need food, *X* does not forbid us to kill, but when we are not hungry, *X* does. And he asks: Who is *X*? And he goes back to his point of departure and answers: *X* is one who forbids us this and that in these and those circumstances, but commands us to do that and this in those and these circumstances; and thus he, the Tellurian, examining the same facts which a Martian would perhaps find describable in physical terms, arrives at a series of explanations and rules which become the basis and the beginning of ethical and religious orders.

Now, perhaps it does make not much difference whether it is a *d* or an *X*, whether it is a pheromone or a god. Both can be deduced. The question is: Can one of them be discovered? The discovery of an *X* will lead us to ethico-theology. Will the discovery of a *d* lead us to ethico-physics?

Let us assume:

(1) that ethical events are something it is legitimate to talk about,*)

(2) that the notions (and terms) now available for thinking and talking about them are obsolete,**)

(3) that no theory based on these notions can be adequate (can avoid begging questions,***)

(4) that it is not excluded that some new and more adequate notions can emerge while some basic research is being done in the field in which ethical events take place,****)

(5) that by 'basic research' (4) is meant research which begins with the notions already established in, or significantly connected with, the whole body of physics,*****)

(6) that the employment of these notions (5) does not exclude the possibility of finding some information concerning ethical events.******)

Now, whether *d* (or *n*, or *factor T* itself) can actually be discovered or not, the very possibility of imagining it in such a context as described above shows that (6) may be true (that there is no logical obstacle, no 'chasm', etc. against its being true). And if (6) is true (4) is possible. And if (4) happens, some genuine science of Ethico-physics, or Moralogy, or Anatomy of Morals, dovetailed with other sciences, may finally emerge.

Here is a letter written by a Martian boy scout to his mother in Mars. Some of its passages seem to illustrate rather nicely certain points of our paper. Asterisks (*, **, &c.) *mark the corresponding places.*

Dear Mama,

... you see, dear Mama, a Tellurian thinks that when he
*) thinks of ethical events in physical terms, he doesn't produce any value judgements. Putting it differently: he thinks that

making value judgements in physical terms is not ethics. On the other hand, he thinks that thinking of physical events in ethical terms is not science. Please, dear Mama, don't shrug your beautiful shoulders at all this terminological messiness. Some time ago, you know, they thought that *all* events were ethical events: an electric discharge was the mood of a god, and so on. Later they discovered that the speed of an arrow was the same whether its destination was a good Tellurian or a bad Tellurian, so they had to accept the notion of at least some events being non-ethical, and they called them 'physical events'. A few even thought that all events are physical events. Well, please don't laugh at me, Mama. I know that I am being terribly chaotic, but, after all, I am writing this to you and not to Teacher, and besides, these primitive phenomena are awfully difficult to describe to a mature mind such as yours. Well, the fact is **)that at the present moment of their history all these terms are awfully mixed up together, the set of their mental tools is most clumsily heterogeneous, what you may truly call a Great Dichotomous Mess, and, what's more, aggravated by the fact that whereas their physical terms have kept developing and have gained considerable precision, their ethical terms are rusted, haven't changed, and are still as primitive as they were when the first Martian visited the planet two thousand years ago. Well, perhaps you had ***)better not quote this last sentence to Teacher. Anyway, let me go back to my Tellurian. As I said, he finds that he cannot define his ethical values in physical terms, and thinks that they are, therefore, absolute. Nor can he define what he calls physical entities in ethical terms, and he states, therefore, that there is an impassable chasm between ethics and physics. All right, dear Mama, I know perfectly well what you are going to say. But it is not exactly as you think. When we, on Mars, face difficulties of the sort, we are wise to try to find out how our problem looks from the independent, outside point of view of Jupiter. And so I see that what you are going to ask is: Why, in their dilemma, do the Tellurians not try to find out how their problem may look from Mars? Well, dear Mama, as a matter of fact they do. In a way. The trouble, however, is that *the* Martian they still, though only occasionally, consult (the one I **)mentioned above), somehow failed to tell them the simple fact that, whether it is physical or ethical events that are taking place, the electrical patterns decipherable in our brains correspond equally to both, and therefore, if one language is sufficient to describe the intricacies of both patterns, one language must also be sufficient to describe both what is ethical and what is physical, and there can't possibly be room for any unbridgeable chasm between them. Well, yes, dear Mama, I know that I needn't explain these things to you. But, you see, the fact that you do not, and that I do not, see the unbridgeable chasm between the ethical and the physical, does not make the Tellurians think that the chasm is an invention of their peculiar way of thinking. Try to remember, dear Mama, that you and I have one sort of nervous system (and therefore one sort of world around us), and they have another nervous system (and therefore another sort of world around them), and so please stop thinking of a Tellurian as if he were a sort of ***)Martian, and try to realise that his nervous system refuses ** to admit (1) that Not-to-investigate ethical events in the terms in which one investigates physical events, because some logical philosophers have proved that there is an impassable chasm between an 'ought' and 'is', is like not-to-build motor-cars because Zeno of Elea has proved that Achilles cannot catch the tortoise;

(2) that to say that ethical values are absolute (and therefore 'physically' untouchable'), because some ethical terms cannot be defined, is like saying that the movement of a car is absolute, because I am not interested in the system of

co-ordinates provided by the map of England;

***) (3) that there is no more of absolute meaning in 'goodness' than there is in 'motion'. That the illusion of its absoluteness is the result of the fact that the word 'good' is a maimed word. That the full-bodied word is 'good-for'. And that it is a pity that when God created various things at the beginning and saw that they were 'good', he did not make the meaning of the word more clear by saying for whom they were good.

69 Bloggins on Professional Biases

ANON

The virtues of the professions are widely known, and there is no advantage in listing them, but the characteristic biases of each, and the ways in which their tasks structure their experiences and condition their opinions, are also important. The topic has not been discussed in print since the acute observations of Sir Henry Taylor, and Archbishop Whately in the last century – perhaps for the reasons which prompted my friend Bloggins to leave the notes, from which the following have been selected, unpublished in his lifetime.

Professions, he wrote, always complicate things, and some he considered even conspiracies against the public. They all thought that their own profession had some special share of common sense, and that life should be interpreted in terms of their own experience, and that they had special qualification for the general, directing, and policy making positions. In detail, he noted:

1. *Administrative Civil Servants:* mandarins, or scholar-bureaucrats with pride of caste, and recruited by public examination. They genuinely know best, except when they are wrong. A pro-

fession that ignores Simon's law that short-term considerations crowd out long-term, and one which believes that successful thinking about the future can be a by-product of every-day work.

2. *Lawyers.* He quoted with approval Sir Henry Taylor:

> It is a truth, though it may seem at first sight like a paradox, that in the affairs of life the reason may pervert the judgement. The straightforward views of things may be lost by considering them too closely and too curiously. When a naturally acute faculty of reasoning has had that high cultivation which the study and practice of the law affords, the wisdom of political, as well as of common life, will be to know how to lay it aside and, on proper occasions, to arrive at conclusions by a grasp; substituting for a chain of arguments that almost unconscious process by which persons of strong natural understanding get right upon questions of common life, however in the art of reasoning unexercised.
>
> The fault of a law-bred mind lies commonly in seeing too much of a question, not seeing its parts in their due proportions, and not knowing how much of material to throw overboard in order to bring a subject within the compass of human judgement. In large matters largely entertained, the symmetry and perspective in which they should be presented to the judgement requires that some considerations should be as if unseen by reason of their smallness, and that some distant bearings should dwindle into nothing. A lawyer will frequently be found busy in much pinching of a case and no embracing of it – in routing and tearing up the soil to get at a grain of the subject; in short, he will often aim at a degree of completeness and exactness which is excellent in itself, but altogether disproportionate to the dimensions of political affairs, or at least to those of certain classes of them.

3. *The pure or mathematical scientist* solves his problem by leaving out the difficulties, always taking the path of least logical re-

sistance. He may be defined as a man who believes that truth consists in the coherence of ideas, who is impatient with the mere fact, and who regards the untidiness of life as a nuisance rather than the main problem. Normally he possesses little sense of history.

4. *The applied or realist scientist.* He defined him as any man who accepts, in his own field, the importance of the three fundamental problems enunciated by Griffiths for engineering:

(i) Dirt, or heterogeneity, or gross errors, or the possibility of a pathology.
(ii) Noise, or random variation, and the limited accuracy of numbers.
(iii) Leaks, or the absence of conservation principles, other than merely conventional ones.

He is a man who respects fact before intellectual brilliance, and believes that truth consists in correspondence with the facts. Occasionally he is noted for dullness, and a lack of interest in speculative thinking.

5. *Operational Research.* The characteristics of this profession, he wrote, are the differences between its literature, and its practice, and the strength of the faith of some publicists. It possesses idols and prophets, and strong expansionist tendencies, linked to an attempt to patent the intellectual virtues. Its practitioners have been described as physicists now re-discovering elementary economics. Current tendencies include a demand for salesmanship and efficient rhetoric from its neophytes.

6. *The Artist.* His bias is the tidiness imposed in the name of artistic unity: as selective as the pure scientist but more dangerous, he succeeds in furnishing the imagination of a majority of men. Only by accident, and not by any discipline of his methods, is he correct. As they are normally devoid of a sense of responsibility, one may understand why Plato wished to exclude many from a rational state.

7. *Critics.* Artistic, literary, and architectural. An occupation distinguished by fluency, and a penchant for fashion, drawing little distinction between rhetoric and analysis. In their defence he noted that in this field analysis leads rapidly to an infinite regress, taking us fast further into mystery, instead of only slowly as in science.

8. *Journalism.* A journalist's problem is to get a story. The truth, as such, is not news.

9. *Historians,* he noted, possess a faith in documentary evidence that can only be astonishing to anyone who has written what he knows may become documentary evidence for a future historian, and who remembers the difficulties he had in determining the facts, and the difference between his writings, and what would have been his verbal comments upon them.

10. *Social Anthropologists* he commended for their clear-sighted recognition that what people said was usually more important than what the constraint of a profession and the conventions of publication caused them to write. But the self-denying ordinance that confined anthropologists to spoken evidence he found astonishing, and one of the clearest pieces of evidence of the artificial nature of the boundaries between disciplines. It was remarkable, he said, that it was thrown up by the very men who should have been free from such restrictions, and should have allowed themselves the pleasures of eclecticism. Like sociologists, anthropologists do not recognise facts which have not been written down, preferably in ritual language, by those of their own totem.

11. *Medical Psychologists,* he noted, tend to have dealings with abnormal rather than normal minds. It would be surprising if this did not influence their judgements.

12. *Accountants.* Men who recognise only one type and measure of value, and are unconcerned at lumping together many variables into one figure. Of importance as among the first practitioners in arithmetic to enter practical life.

13. *Planners.* Every generation of economic planners recognises

the intellectual inadequacy of the tools of its predecessors. The better the planner, the more sceptical he is about his own.

14. *Pragmatists.* Men who are unconcerned at using *post hoc propter hoc* arguments – a characteristically English quality.

15. *Police.* Bloggins wrote only this one sentence which I cannot interpret, and wonder what may lie behind it: 'A policeman is never off duty.'

70 Bloggins's Working Rules

ANON

1. Murphy's edict – if something can go wrong it will.
2. If a problem has less than three variables it is not a problem. If it has more than eight, you cannot solve it.
3. Parkinson's Laws state:
   (i) Work expands to fill the time available for its completion, especially when it is interesting.
   (ii) A man starts to lose his grip five years before retirement age, whatever this may be.
4. Hartree's Law states that whatever the state of a project, the time a project-leader will estimate for completion is constant. A task always takes twice as long as one might reasonably expect.
5. All reports require three drafts.
6. The 20:80 rules: 20 per cent. of the people drink 80 per cent. of the beer. It is prudent to assume the same concentration of effort elsewhere, and Holt's Rule to forecast time series states:
   New forecast = 0·2 (Last result) + 0·8 (Last forecast).
7. When there are unknown scale factors, assume a 0·70 power law.
8. Numbers in real life usually have a 25 per cent. coefficient of variation and rarely less than 10 per cent. Data usually have at least 1 per cent. of gross errors. This applies to people too.

9. The best experts resist innovation, for they wish to remain experts, and they are right only three-quarters of the time.
10. The variance of cumulative chance events is practically infinite.
11. Edie's Limit. Pooled Services may be more effective in theory, but they are soon degraded by difficulties of switching and scanning. Edie found 4–6 channels the most efficient group size for toll booths. The same number must often apply elsewhere.
12. Anyone more than two years younger than oneself is inexperienced. Anyone more than five years older is past his best.
13. Any useful classification has 3–6 sub-categories, but a thirty-fold division provides a fine monument to hard work.
14. Really top brass takes one year to make up its mind in matters in which you are interested.
15. Do not ask questions on which people have no real opinions, or which they will not answer truthfully. Socratic dialogue is more potent than any arithmetic.
16. There are never less than three conflicting criteria of merit. At best, operational research is nearly right.
17. Life is:
    (i) Discrete.
    (ii) Non-linear.
    (iii) Non-zero sum.
    (iv) Non-commutative, and positively irreversible.
    (v) Multiplicative rather than additive; the log normal distribution is more normal than the normal.
18. One nearly always has prior knowledge, but optimisation? It is a delusion. Probabilities are always conditional – very conditional.
19. There are no decision rules to choose decision rules.
20. The only practical problem is what to do next.
21. The art of being correct lies in making the weakest possible statements.

71 Strategy for Wholesalers

by A. R. VAN DER BURG

An important class of problems in economics, not yet studied in detail, concerns the best pricing strategies to be adopted jointly by producers and retailers of 'perishable' goods. This sort of problem arises because the optimal amount of goods for the retailer to buy is usually less than the optimal amount of goods for the producer to make, for a given probability distribution of demand. Owing to this conflict, the situation is a sort of two-person game.

A typical example occurs when the producer is a newspaper company and the retailer is a chain of newsagents. For simplicity, suppose that the retail price of a newspaper is fixed. Then the newspaper company assigns values to the cost of production per day, the wholesale price per copy, and the price paid for the unsold copies bought back from the retailer. It sometimes pays the company to offer to buy back copies not sold by the retailer, because this increases the number of copies that the retailer should buy in order to maximise his expected profit. As the number of copies sold by the company rises, its production costs per copy may be reduced enough to offset the expense of buying back the unsold copies.

Other examples arise in the sale of fashion goods and perishable foods, both of which are unsaleable to the public after a certain date.

Further research on problems of this sort would be in the interests of the consumer as well as of the producer and retailer, and may reveal the suitability of new trade practices.

72 Privately Disposable Taxes, an Incentive for Enterprise

by ALAN J. MAYNE

The resources of a nation may be distributed more democratically by devoting a fairly large proportion of the existing proceeds from income tax, death duties and certain other taxes to projects of importance to the community but largely chosen by the individual taxpayers. In order to increase incentives, the proportion of tax that is 'privately disposable' in this way should rise with the level of income.

The projects qualifying for support from these funds would include:

1. Charities, hospitals, educational institutions, and research projects, approved by the Government.
2. Business enterprises, especially new technological developments currently considered by the Government to be exceptionally important.
3. Special foundations, approved by the Government, which would distribute their funds to projects in categories 1 and 2.

Citizens of outstanding ability would be given a wide discretion in their choice of projects in category 1; for example, a brilliant scientist could use his privately disposable tax to support research projects of his own choice, including those founded by himself. By its choice of the parts of the economy that belong to category 2, the Government would have at its disposal a new instrument of stabilisation, to keep the national economy in control, while individual taxpayers would also influence the trends of the economy through these projects. The special foundations would cater for those people who indicate only the broad classes of project that they wish to support but who prefer to leave the details of the choices to others better qualified than themselves.

73 Greater Efficiency in the Western World

by WASSILY LEONTIEF

I

Four centuries ago the Spanish empire superseded the Dutch republics as the decisive power in the West; two hundred years later Britain wrested the world leadership from Spain and – on the continent – from France, only to pass it some forty years ago to the United States which accepted the new role more as a burden than a prize.

In every instance – except possibly one – the new country, when taking over the dominant position, had a larger population than the country that had to give it up. Such approximate estimates of national income as we possess indicate that in the critical periods of transition, as a rule, the *per capita* real incomes of both countries were rather close. However, as often as not, the standard of living in the ascending nation was still the lower of the two.

Countries with large populations but low income and those with high *per capita* income but a relatively small population can play, on the stage of international power struggle, only secondary and tertiary roles.

These observations about the past can help us to arrive at a realistic appraisal of the future.

II

The *per capita* income of a nation depends in part on the natural resources it possesses or to which it has access and in part on the effectiveness, i.e. the productivity of its economic system. As time goes on, the importance of natural resources gradually diminishes, while the technical-economic efficiency plays a steadily increasing role.

The economic system of a modern nation is like a vast machine:

the individual plants and factories are its wheels and gears, whole industries are its main component parts, while the private or public organisation and initiative provide both the driving power and the over-all co-ordination and control. The performance of a complex machine depends both on the sound construction of its working parts and on the general design. The wheels and gears of the Eastern economic machine are, with a few notable exceptions, inferior to the corresponding components used in ours. The motive drive and the co-ordinating devices used by the Russians seem to be so much more effective than those built into our system that in the post-war economic race the East has been gaining rapidly on the West.

Notable advances have marked in recent years the development of techniques of management itself. Countries willing, and able, to put them into practice, not only on the lowest, but also on the intermediate and the highest levels of economic organisation, will gain more from the application of these new techniques than the countries in which their use will have to be limited to the plant and single-enterprise level.

The underdeveloped countries are aware of all this and they draw the obvious conclusions.

III

The nature of advanced modern technology is such that it cannot be exploited economically without effective, sometimes ruthless, overall co-ordination. Such co-ordination must affect not only all operations within a single plant, but also the relationship between plants within an industry and of all industries within a national economy as a whole. Supra-national co-operation and co-ordination becomes more and more necessary for efficient functioning of the individual national economies as well.

If what I say above is true, scientific research and technical invention alone, however spectacular the results may be, will not solve our problems. To translate technological advances into economic gains, the West would have to revamp its ineffective economic organisation from the ground up and let us not mistake superficial re-decoration, in which we have been engaged up to now, for basic reconstruction.

[It is interesting to note that, after this article was submitted, the British Government avowed its intention to increase national planning, and there is also a more widespread recognition of the need for international planning. 'Planning' is no longer a rude word, and it is to be hoped that it will displace the word 'programming' in the expressions 'mathematical programming' and 'linear programming'. *G.Ed.*]

74 Science-art Unity: an Idea for a Competition

ANON

For each of the subjects: Law, Music, Painting, Linguistics, Philosophy, Mathematics, Physics, Chemistry and Biology, list the names of four men who were respectively law-makers, codifiers, executives and law-breakers.

75 Robotic Croquet

by A. S. C. ROSS

On the Croquet-lawn there stand six hoops and one peg. There are two players, each with two balls. The balls are struck with mallets. The players take *turns* and, in general, a player may choose which of his balls to play with. The object of the game is to go through the sequence of hoops (twice) in a fixed order and hit the peg. A player wins if his two balls hit the peg before the two balls of the other player do this. The rules of Croquet are complicated;* the essentials are the following. Except at the extreme beginning of the game,† the nature of the *turn* is epitomised in the slogan 'Roquet, Croquet, one free shot'. When a player strikes his ball so that it hits another ball, he is said to have *roqueted* the latter. He then picks up his ball, places it in contact with the roqueted ball in any position he chooses and strikes his own ball; this is called *taking croquet.* The player may then strike his own ball once more. In a turn, a player may roquet and croquet each of the other three balls once only. If the ball with which the player is playing goes through a hoop of the sequence, he has a fresh turn with this ball. A rule governs the replacement of balls that go off the lawn and, in the croquet-shot, if either ball goes off, the free shot is forfeited.

Problem. A directs a robot called Able, *B* one called Baker. Both Able and Baker are perfect Croquet executants. *A* wins the toss and can thus decide either (1) which pair of balls he will play with or (2) whether or not to play first. If *A* chooses (1), *B* can choose (2), and vice versa. How should *A* choose and how should he direct Able so as to win?‡

It should be noted that Able and Baker, like lesser players, may well make some use of *wiring*, that is, essentially, arranging two balls so that one cannot hit the other because part of a hoop or the peg intervenes.

Robotic Croquet can, of course, be perfectly simulated by means of a board and four counters of different colours.

* *See* the *Laws of Association Croquet . . . published by the Croquet Association,* 1961; or *Enc. Brit.* article on Croquet.

† The beginning is: (*a*) Starter – plays whichever of his balls he likes; (*b*) Second player – similarly; (*c*) Starter – must play his other ball; (*d*) Second player – similarly. (The normal rules as to roquet-and-croquet and going through hoops obtain when applicable.)

‡ The interest of this question is that it is a non-trivial example of a problem in continuous games: the corresponding problem does not arise for, say, football or cricket, unless one makes assumptions beyond the rules of these games. *G. Ed.*

6

Biology

76 Steak from Sawdust

by MARCUS BISHOP

'. . . and when the meal was completed with dessert of baked locusts and honey à la John the Baptist, the opinion was unanimous that that distinguished prophet no longer deserved our sympathy, and that he had not fared badly on his diet in the wilderness.'

(C. V. RILEY, 1877, cited by Bodenheimer, 1951.)

War-time sausages were, of course, notorious for their lack of meat. So bad, indeed, did they become that disgusted consumers were sometimes moved to assert that sawdust from the butcher's shop had become a major item of their composition. This unsubstantiated and presumably erroneous idea first suggested to me how marvellous it would be if all the odd scraps of wood littering the earth could be converted into protein to feed the world's hungry and expanding populations. Wood remains an almost entirely unexploited source of food for animals simply because most are unable to digest it. Pre-eminent among those that do are certain of the termites. These insects, as many know to their cost, are great devourers of wood, and thrive exceedingly well on it. My first idea for converting the world's unwanted wood into protein was to set up vast termite farms. Termites are readily eaten by many native peoples and there are numerous testimonies to their food value and to the excellence of their flavour. So far as I know, however, the possibilities of termite husbandry have not yet received serious consideration.

From time to time I return to my dream of gastronomic adventures among the termites, but, like other advocates of entomophagy, I have long since faced the reality that most people would prefer beef to termites. Clearly a more sophisticated approach to the problem is required. Striving for this I recalled that termites can live on wood only by exploiting a protozoon, called *Trichonympha*, which inhabits the termite gut. It is the *Trichonympha* and not the termite that possesses the necessary enzymes for breaking down wood into nutritive substances. Part of this digested material is then utilised by the insect host: without *Trichonympha* the termite starves. This brings us to an unsuspected similarity between termites and the herbivorous mammals that provide us with meat, for they too rely upon micro-organisms, protozoa and bacteria, to do most of their digestion for them. This similarity suggests a possible solution to our problem, for if *Trichonympha* or some similar wood-eating protozoon could be induced to inhabit the gut of our meat-producing mammals, and if these in turn could be induced to eat sawdust, the production of socially-acceptable meat from wood would be a reality. Knowledgeable zoologists, acquainted with the complexities of host-symbiont relationships and the aeons of time over which these have evolved, may well find this idea mainly remarkable for its naïveté. Still, even if it gives rise to nothing more substantial, it may contain food for thought, perhaps along lines less controvertible. I wonder, for example, what kinds of protozoa inhabit the stomachs of the notorious tree-eating goats of North Africa, and what use could be made of them. After all, man has already achieved remarkable transformations of his domestic animals with little resort to the exciting techniques of twentieth-century science. The wooden horse is a myth of antiquity: perhaps steak from sawdust will be a reality of the future.

BODENHEIMER, F. S.: *Insects as Human Food*, Junk, The Hague, 1951.

77 Trained Animals as Collectors*

by N. W. PIRIE

One of the advantages that factory synthesis has over natural production is that the latter often involves a great deal of manual labour in collecting scattered small quantities of material. To take an extreme example: if there were a crop that produced as high a yield per acre of rubber as *Hevea*, and could be harvested by running a reaper over the field, it is unlikely that synthetic rubber would have the appeal that it has. Various other plant products – exudates, fruits, seeds, etc. – are even more scattered and expensive to collect but the process of collection does not need the intelligence of a man, all it needs is discrimination and in this respect animals are often our superiors. Monkeys are used in S.E. Asia for collecting coconuts and other objects from trees. With modern knowledge of conditioned reflexes the techniques could be extended. Insects, for example bees, are well-known collectors and flight gives them advantages over monkeys. There are great possibilities in using trained insects† to collect specific products of relatively high value. Bees that have been deliberately misinformed about what they should collect are an obvious possibility but the potentialities of other insects could also be exploited.

78 Procrustean Bananas

ANON

Straight bananas should be cultivated, for ease of packing. [Preferably with regular hexagonal cross-sections! *A.Ed.*]

* Compare: *The Ides of Mad*: page 65, New York, 1961. *G. Ed.*

† PIRIE, N. W.: 'Applied Biology: or getting to work on the muddles', in *An Outline for Boys and Girls and their Parents* (ed. N. Mitchison) Gollancz, 1932.

79 Multipurpose Plants

by N. W. PIRIE

Primitive man recognised that some plants had potentially useful seeds, others tubers, others fibres and so on and he rarely got two useful products from one plant. The use of apple-wood for tools and straw for thatching are not genuine exceptions. We do better now. Cotton-seed is of comparable importance to cotton, and several industries depend on the regular availability of agricultural by-products with useful, if not unique properties. This process could go much further; waste leaves from crops such as jute, ramie, sugar-beet, peas for canning, etc., should be usable as protein sources.* But that would still leave many parts of the plant unused. Plant breeders have shown so much skill and enterprise in combining qualities from different plants into one new variety that it may be worth while defining an ideal. It may prove impossible to attain but we should know what we are striving for:

Its roots should carry tubers, these will probably be of use only as starch sources because that seems to be the way with tubers.

The stem would give a fibre like jute or ramie.

The leaves would have to be large to support the amount of synthesis we envisage. They should also be soft and well adapted to protein extraction.† This extraction separates protein from the water-soluble components of the leaf, so these should include a useful alkaloid, dyestuff precursor such as indican, or an antibiotic.

There should be a seed, preferably with the high protein content that is characteristic of legume seeds, and it might as well have a covering of fibre, like cotton, as well.

* PIRIE, N. W.: *The Circumvention of Waste:* (Eds. F. LeGros Clark and N. W. Pirie) 'Four Thousand Million Mouths', page 180, Oxford University Press, 1951.

† MORRISON, J. E. and PIRIE, N. W.: 'The Large-scale Production of Protein from Leaf Extracts': *J. Sci. Food and Agric.*, volume 12, pages 1–5, 1961.

All that is asking a good deal, and we would be well satisfied with less complete utilisation at first. But it is possible, and the fact that nothing like it exists is irrelevant, for there are no conceivable circumstances in which unaided evolution would give an advantage to so many qualities present simultaneously in one plant. Furthermore, the demands that such a plant would make on water and nutrients in the soil would make it viable only in an environment thoroughly controlled by Man.

80 Nitrogen Economy in Ruminants

by N. W. PIRIE

Plants produce carbohydrate abundantly, but adequate protein supplies for intensive animal production are more difficult to come by. Even a rapidly-growing young animal generally metabolises ten times as much protein as it uses for building muscle and other structures. The rest is excreted as urea, creatine, allantoin and so on. This has probably come about because, in the natural world in which the ancestors of our domestic animals evolved, there would have been no advantage in having it otherwise. In most parts of the world the normal vegetation contains enough protein to satisfy the needs of animals less demanding than a modern cow or pig. Deserts are an exception for here the survival of the species depends on those individuals that are able to survive during periods of adversity when the only food available is withered plant residues, straw, and sticks with a very low protein content. It is not altogether surprising therefore that camels seem to be able to conserve urea.* There is abundant evidence that part of the nitrogen requirement of cattle can be met by feeding them on urea. It

* SCHMIDT-NIELDEN, B., SCHMIDT-NIELDEN, K., HOUPT, T. R. and JARNUM, S. A.: 'Urea Excretion in the Camel': *Amer. J. Physiol.*, volume 188, page 477, 1957.

would therefore seem to be worth while to try to select cattle that are able to secrete urea from the bloodstream through the rumen wall instead of excreting it through the kidney. The same nitrogen would then go round an internal cycle: having been catabolised by the physiological processes of the body it would be returned to the rumen flora which would turn it into protein that would be absorbed farther along the ruminant gut. The protein actually needed for growth or milk production would, of course, have to be supplied but the heavy wastage in urine would be partly avoided.

81 Concerning Life on Stellar Surfaces

by HARLOW SHAPLEY

My first proposal is that there are at least ten times as many stars in the solar neighbourhood as we have heretofore accepted, perhaps a hundred or more times as many. And what holds in this region probably prevails throughout most of the stellar universe. My second speculation is that some of these unrevealed stars tolerate on their surfaces that chemical delicacy which is by us called protoplasmic life. How to present such deductions briefly and still be reasonable and believable is my immediate problem. Several years ago I tried in a paragraph or two to be reasonable in this important matter, and I named these unseen bodies Lilliputian stars;* but apparently nobody thought my hypothesis worthy of comment. Probably I was too brief. Perhaps my surmise tasted too much like science fiction. Some writers may now say that it is obvious what I am about to state, and hardly worth arguing about.

First, I shall make some remarks on star birth, statements rather than speculations. The most acceptable theory of the

* For example, in *Of Stars and Men*, Beacon Press, page 60, 1958.

origin of this solar system is the neo-Kant-Laplacian hypothesis which views the precursor of the sun and planets as a diffuse gravitationally contracting mass – dust as well as gas, the former particles of varied sizes, the latter mostly hydrogen but with the metals weakly present. The chemistry of the earth's crust, as Harold Urey and others have pretty clearly shown, supports this view. It is a better theory of the origin of our solar system than the collisional-disruptive hypothesis that makes planets, or at least planetary material, out of the debris of catastrophe.

No doubt both types of planetary origin could and do produce planetary systems of varied structure. But for our own, the contraction of a slowly rotating mass of dust and gas and rock fragments is the most reasonable. It is not inconsistent with the facts of planetary spacing, with the rotational peculiarities of sun and planets, and with a variety of evidence from meteors, comets, asteroids, and the zodiacal light. It is a pretty satisfactory theory, but much work on it is yet to be done.

Now here is a preliminary searching question: Why is the sun just so big? That query leads us into the heart of the speculation. Are there stars of all sizes, or only stars like those we see and catalogue? Years ago Eddington pointed out that, if the sun were ten or a hundred times as massive as it now is, it could not generally hold together as a single star. The expansive pressure of radiation from within would blow the star up. A double, or a cluster of stars might result from the gravitational evolution of a too-massive gas cloud. It could possibly develop into an 'expanding association' of stars, such as Blaauw, Ambartsumian and others think and write about. A proto-sun five times the sun's mass could be safe enough – we have many such giant stars.

So much for an upper limit of star size. How small can a star be? If it is very much smaller than the sun it will not generate enough internal heat from gravitational compression to shine noticeably in the visual or photographable wave-lengths. It will not tap the nuclear energy source such as that which maintains the sun's radiation.

But on any reasonable theory of planetary formation there must be innumerable free roaming bodies of very small mass. To me they seem inevitable, and in time we may find methods of 'seeing' them. Extra-sensitive, far-to-the-red photographic emulsions may track them down; or transcendent photo-tubes and quantum counters may do it, or, more likely, high resolution radio telescopes.

We know of 'dying' dwarf stars of one hundredth the brightness of the sun, but their masses are not so dwarfish – more like a tenth that of the sun. Such bodies are simply inefficient radiators with a low output of radiation per unit mass. Jupiter, our greatest planet, is in mass one thousandth that of the sun, which is an average star. The earth is but 1/330,000 of the sun in mass, and yet it was able to condense from a nebulous stage to its present size and mass, even though in the near presence of the disturbing proto-sun and proto-planets. I see no reason for not believing that space must be fairly rich in independent dark bodies with masses between one-tenth and one-thousandth the solar mass. The speed of galactic rotation and other stellar motions do not preclude a considerably larger total mass of our Galaxy than that tentatively proposed by most students of stellar dynamics.

Both Jupiter and the earth were gravitationally formed, our most agreeable theory says, at the same time as the sun. Jupiter's mass is great enough to hold its aboriginal hydrogen and other light gases, as do the sun and ordinary stars. The proto-earth, however, was gravitationally too weak to hold its fast-moving light atoms; hence its chemical make-up is now unlike that of stars and of such major planets as Jupiter and Saturn. The atomic 'winds' from the sun helped the earth's atmospheric hydrogen to escape.

The earth's surface is kept livably warm by sunlight, and by its own inner heat that arises from at least two causes: the gravitational compression on its nuclear core, and the energy released by the automatic radioactivity of uranium, thorium, radium, and potassium-40. Jupiter is roughly five times as far from the sun as the earth and gets, therefore, relatively little heat from solar radiation; but it gets a good deal from gravitational pressure, and that source would be available in the complete absence of neighbouring sun or star. Saturn and the other outer planets get still less heat from the sun than Jupiter receives, also less heat from gravitational pressure (being less massive). Those outer planets are of course lifeless because of the cold – that is, without life of the kind we know, which is based on protoplasm and on water in a liquid state.

Among the billions of planetary systems in our galaxy, and the multillion planets in the billions of other galaxies, there must be innumerable instances where the planets are much bigger than Jupiter and which therefore must have richer internal sources of heat. The yet invisible companion of the star 61 Cygni is probably such a body. We know of its existence through its gravitational effect on the motion of its parent star; some day we may glimpse it, if our instrumental ingenuity continues to grow.

To repeat, I suggest that in the interval of mass between Jupiter-size planets and of the known white, yellow, and reddish dwarf stars, there is a continuous spread of masses – that is, innumerable undetected celestial bodies. I can see no reason to maintain that these giant planets must be associated with stars. Of the wandering objects, those of one-hundredth the solar mass are probably much more numerous than those a tenth of the solar mass; and those a thousandth of the solar mass, much more numerous than those a hundredth the solar mass. Indeed, why not accept the probability that the ordinary stars of our photographic plates and of our catalogues are in a small minority among the bodies that have condensed out of the primeval gas clouds? Even if we should prefer the alternate hypothesis that planets are catastrophic products of stellar collisions or explosions, these lightless objects must be vastly more numerous than the lurid stars.

(Here we interpolate that planets are probably scarce or only temporary in tight double or multiple star systems because of the always operating perturbative forces. It now appears that at least in this vicinity more than half of the visible and photographable stars are not singles like our sun. But this tendency to doubling or to congregating into star clusters would have little bearing on the existence and numbers of independent Lilliputs.)

Restating the argument: if Jupiter has contracted out of a part of the original proto-solar-system nebula, should we not also expect that all sorts and sizes of the end-products of contracting nebulae exist in inter-stellar space? It would be reasonable to expect, as mentioned above, many more Lilliputs than stars of sunlike mass. The smaller they are, the more numerous they should be until we get down in size to where gravitation does not hold the proto-planet together against natural gaseous diffusion.

This then is my first speculation: throughout interstellar space there are myriads of celestial bodies that are not orbitally obedient to any star – these in addition to the planets of all sizes that are immediately subservient to stars. These independent bodies are, of course, gravitationally involved in the rotation of galaxies and some of them are involved in localised stellar assemblies. They get but little radiant energy from the stars or from each other. Their surface energy supply is from within, and is probably not significant in amount unless the mass is more than ten times that of Jupiter.

At some mass, however, between that of Jupiter and the dwarf red stars, the surface temperature must be right for a permanent crust to form and for the water molecules to appear in a liquid state – not steaming hot, not frozen cold. Then something momentous can and undoubtedly does occur. Slowly but inevitably, with electric charges (lightning) playing on the primitive atmospheric gases, natural chemical reactions produce amino and nucleic acids, the forerunners of proteins, of biological cells, and of organisms!

I see no escape from this conclusion, for cosmic numbers are big enough and cosmic time is long enough to permit even very infrequent reactions to occur abundantly. The chemical make-up of a lightless star or planet must be much the same as we have here in the solar system; spectrum analyses of stars and nebulae have convinced us of the general similarity of the chemistries throughout the stellar universe – no new kind of atom. Although the light, with wave-lengths that assist in photosynthesis as we know it, will not be generally available, these 'lightless' bodies will have available the energy of lightning that is naturally produced in turbulent atmospheres; and have also the energy sources of radioactivity and of body heat such as is manifested on the earth by volcanoes and hot geysers. Such sources will provide on the surface of some suitable Lilliputian stars the necessary temperature for evolving the lowest animate forms from the inanimate. Moreover, the lack of light refers only to the blue-to-red section of the electromagnetic spectrum; in the longer wave-lengths, where we are blind, there would be the possibility of some kind of practical 'vision', and a different sort of photo-synthesis.

Life on these Lilliputian stars? (Or, borrowing further from Gulliver's Travels, should we call them Brobdingnagian planets?) – What a strange kind of bio-synthesis it might be. There would be no need for eyes of the sort we and many other animals possess. In itself that should not be a very strange limitation for there are many species of eyeless animals. Other sense organs – for hearing, smelling, tasting, feeling – need not be denied Lilliputian organisms, and possibly there could be additional sensations and corresponding sense organs that we earth-bound creatures know nothing about. There might be, for some kinds of organisms, built-in receptors, which we do not have, suitable, for instance, for longwave radiation, and also for magnetic phenomena and for other imaginable reactions. And since Lilliputian stars of all sizes greater than the earth would be likely, with varied physico-chemical conditions, the array of sense organs could differ widely from one planet to another.

Where are these independent dark pseudo-stars, these darkies of interstellar space? Everywhere! For reasons that we can now only roughly surmise, they might be more numerous in some parts of the Milky Way than in others – more numerous in the spiral arms than in the intermediate latitudes; more numerous in the great nebulosities than in the stellar deserts. Apparently there are no massive ones very near the solar system at present; such would be detected through their gravitational perturbations of our planetary orbits. In a century or so we may have advanced in our celestial mechanics and our astrometric precision to the point of saying whether any Lilliputian object of effective mass is in this part of space. Their radiation, as said above, will be 'redder' than infra-red, and probably most potent in millimetre or centimetre wavelengths. The hundreds of radio stars, that have not been and perhaps never can be identified as super-nova remnants or peculiar nebulae or galaxies, may be Lilliputs that steadily send out their low temperature radiation from weak energy sources, so weak that only the most powerful equipment can isolate and accurately locate them.

The imagination boggles at the possibilities of self-heating giant planets that do not depend, as we do, on the inefficient process of getting out warmth through radiation from a hot source, the sun, millions of miles away. What a strange biology might develop in the absence of the violet-to-red radiation! Would we expect only animal and plant kingdoms to exist on Lilliputian surfaces? Or would there be only the non-cellular Protista? What will the stronger gravity do to biological structures? The livable dark Lilliputian star must be big enough to have a warm surface and not so large that all vital molecular aggregates are crushed. Perhaps a different style of evolution will be entailed.

Only one two-billionth of the sun's outgoing radiation is blocked by the earth, and only a fraction of that radiation is used in photosynthesis. It is a very inefficient method of providing terrestial energy compared with the efficient operation of a Lilliput of optimum mass. Such a body, unlike the earth, would not be parasitic on a neighbouring star, not dependent on a parental star for its energy supply. It would be self-warming. Its biology might have to be anaerobic, like some of ours – that is, oxygen free. If so, like the bacteria, its organisms might not rise to what we consider great importance.

To summarise. My first proposal is that interstellar space is populated with myriads of independent celestial bodies, in size and mass between giant planets and dwarf stars, and that some of these are of the right mass to have crusts that are warm enough to maintain water in a liquid state, and not too hot. My second proposal is that such bodies, even in the complete absence of violet-to-red radiation from a neighbouring star, can be suitable for the origin and maintenance of living organisms, which modern astrophysical and biochemical research indicate are natural and inevitable products of cosmic evolution. Thus, the nearest extra-terrestrial life, ignoring Martian possibilities, may not be that which is on planets circling sun-like stars, but may be life that is on the surfaces of small cool stars – objects that cannot now be seen but eventually may be revealed as faint 'radio sources', or otherwise made manifest.

[*Editorial comment.* Shapley suggests that the self-warming of a Lilliputian star would be a more effective method of supplying energy to living things than is the sun's radiation on earth. I doubt this. The synthetic process which would occur in any conceivable form of life requires an external source of energy. On earth, two such sources are used. Green plants utilise the radiant energy of the sun by photosynthesis, and all the rest (animals, bacteria, fungi, etc.) use a chemical source. The common method is the oxidation or anaerobic fermentation of organic molecules which are present only because of plant photosynthesis, but a few bacteria get their energy from inorganic reactions; in principle any exothermic reaction would do, provided there is an external supply of the reagents.

Now consider the Lilliputian star. The radiant energy supply will be very small, though it might be sufficient to support a very sparse living population. Chemical sources will depend on photosynthesis, because a life form which relied on a fuel which was not continually being replaced by some other life-form would soon run out of fuel, just as we shall soon run out of coal and oil. The other possible sources of energy, not used on earth, are mechanical (e.g. in wind or water), electrical (lightning), radioactive decay, and heat. Of these, mechanical and radioactive energy would be difficult to harness, particularly for the necessarily small primitive life forms; lightning, in addition to being difficult to harness, would be too intermittent to be very satisfactory. This leaves heat, which would be virtually useless as a source of energy. Energy can be obtained (e.g. by a gas engine or a thermocouple) only if there is a *difference* in temperature between two points; a steam engine works not because the boiler is hot, but because it is hotter than the condenser. I can just conceive of an organism with its head in a volcano and its tail in a lake, but only just. *B.Ed.*]

82 Mutation Rates and Stellar Explosions

by G. J. WHITROW

Despite the orthodox view concerning the respective roles of mutation and natural selection in relation to biological evolution, the direct evidence for the theory is still extremely meagre and a number of biologists are doubtful if the enhanced survival value given by micro-mutations (in the very rare favourable cases) are sufficient to ensure the establishment of a new variant. This alleged insufficiency of the hypothesis of natural selection of micro-mutations to explain, *inter alia*, primary chemical differences (for example, haemoglobin versus haemocyanin) has led Goldschmidt* to advocate the idea of macro-evolution by

* GOLDSCHMIDT, R.: *The Material Basis of Evolution*: New Haven, 1940.

means of 'systemic mutation'. By this he means a change of structural pattern in the chromosome as a whole, which may lead, possibly in a series of steps, to a new stable pattern yielding a new phenotype (or species) separated from the old by a 'bridgeless gap', non-viable intermediary forms being rapidly eliminated.

This theory has, however, been adversely criticised by Dobzhansky* who argues that, since the mutation rates for most genes are known to be low, a new form arising by systemic mutation would be a unique individual among a mass of unchanged relatives, and would therefore be obliged to mate with such an unchanged relative. Moreover, if, as Goldschmidt suggests, such matings produced inviable offspring, the new type would leave no descendants.

It is the object of the present note to point out that Dobzhansky's claim that 'this consideration is fatal to Goldschmidt's theory' depends on the assumption that natural mutation rates have remained invariable, and that this may not be true, if, for example, they were affected in the past by sudden major changes in cosmic ray intensity, incidence of comparatively near-by novae, supernovae, etc. Novae are observed in our galaxy at the rate of about two a year, so that 200 million would have been observable from the Earth in the last 100 million years. Out of these, one or two might well have occurred comparatively near-by, since the total number of stars in the galaxy is of the order of 100,000 million. Supernovae in our galaxy occur only at the rate of between five and ten in a thousand years, but it is quite conceivable that, say 100 million years ago, one occurred sufficiently near to cause a phenomenal temporary increase in mutation rates on the Earth's surface.

[It is interesting to consider the possible evolutionary consequences of a temporary but dramatic increase in mutation

* DOBZHANSKY, T.: *Genetics and the Origin of Species*, 3rd ed.: page 203, New York, 1951.

rate. But even if such an increase were to occur, it would not alter the validity of Dobzhansky's objection to Goldschmidt's 'systemic mutation'. Even if there were a thousandfold increase in the frequency of all mutations, the frequency of any particular mutation, including a 'systemic' mutation, would still be very small, and the rest of Dobzhansky's argument would therefore still hold. *B.Ed.*]

83 Trouble in Aquila, and other Astronomical Brainstorms

by ARTHUR C. CLARKE

Bitter experience has made me reluctant to go into print with half-baked ideas – at least until I have had a thorough discussion with a patent lawyer. For in 1945 I published a very far-out article ('Extra-terrestrial Relays', *Wireless World*, October 1945) in which I described in some detail the use of artificial satellites, particularly those in the synchronous or 24-hour orbit, as a solution to the problem of world communications. I never expected this idea to be baked in my lifetime; it now appears that I badly misjudged the heat of the oven.

Nevertheless, I have a few suggestions, of varying degrees of seriousness, which I am prepared to donate to the general scientific (and science-fictional) community. Some have been sitting in my writer's note-book for years, and as it now seems unlikely that I will ever use them, here they are for what they are worth – which is probably exactly nothing.

1. According to Norton's Star Atlas, there have been twenty fairly bright novae between 1899 and 1936. *No less than five of them have been in one small area of the sky, in the constellation Aquila.* There were two in a single year (1936), and the 1918 Nova Aquila was one of the brightest ever recorded.

What's going on in this constellation? Why did 25 per cent. of the novae in a forty-year period appear in only 0·25 per cent. of the sky? *Is the front line moving in our direction?*

2. On the same subject, possibly. The galaxy M 87 has, jutting out from its heart, a luminous jet unique in the known universe. (*See* article by Otto Struve, 'Virgo A', in the February 1961 issue of *Sky and Telescope*.) This brilliant jet, according to the Russian astronomer I. S. Shklovsky, liberates as much energy as the explosion of 10,000,000 supernovae. To provide this amount of energy it would be necessary to *annihilate the mass of a hundred suns*.

If this calculation is correct, astronomers have some explaining to do. No natural process is known – and it is hard to conceive of one – that could totally annihilate a hundred suns. Some concentrations of energy are so abnormal that they can be explained only by deliberate and intelligent planning. The explosion of an H-bomb is one obvious example: is the M 87 jet, despite its stupendous scale, another? We, who have progressed from steam engines to thermonuclear reactions in a couple of centuries, should have such powers at our command in a thousand, or a million years.

Perhaps this extraordinary – indeed, unique – phenomenon appeals to me because of some lines I wrote in the novel *The City and the Stars* (1954). If I may be allowed to quote myself:

> Man was about to leave his Universe, as long ago he had left his world. And not only Man, but the thousand other races that had worked with him to make the Empire. They were gathered together, here at the edge of the Galaxy, with its whole thickness between them and the goal they would not reach for ages.
>
> They had assembled a fleet before which imagination quailed. Its flagships were suns, its smallest vessels planets. An entire globular cluster, with all its solar systems and all their teeming worlds, was about to be launched across infinity.

The long line of fire smashed through the heart of the Universe, leaping from star to star. In a moment of time a thousand suns had died, feeding their energies to the monstrous shape that had torn along the axis of the Galaxy and was now receding into the abyss . . .

That last paragraph might well be a description of the M 87 jet. Perhaps the truth, if we ever discover it, may turn out to be even more remarkable.

3. Here is a terse, bald statement from *The Moon*, by H. P. Wilkins and Patrick Moore, which should appeal to anyone with a spark of imagination. They end their detailed description of the lunar crater Atlas as follows: 'On the north-east is a low ring, and west the deep crater A, with a central mountain. Under high illumination many objects on the floor glitter in a remarkable manner.' NASA, please note.

4. One of the most familiar of all dreams is that of levitation. It has many explanations, including the improbable one that it is a racial memory of the days when our arboreal ancestors spent their lives leaping from tree to tree, and sometimes failed to make it.

But suppose the truth lies in the *other* direction – in the future, not in the past. It may well be that, by the time its story is finished, much the greater part of the human race will have lived under conditions of virtual weightlessness in the artificial planets described by Tsiolkovsky in *Beyond the Planet Earth* or J. D. Bernal in *The World, The Flesh and the Devil.* The levitation dream may be not a memory but a premonition – not a flash-back but a 'flash-forward' from the ages to come.

5. Radar – not radio – astronomy is just starting, and will doubtless yield results of great importance in the near future. However, it appears impossible to use radar methods beyond the solar system, because of the distances involved. We can barely contact the sun, and it would require about 10^{24} times the output of our present transmitters to get echoes back from the nearer

stars. (In radar ranging, one has to fight a *fourth* power law, not a square law, as the signal has to travel in both directions.)

However, there are in existence transmitters from 10^{24} to 10^{30} times as powerful as our radar installations: I refer to the natural radio sources in the galaxy. Can we make use of these for radar purposes? It would be interesting to do a few calculations on the subject. The main problem is the lack of modulation since the signal is a steady one, not a pulse. But continuous-wave radars are now highly advanced – and sooner or later, surely, there will be a 'radio nova' when a new source suddenly appears, generating a burst of electromagnetic energy that will go echoing and rumbling through the galaxy. If we can detect these reflected signals, they may provide us with a great deal of information unobtainable in any other way.

Incidentally, do the radio astronomers realise how lucky they are to have a satellite like the Moon on their doorstep? It might have been designed for them; the far-side is an ideal site for a radio telescope for the following reasons:

Two thousand miles of rock provide excellent shielding from terrestrial interference.

Low gravity (0·16 g) enormously simplifies the engineering problems and reduces, perhaps by more than 90 per cent., the amount of material needed.

Complete absence of windage (a major problem with terrestrial telescopes, which must be designed to withstand the worst possible wind conditions).

Low rotation rate, making tracking easy.

No ionosophere (probably).

The optical astronomers have had a good run on Earth, and are only now preparing to move (into space, or to the moon) after some five thousand years. I shall be surprised if the radio astronomers stick it out as long as fifty years.

84 Interstellar Communication for Chemical Research

by I. J. GOOD

Three theories have been suggested to explain the 'handedness' ('chirality') of naturally-occurring sugars, amino-acids, and chromosomes.

(i) They are all descended from a single molecule, or perhaps two or three, which came into existence more or less by accident.
(ii) A society with 50 per cent. left-handed and 50 per cent. right-handed molecules is unstable, and tends to become all left-handed or all right-handed by a sort of war.
(iii) The handedness is a consequence of an asymmetry in the structure of the universe.

The first two of these theories have been familiar for some time. The third, in a weak sense, dates back to Pasteur,[1] and in a less weak sense to Haldane.[2]

It has been found, in nuclear-physical research, that the structure of the universe is indeed unsymmetrical ('non-conservation of parity').[3] Without this discovery, the third theory would be negatively baked.

A test for the theory would be to discover the handedness of sugars and amino-acids on other planets, not necessarily in the solar system. It is liable to be more economical to get this information by interstellar communication than by interstellar travel, since the nearest star is four light-years away, as the crow flies. Each planet of the correct 'handedness' would give a Bayes factor of about two in favour of the odds of the theory, and a single exception would refute it, like testing a coin for being double-headed by tossing it. (Meteorites might also give information: see the footnote on page 23.)

The third theory seems to require that biochemical evolution depends on nuclear phenomena. It would then be inconsistent with Dirac's remark that quantum mechanics entails the whole of chemistry.[4]

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85 Life in the Sun

by A. D. MAUDE

In 1950 a book by Professor Alfvén was published (*Cosmical Electrodynamics*, Oxford) in which a theory of the formation of sunspots was described. While rival theories exist, Alfvén's theory is one of the most successful. It is not the purpose of this article to defend this theory, but to point out an unexpected consequence of it. It is first necessary to describe the theory, although space does not permit more than an outline.

Magnetic lines of force used to be visualised as weightless elastic strings which tended to become short and fat. While this picture is no longer fashionable, it is still perfectly valid. If such a line of force (Fig. 12) ran through a liquid which was a very good electrical conductor, it would not be able to move through the liquid. This is because any such movement would cause eddy-currents to flow, and the magnetic field of these currents would restore the main field to its original position relative to the fluid. The liquid would be, as it were, 'glued' to the line of force. As a result of this, such a line of force would act as if it had a mass, so that it could transmit waves in the usual manner of a stretched

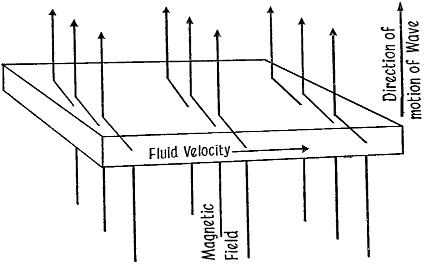

Fig. 12. A plane Alfvén wave.

string. Such waves are known as magneto-hydrodynamic waves or Alfvén waves, and their existence can be deduced rigorously from Maxwell's equations.

In a magnetic field there are, of course, many lines of force, and if one line were displaced sideways, carrying some liquid with it, other liquid would have to move out of its way. One way of arranging for this to occur would be for a circle of fluid to rotate, so that a *whirl ring* would be produced which would travel along the lines of force, as in Fig. 13. Whirl rings need not be perpendicular to the field and their plane may make any angle with it. Here we are principally concerned with rings in which the field is parallel to their plane (Fig. 14). The rings may rotate in either direction, and may travel either way along the lines of force. While the whirl ring is one of the simplest forms of wave, there is no reason why waves of any desired complexity should

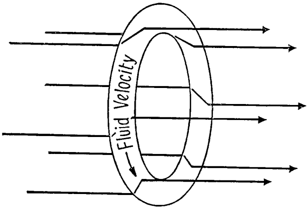

Fig. 13. Whirl ring perpendicular to the magnetic field.

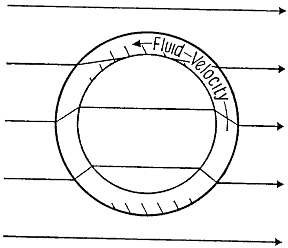

Fig. 14. Whirl ring parallel to the magnetic field.

not exist, and rings with a complex internal structure embedded in them are also theoretically possible.

Professor Alfvén suggested that sunspot pairs were due to whirl rings arriving at the surface of the sun (Fig. 15). He supposed that

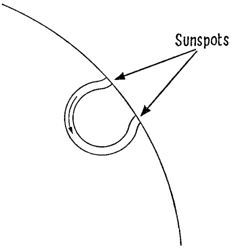

Fig. 15. Whirl ring arriving at the sun's surface producing a sunspot pair.

the rings travelled outward from the centre of the sun along magnetic lines of force.

Consider a whirl ring at *A* in Fig. 16, travelling towards *B*. Professor Alfvén suggested that when the ring reached *B* it entered a particularly unstable region of the sun in which a process similar to convection increased the strength of the ring. One result of this was that a series of echo-like rings were formed, each rotating in the same direction as the original, and each a copy of it, but travelling in the opposite direction. When the original ring reached *C* it was damped, and might sometimes send back an echo which would rotate in the opposite direction. The ring would then continue on its journey, losing energy over much of its path, until it was either damped out altogether, or until it reached the surface at *D* and appeared as a single sunspot or as a sunspot pair.

The echoes starting from *B* would take eleven years to cross the central region of the sun and they, in their turn, would send back a series of echoes from *E* and would continue towards *F*, appearing as sunspots at *G*.

The number of whirl rings would not increase indefinitely, as

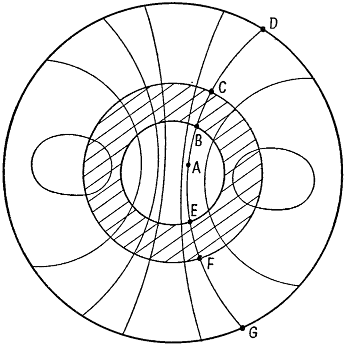

Fig. 16. Cross-section of the sun showing the magnetic field, and the track of the whirl rings.

the available energy would be limited. This would lead to the formation of a group of whirl rings which would cross the sun's core near together, the echoes of which would also cross as a group.

While a group of whirl rings crossed in one direction, a second group with the opposite direction of rotation would be crossing in the opposite direction. This second group would behave in a similar manner to the first one, crossing and sending back echoes which recross the sun's core. These two groups would be entirely independent except for the time when they passed at the sun's centre, and for the occasional echo which would be sent back from D (or F). These would join the other group, and it will be seen they would have the same direction of rotation as the others in it.

This outline of the theory has been too brief to point out how it explains sunspot observations or to explain why it is in just this form. If the reader is interested he should consult Alfvén's book. It is not hard to follow the arguments there, even when the formidable-looking mathematics are not understood at all.

The present author believes that this theory, which accounts for the properties of sunspots in terms of magneto-hydrodynamic whirl rings, leads to the conclusion that rings should undergo a process of evolution. Each ring produces several 'offspring' in the form of echoes at B and E. These offspring are like their 'parents', and any random change which occurs to the 'parent' in its passage across the sun's core is 'inherited' by the 'children'. Not all the 'progeny' can survive to produce a further 'generation'. Under these conditions only those best fitted to the conditions in the sun will be able to survive. It therefore seems highly probable that, if Alfvén's theory is correct, there is a 'struggle for survival' and 'survival of the fittest' leading to some form of evolution. (The author is a physicist, so that he will leave to biologists the decision whether magneto-hydrodynamic whirl rings evolving in the interior of the sun could be called 'alive', but the terminology of life is so convenient in the present case that it will be used, while not intending to beg the question.)

A certain amount of evidence for such evolution may be found in the fact that the whirl rings of one group all rotate in the same direction. With no evolution one might expect a random distribution, but according to the evolutionary hypothesis, if one ring had a particularly favourable form its progeny would displace those of the other rings. This would lead to all the rings of one group having the same direction of rotation. Similarly if one of the two groups evolved to a particularly favourable form then one of the second kind of echo (starting from *D* or *F*) which passes from one group to the other would, after a few generations, take over the other group. The result would be that the two groups would have opposite direction of rotation – as is observed.

As we know so little about conditions near the centre of the sun it is impossible to do more than guess what type of structure will have evolved. By analogy with evolution on the earth it might be expected that the structures would be very complex. There might be various species of whirl ring evolved – perhaps some biologist could say whether this is probable? Some species might be so small in size that they never survive the journey from the core to the surface. Evolution would not fit them for this part of their life, only for that part up to the production of their echo children.

One thing we may guess with reasonable certainty is that, if complex forms have evolved, one of their principle means of controlling their environment will be by means of temperature changes. The rate of production of thermo-nuclear energy within the sun's core is critically dependent on the temperature, so that a small change in the temperature of a region will make a large difference to the available energy there. This would be a most convenient method for a whirl ring to bring up a reserve of strength, or to damp out a rival ring. It is tempting to speculate that the temperature of a sunspot is lower than that of the surrounding material owing to an 'instinctive' reaction on the part of the whirl ring to unfavourable surface conditions.

It is difficult to deduce the form of the whirl ring when it was deep inside the sun from observations of it at the sun's surface. This is partly because conditions in the sun change rapidly with depth so that the magneto-hydrodynamic wave will be distorted. It is also because turbulence in the earth's atmosphere limits our observation to details on the sun of more than about 1,000 miles across.

A scientific theory should predict new results, so it is natural to ask what we could look for to test these ideas. If the rings have a complex internal structure it might be observed by telescopes mounted on artificial satellites. We would then expect that the internal structure of sunspots would resemble each other, or that it would be possible to divide the spots into a number of species, with different internal structures.

It would be a great help if we could make an intelligent guess at the expected structure. We may guess that sunspots have been evolving for the lifetime of the sun (between 3,000 and 8,000 million years), and that the population of a few hundred or thousand whirl rings all have an asexual reproductive life cycle of eleven years. From this knowledge can the biologists give us a guide as to how complex an organism would evolve?

[*Editorial comment.* The hypothesis would be difficult to verify. Compare Olaf Stapleton's *Starmaker,* Fred Hoyle's *The Black Cloud,* or even W. Preyer's theory (1880) that the molten earth was a single mighty organism (*see* Oparin, *The Origin of Life,* page 35). The last comparison is however unfair.

If the theory of continual creation is true, without continual destruction, a galaxy might become so heavy that all the laws of physics would be transformed. Under these circumstances perhaps a whole galaxy might be alive in some sense.

I have noticed, by the way, that ordinary smoke rings in a quiet atmosphere drop 'children' and 'grandchildren', but the umbilical cords are never severed and the new rings do not reach maturity. Note that if an inked nib is drawn across the surface of water a semi-circular whirl ring can be observed. *G.Ed.*]

86 The Blue Haze of Mars

by M. H. BRIGGS

One of the foremost problems facing planetary astronomers at the present time is the composition of the atmospheres surrounding our neighbours in space. Spectroscopic analysis, together with inspired guess-work, has produced the tentative results shown in Table 1. A brief glance at this table is sufficient to show that there are two quite different kinds of planetary atmosphere. The first, which is possessed by the small inner group of high-density planets, contains unreduced compounds; the second, possessed by the outer group of large low-density planets, contains reduced compounds.

Table 1

Composition of Planetary Atmospheres

Planet	Gases present (detected and inferred)
Mercury	none
Venus	nitrogen, carbon dioxide, water vapour, argon
Earth	nitrogen, carbon dioxide, water vapour, argon, oxygen
Mars	nitrogen, carbon dioxide, water vapour, argon
Jupiter	hydrogen, helium, ammonia, methane
Saturn	hydrogen, helium, ammonia, methane
Uranus	hydrogen, helium, ammonia, methane
Neptune	hydrogen, helium, ammonia, methane
Pluto	unknown

If, as seems likely, all the planets once had essentially the same atmospheres, it is clear that something has happened to one of the groups. Studies by Professor H. C. Urey and others have produced evidence that all the planets had atmospheres about the time of their formation, and that these atmospheres were composed of the reduced gases now found only around Jupiter and Saturn and the other big planets. Moreover, actual geological evidence of a reducing atmosphere in the Earth's past has been found by Rankama. The question, therefore, is 'What has changed the atmospheres of the inner planets?'

It is apparent that there is no simple answer to this question. Some of the lighter gases, such as hydrogen, must have been lost by simple gravitational effects, while outgassings of volcanoes must have contributed new gases. But it is of great interest that over the past few years it has been demonstrated that if just this mixture of reduced gases is exposed to ultra-violet radiation, or to electric discharges, many of the important constituents of living matter (amino acids, fatty acids, organic aldehydes, purines, etc.) can be detected in the products. It is quite clear that these processes must have gone on in the atmospheres of all the inner planets when they possessed reducing atmospheres and that organic compounds must have been abundant on their surfaces.

On Earth it is clear that these compounds were the precursors of life. The important problem that remains to be solved is to account for the fate of these compounds on Venus and on Mars.

One characteristic property of all the inner planets is the possession of atmospheres that are opaque to certain visible wavelengths. On Venus the whole planet is surrounded by dense faintly yellow clouds that never clear. I have suggested elsewhere that these are the remains of the primeval organic syntheses that were unable to develop into life owing to the very high surface temperatures (probably about 600° Å).

Both Earth and Mars have two types of clouds. On Earth the most abundant clouds are water vapour, but Dr Went has recently drawn attention to another type of terrestrial cloud, or haze. It occurs particularly over regions of dense vegetation, in such uninhabited areas as the Amazon. This cloud is made of volatile plant products, probably mainly terpenes.

Now Mars too has two cloud types. The first are tenuous white objects that tend to form occasionally over certain Martian surface markings. The second is a much more widespread 'blue haze'. It is easily detected by photographing Mars with plates sensitive to blue light. No surface details can be detected on such plates, though they are quite clear on plates sensitive to longer wavelengths. That this effect is not simply a property of the gases of the atmosphere is demonstrated by the occasional 'blue clearance', when blue-sensitive plates show excellent surface details.

For some reason astronomers have failed to take full advantage of the known properties of organic compounds to explain planetary effects. Yet if the brief account given above concerning the early history of the inner planets is correct, organic compounds were once abundant on both Mars and Venus. The surface temperatures of Mars are surprisingly high. Temperatures as high as 25° C. have been recorded for the equator. On purely theoretical grounds one would expect life to have originated on Mars by precisely the same process that occurred on Earth. The observed properties of the planet go a long way to supporting this prediction. The dark surface-markings have rapid regenerative abilities, while in the infra-red they possess absorptions around 3·5 microns that are characteristic of organic compounds.

With all this evidence pointing to a similarity between Earth and Mars, it is surprising indeed that no one appears to have proposed before that the blue haze of Mars is of a similar nature to the hazes of Earth due to volatile plant products.

Let us take this hypothesis of the blue haze of Mars and see how it works out in practice.

The terrestrial vegetable hazes are blue in appearance owing to their very small particle size (less than 0·1 microns). They appear to be composed of terpenes. The latter are a group of derivatives of hydrocarbons containing ten carbon atoms per molecule. Terpenes are generally fragrant substances, readily volatile, and very resistant to decomposition. According to Went, the amount of terpenes (and similar compounds) evaporated and dispersed from terrestrial plants into the atmosphere is about $1{\cdot}75 \times 10^8$ tons per year. This is a tremendous amount of organic material.

Now, if the surface markings of Mars are some form of vegetation, there is no reason why they should be fundamentally different from terrestrial plants on a biochemical level. And terrestrial plants, almost without exception, manufacture terpenes and related isoprene compounds. Even if these compounds are produced by Martian 'plants' at a much lower rate than on Earth, very appreciable quantities would soon accumulate for a simple reason. On Earth the terpenes must slowly oxidise by reacting with free atmospheric oxygen; but on Mars, which cannot retain oxygen owing to its low gravity, the terpenes would be immune to oxidation. They could also serve a very important role. On Earth there is a layer of ozone, formed from oxygen, in the upper atmosphere that prevents short ultra-violet radiation from the sun from reaching the surface. Such radiation has harmful effects on all terrestrial life-forms. On Mars no ozone layer can exist, but the terpene haze probably presents an equally effective ultra-violet screen.

The hypothesis appears to offer a reasonable explanation of the haze and makes no assumptions other than that Martian surface vegetation is biochemically similar to terrestrial vegetation.

The various hypotheses of inorganic materials proposed by astronomers have all had grave weaknesses. For example, dust clouds are improbable on two grounds: particle size and inability to clear rapidly under Martian conditions. Clouds of carbon-dioxide-ice are much too opaque, while water-ice would require a concentration of water vapour in the Martian atmosphere much higher than that observed. The postulated interaction of carbon dioxide and carbon monoxide to give free carbon seems most improbable under the physical and chemical conditions that prevail on Mars.

This hypothesis may be satisfactory on theoretical grounds, but is there any experimental method of verification?

I think there is (aside from going and collecting some of the haze for analysis). It was mentioned above that infra-red absorptions, probably due to organic molecules, have been detected in the Martian spectrum. According to Sinton, who made the observations, the relative intensity of the absorptions in spectra of the dark surface markings was about two, and of the light markings (which are thought to be deserts), less than 0·5. It seems unlikely that the absorptions detected in the spectra of the desert regions are caused by vegetation. But terpenes in the atmosphere above the deserts would give just such weak absorptions.

Hence the terpene-haze hypothesis of the Martian atmosphere leads to a prediction that is capable of experimental check. If an infra-red spectrum of the Martian deserts can be obtained during one of the rare 'blue clearances', there should be no absorptions at 3·5 microns.

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87 The Limitations of Molecular Evolution

by JOHN MAYNARD SMITH

The analogy between messages in words and genetic instructions on chromosomes is not new, but there is one application of it which has I think been missed, and which has some amusing consequences. This is the analogy between molecular evolution and a particular word-game. In this game, it is required to convert one word into another by changing one letter at a time, with the limitation that each intermediate stage shall also be a meaningful word. Thus WORD can be changed to GENE as follows:

WORD
WORE
GORE
GONE
GENE

In evolution the unit events, or mutations, are changes in the sequence of the four bases in DNA, and are believed to have as their immediate consequence changes in the sequence of amino-acids in the proteins programmed by that DNA. At the protein level, probably the commonest evolutionary event is the substitution of one amino-acid for another, corresponding to a single event in the word game; other possible unit events are discussed below.

I suggest that a population of organisms which cannot now produce a particular protein *X* cannot evolve the capacity to do so unless *X* is connected by a series of unit steps (consequent on unit changes in DNA) to a protein it can now make, such that each intermediate is a functional protein which would, in some environment, be an improvement on, or at least as good as, its predecessor in the series. This is because any particular step will occur with very low frequency, so that the simultaneous occurrence of several favourable steps affecting the same protein molecule is ruled out. Hence each protein arising by a single step must increase in frequency in the population under the influence of natural selection before the next step can be taken.

Consider the evolution of proteins 50 amino-acids long. Since there are 20 different kinds of amino-acid commonly present in proteins, there are 20^{50} different possible sequences. What proportion of them would be biologically useful proteins I don't know, but it seems likely that most of them would be useless.[1] If so, the functional proteins may occur as a series of 'islands' surrounded by a 'sea' of meaningless sequences, just as the words ALLY, ABLY, ABLE, AXLE form an isolated island in the word game. [ABBE, ABYE, and ALLA need not count. *G.Ed.*]

If f is the fraction of amino-acid sequences which are meaningful, in the sense of being functional proteins which would be favoured by natural selection in at least some environment, and if N is the number of transformations of a protein which can occur by unit mutational steps, then if fN is greater than unity the 'landscape' will be a continuous network of meaningful land together with a number of small isolated islands; but if fN is less than unity, the landscape will be a sea with many small islands.

The number N depends on the as yet unknown nature of the genetic code whereby base sequences in DNA determine amino-acid sequences in proteins. So far as we know, the unit changes possible in base sequence in DNA are:

(i) the substitution of one base for another; GORE – MORE
(ii) the elimination of one or more consecutive bases; GORE – ORE.
(iii) the duplication of one or more consecutive bases; GORE – GOGORE.
(iv) the inversion of a group of bases; GORE – OGRE.

Types (ii) and (iii) would cause changes in the number of amino-acids in the protein, and could be involved in the evolution of one protein to another of the same length only by rather roundabout routes.

Type (i) can lead to the substitution of one amino-acid for another. Since there are 20 amino-acids, it might seem that there are 50 x 19 possible transformations of a sequence 50 units long. But this depends on the nature of the code. For example, according to the code suggested by Crick *et al.*[2] each amino-acid is determined by a sequence of three bases. Of the 64 possible triplets, only 20 make sense (i.e. determine an amino-acid), the rest are nonsense. A meaningful triplet can be transformed into an average of 5 others by a single base substitution; thus ACC can change to BCC, ADC, ACA, ACB, but all other changes give nonsense triplets. This would give $N = 250$ approx. for changes of type (i). There will be some additional transformations of type (iv), involving one, or several neighbouring amino-acids (for example, ACA BCB and ACB ACB are both pairs of sense triplets in Crick's code); but long inversions will always be nonsense, because some meaningful triplets are transformed by inversion into nonsense triplets. I have not enumerated the possible inversion transformations for Crick's code, but it seems possible that for this code, allowing for transformations of types (i) and (iv), N is about 500.

Since we know that evolution has in fact taken place, there would seem to be two possibilities. Either there are types of mutational event in addition to those discussed here (for example, the presence of additional DNA with an unusually high mutation rate, not programming proteins necessary for survival, but throwing up a variety of sequences on the off-chance that one might be useful),[3] or fN is greater than unity. But even if the latter is true, there are likely to be isolated islands, comparable to the AXLE group in the word game, consisting of proteins which cannot be produced by evolutionary processes even though they would be selectively advantageous if they arose.[4] These proteins are the analogue of the wheel and the magnet in structural evolution. If they exist, perhaps we shall learn to synthesise the necessary DNA sequences and incorporate them in our domestic animals, to enable them to browse on and digest the nylon and polythene which we may expect by then to cover England to a depth of several feet.

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1. The example of sickle-cell anaemia suggests that the proportion

of proteins which are useful may be much higher in diploids – one good reason for being diploid.

2. Crick, F. H. C., Griffith, J. S. and Orgel, L. E.: 'Codes without commas'. *Proc. Nat. Acad. Sci.*, volume 43, page 416, 1957. While this book was in the press, an article appeared (Crick, F. H. C., Barnett, L., Brenner, S., and Watts-Tobin, R. J., *Nature*, 30 December, 1961) indicating a different 'triple' code which implies perhaps twice as large a value for N.
3. Large duplications occur, and provide stretches of DNA whose programme is not necessary for survival, because the message is already carried elsewhere. Such duplications can be reprogrammed by selection to determine new proteins. This is certainly an important evolutionary process, but it does not greatly help in crossing the 'seas' of meaningless sequences, because the initial duplicated sequence will be a meaningful sequence; if it were not, it would not have been present to be duplicated. The reason for suggesting DNA with an unusually high mutation rate is that in such DNA several favourable steps might be taken simultaneously.
4. If fN exceeds unity, then the equation $p = \exp[(p-1)fN]$ has a solution, less than unity, giving the proportion of viable proteins that are inaccessible. *G. Ed.*

88 Random Synthesis and Subsequent Separation

by N. W. PIRIE

There are two ways of getting a prearranged sequence of objects, for example playing-cards. They can be shuffled and dealt at random over and over again until the right sequence emerges; they can be looked at and arranged deliberately. Which course is adopted depends on the state of development of techniques for dealing and recognising the right sequence, or for arranging. Hitherto, chemical synthesis of large molecules has set out deliberately to put them together step by step in the right order, but techniques for separating complex mixtures are now reaching a state of development that makes the alternative approach possible. It may well be that this is how specific synthesis is managed in the cell and that the structures that Armstrong* postulated as controllers of specificity and compared to an engineer's templates, act more by selecting the right configuration than by directing synthesis towards that configuration. Such a process, working in conjunction with a mechanism for destroying those molecules not selected and using the fragments for another cycle of synthesis, would not necessarily be less efficient, either *in vivo* or *in vitro*, than directed synthesis.

89 Genetical Music

ANON

Birds reared in sound-proof rooms sing tunes appropriate to their species.† The tunes must be genetically represented, presumably in the chromosomes. By means of a study of 'mutant tunes' it might be possible to recover the structure of the genetic determination. If, for example, the notes were represented linearly along the length of a chromosome, one would expect an occasional bird to sing tunes backwards.‡ Since this does not happen, it would appear that the organisation is somewhat hierarchical.

90 Natural Rejection

by I. J. GOOD

A good deal of work in the theory of evolution is concerned with questions of the form 'This species has a certain attribute:

* ARMSTRONG, H. E.: 'Studies on Enzyme Action V. Enzyme Action as Bearing on the Validity of the Ionic Dissociation Hypothesis and on the Phenomena of Vital Change': *Proc. Roy. Soc.*, volume 73, page 537, 1904.

† THORPE, DR W. H., verbal communication.

‡ For a discussion of chromosome 'inversion' *see*, for example, MAYNARD SMITH, J.: *The Theory of Evolution:* page 117, Penguin Books, 1958.

what are its advantages?' I think it may be stimulating to ask quite often, 'This species does not have a certain attribute: what are its disadvantages?'; the obverse of Natural Selection: Natural Rejection.

For example, consider the blindness of cave-dwelling newts. Presumably good vision depends on a large number of favourable genes, so that in the absence of positive or negative selection, the eyes would gradually atrophy.

But now take the example of vitamin B1. Unlike the red mould, we do not have the enzymes for synthesising this vitamin (nor for any other: that is why they are called vitamins). Beck* speculates that our ancestors may once have been able to synthesise vitamin B1. But why did animals which could not synthesise it win the evolutionary struggle against those that could? One would have thought that there must be at least a slight advantage in being able to synthesise it. It makes one conjecture that *the enzymes required for the synthesis may be in some unknown way disadvantageous.*

91 On Living Matter and Self-replication

by L. S. PENROSE

1. *Relation of self-replication to the concept of life*

The object of the paper is to discuss the principles of self-replication and their special relationship to life processes. The essential properties of living substances, as opposed to inanimate material, are difficult to define. One view is that there is a continuous gradation in nature between the two states. Even so there remains the problem of defining the properties which represent the characteristic features of the two poles. Among these

* BECK, W. S.: *Modern Science and the Nature of Life:* page 240, Pelican.

qualities there can be little doubt that self-replication holds a key position. Other main features, accumulation of energy from the environment and maintenance of a steady state, are consequences of the ability to reproduce.

The relationship between these three attributes is also not easy to define but some elementary points seem fairly clear. Thus, if the presence of any object or organism A_1, in given surroundings, automatically causes the construction of a second, identical, object A_2, the whole system A_1 with A_2 has now double the amount of structural energy possessed by A_1. Each newly-formed replica of A_1 will have the same properties as its precursor so that structural energy will be gradually accumulated in the course of self-replication. Furthermore, the repeating process can sometimes reach a steady state. This can occur in what is known as a propagation reaction[4] where the formation of each new molecule coincides with the destruction of the parent molecule.

2. *Variation in structure – alternative forms*

There is another significant property of living matter, namely its range of possible variation, that is to say, out of the same kind of material a variety of different structures can be built. This property differentiates the reproduction in living structures from self-facilitating chemical reactions, like explosions or autocatalytic processes. The stimulus for copying is derived directly from the pattern of the living substance itself. Moreover, if alterations of certain kinds, which can be classed under the general heading of mutations, are made in the seed, these alterations are incorporated in the replica. The result is to produce a new race, which can be called B_1, B_2 and so on, instead of A_1, A_2 and so on. Some types of alteration in A might destroy it or at least destroy its self-replicating ability. Other ehanges might be irrelevant in that they did not change the part of the structure involved in the replicating mechanism. In living organisms, these distinctions were first made clear in Weissmann's idea of separating the soma, which can be altered without affecting replication, and germ track, which contains the hereditary material or instruction programme for building a new organism. Almost the same type of distinction, with due modification, can be made between cytoplasm and nucleus in the single cell. According to modern views based upon experimental cytology, the DNA in the nucleus is the only truly self-replicating substance. It has the full information and is subject to mutation. The RNA, the cell proteins and lipids[3], mostly in the cytoplasm, are secondary structures, alterable for the most part without affecting the hereditary material.

3. *Measurement of self-reproductiveness*

The simplest way in which a self-replicating object can be varied is by quantitative changes. First, let us suppose that two original objects, X and X, change, in response to some rare influence, to an active combination with one bond, $X + X$; this can be called $\overset{1}{X}$. Another change of the same type would produce $X + X + X$ with two bonds which can be called $\overset{2}{X}$, and so on. We may suppose that single molecules like X are built naturally by chance, perhaps even synthesised autocatalytically. At the same time, let us suppose that a chain of given length, with n bonds, $\overset{n}{X}$, arises in the presence of a parent molecular chain of exactly the same length. We are led now to a central principle in self-replication theory. It is this: A structure, which is very unlikely to arise spontaneously, is relatively easily synthesised in the presence of a specimen of this structure itself. It follows that the more complex the structure the more striking will be the result. In the simplest case, using only one kind of molecular unit, complexity is measured by the length of the chain. However, if there is more than one kind of unit in the chain, the potential complexity is enormously increased. In a universe consisting of two kinds of unit, X and x, the number of possible structures (provided the chain has a direction) is 2^{n+1} where n is the number of bonds in the chain. If there is a slight random tendency, p, for bonds to form, the chance that a particular chain of two elements arises will be $p/4$. The random chance of forming any chain of three units will be p^2 and, for a specified chain of three, it will be $p^2/8$. In general, the chance of a specific chain with n bonds arising spontaneously is p^n/S^{n+1}, where S is the number of different kinds of molecular unit in the series. However, if a chain has self-replicating properties, the likelihood of forming a replica of any specific parental chain is equally high whatever the arrangement. It will be limited only by the availability of loose units and the rate of construction, which may depend upon the length.

The concept of self-reproduction of a structure thus appears to be of a graded nature depending upon the probability of spontaneous generation of the same structure. The degree of self-reproductiveness of any structure Z, in a specified environment h, over a given period of time, can be measured by the coefficient R. If $p(Z/Zh)$ is the probability of Z arising in response to the presence of Z in the environment h and $p(Z/h)$ the probability of Z arising spontaneously in the same environment h,

$$R + 1 = p(Z/Zh)/p(Z/h).$$ [12]

In ordinary non-living material the usual value of R is zero. A natural object, such as a piece of rock or a specified molecule, is not likely to arise in response to the presence of another object of the same size and shape. With crystals, however, there is such a tendency. A new crystalline surface will often develop more easily when a surface of the same material to build upon is already at hand than when it is not. Thus, for crystals, R will have low positive values. In an autocatalytic or an explosive chemical reaction the value of R will also be low because factors other than the presence of molecules of the end product, such as the heat generated, facilitate the reaction. In a catalysed chemical reaction producing a given structure, the catalytic agent or the enzyme might facilitate equally the reactions measured in the denominator and the numerator of the coefficient formula and, if so, it would not change the value of R.

For a chemical structure such as a polysaccharide chain, again the value of R is comparatively low. Although a long chain arises easily in the presence of a short primer chain of three sugar molecules[11], in special circumstances a chain can arise spontaneously from single unattached molecules.[5] A single or double strand of DNA, however, has a very high degree of R because just that specificity which characterises it could occur only very rarely indeed by chance concatenation of nucleotides. It may be noted that R can have a negative value if construction of one object inhibits the construction of another either because it disperses suitable elements or uses up rare material. Conversely, with all living objects, whether we think of animals, trees, bacteria or even phage particles, the value of R is, for all practical purposes, infinite and positive.

4. *Theory of single chain replication*

In view of the points already discussed, it should now be easy to see that a basic problem of self-replication can be reduced to the understanding of accurate single-chain reproduction. The process must be such that the presence of the initial parent chain causes an exact copy to be made from pre-formed molecules in the vicinity. In the simplest case an initial chain of two like molecules causes another chain of two and no more to be made. A chain of ten causes another chain of ten to be constructed. For this mechanism to be reliable, it is essential that the new chain should not break or slide away before it is finished. If this should occur the short chain, say with only six units, would act as a new seed; it would then be a mutant type, a monster. In order to prevent these aberrations there must be firm contact between the new chain and the old one at all times during replication, made by cross-links which are finally severed only after the new chain has been completed.

In the method of DNA replication suggested by Watson and Crick[10] no indication was given of how incomplete chains were prevented from becoming detached. The main concern of these investigators was with matching or alignment of complementary molecules on the new chain with those on the old. For various reasons, derived from observations on quantitative estimations of the nucleotide units, they considered only alternating replication. That is to say, the initial chain produced a complementary converse or negative chain which acted as a mould for producing the original positive again. This extra step of converse printing is unnecessary if like molecules have opportunities to pair with one another in a manner which is an everyday occurrence in crystallisation. Watson and Crick did not believe that this type of pairing could be applicable to DNA. In the general, theoretical, case, however, all possibilities may be considered. The simplest solution to the problem of how to make a self-reproducing system involves the use of only one kind of molecule or unit. Complementarity is then not possible and like molecules must tend to align with one another naturally.

The properties required of a molecule which can form precisely self-replicating chains have been discussed in previous papers by the present writer. The basic unit or molecule, besides being capable of alignment with itself, must be able to attach itself permanently to other members of the group when it is suitably activated. There must, moreover, be a method of temporary attachment to the parent chain during building of the replica.[13]

5. *A machine for single chain replication*

The necessary and sufficient conditions for a unit in a self-replicating chain can be most easily appreciated by constructing an actual machine which has the required properties. It might be thought a trivial occupation to build actual machines of this sort because all the details could theoretically be determined by logical planning. However, it is easiest to demonstrate the actual processes concretely to show that they are logically possible and, in the course of the experiment, unexpected pitfalls in the argument are seen and avoided. The first attempt, in 1956, produced a device in which two linked objects facilitated the linking of two others of the same kind when subjected to random horizontal agitation in a linear direction. The junctions were made by using gravity latches. The coefficient R was very low, about 10, because there were only two alternative possible structures and, in practice, one or other was quite frequently produced by chance perturbation.

The R value of a system can be improved either by constructing complex molecules which are well guarded against chance linkage or by making the self-reproducing object itself more complicated by having many alternatives. Devices with different degrees of efficiency have been described by the present writer.[7,8,9] The most satisfactory solution was undoubtedly obtained by the design of units which could form a self-replicating chain of any length.

In the simplest chain, units are of one kind only. They have the capacity of aligning themselves against one another. In three dimensions they would align facing each other, right side of one against left side of the other. In practice, it is more convenient to use a two-dimensional system and here the analogous property is that two sides of the molecule are the inverse of one another in some part of their structure. The units shown in Fig. 17 are supposed to slide freely upon a horizontal surface. Each rests upon a base of the shape shown in Fig. 18 and has a superstructure specified by the letters *A*, *B*, *C* and *D*. The superstructures *B* and *C* contain levers and *A* is a staple. Details of these are given in Fig. 19. Their action here depends upon gravity, though springs could equally have been employed. An additional minor feature, which would be unnecessary in a three-dimensional arrangement, is that each unit is supplied with a

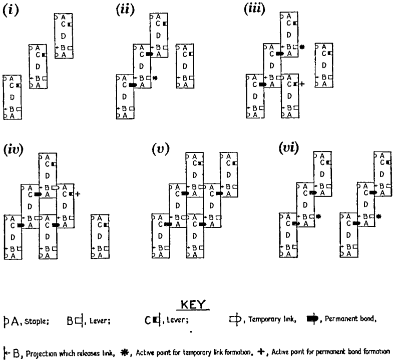

Fig. 17. Diagrammatic representation of a self-replicating chain three units in length.

(i) Three synthetic units, separate and freely movable over a plain surface. The letters *A*, *B*, *C* and *D* represent devices on which the linkage mechanisms depend. They are attached to bases which have aligning properties, shown in Fig. 18, and they are described in Fig. 19(1), (2) and (3), and in Fig. 20. In the separated isolated state these units cannot link to one another.

(ii) A chain made of three units with two permanent *CA* bonds. The asterisk indicates the point where the lever in *B* is active (*see* Fig. 19(5)) so that a *BA* link can be formcd if another staple approaches closely from the right, as in Fig. 19(6). The lever in *B* above the asterisk is also a active, but a free unit, although aligned correctly, cannot approach closely at this point. This is because a second *B*, on account of its projecting ledge, cannot approach closely an active *B* (Fig. 19(11)).

(iii) A free unit can approach the lowest unit of the chain closely enough to form a *BA* temporary link. The plus sign indicates that the lever on *C* is active (Fig. 19 (13)).

(iv) A second free unit can now be added above the first on the new chain because a new *B* can approach the group *ABA* (Fig. 19(7) and (8)) closely enough to enable a new permanent bond *CA* to be formed (Fig. 19(14)). Simultaneously, a second

BA link is made at the upper end of the new unit, and the first *BA* link is released (Fig. 19(8)). At this stage five units are held together by three bonds and one cross-link. With a longer chain the process described here would be repeated many times, always leaving one cross-link until the last unit was added.

(v) The third and last new unit has now been added in the only possible position for close contact, aligning with the top unit of the chain. As before, a fresh *CA* bond is formed. Simultaneously the second temporary *BA* link is released. The two chains are now gently mutually repelled by the released *B* levers (Fig. 19 (8) and (9)).

(vi) Now that the two chains have separated, they are ready to start replicating again.

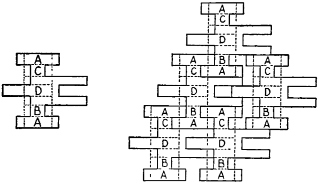

Fig. 18. Bases of units for self-replicating chains. The alignment of the group of interlocking bases of the units shown in Fig. 17(iii) is illustrated.

kind of buffer which prevents more than two aligned units from being in close apposition on the same row at the same time, as shown in Fig. 20. Without this safeguard, a new third chain might begin to be formed, from the second, before the second chain had been completely formed from the first; this is a way in which inaccurate short chains might be produced.

It is supposed that the free units are propelled randomly along the plane surface and that they collide from time to time. The chance of producing a chain merely by random movements is very small because linkage does not occur unless a staple (*A*) is held at the back of a lever (*B* or *C*) to activate it at the same time

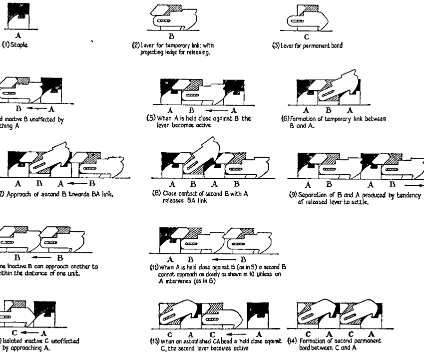

Fig. 19. Scale drawings of staples, levers, ledges and pivots used in the construction of units for self-replicating chains. The mechanisms which allow or prevent linking or release are demonstrated.

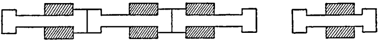

Fig. 20. The buffer, *D*, attached at the centre of each unit. This device prevents close contact of more than two units which are aligned with one another.

as a second staple (*A*) is brought in front of it. The mechanism is demonstrated in Fig. 19 both for the releasable or temporary cross link (*BA*) and for the permanent type of bond (*CA*). Molecular units constructed in this way are mechanically very simple and they do in fact demonstrate how a new daughter chain can be built from a parent chain of the same length. In Fig. 17 the length is three units – chosen for convenience of demonstration.

Unfortunately this machine still has theoretical defects but they are instructive. It is logically necessary that, in building a chain, when each new molecule, after the first, is added, the release of the cross-link should not precede the locking of the permanent bond made during the action. In practice this does not constitute a difficulty because the momentum of the incoming unit has the desired effect in preventing a short chain from being formed by releasing a cross-link too soon.

These considerations about timing may not be obviously important where chemical actions are concerned because we may like to assume simultaneity of junction and release. However, if links are produced by condensation and broken by hydrolysis, it is possible to imagine that, in certain restricted circumstances, when the amount of H_2O is limited, hydrolysis can be delayed until condensation, in forming a bond, has provided an extra molecule of water.

A second obvious defect of the machine is that much power is needed to push the units towards each other, to raise the levers, and, in particular, to release the cross-links. To resemble living matter a machine should gently pick up molecules highly charged with energy. It would then explode or burn them, using the energy to make and break the bonds needed for building the living structure. With suitably charged units, very little force would be required in collision, indeed only enough to trigger the discharge of energy. It has been possible to design and make such elements with springs or rubber bands for storing energy. They are charged before they are placed within range of the self-replicating object and, when it touches them, if there is perfect alignment, the energy is discharged and latching or releasing is efficiently performed.

6. *Application of the theory to the chemistry of living substances*

The first conclusion which seems to follow from the considerations brought forward in this paper is that self-reproduction is a matter of degree. Moreover, it can occur, theoretically, in many systems other than those depending upon DNA. These questions have often been discussed but usually without going into the problem of what actually constitutes self-reproduction. It is generally held that RNA in the cell does not reproduce itself and that it is only built in response to the presence of DNA which is the master substance. There is some difference of opinion about the capacity of virus RNA to replicate. It seems to have less facility than DNA in this matter, as though it had lost some instructions as to how to carry out the process. It requires at least some DNA to be present in order to perform self-replication. Incidentally, this consideration brings out the important point that, if a substance has the power to reproduce itself, it follows that it must also be capable of constructing other things (like shorter chains) less specific than itself, that is to say, with less information.

We may very reasonably ask what is the peculiar property of DNA which makes its R value so high as compared with other substances? The virtue must lie in the design of the molecular units themselves which form the DNA chain, namely, the nucleotides. Experiments on automatic mechanical self-reproduction have shown that, to attain a high R value, a minimal degree of complexity is required in the units. First, they must have good alignment properties. In nucleotides these are provided by hydrogen-bonding propensities. There must be a mechanism for strong permanent bonding down the chain; in nucleotides this is accomplished with the phosphate group by a process of condensation. More obscure is the nature of the cross-linkage which appears to be necessary to ensure stability of the structure during the building of each new chain. It can be provisionally supposed that the cross-links are formed by phosphate groups, probably with triple phosphates, since these are needed in experimental building of DNA chains. The initially charged units have to be nucleoside triphosphates, that is, nucleotides with two extra phosphate groups attached. In all the schemes suggested by the present writer, the nucleotide units are paired like with like during replication. However, a more elaborate system with complementary pairing could be associated with replication using temporary cross-linking.

The particular point, to which the reader's attention is drawn here, is that a nucleotide unit seems to have just about the right degree of complexity to form a theoretically accurate and efficient self-reproducing single-chain structure. Experiments with a DNA primer, in the presence of charged nucleotides and the nucleotide phosphorylase, by Kornberg[6] have demonstrated high *R* values in the system but not yet led to their measurement. However, the simpler question as to whether or not chains built from single nucleotides, like polyadenylic acid, are self-reproducing, in the sense described in this paper, does not seem to have been considered experimentally.

In most discussions on the origin of life it is assumed without question that the only sort of life which could exist anywhere must be based upon nucleotides, amino acids and other organic molecules. However, there have always been a few people who envisaged other possibilities.[1,2] The compounds of silicon might in some circumstances be capable of providing a substitute for compounds of carbon, sulphur for oxygen and vanadium for phosphorus in the construction of complex molecules with self-reproducing properties. The *R* values might be low in such systems and, in these circumstances, there would be little hope of evolution, a development which depends upon accurate inheritance of favourable structural combinations which have first arisen by chance errors. However, when the exploration of extra-terrestrial bodies becomes feasible, no doubt this question will be satisfactorily answered by direct observation.

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13. If an n-unit chain, $C_1, C_2, \ldots, C_n$, forms another one, $D_1, D_2 \ldots$, D_n, then the method used by the author in the model described in § 5, for achieving the 'temporary attachment', is that B_r is attached to A_{r+1} until B_{r+1} is formed ($r = 1, 2, \ldots, n-1$), 'pawn-capture attachment' so to speak. Since A_{n+1} does not exist, the new chain, when completed, separates from the original one. The spiral shape of a DNA chain facilitates pawn-capture attachment. *G. Ed.*

92 Senility as a Result of Increased Cell Effectiveness

by C. H. WADDINGTON

In early embryonic stages it is common to find a considerable number of necrotic and dead cells. It seems possible that at these stages the cells are not yet capable of restoring themselves when they, for some reasons or by purely chance fluctuations, deviate more than a small way from normality. In older stages, the cells may have a great capacity to maintain themselves in a state able to function and remain alive, by processes comparable to regeneration. But the restoration to normality may not be complete, so that the body eventually comes to contain a large number of sub-normal cells, whose presence gives rise to the phenomena of senility. If the restorative capacity had been lower, the damaged cells would have died and disappeared.

93 Hybrid Vigour as a Result of Inefficiency

by C. H. WADDINGTON

The greater part of the more advanced agriculture of the world is based on the exploitation of the hybrid vigour or heterosis which results from crossing slightly different races of animals and plants. Geneticists, who have for many years attempted, with rather little success, to provide an explanation of heterosis, have usually adopted the anthropocentric view that it is a *good thing*, and a sign of efficiency in the plant or animal concerned. I wish to suggest that it may be worthwhile considering the opposite point of view. A living organism takes in certain materials from its surroundings; some of these it processes to carry out its essential functions of living, others it builds into the processing plant, i.e. its body. From the long-range point of view of evolution, the essential function of living is the production of offspring; increased rate of body growth, or final body size, can be regarded as a greater capital investment to produce this return. A totally efficient living being would dispense with the soma altogether. The greater the 'vigour' from the anthropocentric point of view, the less the biological efficiency, measured in terms of 'number of offspring per unit weight of food'.

It is easy to envisage reasons why heterozygosity should lead to inefficiency. A metabolising organism may be compared to a set of multiple-linked trains of gears. If each gear wheel consisted of two sagittal halves, which, when produced by different alleles, differed slightly in radius, it is clear that every heterozygous locus would produce some degree of play or looseness in the mechanism; and with an increase in the number of heterozygous loci, the overall efficiency would decline at something over a linear rate.

In higher organisms, a considerable period of growth occurs before reproduction takes place. Metabolic inefficiency, leading to an unduly high rate of capital investment in the soma, would by the time of reproduction produce an unduly large body. A secondary, and paradoxical, consequence of this might be that more offspring are produced per individual parent. Thus, hybridity may result not only in what has been called 'luxuriance' (increased growth rate and final body size) but also increased reproductive effeetiveness of each of the units (i.e. individual organisms) submitted to natural selection. In that case some degree of biologieal inefficiency (measured in terms of offspring per food-unit) will have positive selective value.

94 Analogies of Language to Life

by H. KALMUS

Symmetry

A necessary condition for successful communication is a common language or code between the sender and the receiver of the message. The greater the detailed symmetry between the reference systems of the sender and receiver, the more easily can the receiver decode the sender's message and the better is his understanding of that message.

Ignorance of the relevant codes is the greatest single obstacle to the attempt of scientists to understand biological systems of communication, especially nervous and genetical systems.

Arbitrariness

Although some symbols of human languages are imitative or descriptive, most of them are highly arbitrary, practically all words having little recognisable relation between object and symbol. Similarly, one must assume that many of the code elements, operating in behaviour and genetics, are arbitrary.

Meaning

Human language depends on a limited number of individually recognisable symbols, organised at various levels (phonemes in speech or letters in writing, syllables, words, sentences). In biological systems, the corresponding levels and their individual units are not known fully; because of this inability to dissect the messages exactly into their proper pieces, it is much harder to decode and interpret them. The biological decoding mechanisms have not yet been discovered.

In the genetical field a somewhat more complex situation may be described, which however is currently held to be fundamental.

It is widely assumed that only two pairs of bases – adenine with thymine and guanine with cytosine – occur in the long desoxyribonucleic acid chains[1] which carry the genetical information in the chromosomes. 'Genes' must thus correspond to finite sequences of these two base pairs. We have little idea which of these Morse-like sequences correspond to genes, how long they are and even whether they are separated into separate letters – corresponding perhaps to the sequence in which twenty odd amino-acids are incorporated into speeific proteins which the genes seem to control; also it is obvious that different parts of these chains must contain longer or shorter identical sequences.[6] For this reason alone, the cracking of any genetical code is a very formidable task. Another reason is that our most powerful tool for the study of genetics, namely the mutations, most closely resemble misprints or copying errors. It would clearly be very difficult to learn a language by concentrating on such lapses!

Transcodability

A common feature of linguistic and biological information is the ease, with which it can be repeatedly transcoded resulting in a multiplicity of codes, such as print, handwriting, shorthand, Braille, and, that powerful obstacle to progress, ideographic script.

We shall only mention two biological situations which involve transcoding. The first concerns the transposition of directional vectors from a visual to a gravitational field, as shown by many animals and in particular by bees.[2] Repeated transcoding also plays an important part in many phases of development. The replication of genes and chromosomes, whether or not it involves the use of templates,[1,3] requires some sort of transposition or equivalence; and so does the chain of specific information which may pass from DNA to RNA, on to amino-acids and proteins – and if the latter are enzymes – to the control of metabolic and, finally, morphological characters. The main difficulty in un-ravelling any of these exceedingly complex situations is that we usually do not know the kind or even the number of transcodings, nor their precise rules, but are by the diversity of our methods compelled to piece together fragments of information which may be only very indirectly connected. I therefore think that a broad consideration of linguistic practice and its application to genetical problems might at this stage usefully supplement the search for chemical minutiae in a very hypothetical framework. In such a task, some discrimination is of course a prerequisite. Language may be considered as consisting of symbols (words) arranged according to certain syntactic rules, and it can become nonsensical if words are either misspelled (mutation), missing (deletion) or wrongly combined (unbalanced phenotypes). Up to a point linguistic sense may then be compared to biological viability.

Language and life both contain conservative elements (the genes perhaps corresponding to printed words), both are variable and subject to selection, both can also be unstable and thus capable of innovation and evolution.

Biological Style

Literary style may be compared with biological type. In any particular human language, there is only a limited number of acceptable sub-languages, such as dialects, jargons, and poetic language; mixtures of these language variants appear incongruous. Similarly, in the course of the evolution of life, only a limited number of types have developed, to which names like 'insects', 'birds', 'elephants' have been given. Often, similar types arise several times but do not necessarily have closely related ancestries. In the organic world, certain characters go together, while other characters are only exceptionally compatible. Trivial examples are the great number of male and female characters in many species. Very general arrangements exist, some of which are entirely unexpected, which ensure the joint development of those bunches of characters which are called types. A striking example of this is the strong positive correlation between the appearance of ocelli and the possession of wings among insects.[4]

Miscellaneous Analogies

There are probably many more aspects that languages and biological systems have in common; biologists for instance may try to find more biological examples analogous to linguistic redundancy ('double safeguards'), or for certain quantitative entities of information theory.[7] Linguists on their part may look for examples of feed-back and other regulative mechanisms in language.

Differences between Biological Communication and Language

What are the chief differences between biological communication and language?

First, language in its widest sense is transmitted *between* the bodies of different people or animals, whereas nervous and genetic information is conveyed *inside* an organism's body, by integral parts of it. These differences of materials and tools for handling information must put different constraints on the types of communications that they carry, even if they serve similar purposes.

Secondly, while a language may have strong direct effects on other fairly different languages, genetical material is rarely incorporated directly into one higher species from a very different higher species.

The purpose of most linguistic communications in the context of human life is fairly clear. We have even invented some two hundred artificial languages, some of which are quite serviceable and have survived for some time. But we have not yet made any form of artificial life – though a beginning has been made with deoxyribonucleic acid chains[5] – and we can only guess at the purpose and role of many organic structures and functions. And for this reason alone our analogies may retain their heuristic value for some time to come.

The following table summarises some of the discussed possibilities and makes them perhaps somewhat clearer.

Level	*Sender*	*Message*	*Vehicle*	*Receiver*
1. Nucleus to cytoplasm	DNA	amino-acid arrangement in protein	Messenger RNA?	Unknown
2. Cell to cell in some organism	Secreting cell	rate order	hormone	hormone-sensitive cell
3. Generation to generation	parent gamete	protein structure instruction	DNA	offspring
4. Organism to organism	individual	operational command	signs, language, etc.	individual
5. Organism to selective forces	organism	gene combination	organisms during lifetime	environment

Summary

Biological and linguistic communication systems both show properties of symmetry, meaning, arbitrariness, transcodability, and style. There are also some important differences between them. In any analogy that may be drawn between them, the biological level at which information is transmitted must be considered.

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95 Pseudo-muscles

by N. W. PIRIE

On many machines periodical linear movements are needed at widely separated parts. These are generally given by an electric motor and screw or by a hydraulic cylinder. It may well be that such arrangements are reasonably satisfactory and that there is no need to look for any other method of getting a push or pull, but, if they have defects, the possibilities of using an arrangement on the general plan of a muscle are worth investigating. Here an array of protein fibres changes configuration as a result of changes in adenosine triphosphate in their neighbourhood, and there is a mechanism, also within the fibre, for restoring the adenosine triphosphate using energy obtained by oxidising glucose. This is a highly telescoped version of what happens, but it serves to illustrate the point. An artificial muscle could either get its energy from electricity fed into it in such a way as to establish an immense number of dipoles along a polymer thread with regularly-spaced charged groups along it, or the muscular method could be copied more exactly so that a polymer with enzyme-like properties, in a bag containing the substrate, forms an enzyme substrate complex only when given an electrical impulse. Contraction and relaxation would destroy the substrate so that after some predictable number of actions the bag would have to be refilled. (Compare pbi 40. One application of artificial muscles would be for the construction of androids, preferably female. *G.Ed.*)

96 Germanium as a Carbon Analogue

by N. W. PIRIE

Some seams of coal, the Hartley seam in Northumberland for example, are rich in germanium. It is generally assumed that this is a consequence of secondary absorption of Ge leached from rocks in the neighbourhood, but it is not easy to see why it should then be so narrowly confined to certain seams. An attractive alternative explanation is that some of the plants that gave rise to these particular seams used Ge in their metabolism. Some present-day plants have unusual abilities to concentrate and use elements such as fluorine and selenium that are little used by other species. Selenium is especially relevant because it appears to replace sulphur; Ge could be an analogue for carbon. If this idea is well founded it should be possible to find, or make, organisms that can use Ge. This would be both interesting and useful. The study should be pursued both in the past and present. In the past by looking for Ge in a wide range of sediments to see whether it is concentrated in bands other than coal; it would be specially interesting if it, or any other trace element for that matter, is ever concentrated in a rock in fossil-sized regions. The analogy here is with the Archean carbon agglomerations, thought by some to be the remains of the organism *Corycium enigmaticum*. A wide range of present-day plant and microbial species should be kept in environments fortified with Ge to see which will tolerate, concentrate, or even use Ge. And the study should be extended to varieties and single individuals because individual tunicates differ in their use of vanadium and niobium*. Such individuality may be common – it is seldom looked for even in man.

97 Taste Differences

by FRED L. WHIPPLE

A chemist, Dr A. L. Fox[1], discovered, much to his surprise, a substance that some people can taste as very bitter but that other people cannot taste at all. The original substance is best called PTC, since its real name is phenyl-thi-carbamide, but a number of similar substances are now known. The ability, or lack of ability, to taste these substances seems to be an inherited characteristic, not only in man but among chimpanzees. 'Tasters' are quite vehement about the bitterness of PTC and run for a glass of water to wash out the bitterness. As a 'non-taster' I detect no flavour at all.

Whereas it is fairly easy to compare individuals with regard to visual sensory receptions, the impressions they receive by taste or smell remain private knowledge. The experience with PTC shows that a great many of the food preferences and flavour intolerances that people express are not primarily the result of conditioning in childhood by good or bad associations, but actually represent inherent differences in the taste sensitivity of a physiological nature.

* CARLISLE, D. B.: 'Niobium in Ascidians': *Nature*, volume 181, page 933, 1958.

Very limited testing on my part indicates that some tasters enjoy certain cooked foods such as cabbage, brussels sprouts, cauliflower, turnips and parsnips, while non-tasters such as myself tend to dislike these and other foods of similar character. My tentative working hypothesis is that non-tasters, being unable to detect the 'interesting' flavours of these foods, are left largely with the sulphide flavour. On the other hand, for the taster, the 'interesting' flavour overwhelms the sulphide flavour.

There may be other large variations and numerous groups of flavours that are appreciated by some individuals and disliked by others because of physiological differences. I believe that a relatively easy experiment could provide a great deal more knowledge about basic taste differences among members of *homo sapiens*. The experiment could be conducted with considerable reliability in high schools and very early college classes. Each individual in a class could be tested for ability to taste PTC, and also any other group of substances for which a comparable difference in tasting ability may prove to exist, and a questionnaire on food preferences could be filled out. The individual would simply check whether he liked, disliked or was neutral about each of a large number of foods currently available in an average home. Such statistical data might be extremely valuable for physiological information. Naturally the experiments could be expanded greatly to determine genealogical properties that may be involved in these taste preferences.

On the practical or commercial level, one can conceive that, if most individuals knew their taste class, the producers of processed foods could label their foods accordingly. The consequences could be of benefit both to the food processers and to the consumer, in that the consumer would have a much smaller chance of being disappointed with a new product if he knew whether or not it conformed to his taste type, while the producer could specialise on extreme types of food which would be disliked violently by some people but appreciated enthusiastically by others. Thus, the culinary world could develop more sparkle and variety without disconcerting so many individuals.

A modern Escoffier could modify his dishes or add variety for Class A, B, etc. palates and gain a mastery in his field far above that of Escoffier, himself, who had to pamper the mean palate. Perhaps we could even attain the ideal in which large banquets could consist of something else than fruit cup, chicken, peas, mashed potatoes and ice-cream.

[*Editorial comment.* The science of the tastes, and therefore also the odours, of foods and drinks, deserves a name, such as 'saporology', in order that endowments should be encouraged.[2] The name 'organoleptics' is often used, but this has a much wider meaning. Also derivative words such as 'saporologist' are more pleasant than 'organolepticist', which sounds as if it has something to do with circumcision.

Since this contribution was received, a relevant letter has appeared in *Nature.*[3] *G. Ed.*]

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7

Physics

98 Remarks on the Mind-Body Question

by EUGENE P. WIGNER

1. *Introductory Comments*

F. Dyson, in a very thoughtful article,[1] points to the ever-broadening scope of scientific inquiry. Whether or not the relation of mind to body will enter the realm of scientific inquiry in the near future – and the present writer is prepared to admit that this is an open question – it seems worth while to summarise the views to which a dispassionate contemplation of the most obvious facts leads. The present writer has no other qualification to offer his views than has any other physicist and he believes that most of his colleagues would present similar opinions on the subject, if pressed.

Until not many years ago, the 'existence' of a mind or soul would have been passionately denied by most physical scientists. The brilliant successes of mechanistic and, more generally, macroscopic physics and of chemistry, outshone the obvious and it was nearly universally accepted among physical scientists that there is nothing besides matter. The epitome of this belief was the conviction that, if we knew the positions and velocities of all atoms at one instant of time, we could compute the fate of the universe for all future. Even today, there are adherents to this view[2] though fewer among the physicists than – ironically enough – among biochemists.

There are several reasons for the return, on the part of most physical scientists, to the spirit of Descartes's '*Cogito ergo sum*' which recognises the thought, that is the mind, as primary. First, the brilliant successes of mechanics not only faded into the past; they were also recognised as partial successes, relating to a narrow range of phenomena, all in the macroscopic domain. When the province of physical theory was extended to encompass microscopic phenomena, through the creation of quantum mechanics, the concept of consciousness came to the fore again: it was not possible to formulate the laws of quantum mechanics in a fully consistent way without reference to the consciousness.[3] All that quantum mechanics purports to provide are probability connexions between subsequent impressions (also called 'apperceptions') of the consciousness, and even though the dividing line between the observer, whose consciousness is being affected, and the observed physical object can be shifted towards the one or the other to a considerable degree,[4] it cannot be eliminated. It may be premature to believe that the present philosophy of quantum mechanics will remain a permanent feature of future physical theories; it will remain remarkable, in whatever way our future concepts may develop, that the very study of the external world led to the conclusion that the content of the consciousness is an ultimate reality.

It is perhaps important to point out at this juncture that the question concerning the existence of almost anything (even the whole external world) is not a very relevant question. All of us recognise at once how meaningless the query concerning the existence of the electric field in vacuum would be. All that is relevant is that the concept of the electric field is useful for communicating our ideas and for our own thinking. The statement that it 'exists' means only that: (*a*) it can be measured, hence uniquely defined, and (*b*) that its knowledge is useful for understanding past phenomena and in helping to foresee further events.

It can be made part of the *Weltbild.* This observation may well be kept in mind during the ensuing discussion of the quantum mechanical description of the external world.

2. *The Language of Quantum Mechanics*

The present and the following section try to describe the concepts in terms of which quantum mechanics teaches us to store and communicate information, to describe the regularities found in nature. These concepts may be called the language of quantum mechanics. We shall not be interested in the regularities themselves, that is, the contents of the book of quantum mechanics, only in the language. It may be that the following description of the language will prove too brief and too abstract for those who are unfamiliar with the subject, and too tedious for those who are familiar with it.[5] It should, nevertheless, be helpful. However, the knowledge of the present and of the succeeding section is not necessary for following the later ones, except for parts of section 5.

Given any object, all the possible knowledge concerning that object can be given as its wave function. This is a mathematical concept the exact nature of which need not concern us here – it is composed of a (countable) infinity of numbers. If one knows these numbers, one can foresee the behaviour of the object as far as it *can* be foreseen. More precisely, the wave function permits one to foretell with what probabilities the object will make one or another impression on us if we let it interact with us either directly, or indirectly. The object may be a radiation field, and its wave function will tell us with what probability we shall see a flash if we put our eyes at certain points, with what probability it will leave a dark spot on a photographic plate if this is placed at certain positions. In many cases the probability for one definite sensation will be so high that it amounts to a certainty – this is always so if classical mechanics provides a close enough approximation to the quantum laws.

The information given by the wave function is communicable. If someone else somehow determines the wave function of a system, he can tell me about it and, according to the theory, the probabilities for the possible different impressions (or 'sensations') will be equally large, no matter whether he or I interact with the system in a given fashion. In this sense, the wave function 'exists'.

It has been mentioned before that even the complete knowledge of the wave function does not permit one always to foresee with certainty the sensations one may receive by interacting with a system. In some cases, one event (seeing a flash) is just as likely as another (not seeing a flash). However, in most cases the impression (e.g. the knowledge of having or not having seen a flash) obtained in this way permits one to foresee later impressions with an increased certainty. Thus, one may be sure that, if one does not see a flash if one looks in one direction, one surely does see a flash if one subsequently looks in another direction. The property of observations to increase our ability for foreseeing the future follows from the fact that all knowledge of wave functions is based, in the last analysis, on the 'impressions' we receive. In fact, the wave function is only a suitable language for describing the body of knowledge – gained by observations – which is relevant for predicting the future behaviour of the system. For this reason, the interactions which may create one or another sensation in us are also called observations, or measurements. One realises that *all* the information which the laws of physics provide consists of probability connexions between subsequent impressions that a system makes on one if one interacts with it repeatedly, i.e. if one makes repeated measurements on it. The wave function is a convenient summary of that part of the past impressions which remain relevant for the probabilities of receiving the different possible impressions when interacting with the system at later times.

3. *An Example*

It may be worth while to illustrate the point of the preceding section on a schematic example. Suppose that all our interactions with the system consist in looking at a certain point in a certain direction at times t_0, t_0+1, t_0+2 . . ., and our possible sensations are seeing or not seeing a flash. The relevant law of nature could then be of the form: 'If you see a flash at time t, you will see a flash at time $t+1$ with a probability $\frac{1}{4}$, no flash with a probability $\frac{3}{4}$; if you see no flash, then the next observation will give a flash with the probability $\frac{3}{4}$, no flash with a probability $\frac{1}{4}$; there are no further probability connexions.' Clearly, this law can be verified or refuted with arbitrary accuracy by a sufficiently long series of observations. The wave function in such a case depends only on the last observation and may be ψ_1 if a flash has been seen at the last interaction, ψ_2 if no flash was noted. In the former case, that is for ψ_1, a calculation of the probabilities of flash and no flash after unit time interval gives the values $\frac{1}{4}$ and $\frac{3}{4}$; for ψ_2 these probabilities must turn out to be $\frac{3}{4}$ and $\frac{1}{4}$. This agreement of the predictions of the law in quotation marks with the law obtained through the use of the wave function is not surprising. One can either say that the wave function was invented to yield the proper probabilities, or that the law given in quotation marks has been obtained by having carried out a calculation with the wave functions, the use of which we have learned from Schrödinger.

The communicability of the information means, in the present example, that if someone else looks at time t, and tells us whether he saw a flash, we can look at time $t+1$ and observe a flash with the same probabilities as if we had seen or not seen the flash at time t ourselves. In other words, he can tell us what the wave function is: ψ_1 if he did, ψ_2 if he did not see a flash.

The preceding example is a very simple one. In general, there are many types of interactions into which one can enter with the system, leading to different types of observations or measurements. Also, the probabilities of the various possible impressions gained at the next interaction may depend not only on the last, but on the results of many prior observations. The important point is that the impression which one gains at an interaction may, and in general does, modify the probabilities with which one gains the various possible impressions at later interactions. In other words, the impression which one gains at an interaction, called also *the result of an observation*, modifies the wave function of the system. The modified wave function is, furthermore, in general unpredictable before the impression gained at the interaction has entered our consciousness: it is the entering of an impression into our consciousness which alters the wave function because it modifies our appraisal of the probabilities for different impressions which we expect to receive in the future. It is at this point that the consciousness enters the theory unavoidably and unalterably. If one speaks in terms of the wave function, its changes are coupled with the entering of impressions into our consciousness. If one formulates the laws of quantum mechanics in terms of probabilities of impressions, these are *ipso facto* the primary concepts with which one deals.

It is natural to inquire about the situation if one does not make the observation oneself but lets someone else carry it out. What is the wave function if my friend looked at the place where the flash might show at time t? The answer is that the information available about the *object* cannot be described by a wave function. One could attribute a wave function to the joint system: friend plus object, and this joint system would have a wave function also after the interaction, that is after my friend has looked. I can then enter into interaction with this joint system by asking my friend whether he saw a flash? If his answer gives me the impression that he did, the joint wave function of friend + object will change into one in which they even have separate wave functions (the total wave function is a product) and the wave function of the object is ψ_1. If he says no, the wave function of the object is ψ_2, i.e. the object behaves from then on as if I had observed it and had seen no flash. However, even in this case, in which the observation was carried out by someone else, the typical change in the wave function occurred only when some information (the *yes* or *no* of my friend) entered *my* consciousness. It follows that the quantum description of objects is influenced by impressions entering my consciousness.[6] Solipsism may be logically consistent with present quantum mechanics, monism in the sense of materialism is not. The case against solipsism was given at the end of the first section.

4. *The Reasons for Materialism*

The principal argument against materialism is not that illustrated in the last two sections: that it is incompatible with quantum theory. The principal argument is that thought processes and consciousness are the primary concepts, that our knowledge of the external world is the content of our consciousness and that the consciousness, therefore, cannot be denied. On the contrary, logically, the external world could be denied – though it is not very practical to do so. In the words of Niels Bohr[7], 'The word consciousness, applied to ourselves as well as to others, is indispensable when dealing with the human situation.' In view of all this, one may well wonder how materialism, the doctrine[8] that 'life could be explained by sophisticated combinations of physical and chemical laws' could so long be accepted by the majority of scientists.

The reason is probably that it is an emotional necessity to exalt the problem to which one wants to devote a lifetime. If one admitted anything like the statement that the laws we study in physics and chemistry are limiting laws, similar to the laws of mechanics which exclude the consideration of electric phenomena, or the laws of macroscopic physics which exclude the consideration of 'atoms', we could not devote ourselves to our study as wholeheartedly as we have to in order to recognise any new regularity in nature. The regularity which we are trying to track down must appear as the all-important regularity – if we are to pursue it with sufficient devotion to be successful. Atoms were also considered to be an unnecessary figment before macroscopic physics was essentially complete – and one can well imagine a master, even a great master, of mechanics to say: 'Light may exist but I do not need it in order to explain the phenomena in which I am interested.' The present biologist uses the same words about mind and consciousness; he uses them as an expression of his disbelief in these concepts. Philosophers do not need these illusions and show much more clarity on the subject. The same is true of most truly great natural scientists, at least in their years of maturity. It is now true of almost all physicists – possibly, but not surely, because of the lesson we learned from quantum mechanics. It is also possible that we learned that the principal problem is no longer the fight with the adversities of nature but the difficulty of understanding ourselves if we want to survive.

5. *Simplest Answer to the Mind-Body Question*

Let us first specify the question which is outside the province of physics and chemistry but is an obviously meaningful (because operationally defined) question: Given the most complete description of my body (admitting that the concepts used in this description change as physics develops), what are my sensations? Or, perhaps, with what probability will I have one of the several possible sensations? This is clearly a valid and important question which refers to a concept – sensations – which does not exist in present-day physics or chemistry. Whether the question will eventually become a problem of physics or psychology, or another science, will depend on the development of these disciplines.

Naturally, I have direct knowledge only of my own sensations and there is no strict logical reason to believe that others have similar experiences. However, everybody believes that the phenomenon of sensations is widely shared by organisms which we consider to be living. It is very likely that, if certain physico-chemical conditions are satisfied, a consciousness, that is the property of having sensations, arises. This statement will be referred to as our first thesis. The sensations will be simple and undifferentiated if the physico-chemical substrate is simple; it will have the miraculous variety and colour which the poets try to describe if the substrate is as complex and well organised as a human body.

The physico-chemical conditions and properties of the substrate not only create the consciousness, they also influence its sensations most profoundly. Does, conversely, the consciousness influence the physico-chemical conditions? In other words, does the human body deviate from the laws of physics, as gleaned from the study of inanimate nature? The traditional answer to this question is, 'No': the body influences the mind but the mind does not influence the body.[9] Yet at least two reasons can be given to support the opposite thesis, which will be referred to as the second thesis.

The first and, to this writer, less cogent reason is founded on the quantum theory of measurements, described earlier in sections 2 and 3. In order to present this argument, it is necessary to follow my description of the observation of a 'friend' in somewhat more detail than was done in section 3. Let us assume again that the object has only two states, ψ_1 and ψ_2. If the state is, originally, ψ_1, the state of object plus observer will be, after the interaction, $\psi_1 \times \chi_1$; if the state of the object is ψ_2, the state of object plus observer will be $\psi_2 \times \chi_2$ after the interaction. The wave functions χ_1 and χ_2 give the state of the observer; in the first case he is in a state which responds to the question, 'Have you seen a flash?' with 'Yes'; in the second state, with 'No'. There is nothing absurd in this so far.

Let us consider now an initial state of the object which is a linear combination $\alpha\, \psi_1 + \beta\, \psi_2$ of the two states ψ_1 and ψ_2. It then *follows* from the linear nature of the quantum mechanical equations of motion that the state of object plus observer is, after the interaction, $\alpha(\psi_1 \times \chi_1) + \beta(\psi_2 \times \chi_2)$. If I now ask the observer whether he saw a flash, he will with a probability $|\alpha|^2$ say that he did, and in this case the object will also give to me the responses as if it were in the state ψ_1. If the observer answers, 'No' – the probability for this is $|\beta|^2$ – the object's responses from then on will correspond to a wave function ψ_2. The probability is zero that the observer will say 'Yes', but the object gives the response which ψ_2 would give because the wave function $\alpha(\psi_1 \times \chi_1 + \beta(\psi_2 \times \chi_2)$ of the joint system has no $(\psi_2 \times \chi_1)$ component. Similarly, if the observer denies having seen a flash, the behaviour of the object cannot correspond to χ_1 because the joint wave function has no $(\psi_1 \times \chi_2)$ component. All this is quite satisfactory: the theory of measurement, direct or indirect, is logically consistent so long as I maintain my privileged position as ultimate observer.

However, if after having completed the whole experiment I ask my friend, 'What did you feel about the flash before I asked you?' he will answer, 'I told you already, I did [did not] see a flash,' as the case may be. In other words, the question whether he did or did not see the flash was already decided in his mind, before I asked him.[10] If we accept this, we are driven to the conclusion that the proper wave function immediately after the interaction of friend and object was already either $\psi_1 \times \chi_1$ or $\psi_2 \times \chi_2$ and not the linear-combination $\alpha(\psi_1 \times \chi_1) + \beta(\psi_2 \times \chi_2)$. This is a contradiction because the state described by the wave function $\alpha(\psi_1 \times \chi_1) + \beta(\psi_2 \times \chi_2)$ describes a state that has properties which neither $\psi_1 \times \chi_1$, nor $\psi_2 \times \chi_2$ has. If we substitute for 'friend' some simple physical apparatus, such as an atom which may or may not be excited by the light-flash, this difference has observable effects and *there is no doubt that* $\alpha\ (\psi_1 \times \chi_1) + \beta(\psi_2 \times \chi_2)$ *describes the properties of the joint system correctly, the assumption that the wave function is either* $\psi_1 \times \chi_1$ or $\psi_2 \times \chi_2$ *does not.* If the atom is replaced by a conscious being the wave function $\alpha\,(\psi_1 \times \chi_1) + \beta\,(\psi_2 \times \chi_2)$ (which also follows from the linearity of the equations) appears absurd because it implies that my friend was in a state of suspended animation before he answered my question.[11]

It follows that the being with a consciousness must have a different role in quantum mechanics than the inanimate measuring device: the atom considered above. In particular, the quantum mechanical equations of motion cannot be linear if the preceding argument is accepted. This argument implies that 'my friend' has the same types of impressions and sensations as I – in particular, that, after interacting with the object, he is not in that state of suspended animation which corresponds to the wave function $\alpha (\psi_1 \times \chi_1) + \beta (\psi_2 \times \chi_2)$. It is not necessary to see a contradiction here from the point of view of orthodox quantum mechanics, and there is none if we believe that the alternative is meaningless, whether my friend's consciousness contains either the impression of having seen a flash or of not having seen a flash. However, to deny the existence of the consciousness of a friend to this extent is surely an unnatural attitude, approaching solipsism, and few people, in their hearts, will go along with it.

The preceding argument for the difference in the roles of inanimate observation tools and observers with a consciousness – hence for a violation of physical laws where consciousness plays a role – is entirely cogent so long as one accepts the tenets of orthodox quantum mechanics in all their consequences. Its weakness for proving a specific effect of the consciousness on matter lies in its total reliance on these tenets – a reliance which would be, on the basis of our experiences with the ephemeral nature of physical theories, difficult to justify fully.

The second argument to support the existence of an influence of the consciousness on the physical world is based on the observation that we do not know of any phenomenon in which one object is influenced by another without exerting an influence thereupon. This appears convincing to this writer. It is true that under the usual conditions of experimental physics or biology, the influence of any consciousness is certainly very small. 'We do not need the assumption that there is such an effect.' It is good to recall, however, that the same may be said of the relation of light to mechanical objects. Mechanical objects influence light –

otherwise we could not see them – but experiments to demonstrate the effect of light on the motion of mechanical bodies are difficult. It is unlikely that the effect would have been detected, had theoretical considerations not suggested its existence, and its manifestation in the phenomenon of light pressure.[16]

6. *More Difficult Questions*

Even if the two theses of the preceding section are accepted, very little is gained for science as we understand science: as a correlation of a body of phenomena. Actually, the two theses in question are more similar to existence theorems of mathematics than to methods of construction of solutions and we cannot help but feel somewhat helpless as we ask the much more difficult question: how could the two theses be verified experimentally? i.e. how a body of phenomena could be built around them. It seems that there is no solid guide to help in answering this question and one either has to admit to full ignorance or to engage in speculations.

Before turning to the question of the preceding paragraph, let us note in which way the consciousnesses are related to each other and to the physical world. The relations in question again show a remarkable similarity to the relation of light quanta to each other and to the material bodies with which mechanics deals. Light quanta do not influence each other directly[12] but only by influencing material bodies which then influence other light quanta. Even in this indirect way, their interaction is appreciable only under exceptional circumstances. Similarly, consciousnesses never seem to interact with each other directly but only via the physical world. Hence, any knowledge about the consciousness of another being must be mediated by the physical world.

At this point, however, the analogy stops. Light quanta can interact directly with virtually any material object but each consciousness is uniquely related to some physico-chemical structure through which alone it receives impressions. There is, apparently, a correlation between each consciousness, and the physico-chemical structure of which it is a captive, which has no analogue in the inanimate world. Evidently, there are enormous gradations between consciousnesses, depending on the elaborate or primitive nature of the structure on which they can lean: the sets of impressions which an ant or a microscopic animal or a plant receives surely show much less variety than the sets of impressions which man can receive. However, we can, at present, at best, guess at these impressions. Even our knowledge of the consciousness of other men is derived only through analogy and some innate knowledge which is hardly extended to other species.

It follows that there are only two avenues through which experimentation can proceed to obtain information about our first thesis: observation of infants where we may be able to sense the progress of the awakening of consciousness, and by discovering phenomena postulated by the second thesis, in which the consciousness modifies the usual laws of physics. The first type of observation is constantly carried out by millions of families, but perhaps with too little purposefulness. Only very crude observations of the second type have been undertaken in the past, and all these antedate modern experimental methods. So far as it is known, all of them have been unsuccessful. However, every phenomenon is unexpected and most unlikely until it has been discovered – and some of them remain unreasonable for a long time after they have been discovered. Hence, lack of success in the past need not discourage.[17]

7. *Non-linearity of Equations as Indication of Life*

The preceding section gave two proofs – they might better be called indications – for the second thesis, the effect of consciousness on physical phenomena. The first of these was directly connected with an actual process, the quantum mechanical observation, and indicated that the usual description of an indirect observation is probably incorrect if the primary observation is made by a being with consciousness. It may be worthwhile to show a way out of the difficulty which we encountered.

The simplest way out of the difficulty is to accept the conclusion which forced itself on us: to assume that the joint system of friend plus object cannot be described by a wave function after the interaction – the proper description of their state is a mixture.[13] The wave function is $(\psi_1 \times \chi_1)$ with a probability $|\alpha|^2$; it is $(\psi_2 \times \chi_2)$ with a probability $|\beta|^2$. It was pointed out already by Bohm[14] that, if the system is sufficiently complicated, it may be in practice impossible to ascertain a difference between certain mixtures, and some pure states (states which *can* be described by a wave function). In order to exhibit the difference, one would have to subject the system (friend plus object) to very complicated observations which cannot be carried out in practice. This is in contrast to the case in which the flash or the absence of a flash is registered by an atom, the state of which I can obtain precisely by much simpler observations. This way out of the difficulty amounts to the postulate that the equations of motion of quantum mechanics cease to be linear, in fact that they are grossly non-linear if conscious beings enter the picture.[15] We saw that the linearity condition led uniquely to the unacceptable wave function $\alpha(\psi_1 \times \chi_1) + \beta(\psi_2 \times \chi_2)$ for the joint state. Actually, in the present case, the final state is uncertain even in the sense that it cannot be described by a wave function. The statistical element which, according to the orthodox theory enters only if *I* make an observation enters equally if my friend does so.

It remains remarkable that there is a continuous transition from the state $\alpha(\psi_1 \times \chi_1) + \beta(\psi_2 \times \chi_2)$ to the mixture of $\psi_1 \times \chi_1$ and $\psi_2 \times \chi_2$, with probabilities $|\alpha|^2$ and $|\beta|^2$, so that every member of the continuous transition has all the statistical properties demanded by the theory of measurements. Each member of the transition, except that which corresponds to orthodox quantum mechanics, is a mixture, and must be described by a statistical matrix. The statistical matrix of the system: friend plus object is, after their having interacted ($|\alpha|^2 + |\beta|^2 = 1$)

$$\begin{Vmatrix} |\alpha|^2 & \alpha\beta^* \cos\delta \\ \alpha^*\beta \cos\delta & |\beta|^2 \end{Vmatrix}$$

in which the first row and column corresponds to the wave function $\psi_1 \times \chi_1$, the second to $\psi_1 \times \chi_1$. The $\delta = 0$ case corresponds to orthodox quantum mechanics; in this case the statistical matrix is singular and the state of friend-plus-object can be described by a wave function, namely $\alpha\, \psi_1 \times \chi_1 + \beta\, \psi_2 \times \chi_2$. For $\delta = \frac{1}{2}\pi$, we have the simple mixture of $\psi_1 \times \chi_1$ and $\psi_2 \times \chi_2$, with probabilities $|\alpha|^2$ and $|\beta|^2$, respectively. At intermediate δ, we also have mixtures of two states, with probabilities $\frac{1}{2} + (\frac{1}{4} - |\alpha\,\beta|^2 \sin^2\delta)^{\frac{1}{2}}$ and $\frac{1}{2} - (\frac{1}{4} - |\alpha\beta|^2 \sin^2\delta)^{\frac{1}{2}}$. The two states are $\alpha\,\psi_1 \times \chi_1 + \beta\,\psi_2 \times \chi_2$ and $-\beta^*\,\psi_1 \times \chi_1 + \alpha^*\,\psi_2 \times \chi_2$ for $\delta = 0$ and go over, continuously into $\psi_1 \times \chi_1$ and $\psi_2 \times \chi_2$ as δ increases to $\frac{1}{2}\pi$.

The present writer is well aware of the fact that he is not the first one to discuss the questions which form the subject of this article and that the surmises of his predecessors were either found to be wrong or unprovable, hence, in the long run, uninteresting. He would not be greatly surprised if the present article shared the fate of those of his predecessors. He feels, however, that many of the earlier speculations on the subject, even if they could not be justified, have stimulated and helped our thinking and emotions and have contributed to re-emphasise the ultimate scientific interest in the question which is, perhaps, the most fundamental question of all.

REFERENCES AND NOTES

1. DYSON, F. J.: *Scientific American*, page 74, Sept. 1958. Several cases are related in this article in which regions of inquiry, which were long considered to be outside the province of science, were

drawn into this province and, in fact, became focuses of attention. The best-known example is the interior of the atom, which was considered to be a metaphysical subject before Rutherford's proposal of his nuclear model, in 1911.

2. The book most commonly blamed for this view is E. F. Haeckel's *Welträtsel* (1899). However, the views propounded in this book are less extreme (though more confused) than those of the usual materialistic philosophy.
3. W. Heisenberg expressed this most poignantly (*Daedalus* 87.3, 95, page 99, 1958): 'The laws of nature which we formulate mathematically in quantum theory deal no longer with the particles themselves but with our knowledge of the elementary particles.' And later: 'The conception of objective reality . . . evaporated into the . . . mathematics that represents no longer the behaviour of elementary particles but rather our knowledge of this behaviour.' The 'our' in this sentence refers to the observer who plays a singular role in the epistemology of quantum mechanics. He will be referred to in the first person and statements made in the first person will always refer to the observer.
4. VON NEUMANN, J.: *Mathematische Grundlagen der Quantenmechanik:* Chapter VI, Julius Springer, Berlin, 1932; Eng. tr.: Princeton Univ. Press, Princeton, 1955.
5. The contents of this section should be part of the standard material in courses on quantum meehanics. They are given here because it may be helpful to recall them even on the part of those who were at one time already familiar with it, because it is not expected that every reader of these lines had the benefit of a course in quantum mechanies, and because the writer is well aware of the fact that most courses in quantum mechanics do not take up the subject here discussed. *See also*, in addition to references 3 and 4, PAULI, W.: *Handbuch der Physik:* Section 2.9, particularly page 148, Julius Springer, Berlin, 1933. Also LONDON, F. and BAUER, E.: *La Théorie de l'Observation en Mécanique Quantique*, Hermann and Co., Paris, 1939. The last authors observe (page 41), 'Remarquons le rôle essential que joue la conscience de l'observateur . . .'
6. The essential point is not that the description of objects, by means of position and momentum co-ordinates is, because of the uncertainty prineiple, impossible. The point is, rather, that the valid description, by means of the wave function, is influenced by impressions entering our consciousness. See in this connection

the remark of London and Bauer, quoted above, and S. Watanabe's article in *Louis de Broglie, Physicien et Penseur* Albin Michel, Paris, 1952, page 385.

7. BOHR, N.: *Atomic Physics and Human Knowledge:* Section on 'Atoms and Human Knowledge', in particular, page 92: John Wiley and Sons, New York, 1960.
8. The quotation is from BECK, WILLIAM S.: *The Riddle of Life, Essay in Adventures of the Mind:* page 35, Alfred A. Knopf, New York, 1960. This article is an eloquent statement of the attitude of the open-minded biologists toward the questions discussed in the present note.
9. This writer does not profess to a knowledge of all, or even of the majority of all, metaphysical theories. It may be significant, nevertheless, that he never found an affirmative answer to the query of the text – not even after having perused the relevant articles in the earlier (more thorough) editions of the *Encyclopaedia Britannica.*
10. LONDON, F. and BAUER, E. (op. cit. reference 5) on page 42 say, 'il [l'observateur] dispose d'une faculté caracteristique et bien familière, que nous pouvons appeler la "faculté d'introspection": il peut se rendre compte de manière immediate de son propre état.'
11. In an article which will appear soon (*Werner Heisenberg und die Physik unserer Zeit*, Friedr. Vieweg, Braunschweig, 1961) G. Ludwig discusses the theory of measurements and arrives at the conclusion that quantum mechanical theory cannot have unlimited validity (*see*, in particular, Section 111a, also Ve). This conclusion is in agreement with the point of view here represented. However, Ludwig believes that quantum mechanics is valid only in the limiting case of microscopic systems whereas the view here represented assumes it to be valid for all inanimate objects. At present, there is no clear evidence that quantum mechanics becomes increasingly inaccurate as the size of the system increases and the dividing line between microscopic and macroscopic systems is surely not very sharp. Thus, the human eye can perceive as few as three quanta and the properties of macroscopic crystals are grossly affected by a single disloeation. For these reasons, the present writer prefers the point of view represented in the text even though he does not wish to deny the possibility that Ludwig's more narrow limitation of quantum mechanics may be justified ultimately.

12. This statement is certainly true in an approximation which is much better than is necessary for our purposes.
13. The concept of the mixture was put forward first by LANDAU, L.: *Zeits. f. Physik*, volume 45, page 430, 1927. A more elaborate discussion is found in J. VON NEUMANN's book (reference 4) Chapter IV. A more concise and elementary discussion of the concept of mixture and its characterisation by a statistical (density) matrix is given in L. Landau and E. Lifshitz, *Quantum Mechanics* (Pergamon Press, London, 1958), pages 35–38.
14. The circumstance that the mixture of the states $(\psi_1 \times \chi_1)$ and $(\psi_2 \times \chi_2)$ with weights $|\alpha|^2$ and $|\beta|^2$ respectively, cannot be distinguished in practice from the state $\alpha(\psi_1 \times \chi_1) + \beta(\psi_2 \times \chi_2)$, if the states χ are of great complexity, has been pointed out already in Section 22.11 of D. BOHM's *Quantum Theory*, Prentice Hall, New York, 1951. The reader will also be interested in Sections 8.27, 8.28 of this treatise.
15. The non-linearity is of a different nature from that postulated by W. Heisenberg in his theory of elementary particles (cf. e.g. H. P. Dürr, W. Heisenberg, H. Mitter, S. Schlieder, K. Yamazaki, *Z. Naturf.*, volume 14a, page 441, 1954), in our case the equations giving the time variation of the state vector (wave function) are postulated to be non-linear.
16. An example, more familiar to the man in the airport, is the use of a photo-electric cell for controlling the opening of a door. In this example, light has large-scale mechanical effects via electrical effects. *G. Ed.*
17. The challenge is to construct the 'psycho-electric cell', to coin a term. *G.Ed.*

[Prof. Wigner's 'friend' in Section 3 can be regarded as a kind of recording apparatus. Let us imagine him replaced by a semi-transparent photographic plate for recording the flash, together with a photo-electric cell behind the plate which activates a camera which photographs a clock. The question whether a flash was 'observed' by the semi-transparent plate at a certain time can be decided by looking at the photograph of the clock. If we accept quantum mechanics for all inanimate matter we are driven close to the limited form of solipsism that was embraced by George Berkeley (1710), and if we accept it unreservedly we are driven close to true solipsism at which the author draws the line. Bohm (*Brit. J. Phil. Sc.*, Volume 12, page 114, 1961), and presumably Ludwig, draw the line at even the approximation to the limited solipsism of Berkeley. It is interesting to note that a consciousness can regard its own body as an inanimate system to which the laws of quantum mechanics might rigorously apply except when the consciousness affects it. *G.Ed.*]

99 A Proposed Topological Formulation of the Quantum Theory*

by DAVID BOHM

It is generally recognised that with the Theory of Relativity there began a new line of development of our concepts of space and time, the essential feature of which may be stated as follows:

There is no pre-existent absolute frame of space and time, but, rather, this frame has a meaning only in relationship to those physical processes by which space and time are actually defined and measured. In the special theory of relativity, it was shown, for example, that the appropriate frame of space and time depends on the speed of the measuring apparatus, while in the general theory, it was found that the frame is fundamentally related to gravitational fields.

Thus far, however, only the *metrical* properties of space and time have been considered as being related to physical conditions in the way described above (e.g. the 'length' or 'interval' between two events). In this paper, some reasons will be given in favour of the suggestion that the *topological* properties of space and time (e.g. before, after, between, contact, inside, outside, etc.) must also be considered in connexion with the physical processes that

* Some of these ideas are developed in more detail in a paper to be published in the *British Journal for The Philosophy of Science.*

are actually taking place. Hitherto, these properties have just been taken for granted without further discussion, but here we shall propose that such properties (which evidently involve relationships that are discrete and discontinuous) are linked with the discrete aspects of quantum mechanical processes in somewhat the same way that the metrical properties are linked to the relative velocity and to the gravitational field.*

In order to introduce the idea, we shall begin with a suggestion of G. N. Lewis† for explaining the ability of light to carry discrete quanta of energy over indefinitely long distances. Lewis noted that, because the four-dimensional interval between two events connected by a light ray is zero, these events can be regarded as as being in a 'virtual contact', such that one can *act physically* in the other (e.g. energy can be transferred directly from an atom of the emitter to one in the absorber). As a result, there is no need to suppose that the energy spreads out continuously through space as a wave, nor is it necessary to assume a localised particle, which carries the energy in a single discrete packet. By thus considering emitter and absorber to be in a kind of immediate contact, one can, as Lewis showed, go a considerable way toward understanding the puzzling 'wave-particle duality' in the properties of light.

We are still left, however, with a serious problem. For how can 'physical contact' of emitter and absorber be consistent with the assignment of different space-time co-ordinates to them? One can gain a further insight into this problem by considering a question which Einstein raised and which ultimately led him to the Theory of Relativity: 'What would happen if one could follow a light beam?' It is well known that as any system approaches the speed of light, the 'proper time' (which is measured by every process within such a system) becomes slower and slower in relation to that of a system of fixed velocity, and that the co-

* To the author's knowledge, the idea that there may be a fundamental relationship between quantum theory and topology was first suggested by WHYTE, L. L. *See* his *Critique of Physics*, London, 1931.

† LEWIS, G. N.: *Nature*, volume 117, pages 236–8, 1926.

moving observer will see the world suffering a Lorentz contraction in the direction of relative motion, in the ratio $\sqrt{1 - v^2/c^2}$. If these results are extrapolated to the speed of light, one finds that *in the proper frame of the co-moving observer*, no time at all would pass between emission and absorption of a light quantum and there would be no spatial separation between them. In this way we can justify Lewis's idea of contact between these two events, but in so doing, we show that it is only in the proper frame of the photon that this contact will be described as a coincidence of space-time co-ordinates.

Of course, frames moving at the speed of light have not hitherto generally been considered to be permissible. Nevertheless, it has been recognised that every process moving at less than the speed of light is most appropriately studied in its own proper frame. (Such a study shows for example that observers who travel in different paths in space and time, suffering different accelerations, will have aged differently when they meet.) It seems somewhat arbitrary not to apply the same considerations to processes moving at the speed of light itself. And if we do so, then, as we shall see, we can develop some interesting connexions between relativity and quantum theory.

It is clear first of all that if we allow proper frames at the speed of light, then not only are the metrical properties of space-time relative, but so also is the topological property of *incidence* or *contact* between two events. For example, the emission and absorption of a photon are, as we have seen, in contact in the proper frame of the photon, and separated in the laboratory frame. In order to understand how such contact and separation are possible together, we may say that, roughly speaking, each space-time frame furnishes a kind of projection of the total process under investigation, which correctly expresses certain relationships and distorts others. Thus, in three-dimensional space, the projection of an object (e.g. its shadow) on a generally curved surface will distort certain features of its shape and may even distort the question of its connectivity, so that an undivided object can have a divided projection and vice-versa. Similarly, in relativistic space-time, the co-ordinates as measured in the laboratory frame may be regarded as projections which distort more and more as the velocity is raised (recall the observers who have aged differently when they meet) and which distorts even the question of connectivity at the speed of light. In this way we see that the assumption of direct physical contact between emitter and absorber (which contact is most appropriately described in the proper frame of the photon) is in no way incompatible with their lack of contact in a laboratory frame, because the latter completely distorts the relationships of contact and separation for processes at the speed of light.

If we once accept the suggestions outlined above, then we are led to far-reaching changes in our basic conceptions of space and time; for the topological properties can no longer be taken for granted but must be brought explicitly into the theory. Now (as is implied by the Greek 'topos', which means 'place') topology is, in essence, the study of the order involved in the placing of one thing in relationship to another. It must be remembered, however, that what is relevant in the study of physical process is not only the order of places, but also the time order of events, which may appropriately be called 'chronology'. Therefore, the theory that must be developed is really 'topo-chronology', which is *the study of order and relationship in process;* or in other words, the study how one event or moment *acts physically** in another. Such a branch of mathematics cannot be said to exist in a proper sense as yet, although, as we shall see, its rudiments and basic outlines are already visible in various forms of mathematics that have thus far been developed.

A special case of a topo-chronological relationship was seen in the discussion of the light ray; for in the proper frame of this ray, the emission and absorption are not separated, so that one can act in the other. But let us now express this fact in terms of our new point of view. We do not begin by imagining that we are

* In the sense explained in the discussion of G. N. Lewis's suggestion.

given a set of points in space and time, into which all events and processes must fit. Instead, *we begin with the process itself*, and from the actual relationships in this process, we shall abstract the notions of space and time, as descriptions of certain features of the order in this process. Our starting point is, therefore, that each event is in fact *immediately linked* with certain others, viz. those which in current theories are said to be on the light cone through that event. But here, we are regarding this relationship of immediate contact as more fundamental than the space-time frame, in terms of which the light cone usually obtains its meaning.

What about events that are not in this kind of immediate contact? We are here adopting the point of view that all action – all contact – is primarily an immediate contact at the speed of light. Actions that go slower than this take place by second, third and higher order contacts, built up out of primary contacts at the speed of light. For example, in the Minkowski diagram (*see* Fig.

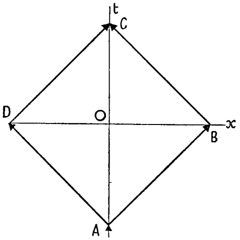

Fig. 21.

21), the line *AB*, which represents a light ray, corresponds to a first order or primary contact while *ABC*, *ADC* are second order contacts. Now every first order contact is *directed*, signifying that the action goes only in one way (e.g., *A* acts on *B* and not vice versa). If two contacts (e.g. *AB*, *BC*) are in the same direction, they combine to give a time-like interval (in this case *AC*), while if they are oppositely directed (e.g. *AB*, *AD*), they give rise to a space-like interval (in this case *BD*). In this way, we obtain the basis of the distinctions between time and space. That is to say, the primary actions along a light ray have in them neither space nor time as such (since in their own frames, they represent immediate contact without separation). It is only in the combination of such primary actions that space, time and their distinctions properly arise.

More general types of action at a speed less than that of light will then consist of a zig-zag series of primary contacts at the speed of light, resulting from a process of *reflection* of action (*see* Fig. 22). All apparently continuous movements at speeds less

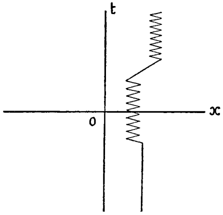

Fig. 22.

than that of light are to be explained as the result of such a trembling movement at the velocity of light. This fits in with modern relativistic quantum mechanical ideas concerning the movement of the electron. Dirac's equation implies, for example, that the electron moves at the speed of light in trembling movements (called *Zitterbewegungen*). (The average velocity, which is less than that of light, then corresponds to a kind of circulation in a spiral path that is responsible for the phenomena associated with the electron 'spin'.)

It is clear that in ordinary experience, most processes will involve an enormous number of primary actions, so that there appears to be a continuous connexion between any two events. Moreover, in a connexion of this kind, there will be an even larger number of possible variations, most of which do not involve any appreciable change in the long run or average properties of the interval. If, among these possibilities, one finds the 'most numerous' or 'most probable' type of connexion (i.e. one near which there is the largest number of essentially equivalent possibilities) one can use it to approximate a typical path. In the limit, as the number of steps approaches infinity, such a procedure is equivalent to minimising the 'distance' or 'interval' between two events (which can be shown in a more detailed treatment to be proportional to the number of discrete primary steps in any path). Such an approximation will not, however, always be adequate, and, more generally, one will have to take into account those paths involving only a few steps, and even immediate contact. But these will be relevant mainly in the domain studied by relativity and the quantum theory.

If the movements in *every* process are compounded of movements at the speed of light, then we have a direct explanation of why the speed of light is absolute. Indeed, in the primary sense, *there is no other velocity*; and as a result, the movement of light determines the natural physical unit of velocity, while all other units are only accidental and empirical. Our measurements therefore do not tell us the speed of light in terms of empirical units of velocity, but, rather, they tell us how many empirical units happen to be contained in the natural and universal unit, given by the speed of light. (Similarly, a measurement of the spacing of atoms on a crystal may most appropriately be regarded as determining how many interatomic distances are in the accidental empirical units, such as the inch, rather than as beginning with the inch as the fundamental basis of physics, and then telling us how many interatomic spacings there are to the inch.)

In order to make further progress in the development of a theory of 'topo-chronological' order in physical processes, we must consider the Minkowski diagram more critically. Is there in reality a four-dimensional continuum, with all the times present together? For example, if one wishes to measure the time between today and yesterday, can one take a clock and go back to yesterday, counting the steps in time needed to do so? Evidently this is impossible. All that we have at any given moment is a trace (or a set of marks) of earlier moments. This trace may be in our memories, in a photographic plate, or it may be left in the structure of the system (e.g., layers of structure in the Earth?). By considering this structure as showing the trace of the process by which things became what they are, we can reconstruct the past. In other words, what we always do is to relate an immediate event existing at a given moment with another set of events existing at the same moment, which constitute traces of processes that took place earlier (and in general, elsewhere).

These traces, which are present in each moment in a highly involved, tangled, and folded form, are *conceptually* disentangled, unfolded, and as it were, 'disinvolved'. The result is *mapped* on to the Minowski diagram. But no process ever actually *is* as shown on the map. Indeed, all maps are abstractions, useful in that they exhibit certain relationships, but not to be taken too literally. For example, Mercator's projection of the earth suggests that Northern Greenland is infinite, but if one goes there, he will not find any infinite spaces. Similarly, the Minkowski diagram leads one to expect a co-existent continuum of moments, but one moment is all that there ever actually *is*.

When any moment *is*, its past is always gone, and what remains of this past is only a trace, as described above. Its future is always 'not yet', but is still only a projection or an expectation. Then comes another moment, within which the earlier moment is likewise contained as a trace, while the later moment contains not only a part of what was previously projected, but also something new and not thus projected. In addition, it has in it, of course, a projection of still later moments. In other words, every moment has *its* past and *its* future, and the general features of the relationship of each moment to this past and to this future are universally shared by all moments.

It follows from the above that the space-time order of events is basically contained *within* each moment, in the sense that this order is implied by the inner structure of any event in the total process. Consider, to begin with, the time order of events. Each moment has within it a trace of earlier moments, while the earlier moments do not have a trace of it. (As a person today remembers yesterday whereas yesterday he did not remember today.) Each moment is therefore *inwardly later* than all those which are traced in it.*

Every moment not only traces earlier moments directly, but also does so indirectly, in the sense that it traces the traces that were present earlier. (As a person not only remembers yesterday and the day before, but also remembers that yesterday he remembered the day before.) It is through the series of relationships of trace, trace of a trace, etc. (suggestive of Russell's notion of classes, classes of classes, etc.) that the order of each event in the total process is contained implicitly in the inner structure of that event. Not only the time order but also the space order are implicit in this way. For the spatial structure of each trace evidently reflects corresponding structures in the moments which are thus traced. Therefore, the inner structure of each event contains implicitly its place in space and time. Or to put the same idea into more picturesque terms, where and when an event is, in relation to the whole process, is implied by how the universe would 'look' from the standpoint of that event. The notion of

* It is true, of course, that the earlier moment has a *projection* of the later one, but the relationships of trace and projection are so basically different that there is no possibility of confusion between the two.

space and time as outer relationships, expressed, for example, by mapping on to a Minkowski diagram, should then be regarded as an abstraction, the main role of which is to help show some of the implications of the inner relationships described above.

With the above-described conception of space-time relationships let us now ask, 'Does a photon pass through the space between emitter and absorber?' The answer is that it does not. Of course, as has already been pointed out, in large-scale experiences with objects moving at low velocities, typical processes will have a very high order trembling movement that is, for practical purposes, continuous. Extrapolation of this experience then leads us to expect that any influence whatsoever that passes from one point to another must go through a non-countable infinity of intermediate points. But is there any evidence that this non-countable infinity of points actually exists outside our customary maps of space and time? Are those points more real than the infinite spaces of Northern Greenland?

In the theory that is being proposed here, only the *actual* physical points need be considered (i.e. those at which an action of some kind takes place), so that there is no reason why a photon cannot connect two such points without passing through any intermediate points. Similarly, an electron in its normal trembling movement at the speed of light is *always* passing from one point to another, with immediate and direct contact between such points, even when it is a stationary state. If energy can be exchanged with its environment, it may then occasionally cross from one orbit to another and remain in this other orbit. It follows then that the characteristic quantum mechanical notion of discrete jumps in the properties of the electron will create paradoxes only if one insists that the usual continuous maps must lie at the basis of every theory, and that no matter what else happens, everything must fit into such maps.

In the brief space remaining, we shall sketch the general lines along which a mathematical theory that embodies in a more precise way some of the ideas that have been suggested here, can be developed. First, let us represent every *actual* moment by a point, and every *actual* process of immediate contact between such moments by a line (i.e. points not in such contact do not have a line between them). If we could find a law determining what are the actual lines (of which there would be such a large number that they would, in a rough description, resemble a continuous set), then we would know the order of action in the total process of the universe. To know this would be equivalent to knowing all the laws of physics and a great deal else besides. Of course, we will have to be content with something less than such complete knowledge. Indeed, to begin with, it would be quite instructive merely to find how the order of action (and therefore the laws of physics) have to be restricted, so that some of our customary ideas on space and time, and physical processes come out as suitable limiting cases.

In carrying out the above programme it is necessary first to develop a way of describing mathematically the set of relationships of trace, trace of a trace, etc., which define each moment in relation to its past and its elsewhere. It turns out that such a set of relationships can be expressed in a very natural way in terms of certain kinds of matrices, having the property that each matrix is a member of a certain kind of sequence, such that it can be obtained from the previous one, by a characteristic matrix operation. These matrices turn out to be closely related, in fact, to the creation and destruction operators of the boson and fermion particles treated in modern quantum mechanical field theory.

The above-described matrices enable each moment to be located in relation to the total process. The next requirement grows out of the study of how a space can be discrete and yet have a kind of homogeneity that we usually associate with the notion of continuity. We shall, in fact, regard the space as built of a discrete set of structures called 'simplices', consisting of points, lines, planes, pyramids, etc. We require, naturally, that these simplices generally fit together, boundary against boundary.

The places where such a failure of perfect fitting occur (which are analogous to dislocations in crystals), would then correspond to matter, while homogeneous fitting would correspond to 'empty' space. As further study shows, the relationships arising in the fitting of boundaries of regions (the so-called 'homology groups' of topology) are isomorphic to those arising in the algebra of the anticommuting fermion operators in quantum mechanical field theory.

On the other hand, the notion of the homogeneity of space also contains implicitly the notion of congruence of one region with another. The transformations between regions that express this relation of congruence, correspond to another group (the so-called Lie group) which is, in fact, very deeply related to the homology groups describing boundary relationships. (Mathematically the two groups are said to be dual to each other.) A further mathematical study shows that the algebra associated with these congruence groups is isomorphic to the algebra of the 'boson' particles of quantum mechanics.

The above results suggest that the modern quantum mechanical field theory is basically a treatment of order and relationships of actions, of such a kind that the wave function of the universe in principle determines the topo-chronology of the total process. The so-called particles then correspond to localised and persistent failures of homogeneity in the topo-chronological relationships. Fermions and bosons appear as descriptions of two aspects of the same total process, and their interaction is now automatic and necessary, just because they must complement eaeh other in a description of this process.

There is room for a whole range of kinds of elementary particles, corresponding to various kinds of inhomogeneities in the local structure of space. In principle, all properties of these partieles and their interactions are determined, once we know to which kind of inhomogeneity in the topo-chronological process they correspond. Moreover, there can be inhomogeneities in various levels, allowing for a range of masses, strengths of coupling constants, etc. In other words, it appears that the topo-chronological analysis of the process can not only provide an interpretation of the fact that the basic quantum theoretical description of the world is in terms of bosons and fermions, but also, that there are distinct possibilities for understanding the structures of so-called 'elementary particles' and the relationships between them in a deeper way than is possible in terms of current theories. Finally, it is worth mentioning that with the replacement of the currently assumed non-countable infinity of points by some discrete or at most countably infinite set of simplicial regions, one of the principal sources of the infinite results of quantum mechanical field theories is eliminated. It may, therefore, be hoped that in this way a self-consistent form of this theory can be developed.

100 Two-way Determinism

by I. J. GOOD

'Backward time isn't such a new thing, backward time will start long ago.' – Doog (after a popular song).

G. N. Lewis* outlined a theory of light in which the present is determined as much by the future as by the past. Popper,† contradicting a familiar interpretation of Heisenberg's uncertainty principle, claimed that the position and momentum of a particle can both be determined with arbitrarily high accuracy at a single moment of time, provided one has accurately observed both its earlier position and its later momentum at two specified moments. It is natural then to raise the following question.

* LEWIS, G. N.: *Nature*, volume 117, pages 236–8, 1926. *See also* pbi No. 45, note 8.

† POPPER, K. R.: *The Logic of Scientific Discovery:* page 231, 1959. *See also* Sir Arthur Eddington, *The Nature of the Physical World*, London, 1928, chapter 14.

Given a connected bounded piece of space-time, are all the elementary subatomic events within it that are classically describable (i.e. without explicit reference to quantum mechanics) fully determined by all the classically describable events outside of it? Or, if not, is there any neat way of describing how much indeterminacy is left? Can these questions be answered in terms of existing quantum mechanics, and do they raise interesting new mathematical problems?

If the answer to the first question is *yes*, then we could say that we have two-way determinism, since the present would be mathematically determined jointly by the past and future, however remote. Note however that two-way determinism is a special case of what is usually called 'indeterminism', since the past alone would not determine the present. This merely shows that language does not always behave very well.

If two-way determinism is true it raises another, more philosophical, question, namely whether we should then say that future events are contributory causes of present ones.*

101 Physical Numerology

by I. J. GOOD

The term 'numerology' was applied in conversation in 1947 by P. M. S. Blackett to the activity of finding simple numerical expressions for the fundamental physical constants. Numerology plus half-baked theory remains numerology, but is a little more likely to be 'causal' than numerology unadorned by theory. I imagine that, if the bakedness is p, the odds are multiplied by something like $\exp\{p/(1-p)\}$. Of two pieces of numerology for the same physical constant, both unadorned, the simpler piece is more likely to be causal. By 'simpler' I do not mean easier to

* Compare pbis Nos. 104, 45, and 59.

refute: I disagree very emphatically with Popper on this issue, but this is not the place to elaborate. By 'causal' I mean understandable in terms of a correct or approximately correct, but possibly unknown, theory.

I should like to see a fairly comprehensive collection of examples of physical numerology. If the average probability of the items was one in a hundred, then a collection of a hundred items would have a chance of at least 0·7 of containing something 'correct', and might provide a clue for a correct theory.

In the history of science there have been several examples where numerology preceded theory. Let us consider three of these.

1. 'Kirchoff in 1857 noticed the coincidence between the velocity of light and that of the ratio of the electrical units. In 1858 Riemann presented a paper to the Göttingen Academy in which he assumed a finite velocity of propagation, and deduced that this must be equal to the ratio of the units, hence to the velocity of light.'[1]

2. In 1885, Balmer gave a formula for the frequencies of the spectral lines of hydrogen. This numerology was clearly causal since it was so accurate. It was explained by Bohr in 1913, and better still by Dirac and Pauli in 1926, in terms of the newer quantum theory.[2] What some of us would like now is an explanation of quantum theory.

3. In 1747, J. E. Bode gave a neat formula which well approximated the distances from the sun of six planets. Later the formula 'picked up' Uranus and the asteroids, but not Neptune or Pluto. No explanation has yet been generally accepted. It is possible that the magnetohydrodynamic theory of the origin of the solar system will lead to an explanation.[3]

The collection of physical numerology that I have in mind would include the following examples, all related to the masses of fundamental 'particles'. (Fundamental 'things' would perhaps be a better term.)

(*a*) Eddington[4] wrote $\beta = 137/136$ (Bond's constant) and suggested that the ratio of the roots of the quadratic equation

$$10m^2 - 136mm_0 + \beta^{5/6}m_0^2 = 0$$

is equal to m_p/m, the ratio of the mass of the proton to that of the electron. This gives the value 1836·34, whereas the experimental value[5] is now 1836·12 $\pm$ 0·02, so that Eddington's value is very significantly in error. Eddington also suggested that there should be particles of masses $174m$ and $2{\cdot}38m_p$. Slater[6] says that there was formerly believed to be a meson with mass near $174m$, but the nearest is now the μ–meson, whose mass[5] is 206·9 $\pm$ 0·2. There is no evidence for a particle of mass $2{\cdot}38m_p$.

The numbers $r^2(r^2 + 1)/2$ are 0, 1, 10, 45, 136, ... Eddington singled out 10 and 136 as being of special significance, and ignored 45. I shall however write $\gamma = 46/45$, by analogy with the definition of β. Then:

$$137^2/10\gamma = 1836{\cdot}10, \quad 46^2/10\gamma = 207,$$

which *give the masses of the proton and of the μ–meson within experimental error.*

It so happens that γ / β^3 is very close to 1, being 1·0000008, so the above expressions may equally be written as $137^2/10\,\beta^3 = 136^2/10\beta$ and $46^2/10\,\beta^3$.

I have not tried to dress up this numerology in an unintelligible theory.[7]

(*b*) It seems reasonable to conjecture that the masses of the elementary particles are related either to the eigenvalues of a simple operator or to the zeros of a simple function. In view of the wave properties of the elementary particles, the functions that first spring to mind are the Bessel functions. Furthermore, since mass is, in a sense, a three-dimensional property, it seems natural to work in terms of the cube roots of the masses when expressed in units of the electron mass. Let then y_n be the cube of the nth positive root of the Bessel function $Y_0(\pi x)$. Then

$$y_{13} = 1{,}838{\cdot}73, \quad y_{14} = 2{,}326{\cdot}73,$$

which are equal to the masses of the neutron and Σ hyperon (in units of the electron mass) within experimental error.[8] (The experimental values are $1838{\cdot}66 \pm 0{\cdot}04$ and 2327 ± 3.[11] It is perhaps worth noting also that $(7/2)^6 = 1838{\cdot}27$.)

(*c*) Haenzel[9] has shown some close relationships between Dirac's relativistic wave equations for the electron and the groups of rotations of the icosahedron. It is natural then to see if Klein's book[10] suggests any numerology. Associated with the icosahedron, there are certain algebraic expressions, T, H, and f, known as 'ground-forms', and they are related by the equation $(T^2 + H^3)/f^5 = 1{,}728$. (I shall not define a ground-form here, but refer the reader to Klein's book.) For the octahedron there are three ground-forms related by $(W^3 - \chi^2)/t^4 = 108$. It may be noted that $1{,}728 + 108 = 1{,}836$, which is close to the proton mass. This suggests that, in order to obtain a relativistic wave equation for the proton, we should try thinking in terms of the rotation groups of the icosahedron and octahedron simultaneously, for example, by starting with the direct product of these two groups.

I should like to emphasise that I do not regard the above examples of numerology as very likely to be 'causal'. They give little evidence against other models of the fundamental particles, even concrete ones, like vortices and *knotted* vortices. Incidentally it would be interesting to know the behaviour of knotted smoke-rings, just as a problem in hydrodynamics.[12] Kelvin had a vortex theory of the atom, long since abandoned, but it might be revived for the electron.[13]

The whole field of physical numerology helps to indicate a requirement for a new theory of simplicity of scientific hypotheses and laws, especially as regards the simplicity of the numerical parameters in them; and a rule for converting the measures of simplicity into initial probabilities. Methods of assigning initial probabilities to laws according to the *number* of parameters in them have already been proposed.[14]

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102 Towards a New Theory of Nuclear Structure

by R. H. MACMILLAN

Suppose that the neutron has a charge which is not uniformly zero but which consists of positive and negative regions such that the *net* charge is zero. Then, if these regions were suitably distributed, some very interesting properties emerge for groups of neutrons and protons. In particular, suppose the positive charge to be localised in one half of the volume occupied by the neutron and the negative charge in the other (rather like two hemispheres). If now the neutron spins about any suitable axis passing between the two parts (cutting the 'equator'), its resultant external electrical effect will be zero; if, however, it spins about any other axis the particle has an electrical axis with net negative charge at one end and net positive charge at the other. (The effect is greatest when the axis of rotation is perpendicular to the equator.)

Working on this hypothesis, we find the possible existence of regions of negative charge within a nucleus consisting solely of protons and neutrons and this leads to the possibility of the formation of nuclei which are stable solely under the influence of electric forces, as can easily be checked for the nuclei of the lightest elements and their isotopes. The problem of the stability of heavier nuclei appears to be much more complex, involving as it does the interaction of many particles.

It is clear that this concept of electrically non-homogeneous elementary particles – it can be applied equally to others, such as the proton – gives special significance to particle spin and might help to elucidate the physical significance of 'strangeness'. It also presents a possibility of accounting for the odd selection of stable 'elementary' particles including *mesons* and the rest.

Of course, even if this hypothesis should prove capable of explaining currently unrelated observed phenomena, it would only have replaced present nuclear mysteries with an even greater one: what causes the charge to be segregated within the particles, and how is cancellation avoided?

103 Non-linear Quantum Mechanics

by E. H. HUTTEN

Most physical events, certainly on the microscopic level, are interactions. Non-linear differential equations may be interpreted, in physical terms, as interactions between two (or more) entities. Linear equations represent isolated action of one entity on another; this can never be more than an idealised description of reality, an approximation applicable only under certain conditions. There are several quite general arguments that support this interpretation of non-linearity:

1. The action of a force is usually described by one of the variables in an equation, e.g. the distance. Interaction is conceived of as mutual action and so must involve the variable or variables to a second or higher degree, if two or more entities participate.

2. The superposition principle does not hold for non-linear equations, only for linear ones. Superposition means that solutions of an equation, i.e. physical actions, simply add without modifying each other. Interaction, by definition, describes the fact that two, or more, entities acting together modify their actions; thus a non-linear equation is needed to represent it.

3. Perturbation theory shows the solution of a many-body problem, e.g. that of the helium atom, to improve the higher the powers of the variable in the solution. This is generally taken to mean that the perturbation – say, the interaction of the second electron with the hydrogen-like residue of the helium atom – is more completely described by the higher approximation.

4. In order to describe interaction between particles, it is most likely necessary to regard them as having a finite size rather than as point-particles. Within the field of classical electron theory, this leads to a non-linear extension of Maxwell's equations.[1] A theory of elementary particles will probably require a quantis-

ation of space (and time), i.e. a fundamental length; we may therefore expect a non-linear equation in such a theory.[2] This would also seem to be the gist of Einstein's remarks: speaking about Maxwell's equations he said that 'the true laws cannot be linear. Such linear laws fulfil the superposition-principle for their solutions, but contain no assertions concerning the interaction of elementary bodies.'[3]

It seems reasonable to say, then, that a more exact and complete description of elementary particles can be achieved only by directly solving a non-linear equation.[4] This is true the more since we understand that spin is a property describing interaction in some manner; and that elementary particles can be the result of interaction (pair creation). It is well to remember, too, that the other properties of elementary particles, i.e. isotopic spin and strangeness, are generalisations of the spin property and are ascribed to a particle only by virtue of its interaction with other particles.

Modern quantum mechanics could be said to begin with Dirac's relativistic wave equation. Dirac approached the problem of finding his equation by starting from the Hamiltonian form

$$i\hbar \frac{\partial}{\partial t} \psi(r,t) = H\psi(r,t) \qquad (1)$$

($\hbar = h/2\pi$ refers to Planck's constant, ψ is the wave function). The classical relativistic Hamiltonian for a free particle is the positive square root of the right side of

$$E^2 = c^2 p^2 + m^2 c^4 \qquad (2)$$

However, if this is substituted in eqn. (1) and p is replaced by $-i\hbar$grad (the corresponding quantum mechanical operator), the resulting wave equation is unsymmetrical with respect to space and time derivatives, hence not relativistic. Dirac therefore modified the Hamiltonian in such a way as to make it of the first order in the space derivatives.[5]

Dirac solved the equation by means of linear operators and of the superposition principle. Superposition shows that no true interaction is allowed for by this solution; otherwise it would be impossible to treat one energy state as independent of the other and simply superpose them. All the same, the existence of the first of the new, unstable elementary particles was predicted by Dirac's equation. Both the properties of spin and of charge dualism (including the pair creation of electrons) are derivable from this equation.[6]

Dirac solved his equation by making it first order and by making use of a novel mathematical entity, the spin matrix. This has always appeared to physicists as a compromise. The peculiar properties of the spin matrices represent interaction which would have been lost if the first order equations resulting from the splitting of the original, second order, equation had been solved by conventional mathematics. Statistical methods can also describe interaction, though only 'in bulk', and not of individual particles as required here. Bose-Einstein and Fermi-Dirac statistics are an example. Perturbation methods are useless, by definition so to speak, since the interaction here is of the same order of magnitude as any of the other actions.

The problem is therefore to find a direct way of solving a non-linear equation, e.g. of the Dirac type, in order to describe *all* the properties of elementary particles, including their mass spectrum. If we assume that Dirac's equation suffices to describe all particles of spin $\frac{1}{2}$, then we need another method for solving it. There are, however, good reasons for saying that Dirac's equation does not suffice. It would be better to construct a new, non-linear equation containing second, or higher, powers of the variable which thus describes true interaction. My *first half-baked idea* is to find such an equation either by a hydrodynamical analogy (as in the instance of the original Schrödinger equation) or to generalise an existing non-linear equation describing oscillations, e.g. Duffing's equation

$$\ddot{x} + \omega_0^2 x + hx^3 = G\cos\omega_1 t \qquad (3)$$

or van der Pol's equation

$$\ddot{x} - \varepsilon(1 - x^2)\,\dot{x} + x = 0 \qquad (4)$$

(ω_0 is the frequency of the free oscillation of the non-linear spring to which unit mass $m = 1$ is attached, ω_1 is the frequency of the driving force, G its amplitude, h and ε constant coefficients). The generalisation would consist, in part, of introducing suitable, quantum-mechanical operators.

This is a very difficult task since, obviously, knowledge of the physical situation has to be brought in; it may well be that we still do not know all the relevant facts about elementary particles. Failing to achieve this, my *second half-baked idea* is to interpret Dirac's equation as describing non-linear oscillations of a continuous substratum. From the theory of non-linear vibrations it is known that they display abrupt changes. It would then be possible to explain, for example, the various mesons changing into one another as different states of excitation of the substratum. This is, of course, only one of many possible interpretations. Since spin is a rotation and quantised (by introducing suitable momentum operators), it suggests ideas like non-linear rotation and non-linear quantisation. The quantisation of the unharmonic oscillator might be visualised by a spiral, just as the quantisation of the harmonic oscillator is pictured as an ellipse in phase-space. Certain graphical solutions of non-linear equations are spirals. I have not been able, so far, either to find a new equation or to obtain a direct, non-linear solution of the Dirac equation.[7]

For this reason, it might be advantageous to proceed the other way round. This is my *third half-baked idea*. It should be possible to solve a standard non-linear equation, e.g. of the unharmonic oscillator,

$$m\ddot{x} + ax + bx^3 = 0 \qquad (5)$$

by Dirac's method (m is the mass, a and b are constants). In other words, linearise it so that it generates spin matrices of some sort: we might find in this way a generalisation of Dirac's method which could then be applied in quantum mechanics. Though I have carried out this programme for the unharmonic oscillator, I have not been able to see the wider implications of this method. Perhaps a more complicated non-linear equation must be studied.

Finally, and this is my *fourth half-baked idea*, it may be possible to invent a new kind of non-linear (and non-commutational?) algebra for the purpose of solving the problem. My *cri-de-coeur* is then for a mathematician knowledgeable and inventive in the non-linear field.[8]

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3. EINSTEIN, A., in *Albert Einstein: Philosopher-Scientist*, edit. by P. A. Schilpp, 2nd edition, page 89, New York, 1951. *See also* EINSTEIN, A., INFELD, L. and HOFFMAN, B.: *Annals of Math.*, volume 39, pages 65–100, 1938.
4. CUNNINGHAM, W. J.: *Introduction to Nonlinear Analysis:* McGraw-Hill, 1958. This is the latest book on the subject; it contains an exhaustive list of references.
5. DIRAC, P. A. M.: *The Principles of Quantum Mechanics:* Oxford Univ. Press.
6. I allude here to the well-known interpretation of Dirac's equation, according to which a negative electron, when knocked out of the substratum, leaves behind a hole which is observed as a positive electron.
7. In his recent book, *Non-Linear Wave Mechanics*, Elsevier, 1960, L. de Broglie is concerned with a problem completely different from the one here, namely, with reconciling the wave-particle dualism. He is inspired by the equations of General Relativity which are non-linear. However, he merely discusses possibilities and does not attempt to construct a non-linear equation.
8. *See also* ROMAN, P.: *Theory of Elementary Particles:* pages 555–9, Amsterdam, 1960.

104 Computers, Causality, and the Direction of Time

by I. J. GOOD

Reichenbach, in his very interesting, but perhaps too long, book, *The Direction of Time* (1956), p. 29, says that mechanical processes are reversible, i.e. that they could be run backwards without defying the laws of mechanics, but that thermodynamic processes are usually irreversible. But consider an object sliding to rest on a rough horizontal plane. This would often be described as a mechanical set-up, but it is not reversible, or, at any rate the probability that the molecules would happen to move in such a way as to produce the reversed motion is so small that if it happened it would be more sensible to call it a miracle than a coincidence. The Bayes factor in favour of a miracle would be enormous.

What Reichenbach should have said is that mechanical processes are reversible when they are conservative, and so generate no heat.

The above experiment is perhaps the simplest one for defining a time direction. I should now like to consider another method.

Charles Babbage[1], starting in about 1833, designed a mechanical 'analytic engine' capable in principle of performing any calculation that an electronic computer can do, more slowly. If the 'input' were shut down, the machine would have only a finite number of possible 'states', such that each state has a unique successor, since the machine is deterministic. But some states would have more than one, or no, predecessors. Let us call any mechanical machine having the latter property (and only a finite number of states) a 'Babbage machine'. Clearly a Babbage machine cannot be run backwards deterministically along all its paths through its 'state space'. *Therefore a Babbage machine generates heat.* (In fact if a state, S, has two predecessors, S_1

and S_2, then heat is generated in the transition $S_1 \rightarrow S$, or in $S_2 \rightarrow S$, or both. Likewise, a ball cannot be capable of running quite smoothly inside two glass tubes that coalesce into a single tube.)

The same conclusion about Babbage machines can be reached by another argument. The number of possible states of the machine decreases while it runs, being eventually equal to the total length of the cycles of its state diagram (an 'oriented linear graph'). So, if it starts from a random state, there is a decrease in its *mechanical* entropy, defined as $-\Sigma p_i \log p_i$, where p_i is the probability of the ith state. Therefore its thermodynamic entropy must *strictly* increase, in order that the second law of thermodynamics should not be violated. Therefore heat must be generated.[2]

If, by a miracle, a Babbage machine did run backwards, it would be not a computer, but a refrigerator.[3]

That would be an appropriate note to end this contribution on, but I should like to mention another incompletely formulated concept.

There are some subatomic phenomena, first described by Stückelberg and Feynman[4], in which the direction of time is disputable. I should like to propose that the problem can be resolved by assuming, as a sort of conventional law of nature, that the interpretation to be adopted is the one that *maximises the total amount of probabilistic causality*. The latter expression can be given a precise meaning.[5] I suspect that this 'law' implies that local time direction is that of increasing entropy, for isolated systems.

It seems appropriate to add a remark here concerning reversed causality.

Let us take for granted that there are true laws of nature irrespective of whether we know or ever will know what they are. Every fairly isolated physical system may be regarded as generating a causal chain that exhibits some of the laws of nature. Most physical systems consist of a large number of sub-systems, so that a causal chain may also be regarded as a bundle of thinner chains that both coalesce and branch. Also, if a system blows up it would be natural to say that its causal chain had branched out into many chains. Branching *in*, on the other hand, seems very seldom to occur unless a system is interacting strongly with other systems: unfortunately broken Ming vases do not reassemble themselves; and this is one reason why time seems to have a direction. It is conceivable however, especially in an imaginary universe, that the simplest description of nature would be given by assuming that some of the chains run both forwards and backwards in time, according to some clocks. We may then reasonably say that the future can affect the past.

Let us develop this idea a little further. Suppose that we had *n* blue-prints for making clocks, and that all clocks made in accordance with the *r*th blue-print kept the same time, for each *r*; but that clocks in any one of the *n* sets would be inconsistent with clocks in any of the other sets. Suppose, in fact, that the time of any one set went backwards and forwards with respect to that in any other set. Suppose also that many other varieties of physical system could also be regarded as *some sort of a* clock; for example, each man could be regarded as an approximate biological clock. The measurement of time would then be much more 'relative' than in the theory of relativity.

A man on a desert island would tend to use his own biological clock for the estimation of time, but in a society this would be regarded as too egocentric. It would be generally recognised that the adoption of a particular set of mutually consistent clocks was to a large extent conventional, and that, whatever set of clocks was adopted as standard, there would be many causal chains that went both forwards and backwards in time. Whatever standard was adopted there would always be examples of the future affecting the past.

Thus, it is not false *by definition* that the future may affect the past, although it may well be false. When there are two different ways of measuring time with very great precision we may find that it is true. *Conceivably* it will be shown by means of parascientific research.

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2. Similar, but not identical, ideas were independently proposed (and developed) by LANDAUER, R.: *IBM J. Res. Dev.*, pages 183–91, 1960.
3. A very small prize indeed is offered for converting this idea into a limerick.
4. *See* Note 8 of pbi No. 45.
5. *See* 'A causal calculus': *Brit. J. Phil. Sc.*, volume 11, pages 305–18, 1961; volume 12, pages 43–51, 1961; in an early draft of which the above idea was formulated more completely.

105 Physics, Stereochemistry, and the Fourth Dimension*

by MIGUEL MASRIERA

According to the general theory of relativity, the universe is finite but unbounded, and can be represented approximately as a three-dimensional hypersphere in a space of at least four dimensions. I suggest that the 'real' directly observable universe is better represented by supposing the hypersphere to have a very small thickness, perhaps between 10^{-8} cm and 10^{-12} cm. This hypothesis† gives some reason why elementary particles,

* This contribution has been condensed by the editors from two much fuller papers, to appear in Spanish.

† References to more fully formulated five-dimensional theories are given in a footnote to pbi No. 45. Note also that EINSTEIN, A., and ROSEN, N, (*Phys. Rev.*, volume 48, pages 73–7, 1935), suggested that physical space should be represented by a mathematical space of two identical hypersheets, a particle being represented by a 'bridge' connecting these hypersheets. But the author's idea is quite distinct. *G.Ed.*

and perhaps atoms, behave quite differently from macroscopic bodies, and offers a possibility of unifying quantum theory with relativistic cosmology.

Wave-particle duality can be viewed as the result of the impact of a particle from inside the thickness of the hypersphere, generating surface waves like a stone falling in a lake. Atomic phenomena will appear to be indeterministic because the events inside the thickness of the hypersphere will be unobservable by normal means.

The short life of some of the elementary particles may be explicable by assuming them to stay in the vicinity of the hypersurface for a short time. The uncertainty principle and probability waves also seem no longer paradoxical.

I suggest that positive and negative electricity may be closely related to the inside and outside of the hypersphere.

The hypothesis should have application to stereochemistry. The dependence of optical activity on symmetry properties of molecules has been explained only qualitatively by existing theories.

In living matter, molecular asymmetry appears through the unequal production of right-handed and left-handed forms of the same compounds. I suggest that the specific rotatory power of the two forms can be derived from the asymmetry of a four-dimensional body, and that the two three-dimensional forms correspond to two projections of this body into the hypersphere. All these considerations need mathematical development.

106 Winding Space

by I. J. GOOD

A problem that naturally fascinates philosophers of science and theoretical physicists is why space has three dimensions, or whether it does.[1] In this paper I shall first briefly discuss this question[2], and then speculate on the possibility that ordinary three-dimensional space is embedded in space of higher dimensionality. The suggestion I shall make is that ordinary space is of infinite extent, but winds around in space of say seven dimensions, without intersecting itself.

If you take six rods of equal lengths you can fit them together as a tetrahedron, but, try as you may, you cannot add four more rods as you should be able to do in four spacial dimensions. So it is clear that in some sense space has just three dimensions when we are concerned with ordinary lengths. But we should remember, with A. N. Whitehead,[3] that different numbers of dimensions may be appropriate for different kinds of phenomena.

We may just *happen to be the right size* to think that space has just three dimensions. Perhaps if our lengths were 10^{24k} metres ($k = -3, -2, -1, 0, 1, 2, \ldots$) we would think of space as having $k + 3$ dimensions. At certain intermediate sizes we may even suppose that the number of dimensions is fractional. Ulam[4] has indeed suggested that fractional dimensions may have physical significance.

Courant[5] has stated that only in three dimensions is high fidelity communication possible. We may speculate that there are really an infinite number of dimensions, but that we can appreciate only three of them because we ignore low-fidelity signals. I have not thought of any reasonably rigorous formulation of this idea.

We may try to run away from the question by saying that 3 is a small enough number not to need an explanation. An explanation would have been more in demand if the dimensionality had been 32650494425.

Finally, it is possible that God selected just three dimensions in order that communication systems, such as ourselves, should be possible.

In the theory of relativity, space is assumed to be of three dimensions, not embedded in more than the four dimensions of space-time. In most of what follows, I shall ignore the time dimension. In the *Einstein universe*, the mean radius of curvature, R, of space is related to the total mass, M, by the formula

$$4GM = \pi c^2 R, \quad (1)$$

where G is the gravitational constant and c is the velocity of light.[6] In this model, space is assumed to be a 3-sphere, the hypersurface of a hyperball. The curvature is an 'intrinsic' property of space, defined in terms of the metric of space. For example, the curvature can be deduced by measuring the angles of a very large triangle, and seeing by how much the sum exceeds 180 degrees. Thus, the curvature can be defined without assuming the real existence of the hyperball, but the hyperball is valuable in providing a proof of the self-consistency of the model, if the self-consistency of more elementary mathematical ideas is taken for granted. Without the idea of the hyperball, the idea that space is finite but unbounded would be intuitively difficult to accept.

It is interesting to consider some of the implications of the above formula if we assume instead that the universe is infinite. Then the natural interpretation of M is the mass of the *observable* universe, i.e. of those parts that are not receding from the observer faster than light. Owing to the random distribution of matter in space we should expect M to vary slightly when we switch from one observer to some other very remote observer. Since it is thought that there are about 10^{78} particles in any one observable universe, we may assume, on the basis of binomial (heads-and-tails) random variation that the proportional variation will be of the order of 10^{-39}. Since the radius of curvature is about 10^{28} cm, the variations will be of the order of 10^{-11} cm, or very roughly, $h/(2\pi mc)$, where h is Planck's constant, and m is the mass of the electron.

The above argument should be compared with Eddington's method of arriving at his 'uncertainty constant', which measures the 'uncertainty of the reference frame'.[7]

If, after smoothing out local irregularities, such as clusters of galaxies, R varies, by any amount however small, then space would not necessarily close on itself. It could, for example, bear much the same relationship to a 3-sphere as a helix bears to a circle. The local properties of a helix of very small torsion would be indistinguishable from those of a circle, and likewise the local properties of space might be indistinguishable from those of a 3-sphere.[8] But since space apparently has 'handedness',[9] one's belief in its isotropy is somewhat undermined, and the 3-sphere seems a little less probable than it was before.

If the variations of R are somewhat random, as suggested above, then space would be more analogous to a badly wound reel of cotton than to a helix. We are thus led to a theory of 'winding space', but I have not yet determined whether the assumptions are mathematically self-consistent.

Suppose that optical or radio telescopes are one day powerful enough to see up to a distance $2\pi R$ (along a geodesic), that is 'right round the universe'.[10] If the universe is Einstein's, then, if we looked in various directions we would see the same part of space from various aspects, namely 'ourselves' $2\pi R/c$ seconds ago. But on the present hypothesis of winding space we would see two different parts of space when we looked in two opposite directions. Thus, there is some hope of an experimental test. But even if no test could distinguish Einstein's universe from 'winding space', it would still be of cosmological interest that space could be infinite and yet all the physical consequences of the Einstein universe be valid. It would exemplify the well-known hazards of extrapolation from our limited knowledge to the universe as a whole.

Other consequences may follow from an elaboration of the hypothesis. Suppose for example that our 3-space is embedded in an n-dimensional space ($n > 3$) having physical meaning, just as the hyperball could have. Imagine a trip taken along a geodesic with infinite speed. After going round 'full sphere', a point would be displaced by a small distance in a direction perpendicular to ordinary space. If we imagine the process repeated indefinitely, the displaced point would perform a random walk in space of n-3 dimensions. (This is only an approximate description since there are three independent routes around the universe.) Now there is a theorem of Pólya's[11] to the effect that if a random walk is performed on a p-dimensional lattice, with unit steps, then return to the origin infinitely often is certain if $p < 3$. It is true that the conditions of this theorem are not quite applicable here, but it does suggest that we need $n \geqslant 6$ in order that the hypersheets should not be packed indefinitely densely. In fact, owing to the 'independent routes around the universe', which were mentioned parenthetically above, we need to take $n = 7$. It may be noted that $n = 7$ is just large enough so that the hypersheets do not intersect. Allowing for time, the embedding space would be eight-dimensional, and this suggests that some eight-unit algebra[15] should be relevant. It is perhaps worth mentioning also that dimensions $n \geqslant 7$ have a distinctive topological property.[12]

We may now conjecture that the elementary particles are 'wormholes' connecting close pairs of hypersheets. Wormholes have already occurred in the literature of the 'geometrisation' of physics.[13] We may further conjecture that the possible elementary particles and their properties depend on the local distribution of hypersheets. This is a reasonable conjecture because nearly all the local hypersheets will be locally nearly flat since large concentrations of matter are extremely rare in space. As a consequence of this flatness, our conjecture does not lead immediately to observational contradictions: it may be necessary to go to very remote galaxies to find appreciable differences in atomic physics. At any rate the appearance from the earth of remote galaxies might be found to be statistically different in different directions, even before we have telescopes as powerful as those previously mentioned.[14]

If the velocity of recession is proportional to distance, then the local velocity of neighbouring hypersheets relative to us is liable to exceed *c*. It seems more reasonable to assume that $u = \text{arctanh}\ (v/c)$ is proportional to distance, where v is the vclocity of recession and u is the usual modified (additive) vclocity of Special Relativity. Then the various hypersheets would all be 'sliding' over each other with relative velocities close to *c*. This sliding might be responsible for the spin of elementary particles, if they are wormholes joining 'our' hypersheet *to itself*, as is assumed in Wheeler's theory. Particles without spin might very well correspond to suitably *knotted* wormholes.

On this assumption, the cxpression 'observable universe' ccases to have a precise meaning, and M must be redefined as a *weighted* mass, for example as the integral of $m.\exp(-d)$, where d is the distance from us, along a geodesic, of a mass m. Or, as Professor Wigner suggested in a private communication, M could be interpreted as a density.

The model has great potentialities for metaphysical speculation. Who knows what peculiar particles, fields, forms of consciousness, and categories other than matter and mind may be all around us in the extra four dimensions? It is tempting to suppose that an act of consciousness occurs when there is some kind of pattern of interaction between a pair of hypersheets. If the interaction takes place only through elementary particlcs and quantum-mechanical fields, then a cog-wheel machine would not be conscious however much information it handled.

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15. *See*, for example, DICKSON, L. E.: *Linear Algebras:* page 14, Cambridge, University Press, 1930.

[*Editorial comment.* When it comes to special relativity, the author of the next article has been fighting fearful gods for some time. In the course of correspondence, in which I have been a devil's advocate on their behalf, it has emerged that the author's case depends largely on the validity of the following statement:

'To say that two bodies remain relatively at rest is to say that at every moment of time, in whatever co-ordinate system (frame) you choose, the distance between them remains the same.'

It follows from this, for example, that a co-ordinate frame is uniquely specified as one in which a particular body is at rest, apart from transformations of the *spacial* co-ordinates.

Personally, I do not believe that the notion of two bodies' being relatively at rest can be given a self-consistent operational meaning, unless they are at rest in an inertial frame, i.e. a frame in which Newton's first law of motion is locally valid. All the same I think the paper is stimulating, and should the author prove to be right he will, to quote him,* have established his descent from the little girl who saw that the Emperor had no clothes.

There are other authors who question the theories of special or general relativity. *See*, for example, Julio Palacios, *Rev. R. Acad. C. Madrid*, volume 55, pages 1–12, 1961; also the reference to 'Weyl's postulate' by H. Bondi, *Cosmology*, page 150, Cambridge, 1952. *G. Ed.*]

107 The Observation of Line Events

by HERBERT DINGLE

This idea was first put into the oven in a paper in *The British Journal for the Philosophy of Science*[1] in which I posed the following problem:

Consider two exactly similar observers, A and Z, alone in space and relatively at rest at a distance X apart. They carry

* Private communication.

synchronised clocks, and continuously radiate the same monochromatic light. At the same instant, $t = 0$, they fire identical rockets in the direction $Z \rightarrow A$ with equal momenta. Will either of them observe a Doppler effect, and, if so, when?

If, according to its definition, a Doppler effect is necessarily associated with relative motion between the bodies concerned, they will not, for at no time is there any relative motion between A and Z. But if there were a third observer, A', originally at rest beside A, who did not fire a rocket, we have no reason to doubt that they would both observe A''s light displaced from the moment of firing the rockets. If, then, Z does not observe A's light displaced, it must be that the effect of A's motion on A's light must be transmitted instantaneously to Z, although the observation of A's rocket-firing operation would not be possible to Z until time X/c later. This is difficult to believe in the light of present conceptions, and, in fact, it is often assumed without question that the Doppler displacement would take the same time to reach a distant point as the light itself. Suppose, then, that it does take that time. Then during the interval from $t = 0$ to $t = X/c$, Z, because of his own motion, observes A's light displaced by a definite amount, $d\lambda$, from which he can calculate a velocity dV. What is that velocity? There are only two bodies concerned, A and Z, and they are never in relative motion (A' is imaginary, and the rockets cannot determine dV, for their velocity is quite indefinite; only their momentum determines the subsequent motion of the bodies from which they are fired).

(It should be added, to prevent a misunderstanding which has arisen, that the problem is not concerned with the brief interval of time during which the rockets are operating. If A and Z are one light-year apart, and the rocket-firing lasts one second, what will be observed by Z one month after the firing?)

It would appear that there are only two possibilities compatible with the relativity of motion – i.e. compatible with the postulate that 'velocity' has no meaning unless it can be expressed as 'velocity with respect to something'. First, no spectrum shift will be observed, in which case Doppler effects must be transmitted instantaneously, and the motions which we infer in distant stars, extra-galactic nebulae (if the red-shifts are really Doppler effects) etc. are the motions which those bodies possess *now*, and not necessarily t years ago, where ct is their distance in light-years ($c =$ velocity of light). The second possibility is that light itself can act as a reference body with respect to which velocities can be expressed. In that case, if Z observes a displacement corresponding to a velocity dV, this quantity is defined by the fact that the velocity of A's light with respect to Z is $c + dV$.

Einstein's special theory of relativity is based on two postulates – the 'postulate of relativity' and the 'postulate of constant light velocity' – and both are absolutely essential to it. It appears, therefore, that in this situation, in order to preserve the first postulate the second must be violated, for the theory, as is well known, requires that no information of an event can be transmitted to a distant point with a velocity greater than c, and that the velocity of light is c with respect to all bodies, no matter how they are moving. If the Doppler effect is transmitted instantaneously, then knowledge that a body has been set in motion is so transmitted; and if the velocity of A's light with respect to Z is $c + dv$, then clearly it is not c.

I can conceive of no solution of this problem that is consistent with Einstein's theory, and none has been suggested to me. I have no preconceptions as to what would be observed, nor any preference for one eventuality over another, but the purpose of this paper is to follow the cooking of the first idea, namely, that the Doppler effect is transmitted through space instantaneously.

So far as I can see, there is nothing in our present knowledge to refute this possibility. What immediately occurs to one is that it is inconsistent with the fact that in eclipsing variable stars the Doppler shifts are always in phase with the brightness changes. It is impossible to regard this as accidental, so if the Doppler effect is transmitted instantaneously, the eclipsing of one star by the other must also be transmitted instantaneously. But why not?

In this connexion we must recognise a limitation of relativity theory which is indeed well known but which, so far as I know, has not been recognised as a limitation. It arises from the fact that the theory presupposes that every physical event is a *point-instant*, or at any rate can be analysed in terms of point-instants. For example, Eddington writes[2]: 'Our fundamental hypothesis is that

Everything connected with location which enters into observational knowledge – everything we can know about the configuration of events – is contained in a relation of extension between pairs of events.'

(his italics), and he has previously defined an *event* as something represented by four co-ordinates, x, y, z, t, i.e. as a point-instant in a four-dimensional continuum. The same idea is sometimes expressed in the form that our only possible knowledge is that of intersections of world-lines. But there are some events which are intrinsically inexpressible in terms of point-instants. In the paper above referred to I mentioned one such event – namely, that at which two bodies, which first move apart and then reverse their motion, have their maximum separation. In terms of point-events we can regard this as A reaching its maximum distance from B or B reaching its maximum distance from A, and, no matter what co-ordinate system we choose to adopt, these are two different events, occurring at different places and, in general, different times. Yet it is a single event, and since it is just the sort of event which is associated with the Doppler effect, it is not surprising that current relativity theory is in difficulties in problems concerning that effect.

Another such event is that of the collinearity of three bodies. This cannot possibly be expressed in terms of point-events. Everything that happens to any one of the bodies is exactly the same, no matter whether two other bodies, somewhere else in space, happen to be in line with it in some co-ordinate system or not. In general, we may say that any event at all that *necessarily* involves a finite extension in space or time is outside the scope of current relativity theory; it cannot possibly be described as an intersection of world-lines. No matter how we try to apply the theory to it, we meet with ambiguities. I will call such events *line-events*: their characteristic is that they have no meaning in terms of the separate bodies involved in them, but are concerned solely with the relations between those bodies. Let us consider in particular the type of line-event just mentioned, in which three bodies are collinear.

In the first place we may note that, in a single co-ordinate system, such an event can be uniquely described. In such a system a straight line has a perfectly definite meaning, and time has a definite meaning at all points of space. Hence, if we have three bodies, A, B and C, moving in any way at all, it has a definite meaning to say that, at a particular instant, they are all on a particular straight line, and therefore are collinear. The ambiguity arises when we consider the *observation* of the collinearity. Suppose there is an observer on A. If he regards the collinearity as occurring when A comes into the line joining B and C, it occurs at his own position and he observes it at once. If it occurs when B or C comes into the line joining the others, then it occurs at B or C, and he observes it after a time AB/c or AC/c. Which is right? In terms of current relativity theory we have no means of knowing.

It follows that there is nothing at all in the fact that the spectrum and brightness changes of eclipsing binaries are in phase to prevent us from supposing that the Doppler effect is transmitted instantaneously. If it is, then we must say that we observe the eclipse when the Earth enters the line joining the components of the star.

But what about the eclipses of Jupiter's satellites? Here we have definite evidence that these phenomena are observed some 40 minutes after the eclipses occur, and since the dynamical theory would be impossibly upset if eclipses were observed after 40 minutes delay and occultations immediately, it would seem that this determines how the occurrence of a collinearity is related to the observation of it. But in fact this is not a proper conclusion. An eclipse of a satellite, which occurs when the satellite enters the shadow cone of Jupiter cast by the Sun, is quite definitely a point-event. An observer on the satellite, who knew nothing of what went on outside, could observe it by the sudden darkening, and it is that sudden darkening that we observe 40 minutes later. But when we say that the darkening occurs at the moment when the Sun, Jupiter and satellite (or particular points on them) are collinear, we are again making an assumption. Does the *collinearity* occur when the satellite enters the line joining the Sun and Jupiter, or when the Sun enters the line joining Jupiter and the satellite? We can take our choice.

There is exactly the same ambiguity in relating the time of observation of an occultation of the satellite with the time of collinearity of the bodies concerned. Gravitational theory deals only with collinearities; it would be exactly the same if all the bodies were completely dark. Observation deals only with events in which light enters; it would not occur at all if the bodies were dark. The continuity of the phenomena, coupled with that of the events assumed in gravitational theory, ensures that *if* we suppose that the moment of eclipse is a moment of collinearity, then we must suppose that the moment of reappearance of the satellite from behind Jupiter is a moment of collinearity also, but it tells us nothing about the actual relation between the moment of eclipse (or occultation) and the moment of the corresponding collinearity.

Let us try another approach. Let Fig. 23 represent, in a co-ordinate system in which the Sun is at rest, the objectively significant positions (i.e. positions quite independent of any observation of them) of Jupiter, J, one of its satellites, M, and the Sun, S, when they are collinear at opposition,* the Earth being

* Any apparent reference to the Biology Editor is purely coincidental. *G.Ed.*

collinear with them at E_1. If this objective configuration is observed as such instantaneously, it will be observed as an occultation when the Earth is at E_1, and if it is observed after 40 minutes delay, the Earth will be at E_2. It is easily calculated

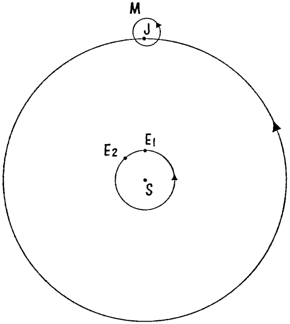

Fig. 23.

that the angle JE_2M in that case will be about 0·07 seconds of arc. This is too close to zero to be observationally distinguishable from an occultation. Hence, we cannot in this way determine how the time of a collinearity is related to the time of observation of it.

It is possible, of course, to assume that a ray of light which meets M grazes J at a time JM/c earlier, and left the Sun at a time SJ/c earlier than that, the positions of J and M at the various times being calculated from their motions. In that way it would be possible to calculate a time, relative to the time of eclipse (or occultation, as the case may be) at which S, J and M were collinear. But that would be to locate particular pulses of light in space at particular instants (which is the second suggestion at the beginning of this article, *alternative* to that of instantaneous transmission of the Doppler effect), and this, in order to escape contradiction, would inevitably demand the simple addition formula, and not the generally-accepted relativity formula, for composition of velocities. Moreover, it is a purely theoretical approach, and what I am looking for now are *observational* means of relating the times of occurrence to the times of observation of line-events. Theories are here in the dock, and their plea of 'Not Guilty' merely starts the trial, not decides it.

It is much to be hoped that space research will eventually increase our, at present, very scanty experimental knowledge of the Doppler effect. At the moment, the only test of the time of its transmission that I have been able to think of appears to be beyond our experimental resources. Consider a spectroscopic binary star, and suppose for simplicity that the Earth lies in its orbital plane and is at rest with respect to its centre of gravity. Fig. 24 shows the orbit, assumed circular, of radius a, of one com-

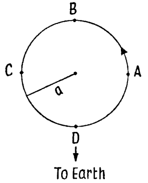

Fig. 24.

ponent round the centre of gravity. Suppose the star is at A at time T, at B at time $T + t$, at C at time $T + 2t$, and at D at time $T + 3t$. Now suppose the star is r light-years away, and that the Doppler effect is transmitted with velocity c. Then

maximum red displacement will be observed at time $T + r/c$
$T + t + (r + a)/c$;
undisplaced lines will be observed at time $T + t + (r + a)/c$;
maximum violet displacement will be observed at time $T + 2t + r/c$;
undisplaced lines will be observed at time $T + 3t + (r - a)/c$.

Hence the interval between maximum red displacement and the next undisplaced spectrum will be $t + a/c$, and the interval between the latter and maximum violet displacement will be $t - a/c$. The time of observation of the undisplaced spectrum will therefore not be midway between the times of maximum red and violet displacement. On the other hand, if the Doppler effect is transmitted instantaneously, this asymmetry will not occur.

The conditions favourable for this test are a short period and large orbital radius, and these are mutually antagonistic. Moreover, the orbits of most binary stars are eccentric, and this introduces another possible cause of asymmetry. There seems little hope of a decisive observation here.

In the absence of knowledge, the proximity of a heated oven invites the introduction of a speculative dish, not yet begun to be baked. Suppose all point-events occurring at a distance r can be known only after an interval r/c or later, and all line-events are knowable immediately: what sort of confection will that produce when cooking is complete?

The fact that a collinearity has a precise meaning in a single co-ordinate system opens the way to a pretty mathematical theory, which may possibly have been developed, but I do not know of it. For example, let Fig. 25 represent a simple case in

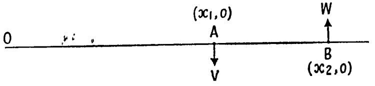

Fig. 25.

which O, A and B are collinear along the x-axis at time $t = 0$. O is at rest, and A and B move as shown, with uniform velocities $-V$ and W, respectively, in the y-direction. We can calculate the time of collinearity of these bodies in the primed co-ordinate system of the usual special relativity theory, which moves with velocity $(v,0)$ with respect to O. We find that in this system collinearity occurs at time

$$t' = -\frac{vx_1x_2(W + V)}{c^2\,\alpha\,(Wx_1 + Vx_2)}$$

where $\alpha = \sqrt{(1 - v^2/c^2)}$. The *point*-event on the x-axis occurring at this time would be distant

$$\frac{x_1x_2(W + V)}{Wx_1 + Vx_2}$$

from O, and we may call this the *equivalent point-event* of the line-event which is the collinearity of O, A and B.

As a practical example of this – provided the values are such that t' is not too large – we can suppose the Sun to be at O, and the components of an eclipsing binary, at the time of a collinearity corresponding to an eclipse, to be at A and B. If the star is in the plane of the ecliptic the Earth may then, at the proper time of observation, be regarded as fixed at the origin of the primed system. In that case, $x_1 \simeq x_2$, and the equivalent point-event is at the star. For an occultation of a star by the Moon, the equivalent point-event is at $x_1(1 + W/V)$, but observations from the Earth in that case have no direct significance.

Various possibilities are opened up which mathematicians might find useful for examination problems, but I have some pangs of conscience in making the suggestion because it is practically certain that some mathematical physicists will think there must be some physical meaning in this, and will be sure that they know what it is. They *may* be right in the first supposition, and will assuredly be wrong in the second. The significance of all this depends on the significance of the Lorentz transformation in kinematics, and in view of the considerations at the beginning of this article, and also the fact (among others) which I have pointed out elsewhere[3], that if the Lorentz transformation is applicable to kinematics, a moving clock goes both faster and slower than a resting one *in a single co-ordinate system*, I have no doubt whatever that the special theory of relativity is no longer tenable.

That idea is cooked to a turn, but its implications with regard to electromagnetic theory have scarcely reached the recipe stage. Either current electromagnetic theory is correct, and we must adopt Lorentz's *ad hoc* modifications of bodies caused by motion through the ether, or else a new electromagnetic theory is required – possibly on lines suggested by Ritz[4] or by Faraday[5], or something not yet thought of.

The most immediate way of reducing the possibilities would be an experimental test of Einstein's second postulate *by a purely kinematical method*, such as that which I suggested in *Nature*, 20 June 1959. An experiment by Bonch-Brucvich,[6] which appears to show that beams of light from opposite limbs of the rotating Sun travel to the earth with the same velocity, does not in fact do so, for the beams were reflected into a new direction before the measurement was made, and Ritz's theory also predicts that in that case their velocities should be the same. But if an appropriate experiment should show that light from relatively moving sources travels as a single beam, then Lorentz's theory, with its static ether with respect to which velocities can be measured, would seem to be the only possibility. If, on the other hand, the velocity of light depends on the motion of the source, then a larger cookery book is available. Good luck to the cook!

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2. *The Mathematical Theory of Relativity*, page 10: Cambridge, 1930.
3. *Philosophy of Science*, volume 27, page 233, 1960.

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5. *Phil. Mag.*, volume 28, page 345, 1846.
6. *Physics Express*, volume 3, No. 3, page 11, 1960; *Optika i Spectroskopia*, volume 9, No. 1, page 134, 1960.

[*Editorial footnote.* The notion that point-events are not enough to explain larger ones is consistent with the philosophy of holism. Also relevant is work by George Adams*. He has emphasised that it may be stimulating to apply three-dimensional projective geometry† to physics and paraphysics, in the following sense. In projective geometry there is a principle of duality between points and planes, which gives planes as much right to be considered basic as points. Following up an idea originally propounded by Rudolf Steiner, he proposes that this dual position is especially appropriate in the science of life: that, for example, it is better to think of forces as acting inwards from surfaces, in biological problems, than from points outwards. There may perhaps be a connexion between this duality and particle-wave duality in physics.]

108 The Light-mile‡
by ISAAC ASIMOV

If we are permitted to speak of a light-second as equal to the distance covered by light (in a vacuum) in one second and find it equivalent to 186,273 miles; why not speak of a 'light-mile' as equal to the time required for light (in a vacuum) to cover a

* ADAMS, GEORGE, and WHICHER, OLIVE: *The Plant Between Sun and Earth*: Goethean Science Foundation, Clent, Worcestershire, 1952; and *Die Pflanze in Raum und Gegenraum. Elemente einer neuer Morphologie*: Stuttgart, 1960; and a chapter by Adams in *The Faithful Thinker*: Hodder and Stoughton, 1961.

† For a *four*-dimensional projective theory, due to VEBLEN, O., HOFFMAN, B., and others, *see* the references given by WHITTAKER, E. T.; *History of the Theories of Aether and Electricity*, 1900–26: page 191, London, 1953.

‡ This article first appeared in *The Magazine of Fantasy and Science Fiction*: Mercury Press, August 1959.

distance of a mile and find it equivalent to 1/186,273 seconds?

Why not, indeed? It would enable us to use the speed of light in a vacuum, that universal constant, in measuring not only the vast distances encountered in astronomy, but also the ultra-short intervals of time dealt with in nuclear physics.

The only drawback is that 186,273 is such an uneven number. However, by a curious coincidence, the metric system comes to the rescue, since 186,273 miles is almost equal to 300,000 kilometres, so that a 'light-kilometre' is approximately equal to 1/300,000 sec. Or, to put it another way, $3\frac{1}{3}$ 'light-kilometres' equals 10^{-5} sec.

To express still smaller units of time, it is only necessary to consider light to be covering smaller and smaller distances.

We can begin by pointing out that a kilometre (km.) (10^5 cm.) is equal to a million mm.; that 1 mm. (10^{-1} cm.) is equal to a million millimicrons (mμ); and that 1 mμ (10^{-7} cm.) is equal to a million fermis. The fermi, named in honour of Enrico Fermi, is equal to 10^{-13}cm.

Well, then:

1 'light-km.' = 1,000,000 'light-mm.'
1 'light-mm.' = 1,000,000 'light-mμ'
1 'light-mμ' = 1,000,000 'light-fermis'.

Now we can relate these units to conventional units of time:

$3\frac{1}{3}$ 'light-km.' = 10^{-5} sec.
$3\frac{1}{3}$ 'light-mm.' = 10^{-11} sec.
$3\frac{1}{3}$ 'light-mμ' = 10^{-17} sec.
$3\frac{1}{3}$ 'light-fermis' = 10^{-23} sec.

Is there any point in going further down the scale? Probably not, at least now. One fermi is approximately the diameter of subatomic particles. A 'light-fermi' is therefore about the time required for a ray of light to travel from one end of a proton to the other. It is the time required for the fastest known motion to cover the smallest known tangible distance. Until the day comes that we discover something faster than the speed of light or something smaller than subatomic particles we are not likely ever to have to deal with a time interval smaller than the 'light-fermi'. As of now, it is the ultimate split of the second.*

Of what value is this new type of unit?

Well, to say that the half-life of one type of meson is of the order of 10^{-8} sec. and of another of 10^{-15} sec. leaves very little impression on the mind. A hundred-millionth of a second and a quadrillionth of a second fade into the common denominator of 'the unimaginably small'.

Say instead that the half-life of one type of meson is of the order of a 'light-metre' and of the other of a 'tenth of a light-micrometre' and visualisation is easy. We are used to thinking of distances and the difference between a metre and a tenth of a micrometre is a vivid one to our consciousness.

Furthermore, subatomic particles often move at speeds near that of light. A meson with a half-life of about 30 'light-mm.' will have a chance to leave a bubble-chamber track 30 mm. long before breaking down.

The one implies the other. By using conventional units, you might say that a length of track of about 30 mm. implies a half-life of about a trillionth of a second (or vice versa) but there is no obvious connexion between the two numerical values. To say that a track of 30 mm. implies a half-life of 30 'light-mm.' would be equally true and how neatly the two would fit together!

* It is approximately equal to a 'chronon'. *See*, for example, WHITTAKER, E. T.: *From Euclid to Eddington*: page 41, Cambridge, 1949, or *Brit. J. Phil. Sc.*, volume 9, page 317, 1959. *G. Ed.*

109 An Improvement for Eye-glasses

by OLIVER G. SELFRIDGE

Eye-glasses, as wearers will vouch, suffer from scratches and scrapes, especially on the convex side. A way to cure this would be to coat each side with a piece of uniformly thin plastic film which could be easily and cheaply replaced when it was damaged. If the thickness was just right it could also serve as a quarter-wave absorber to make the glass non-reflecting.

8

Mathematics, Logic, Probability, and Statistics

110 The Teaching of Mathematics and the Biogenetic Law

by GEORGE PÓLYA

1. *Rational Choice of the Curriculum?*

The rapidly increasing importance of science in the affairs of mankind should raise, and in many countries has already raised, the demand for teaching more science and for teaching it better. In some countries, the demand became public clamour, answering which various schemes and programmes sprang up for the improvement of science instruction the conflicting claims of which are now widely debated. The participants in this debate are supposed to have more than nodding acquaintance with science, and so it is disconcerting to observe how little of rational argument is brought into the discussion and how little rational the ways and means appear by which the decisions are finally reached.

The task is to choose the facts that should be taught, the sequence in which, and the methods by which, the facts should be taught – to choose, in short, the *curriculum*. And so the question arises: *Can we choose the curriculum rationally?* Or must we depend on the opinions of the few who happen to be in key positions? Or should we rely on the hazardous outcome of a blind struggle between inarticulate beliefs, emotional judgements, incomplete experiments, and old and new vested interests thinly veiled by specious arguments?

To choose the curriculum is a complex and responsible task. It should be obvious from the start that, in complex human affairs of this kind, decisions cannot be expected to be completely rational. And so, I must admit, I asked the wrong question; I should have asked something of the sort: Can we introduce some approximately rational element into the choice of the curriculum?

I must confess that I was led to this question by my acquaintance with (and my concern for) the present situation of the teaching of mathematics in the secondary schools. I do not want to discuss here this situation in detail, but I wanted to voice my concern because this concern, if it cannot excuse, can at least explain to some extent the very imperfect speculations which I am going to sketch.

2. *A Biological Comparison*

The struggle for life leads to the survival of the fittest. If you have to choose between rival curricula, let the teachers use them side by side, let the curricula freely compete, let them struggle for life and let the fittest one survive.

I do not think that we should reject a suggestion of this kind off hand. At any rate, let me quote a somewhat similar case: we have to choose between rival scientific terminologies or rival mathematical notations. The rival terminologies and notations may be used side by side for a while; yet if one of them is definitely easier to remember and more helpful in handling the concepts denoted than the others, in short, if it is definitely the fittest for use, there is a good chance that only the fittest will survive and the others will fall into oblivion.

Free competition may be a quite rational procedure to choose between rival terminologies and notations, and so the biological comparison may have some merit in this case – but I am afraid it has little merit in the case of the curriculum. Let me note two grave difficulties.

Let us consider two curricula, the first of which is much easier to teach, although the second may offer definitely more lasting benefit to the students. In free competition, very likely the first, the easily teachable, will survive and the second will fall into oblivion. And this will happen not only because the teachers are liable to prefer their convenience to the public good. There is another strong reason: convenience or inconvenience in teaching is readily observed whereas the good or bad effects on the student may show up only in the long run, years later, and may easily pass unobserved.

There is another deficiency. Elimination of the unfit by the struggle for life may be a very slow process. Yet, at present, science and technology advance so rapidly that it may be necessary to change the curriculum at short notice.

In fact, I mentioned the foregoing just to put the reader on guard against the next suggestion.

3. *Another Biological Comparison*

Ernest Haeckel's 'fundamental biogenetic law' states that 'Ontogeny recapitulates Phylogeny'. Ontogeny is the development of an individual animal, phylogeny is the evolutionary history of an animal species. Thus Haeckel's law means: the development of the individual animal is a (shortened) recapitulation of the evolutionary history of the species to which the animal belongs. That is, in the course of its development from the fertilised ovum to its adult form, the animal passes through successive stages corresponding to its successive ancestors: at each stage the animal resembles the corresponding ancestor.

As I am not a biologist, I cannot tell how far, in what sense, with what limitations or qualifications the biogenetic law is accepted, if it is accepted at all, at present. Yet even as a layman

I cannot help admiring how much light is shed by one concise statement on an inexhaustible variety of phenomena and I readily believe that Haeckel's law provoked an immense mass of useful research. In fact, it connects distant branches of biology, it prompts the embryologist and the palaeontologist to take mutual interest in each other's work.

Is the biogenetic law applicable to mental development? This is my question or, rather, the embryonic form of my question. It would be a little better to ask: *In what respects* is the biogenetic law applicable to mental development? How far, in what aspects, does the mental development of the human child parallel the mental development of the human race? This question has many facets and I raise it in the hope that some of the many potential sub-questions implied by it will not be barren. In fact, the question raised opens new connexions between distant spheres of interest, it could prompt the historian of science, the psychologist, the ethnologist and the educationalist to take mutual interest in each other's work. And so I have some hope that the question raised may provoke useful research.

4. *A Sub-problem of a Sub-problem*

Moved by certain recent goings-on in my sphere of interest, I am particularly concerned with one facet of the general question just raised: How far, in what respects should the secondary school *mathematics curriculum parallel the historical evolution* of mathematical science?

One could usefully devote one's lifework to some sub-problem of this sub-problem. Take for instance the role of mathematical proofs in the curriculum of the schools and in the history of science.

We may presume that, at the dawn of civilisation when the first relations between numbers and the first properties of geometric figures emerged, these relations and properties were accepted without proof as they are accepted today by children of kindergarten age. It is a long way hence to the mathematical logician who, in confining himself to an 'atomised' or 'anatomised' view of mathematics, regards a mathematical proof as a (long) sequence of (small) steps; each step consists in introducing a new formula of which we ignore the meaning but which we are supposed to put down strictly in accordance with certain axiomatically accepted formulas and with certain rules of inference fully specified *ab initio*. Now, between these two extremes there are other levels of proof which a primitive savage or a kindergarten tot or a professional logician may not understand or ignore – but in planning the curriculum we cannot afford to ignore them.

It seems to me that the study of those intermediate levels of proof would deserve to be pursued and that such a study needs a carefully balanced combination of at least three things: (1) bona fide experience in mathematical research; (2) sympathetic observation of people of your own standing and of children of various ages in the classroom as they struggle to convince themselves of a mathematical proposition, and (3) familiarity with at least some phases of the history of mathematics, sufficient to recognise and to document the then-prevailing level of proofs. Moreover, it may help a little to view the various levels of proof as successive stages of an evolutionary sequence.

Perhaps some day I shall be able to write about these matters more concretely. In the meantime, I wish to mention two references. In a brief passage, Descartes explains what he regards as the essence of deduction – it is a remarkably lucid description of an important level of mathematical proof.* In a recent article, Árpád Szabó attempts to reconstruct different levels that the proofs attained in Greek mathematics.†

* *Œuvres*, edited by Charles Adam and Paul Tannery, volume 10, pages 369–70.
† 'ΔΕΙΚΝΥΜΙ als mathematischer Terminus für "Beweisen"', *Maia*, volume 10, 1958.

111 A True Mathematical Statement that will Probably Never be Proved

by I. J. GOOD

The number $2^{3217} - 1$, which in the binary scale consists simply of a string of 3217 ones, is the largest known prime number to date.* Call it P. It exceeds the tenth power of Eddington's estimate of the number of particles in the universe.

Now consider the number $2^P - 1$, which consists of a string of P ones in the binary scale. It can be shown to have no factor smaller than $P + 1$, so it is certainly not divisible by any known prime number. On the other hand, it is so large that I confidently conjecture that it is composite (not prime). This conjecture could 'in principle' be verified in a finite number of steps. But the number of steps would be so large that it seems safe to say that it is physically impossible to perform them.†

112 Does it Make Sense to Speak of 'Good Probability Appraisers'?

by BRUNO DE FINETTI

A cluster of 'half-baked ideas' arises when one tries to answer such a question as: 'Does it make sense to speak of "good probability appraisers"?'

The whole discussion of this topic could have been completely developed in the light of the notion of subjective (or personal)

* RIESEL, H.: *Math. Tables and other Aids to Computation:* volume 12, pages 207–13, 1958.

† ibid., volume 9, pages 314–15, 1955.

probability, without reference to any particular device for the evaluation of such probabilities. Nevertheless, the opportunity to start musing and then thinking seriously about this kind of question was afforded by work concerning the construction of such a device, and the practical use and testing of it.

The particular device referred to in this paper is a simple example (perhaps the simplest) of a family of similar ones to be presented in a joint paper by L. J. Savage and the present author.* The first practical experiment on it is now under way at the University of Rome. This experiment concerns the probability evaluations made weekly by a group of people† in connexion with the football matches of the Italian (A-Series) championship.

The method is easily illustrated (hopefully, in a form sufficient for our present purpose) by stating the scoring rule applied in this probability evaluation competition together with a geometrical interpretation of it. The evaluation is expressed, for any match, by three non-negative numbers x, y, z adding up to 1 ($x + y + z = 1$) that indicate the probability for the home team to *win* (x), to *tie* (y), or to be *defeated* (z). Actually, only the percentages 0, 1, 2 ... 99, 100 per cent. are allowed. When the outcome of the match becomes known, a negative score is computed for the match according to the probabilities assigned to the two other possibilities, by the formula $-L = x^2 + y^2 + xy$ (in case they are x and y).

Geometrically, any point P in the equilateral triangle (Fig. 26) V_x, V_y, V_z represents a probability evaluation by its three

* Similar suggestions were independently made in the following papers, drawn to my attention after writing this one. (I have so far been able to obtain and read only the first of these.) (i) GOOD, I. J.: 'Rational decisions': *J. Roy. Stat. Soc.* series B, 14, pages 107–14, 1952; (ii) MARSCHAK, JACOB: 'Remarks on the economics of information', in *Contributions to Scientific Research in Management*: Berkeley, 1960; (iii) MCCARTHY, J.: 'Measures of the value of information': *Proc. Nat. Acad. Sci.*, volume 42, pages 654–5, 1956. The joint paper in preparation will take account of all these references.

† Some thirty students from the author's class in probability and actuarial mathematics, together with the author and three other people, all in the Faculty of Economics.

distances from the edges (that is, by its baricentric co-ordinates) x, y, z (where, necessarily, $x + y + z = 1$). The score is then minus the square of the distance of P from the vertex V_i representing the actual outcome. (Here the unit of distance is the edge of the triangle, whereas for x, y, z it is the height.)

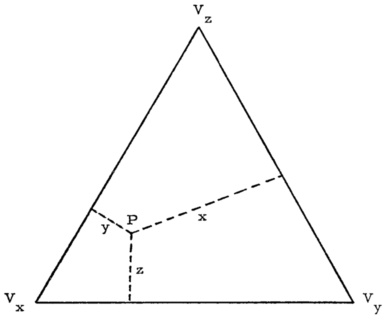

Fig. 26.

The scoring rule is constructed according to the basic idea that the resulting device should oblige each participant to express his true feelings, because any departure from his own personal probability results in a diminution of his own average score as he sees it. This is only a simplified way of exploiting the well-known conclusion of decision theory that the choice of a particular action among a sufficiently wide set of permitted possibilities is equivalent to an evaluation of the probabilities concerned.

The use of such a device seems to be a great improvement for psychological experiments on human behaviour in probability evaluation, in as much as it unifies the advantages of both the verbalistic and the behavioural outlooks* and avoids the usual shortcomings of both. It gives, in fact, a direct behavioural meaning to the familiar expression of a belief in terms of a numerical probability, without introducing questions about preferences in hypothetical conditions or between complicated and remotely related alternatives. Moreover, this method leads automatically to an overall comparison between the outcomes of different personal evaluations concerned with the same totality of events. The accumulated loss (that is, the sum of all penalties incurred in the individual forecasts) is indeed a thoroughly concrete measure of success.

This fact seems to bring somewhat closer the kinds of reasoning proper to the objective theory of probability, on one side, and to the subjective on the other, raising for both a set of new questions. The objectivists, who reject the notion of personal probability because of the lack of verifiable consequences of any evaluation of it, are faced with the question of admitting the value of such a 'measure of success' as an element sufficient to soften their forejudgement. The subjectivists, who maintain that a probability evaluation, being but a measure of someone's beliefs, is not susceptible of being proved or disproved by the facts, are faced with the problem of accepting some significance of the same 'measure of success' as a measure of the 'goodness of the evaluation'.

I do not venture to discuss the possible reactions of other people; as for myself, though maintaining the subjectivistic idea that no fact can prove or disprove a belief, I find no difficulty in admitting that any form of comparison between probability evaluations (of myself, or of other people) and actual events may be an element influencing my further judgement, of the same status as any other kind of information. It is quite possible and reasonable, I think, that I should take into account, for example, the fact that my own opinions in the past were unsuccessful on

* *See* SAVAGE, L. J.: *The Foundations of Statistics:* page 27, John Wiley and Sons New York, 1954, and elsewhere.

the average owing to an excessive trust in the chances of a particular football team. This could lead me to modify the evaluation I had spontaneously adopted. Or the fact that a given person obtained a very favourable score could incline me to put my future beliefs in better agreement with his. But, as with any other experience, these modifications would not be governed by a mechanical rule; it is, in each case, my personal judgement that is responsible for giving a weight to the facts (for instance, according to my feelings about the success of the other person being due to his skill and competence or merely to a meaningless chance).

Regardless of the different possible conceptual interpretations of the whole subject, a number of particular questions can be considered; let us mention and comment on a few of them.

How does the practice of probability evaluation based on the device improve our aptitude to express differences in beliefs as differences in numbers? The little experience I have had in the current experiment seems significantly affirmative. Each number (such as 68 per cent., 70 per cent., 73 per cent.) becomes familiar from usage in past cases, and the choice of numbers expressing an opinion becomes thereby less open to vagueness.

How does such a practice improve 'competence' or the capability of making 'reasonable' probability evaluations? Thus formulated, the question is, strictly speaking, meaningless, but it can be interpreted according to my point of view, expressed above, and probably according to any other more or less similar point of view. The difference between this question and the preceding one is, to clarify it by a comparison, that between being able to perceive a temperature more or less exactly, and the facility to translate it into a measure in some familiar scale. The question has very many facets, and I do not wish to enter into details. Briefly one should investigate: how the 'competence' of a given person may vary from field to field of events (for example, from Football Series A to B or C, or to meteorological, political, economical, or medical forecasting); how 'competence' might be influenced by other factors such as short or long reflection before writing down the figures, reading news or opinions of columnists about the subject, conditions of health or of spirit, etc.; how one's evaluations change if repeated after some delay in time (when one does, or does not, remember one's former evaluations); and how it is possible to explain agreements or systematic differences between independent evaluations by different people.

It is of course necessary to distinguish, above all, those people who are aware of the mechanism and the spirit of the device and who try to use it correctly to express their own beliefs from those who prefer, owing to lack of right understanding or of willingness, to behave as gamblers, for example, by trying to attain the maximum of the scores by giving 100 per cent. probability to the most probable case. The latter must be studied separately, as a pathological class.

Considering the 'skilful' people, as a group, what about their 'average opinion', that is, the evaluation in which each probability is calculated as the arithmetic mean of the corresponding evaluations of the individual members? Would such an average opinion be more successful than any single one? And what about the average of the few most successful members? In the current experiment, the 'average opinion' corresponds weekly to a rather high position, though all participants are included, even some 'gamblers', but the extent of the data is not yet sufficient for discussion.

Analysing the behaviour of some one person, say the one who ranks highest, might it be possible to infer some (probably unconscious) rule by which he takes into account the pertinent facts (for football, perhaps the outcomes of recent matches and present relative rank of each pair of teams)? Again, analysing rules of this kind as such, might some be discovered that would have been more successful than most or all of the contestants? If so, are there sufficient grounds to accept such a rule as 'significantly good' in the sense that we believe it will probably continue to work in the future in other circumstances, and how far say, for football in other countries or for other games like basketball and rugby in the same country?

I hope it will already be clear to the reader how many further questions of a similar kind should be added, were it not better to stop here, in this exposition, which has no immediate purpose other than to stimulate thought and work along the lines of such problems.

As for the importance of questions of this kind, it is not confined, I think, to the domains of psychology and the philosophy of probability and human behaviour but greatly concerns many thoroughly practical fields.

Often, important economic decisions depend on the answer of an expert; the opinion he is asked to reveal is, explicitly or implieitly, his evaluation of certain probabilities. This is particularly clearly brought out by G. J. Grayson,* discussing the geologist's evaluation of the suceess of a proposed oil-well drilling. Grayson says (on pages 255–6):

'Actually, operators are obtaining a form of personal probabilities from their geologists at the present time, although they do not refer to them by such a term.

'Numbers . . . are merely another form of language, permitting subjective judgement to be put into a more precise form, a form which is tractable when relating the expert's evaluation to other facets of the drilling problem.

'But . . ., how can such numerical personal probabilities be obtained? There are several possible ways.

'The simplest is to *ask* the geologist. . . . The geologist looks at the evidence, thinks, and then gives a figure such as 1 in 5 or 50–50. Admittedly, this is difficult. . . . Thus, several ways have been proposed to *help* the geologist to make his probability estimate explicit.

'The leading proponent of personal probabilities, Savage,

* *Decisions under Uncertainty: Drilling Decisions by Oil and Gas Operators:* particularly 'Obtaining Possibilities', pages 250–63, Harvard University, 1960.

proposes what seems to be the most workable method. "One can, namely, ask the person, not how he feels, but what he would do in such and such a situation." Accordingly, a geologist would be confronted with a choice-making situation.'

Several hypothetical gambles are presented by Grayson as useful devices 'in helping the geologist to state the personal probabilities that actually exist in his mind'. The device illustrated here is but another with the same purpose. The fact that it can be applied as a 'punitive device' to 'discourage falsification', without recourse to complicated or hypothetical bets, and avoiding the shortcomings of other methods (clearly illustrated by Grayson), raises the hope that it will be found suitable for every practical application of the kind illustrated by the geological consultant.

113 The Logic of Logic

by A. A. MULLIN

What would be the nature of a discipline that would do for logic what logic seems to be doing for mathematics?

114 Scientific Inference

by CHRISTOPHER S. O'D. SCOTT

You are given a large number of identical inscrutable boxes. You are to select one, the 'target box', by any means you wish which does not involve opening any boxes, and you then have to say something about what is in it. You may do this by any means you wish which does not involve opening the target box.

This apparent miracle can easily be performed. You only have to select the target box at random, and then open a random sample of other boxes. The contents of the sample boxes enable you to make an estimate of the contents of the target box which will be better than a chance guess. To take an extreme case, if none of the sample boxes contains a rabbit and your sample is large, you can state with considerable confidence: 'The target box does not contain a rabbit.' In saying this, you make no assumption whatever about the principles which may have been used in filling the boxes.

This process epitomises scientific induction at its simplest, which is the basis of all scientific inference. It depends only on the existence of a method of randomising – that is, on the assumption that events can be found which are unrelated (or almost) to given events.

It is usually thought that scientific inference depends upon nature being orderly. The above shows that a seemingly weaker condition will suffice: scientific inference depends upon our knowing ways in which nature is disorderly.

115 The Bitter End

by I. J. GOOD

Much of the appeal of Fisher's case[1] that smoking of cigarettes may not be a cause of lung cancer was based on the observation that, in the first half of the retrospective inquiry by Bradford Hill and Doll,[2] inhalers less often got lung cancer than non-inhalers, the significance level being about 1 in 80. Let us provisionally call this effect the 'inhalation paradox'. Fisher ironically demanded why the investigators had not recommended that smokers inhale for the good of their health![3]

It has recently been suggested[4] that inhaling might *indirectly*, in some populations, tend to prevent lung cancer since it might encourage the smoker to throw away a longer cigarette butt. The butt of a cigarette is less pleasant to smoke than the rest of it (as is that of a cigar), and this unpleasantness may well be accentuated by inhalation. It may be recalled that certain chemical constituents become more concentrated the farther down the cigarette is smoked, namely the polycyclic hydrocarbons including benzpyrene, which can in larger quantities cause skin cancer in animals.[5] Note that the reasons for the unpleasantness of the butt might be independent of the cancer-producing chemicals (so that the butt of a cigar might be less harmful than that of a cigarette).

The conjecture then is that *inhaling is positively correlated with butt-length*, within a tolerably homogeneous population. It might be thought that, even if this conjecture is true, the extra damage from inhaling the first part of the cigarette would more than counterbalance the avoidance of the last part, but it is easy to construct an infinity of hypothetical mathematical models for which this objection would not be valid. Our conjecture would be fairly easy to test, but meanwhile it is a very reasonable explanation of the inhalation paradox. It gives support to the recommendation that, *whether he inhales or not*, the smoker should avoid smoking cigarettes to the bitter end. Perhaps a very small amount of explosive should by law be inserted in the butt in order to encourage smokers to adopt this recommendation.

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3. The total of Hill and Doll's evidence was rather less significant, but it was still of the same nature and certainly did not indicate a positive correlation between lung cancer and inhaling. On the other hand, there has recently been some evidence of a positive correlation in Paris. (SCHWARTZ, D. and DENOIX, P. F., *Sem. Hôp. Paris*, volume 33, pages 3630–43, 1957.) Variation from

country to country is not surprising since the taste and odour of cigarette smoke varies from one country to another, and a high cost may encourage people to smoke more of the cigarette. (One Scottish friend attributed his habit of smoking nearly all of a cigarette to his nationality!) Also pertinent are the reasons for international variability of the correlation between the numbers of cigarettes smoked and the death rate from lung cancer.[5]

4. GOOD, I. J.: *J. Roy. Stat. Soc.*, volume B. 23, page 29, 1961.
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116 Record Sequential Trials

by MERVYN E. WISE*

In a sequential trial the objects or subjects are tested one by one. The testing is costly and we want to stop it as soon as the results look definite enough.

In the Wald (1947)[7] system† shown in Fig. 27 the trial is on a batch of articles which are either good or bad and only a sample from the batch can be tested. The middle line represents a standard batch. If the number of defectives found during the trial reaches a given excess over what would be the standard number, then the batch is accepted, and if another (or sometimes the same) number below the standard is reached the batch is rejected. The total of bad units found is plotted after each test and the decision depends on whether the top or bottom line is reached first.

The test could be on a new drug, which either does or does not cure. If its effect can be measured, a scheme like the one shown in Fig. 28 for a clinical trial should be better. There are many

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† Essentially the same system was suggested by Turing (1940) for a classified application. *G. Ed.*

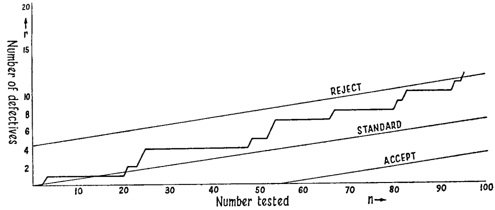

Fig. 27. A sequential scheme for accepting or rejecting batches. Each unit tested is found to be either 'good' or 'bad'; the number (r) of bad units (defectives) is plotted against the total number of tests (n), and the testing is stopped when the graph reaches one of the boundaries. The form and location of boundaries depend on criteria introduced by Wald.[7]

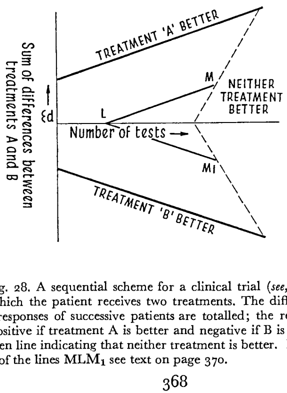

Fig. 28. A sequential scheme for a clinical trial (*see*, e.g. Armitage[2]) in which the patient receives two treatments. The differences between the responses of successive patients are totalled; the response is scored as positive if treatment A is better and negative if B is better. Note the broken line indicating that neither treatment is better. For the explanation of the lines MLM_1 see text on page 370.

others. (*See*, e.g. Armitage[2].) Two treatments are given to each patient, and the differences between the responses are added and plotted as they come in. If the total reaches a boundary the winning treatment is given to all future patients. If the middle broken line is reached the result is adjudicated a draw.

These ideas seem obvious, but it is harder to make sense of statistical data when the number of tests is not fixed in advance and depends on the results during the trial. The reasons for choosing any one system are well understood for batch-testing. But for medical trials I think the wrong questions are often asked and I would like, in this note, to suggest better ones.

The difference is that in industry we can often estimate the cost of each test, of losing an article, of losing a whole batch, and even of passing on a bad batch that ought to have been stopped. We can calculate this risk, and then choose the scheme with the lowest expected total cost in the long run.

This is seldom or never possible in medical research. We might be comparing an old with a new treatment, and be willing to accept only a small risk of wrongly concluding that the new treatment is better. A simple system can be worked out, for which the probability of reaching the upper boundary is 5, or $2\frac{1}{2}$, or 1 per cent., when there is really no difference between the treatments. The smaller the percentage risk that is chosen, the farther out the line is, and then the chance of detecting a real difference from a reasonable number of tests gets correspondingly smaller. How do we decide which line to choose?

In fact, is this really what we need anyway? Suppose we record each result as it comes in, and that we know little or nothing about sequential theory. We should still plot the totals, and whenever an extreme total is reached we would stop and consider it. But we do not know where the boundary lines are, hence we cannot as yet decide whether the accumulated total is extreme enough to justify stopping the trial. There is a whole series of possible boundary lines. For the scheme of Fig. 27 these would all be parallel to the middle line; for Fig. 28 it is less obvious, but the upper boundaries might be made parallel to one another and the lower ones also, sloping, of course, the other way. Whichever boundaries are finally chosen, we always stop when, and only when, one or other boundary is *first* reached. If this happens at, say, the *n*th test, in the case of Fig. 27 the distance from the middle line is then greater than any previous value. In the second case the distance from one of the middle sloping lines LM, LM_1 similarly breaks its previous record.

This suggests that there is no need to have any boundary lines in a sequential system. Instead of this, the rule can be to stop whenever a record is broken.

Having thus stopped, we need something to take the place of the decision rules. In the first example, we want to estimate the true proportion of defectives (or of cures), and decide whether it is much better or worse than the standard. We decide that it is better if the observed result, or this and all better results, are unlikely enough to have arisen by chance when the real improvement is zero; the probable limits for the true proportion must not then extend to zero (Fig. 29). In statistical terms, given this estimate, we ask how much of the lower end of the confidence (or the fiducial or the posterior probability) distribution of the true value – what proportion of its tail area – is below the standard, or below zero. Similarly, if we hope for an improvement and the results suggest none, we look particularly at the upper end of the confidence (or etc.) distribution.

When the response is a continuous variable, its mean could be estimated, or, for the scheme of Fig. 28, the mean difference between the two treatments. A more suitable measure of a record might be obtained from a set of boundary lines through the origin, as in Fig. 30. Then what is plotted could be the *square* of the observed sum of differences, with the same sign, + or −, as the sum. The record being broken will be the square of the ratio of Σd to its standard error. If this standard error is not known, we could plot $\pm(\Sigma d)^2/\Sigma d^2$. The boundary lines in the set are now all horizontal, and we have a generalisation of the

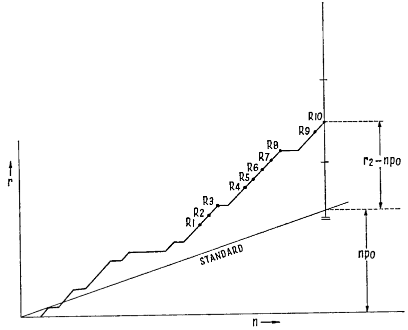

Fig. 29. By the proposed new rule, the test can be stopped at $R_1, R_2, R_3, R_4 \ldots$ up to R_{10}. At each stop, limits of estimates of the true proportion of defectives could be calculated. In the figure are shown possible limits of ± one standard error of the estimate of the expected number of defectives after n tests. The outer limits could be like $2\frac{1}{2}$ and $97\frac{1}{2}$ per cent. confidence limits, but they will not be the usual ones as they must allow for the bias in stopping at a record value. (The 'standard' number of defectives is np_0 and r_2 is (say) the second observed number considered in detail.)

well known sequential *t* test (*see* Armitage,[2] also for other references). Evidently many existing schemes could be generalised.

We must also have a rule for stopping when the result looks most like a draw. Clearly we will do so whenever the plot reaches a middle line, or a zero line as the case may be (Fig. 31).

If such record rules can be put into practice, more and better information will be obtained from costly experiments. But before this can be done some awkward estimation problems will have to be solved; it is not even certain that the problems have useful solutions. Statisticians do not completely agree over the con-

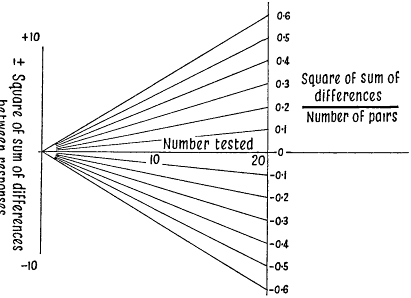

Fig. 30. In this system we can stop at any point where the polar co-ordinate is higher than all previous values.

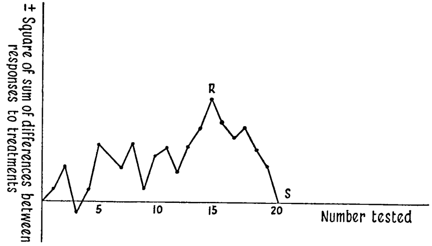

Fig. 31. If the estimate based on the record value at R was not definite enough we might go on testing and then reach S. Then we need an objective estimate of the probable limits of the true mean treatment difference, allowing, if we can, for the bias introduced by stopping when zero is reached.

clusions to be drawn from a sample of fixed size; but at least these do not depend upon the order in time of the results. Our sequential estimates, given that a record has just been broken, must always do so.*

The estimates of this kind will therefore not be the same as the usual ones. In a series of measurements or tests subject to random variation, if we stop at points most favourable to a hypothesis, we have a better chance of confirming it than if we stopped after a number of tests determined in advance! Similarly we could wait until the plot of accumulated totals crosses a given line, and present the result as confirming that it should lie close to this line. If this, as it stands, is not lying with statistics, it is suppressing some of the truth.[1,5,6] But, of course, any objective estimate based on record values, or on crossings of predetermined lines, will have to allow for this kind of bias.

There is also the awkward question of what can be concluded if the trial has to be stopped because no more results are coming in, or because results of the record estimates in one trial have not been definite enough. We cannot specify an upper limit to the number of tests.

It would be advantageous to make the new procedure still more flexible. There are other plausible measures of accumulated results besides the ones given so far, and an extreme total could be a record when plotted in one way but not in another.

If these problems can be solved, it will be by professional statisticians. The medical or other research worker would have to consult his statistical colleague every time he stops the trial. But since this record sequential analysis is only justifiable for costly trials it would be penny wise and pound foolish not to do so. One of our most distinguished medical statisticians has stated that they (the statisticians) should teach the medical man to be his own statistician, in other words, 'teach themselves out of their

* In a Bayesian approach, the order in time of the results is irrelevant, even in a sequential experiment, provided that the 'trials' are statistically independent. *G. Ed.*

job'.* Some analyses can certainly be performed and (more or less) understood by all medical workers. But nowadays research workers seldom build their own apparatus or prepare their own sections and slides. Why should they be expected to do all their own statistical analysis? Provided that what goes into the analysis is clear, and that what comes out at the other end can be reasonably well understood, it matters little to the research worker how difficult is the stage in between.

Existing sequential trials are a real contribution to biological research, and the rules for making the decisions are easy to carry out. But even when the results look very definite it is hard to assess their precision, and one is left in the air if a plotted path nearly reaches a boundary. It is better that the statisticians should have the worry of improvising a solution to the right problem than that a precise and elegant technique should be applied to the wrong problem.

I would like to thank Dr I. J. Good for showing me some of his unpublished correspondence and pointing out the published discussions on the very relevant question of whether the statistician can cheat by stopping just when the results best fit a hypothesis, or as he puts it, 'by pretending he has a train to catch'.

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* *See* BRADFORD HILL, 1956, 1960. His second contribution to the symposium on controlled clinical trials (*see* reference) is an interesting discussion of the desiderata and the difficulties.

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9

Technical Ideas

117 Paraffin by Fermentation

by N. W. PIRIE

A great deal of effort has gone into the study of the microbial production of methane from organic wastes but the process lacks general appeal because of the difficulty of storing and transporting a gas that is being made in small units at scattered places. Methane has, however, the great advantage over fermentation products such as ethanol and acetone that it is immiscible with water so that it separates from the fermentation fluid spontaneously without the need for distillation. If micro-organisms could be found, or trained, that could knit as well as ferment and make longer chain hydrocarbons, the process would be much more attractive. On the conventional picture of petroleum formation this can happen, and organic residues such as cellulose and proteins get converted to hydrocarbons. The rival pictures – that petroleum is derived from the oils initially present in plants* or is of inorganic origin† – do not at present find much favour.‡ But the possibility of getting butane or some higher hydrocarbon directly by fermentation does not depend on the mechanism of oil

* KROPOTKIN, P. N.: 'The geological conditions for the appearance of life on the Earth and the problems of petroleum genesis', in *The Origin of Life on Earth* (Eds. CLARK, F. and SYNGE, R. L. M.), page 84, Pergamon Press, London, 1959.

† WENT, F. W.: 'Organic matter in the atmosphere and its possible relation to petroleum formation.' *Proc. N.A.S. Wash.*, volume 46, page 212, 1960.

‡ But *see* pbi No. 118. *Eds.*

formation; it is in principle possible to select or create a culture that would do it and it would be very advantageous if that culture were then used to seed tanks of refuse.

118 The Duplex Origins of Petroleum

by SIR ROBERT ROBINSON

Summary. It is believed that the arguments for a biological *and* an abiological origin of petroleum are alike incontrovertible. Hence a duplex origin of mineral oil is envisaged.

P. N. Kropotkin (Int. Union of Biochemistry Symposium on *The Origin of Life on the Earth*, Moscow 1957, c.f. Proceedings in English-French-German, Pergamon Press, 1959 – Later references are to I.U.B. Symp. No. 1) has pointed out that the idea of the inorganic origin of simple hydrocarbons first postulated by Mendeleev was warmly approved in the later nineteenth century but has been overlooked since about 1920, largely on account of the emphasis laid on the assumed biological origin of petroleum. Quite recently there has been a revival of interest in abiological origins of naturally-occurring hydrocarbons and largely on geological grounds. These concern the great depths to which the oil- and gas-bearing sources descend (op. cit. page 86) and the non-coincidence of petroleum sites with those rich in solid organic matter. This is very interesting, since the writer has always been assured that the geological arguments in favour of the solely biological origin of petroleum are incontrovertible. Nevertheless, while technically not equipped to discuss the geological aspects, he found the full biological theory quite unacceptable on organic chemical grounds. In the significant development of Kropotkin's discussion, some of these difficulties are clearly pointed out. For example, the 'saturated' character of petroleum constituents is rightly considered inconsistent with a modification of biological material at relatively low temperatures. The writer is expressing only his personal views, which are based on published information.

Facts Suggesting a Biological Origin of Mineral Oil

First we may consider the chief proofs (a strong word used advisedly with full implication) of biological origin, remembering however that these do not necessarily relate to the whole of the petroleum samples but possibly, even probably, to only a part of them:

1. Optical activity is noted in mineral oils and this is found at certain peaks in the carbon-number distribution. One of these could arise from a steroid or more generally polyterpenoid precursor.

2. There exists a predominance of naphthenes with one or four rings per molecule. The latter again suggests a steroid origin.

3. Petroleums contain porphyrins which are those related to both animal and vegetable pigments. One is mesoporphyrin IX and another is desoxophyllo-erythro-aetioporphyrin, especially significant since it contains the characteristic cyclopentene ring of chlorophyll.

Our knowledge of this field is due to A. Treibs (1939), who noted that mesoporphyrin was completely decarboxylated when heated in a neutral solvent at 240°C. He concluded that the oil had never been heated above about 200°C.

Leaving out of the argument the effects of chemical modifications (e.g. occurrence as salts or esters) this experiment could be accepted as an indication that the oil in question was never heated above 200°C *after the introduction of the mesoporphyrin.* The central co-ordinated metal in the oil-porphyrins is usually vanadium (as vanadyl VO – O – VO), associated with two porphyrin residues, but nickel has also been found to function in this manner in oil from fresh-water sediments.

4. A zigzag distribution of hydrocarbons as we ascend the

C-number series is conclusive evidence of biological origin, that is of the whole *or* a part of the oil. The hydrocarbons with an odd number of carbon atoms in the molecule are often present in much larger relative amount than the even-C-numbered hydrocarbons on either side. This odd-C-predominance is doubtless due to biogenesis from natural fatty acids which normally contain an even number of carbon atoms in their molecules. In its turn this regularity among the fatty acids has been shown, by isotopic tracer techniques, to be due to their biogenesis from acetic acid, $C_2H_4O_2$, without loss of carbon (Rittenberg, Bloch *et al.*). The conversion of fatty acids to hydrocarbons is by loss of one carbon atom from two molecules (theoretically a carbon could be lost from a single molecule). Reduction of the ketone so produced affords the hydrocarbon.

A saturated paraffin found in many plants is hentriacontane, $C_{31}H_{64}$, the composition of which was established by Channon and Chibnall (1920). A common fatty acid is palmitic acid, $C_{16}H_{32}O_2$, which is $C_{15}H_{31}{\cdot}CO_2H$ and two molecules of which, by loss of H_2O and CO_2, afford palmitone $C_{15}H_{31}{\cdot}CO{\cdot}C_{15}H_{31}$. Reduction of CO to CH_2 then gives the straight-chain $C_{31}H_{64}$. Naturally the odd number of carbon atoms in the hydrocarbon is no proof that the precursor was an even carbon-numbered fatty acid. Clearly $2n-1$ is odd for all values of the integer n. A specially high content of C_{29} is noted and this could be derived from a C_{15} acid, or from coupling of C_{12} and C_{18}, that is to say, the widely-distributed lauric and stearic acids. The detailed discussion of the relative merits of these possibilities would take us too far afield. On the whole, the use of odd-numbered fatty acids seems preferable and it is perhaps relevant to note that the straight-chain primary alcohols of certain Australian grasses include all carbon numbers over the range examined. Furthermore, the range of fatty acids in phytoplankton is wider than in plants, and odd-numbered unsaturated straight-chain substances are metabolic products of basidiomycetes (E. R. H. Jones *et al.*, Oxford).

The occurrence of odd-predominance is not only evidence of biogenesis of the hydrocarbons but also shows that the biochemical system that operated was substantially the same as that which exists today. The whole thing has a rather modern look though it must be admitted that we do not know how far back modern biochemistry extends: possibly millions or tens of millions of years. However, crude oils are certainly much older, at least 500 million years, and this could be a gross underestimate on an abiological basis.

Taking these four indications together, the biological origin of petroleum cannot be doubted, again with insistence on the proviso that the proof may apply only to a *part* of the oil.

P. V. Smith jun. (1954) found hydrocarbon oils, approximately CH_2 in composition, in recent marine sediments. One example was the Grande Island core, which was dated by ^{14}C isotopic analysis as 11,800 to 14,000 years old. His extensive researches were hailed as strongly supporting the biological hypothesis, since it was clearly proved that marine organisms could generate hydrocarbon oils in such sediments. Strong odd-predominance was observed. Later studies in several different laboratories showed, however, that these recent oils differed in many characteristics from the oils from ancient sediments or reservoirs, which may be termed 'crude oil'.

Difficulties in Accepting a Biological Origin

In order to explain these divergencies a hypothesis of modification of biologically-produced oil into 'crude oil' through intermediate types has been widely approved and may be said to be the currently accepted view. Unquestionably the hallmarks of bio-oil gradually become less legible as we descend the rock strata to greater depths and correspondingly greater ages. This applies especially to odd-predominance and optical activity; the question of porphyrin content is less clear owing to a lack of adequate comparative analytical data. The alternative of a primeval abiogenic petroleum with zero optical activity and zero odd-predominance, which became more and more admixed with bio-products, seems in much better accord with the facts.

It is this conception of tailoring bio-oil to fit 'crude oil' that the writer rejects as being unacceptable on organic chemical grounds. Bacteria and radiation as modifiers have been considered and rejected as inadequate. The general view seems to centre on a rather vague idea of catalysis. It is quite imprecise and there are no known organic chemical analogies for the type of changes that must be assumed, at least that is so under the conditions that obtain. Aluminium silicates have been mentioned as possible catalysts. Even assuming that these could operate under wet conditions, all experience suggests that they would effect much branching of the chains and would produce far more individual hydrocarbons than are actually found. Complex as the composition of petroleum may be thought, it must be regarded as a simple mixture when one considers the thousands of isomerides that are possible or that could be formed in a real catalytic free-for-all. The straight-chain hydrocarbons need to be degraded by loss of one terminal carbon atom if odd-predominance is to be ironed out. Unfortunately we have no inkling how this could be done in accordance with established transformations. It is easy to write $R{\cdot}CH_3 \rightarrow R{\cdot}CO_2H \rightarrow RH$ and these changes have been accomplished under special conditions (e.g. oxidation of hydrocarbons in presence of Co and other catalysts for the first stage and electrolysis of salts and various pyrolytic methods for the second stage) but it remains true that this sequence of reactions is a very unlikely one under the circumstances contemplated in the hypothesis of bio-oil modification.

The Nature of Crude Oils

It is well known that crude oils vary very much in composition from place to place. For example, Pennsylvanian oils are largely paraffinic whereas much of the East Indian oil is of aromatic character, at least in contrast to other sources. Nevertheless, all crude oils are essentially saturated in the sense of difficultly reducible. The formal unsaturation is associated with alicyclic or aromatic structures. Sulphur is present as cyclic sulphides or thiophenes and nitrogen as pyridine derivatives. To all these statements there are exceptions of the kind that prove the rules.

Chemically, 'crude oil' composition suggests that it was produced under powerful reducing conditions from a mixture of normal paraffins by chain-branching isomerisation, aromatisation, and involvement of S and N in cyclic structures resistant to hydrogenation. It should not be difficult to synthesise 'crude oil' by catalysis of Fischer-Tropsch hydrocarbons at the appropriate temperatures and pressures.

Whereas the recent biologically-produced oils show odd-number predominance, this is all but absent in crude oil. Apart from a small anomaly at C_6 (which is a little low) and perhaps C_{29} (a very small increase above C_{28}) the volume percentage of individual straight-chain hydrocarbons slowly falls from carbon number to higher carbon number in a remarkably regular fashion. There are, of course, branch chain hydrocarbons as well but the normal member of a group is present in far larger amount than any one branched isomeride. The latter, taken together, may slightly exceed in content the normal representative. The picture is plainly that of a product built up by synthesis, adding one carbon atom at a time to the end of the chain. This is followed by isomerisation, cyclisation and aromatisation.

The constituents of natural gas and the waxes follow the same pattern. In the former, the distribution curve is steeper (than for oil) and in the latter it is flatter. Methane is the main constituent of natural gas, but it is followed by substantial amounts of ethane and higher hydrocarbons. In contrast, methane derived from the fermentation of cellulosic material contains a mere trace of ethane.

The Duplex Origin of Oil

The saturated character of petroleum appealed to Kropotkin (I.B. Symp. No. 1, page 92, cf. E. I. Klabunovskiĭ, *Origin of Life*, page 158) as an argument *contra* the biogenic origin of the oil and this is undoubtedly a strong argument. In fact crude oil is not unlike coal hydrogenation oil if due allowance is made for the different starting point. The writer is greatly indebted to Dr R. Holroyd for information concerning the composition of oil made by the hydrogenation of coal and creosote oil. The theories of biological origin of petroleum do not account for the characters of the older crude oils, except by processes which are unacceptable to organic chemists. The alternative of a primeval abiogenic petroleum with zero optical activity and zero odd-predominance, which became more and more admixed with bio-products, seems in much better accord with the facts.

Possible Syntheses of Hydrocarbons

The starting point could be either carbon dioxide in strongly reducing conditions, or methane under dehydrogenating eonditions. It goes without saying that appropriate catalysts would be required as well as elevated temperatures and possibly pressures above the normal. The further synthesis might go via CH_4 or more directly from CO and a hydrogen source by a Fischer-Tropsch type of proeess. As regards CH_4 in the primeval atmosphere, some writers have assumed its presence in quantity but more lately it has been considered a possible but minor constituent. Valuable detailed discussions of this and related questions have been contributed by A. P. Vinogradov, and by V. A. Sokolov and by B. Yu. Levin (I. B. S., No. 1, pages 23, 54 and 67 respectively). If submitted at any stage or in any place to dehydrogenating conditions, be they associated with heat, radiation, or chemical oxidation, higher hydrocarbons could be formed. There are so many possibilities that it is hardly worth while to discuss them in detail. The steam-methane reaction gives CO and H_2 and thus affords the basis of a Fischer-Tropsch synthesis of normal paraffins. It may be pointed out, however, that attack of hydrocarbon by radicals is rather an unlikely mode of chain extension of complete hydrocarbon molecules. Doering has shown that these reagents are so 'hot' that they attack all positions indiscriminately, so that the yields are controlled by the relative numbers of hydrogen atoms in similar positions in the molecule. Thus methane should give ethane and ethane, propane, $CH_2{\cdot}CH_2{\cdot}CH_2$. Propane (8 mol.) would give 6 mol. of *n*-butane for every 2 mol. of isobutane and *n*-pentane (12 mol.). $CH_3{\cdot}CH_2{\cdot}CH_2{\cdot}CH_2{\cdot}CH_3$, the case fully examined, gave 6 mol. *n*-hexane to 4 mol. of 2-methylpentane and 2 mol. of 3-methylpentane, on reaction with methylene (carbene). As the chain lengthens the proportion of normal isomeride produced is theoretically lowered and, for example, at C_{10} it has become only 27·3 per cent. of the total.

Other reactions that might be considered are the oxo-process and the addition of carbenes to α-olefins followed by pyrolysis of the alkylcyclopropane to a higher α-olefine and later hydrogenation to a paraffin. The difficulty inherent in these suggestions is the awkward succession of reactions and the non-occurrence of α-olefines in crude oils. As we find the hydrocarbons saturated, it is hard to accept an idea which requires unsaturated intermediates, since the latter would hardly survive long enough under the conditions.

Petroleum in Relation to Organisms

It is now thought that the earth is about 4–5 x 10^9 years old and that well-developed living organisms occurred before 2 x 10^9 years ago. There must have been primeval forms at a much earlier epoch and one may say that the origin of life occurred when asymmetric carbon compounds, probably polypeptides, found a chemical mechanism for reproducing their kind and thus proliferated. Small molecules led to larger ones, which were built into structures. The rudimentary living molecules and the first organised structures were surely non-porphyrinic but probably made use at an early stage of metal-containing catalysts and of oxidation-reduction processes. The subsequent biochemical evolution towards the carbon cycle was, from our point of view, the most important event in the history of the Earth.

The first stages in the synthesis of amino-acids and other simple organic compounds have been forcibly suggested by the brilliant experiments of S. L. Miller (1953, 1955, 1957). Physical activation of various mixtures of gases by spark or silent electric discharges afforded amino-acids and other substances corresponding in a most remarkable manner to clearly recognisable units of protein structures. In 1954 Miller used $CH_4 + NH_3 + H_2O + H_2$; he has since replaced the NH_2 by N_2 and, as might have been expected, the same products were formed although in lower yield.

P. H. Abelson (1956) applied the spark discharge to a mixture proposed by W. W. Rubey (1954) as a primitive earth atmosphere, viz. CO_2, CO, N_2, H_2O. No amino-acid was obtained but when H_2 was added, amino-acids were produced. Hence it is certain that a reducing atmosphere, as first suggested by Oparin (1938) and Urey (1952), is necessary. The actual composition of the primeval atmosphere is, of course, unknown. A. P. Vinogradov (loc. cit.) considers that the present-day composition of volcanic gases gives some hints, and the main constituents are H_2O, CO_2, CO, HCl, HF, H_2S, N_2, NH_3Cl and perhaps CH_4. Here the reducing constituents are somewhat poorly represented by H_2S and NH_3. In that connexion the role of metals must not be overlooked. The formation of carbides, nitrides and even hydrides might be intermediary in the production of CH_4, NH_3 and H_2. Since H_2 would tend to escape from the atmosphere, other reducing agents must have been effective and the most likely are NH_3, H_2S, CH_4 and even CO (e.g. $CO + H_2O \rightarrow CO_2 + H_2$).

Assuming a Miller synthesis as the first fundamental step, the second is the synthesis of peptides and here the recent experiments of Bahadur and Srivastava are significant. These workers have obtained peptides by irradiation of aqueous solutions of, e.g. glycine and aspartic acid (two of the products isolated by Miller). Parenthetically, Bahadur has obtained amino-acids from paraformaldehyde and nitrogen in an aqueous medium and in presence of colloidal molybdenum oxide, using an ordinary 500-watt electric bulb as the source of radiation.

The first non-chlorophyllic organisms may well have used primevally synthesised hydrocarbons as a source of reduced carbon and thus become associated with the oil. It is a splendid, but perhaps not very profitable, field for speculation to consider how this could have occurred and by what stages of biochemical evolution the present system was approached. The very long time available allowed of false starts and failures, but also of more than one successful development and these could have taken somewhat differing courses.

If we are justified in assuming an early association of primeval oil and rudimentary life forms, this would in all probability persist over a long period and in the later phases (still a very long time back) the organisms could contribute constituents to the oil; they having meanwhile passed to a higher type of organisation.

Distribution of Petroleum

This problem in respect of the later stages is one of the chief concerns of the research laboratories of the great industrial oil groups. In so far as source rocks and reservoirs are concerned, the matter is of the greatest economic significance. However, the problem of the origin of oil takes us back to earlier epochs and the only question that may be significant is whether the present distribution of natural gas, petroleum, bitumen, etc. prohibits the conception of an abiological origin of a part of the materials.

Admittedly many authorities, impressed by the 'direct' interpretation of the facts, have taken the point of view that observed distribution can only be reconciled with the purely biological hypothesis. Nevertheless, there has been a swing of the pendulum and the present writer cannot agree that the arguments adopted in this connexion by most writers in journals and reports devoted to oil topics are necessarily valid.

The suggestions of E. G. Baker are of considerable interest. He notes the solubility of hydrocarbons in water containing micelles formed from fatty acids and considers that these could have aided the selective distribution of oil constituents. The hydrocarbons would be released on dilution with water. This hypothesis represents one possibility in the redistribution of oil. If primeval oil was at one time on the surface of the ocean, it would probably find its way to the littoral and eventually into the sediments. There is no more difficulty in explaining the capricious distribution of oil than of diamonds, gold, uranium and of most other constituents of the earth's crust.

119 Parenteral Nutrition

by N. W. PIRIE

Biochemistry has now reached the stage that much of it is summarised in an elaborate series of cycles, and diagrams of them can be seen hanging on the walls of most biochemical laboratories. The outstanding feature is that this complexity is mainly used to restore a few key substances – adenosine triphosphate, diphosphopyridine nucleotide, flavine-adenine dinucleotide, etc. – to their original, reactive, state. These carriers reappear over and over again in different reactions but such a small number seem to be responsible for such a large proportion of our metabolism that it would be worth while trying to find means for restoring them by methods other than those that have evolved in the cell. Hints at methods by which this might be done come from the idea of Armstrong* and Szent-Györgyi† that a protein can be an electron conducting system, and of Karreman and Steele‡ that energy is transferred from system to system in the cell by radiation and resonance. If these ideas are well founded it should be possible to feed electrical or radiant energy into a system in such a way that it restores the exhausted intermediate.§ At present nothing is known about the way in which the energy released by one reaction is used to drive another but it seems to be established that this 'coupling' takes place. It is therefore not at present reasonable to dismiss electrical or radiant 'feeding' of a system as impossible; that will only be legitimate after a satisfactory explanation of normal energy transfer has been given.

120 Artificial Enzymes

by N. W. PIRIE

Almost all studies on enzymes are directed towards an interpretation of the metabolism of organisms that are of importance to us. As a result we study enzymes coming from one evolutionary sequence and they, so far, have turned out to contain protein. Bertrand tried to make artificial enzymes and had some success with laccase. Some examples of enzyme-like reactions catalysed by systems without protein were discussed by Langenbeck[1] and

* ARMSTRONG, H. E.: 'Studies on Enzyme Action. V. Enzyme Action as Bearing on the Validity of the Ionic Dissociation Hypothesis and on the Phenomena of Vital Change': *Proc. Roy. Soc.*, volume 73, page 537, 1904.

† SZENT-GYÖRGI, AVO: 'Towards a new Biochemistry?' *Science*, volume 93, page 609, 1941; and in *Muscular Contraction*, Academic Press, New York, 1947.

‡ KARREMAN, G. and STEELE, R. H.: 'Long-distance Energy Transfer by Resonance in Biology': *Biochim. Biophys. Acta.*, volume 25, page 280, 1957.

§ PIRIE, N. W.: 'The Maintenance of Life in Space Ships: Synthesis, Recycling, and the Steps Towards a Microcosm', in: *Biology of Space Travel*: *Inst. Biol. Symp.*, 1961.

more recent examples are: phthalocyanin or its decomposition products acting as hydrogenase,[2] lysine acting as an esterase,[3] and some of the lanthanides acting as phosphatases.[4,5] Similarly, apart from carbonic anhydrase, enzymes catalysing inorganic reactions have been little studied but there have been some publications on the biological breakdown of clay minerals[6] and calcium phosphate.[7,8] Work of this type will probably soon be extended with the use of more complex catalysts (which might as well be called enzymes) acting on a more diverse range of substrates, possibly in non-aqueous solvents, to give products having nothing whatever to do with biology. We are still too much obsessed by the biological origin of the conventional enzymes and fail to see that the apparent absence of systems with more diverse catalytic capacities may only be a consequence of the absence of naturally occurring substrates for them.

The synthesis of organometallic compounds would be one interesting and potentially useful direction for this work, another would be the enzymic synthesis of diamond. Normal enzymes can make such insoluble products as cellulose and sulphur; so insolubility is no obstacle. What is needed is a suitable substrate. Using the biological analogy this would be a phosphate or silicate that can give the end-product exergonically, and a catalytic surface that holds the substrate molecules in a suitable array and gets them sufficiently close together. An absorbing surface may be able to do this as effectively as the present high-pressure systems.

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121 The Riemann Hypothesis

by J. E. LITTLEWOOD

[The zeta function is defined as the sum of the *s*th powers of the reciprocals of all positive integers. It was first studied by the fantastic mathematical genius, Bernhard Riemann (1826–1866), both for its intrinsic interest and for its applications to the theory of prime numbers. The series is divergent when the real part of *s* is less than 1, but the function satisfies an equation which enables it to be defined over the whole complex plane. The Riemann hypothesis is that the only complex roots have real part equal to one half. It is known that the first thousand zeros satisfy this condition, so that a crude application of 'scientific induction' appears to support the hypothesis. Professor Littlewood here maintains that a deeper understanding shows the argument by scientific induction to carry little weight, since, even if the hypothesis were false, the observed results would have probability close to 1. *G.Ed.*]

I believe this to be false. There is no evidence whatever for it (unless one counts that it is always nice when any function has only real roots). One should not believe things for which there is no evidence. In the spirit of the anthology I should also record my feeling that there is no imaginable *reason* why it should be true.

Titchmarsh* devised a method, of considerable theoretical interest, for calculating the zeros. The method reveals that for a zero to be off the critical line a remarkable number of 'coincidences' have to happen. I have discussed the matter with several people who know the problem in relation to electronic calculation; they are all agreed that the chance of finding a zero off the line in a lifetime's calculation is millions to one against. It looks then as if we might never know.

It is true that the existence of an infinity of L-functions raising the same problems creates a remarkable situation. Nonetheless life would be more comfortable if one could believe firmly that the hypothesis is false.

122 A Question Concerning Fourier Series

by J. E. LITTLEWOOD

[A set of objects is said to be *countably infinite* if it is possible to count them, so that sooner or later any given object gets counted. The set of all real numbers is not countable, although the set of all terminating decimals is. A set of points on a straight line is said to be of *zero measure* if it can be covered by a countably infinite set of intervals of arbitrarily small total length. To say that $s_n(\theta) \rightarrow f(\theta)$ for *almost all* values of θ means that the exceptional values of θ form a set of zero measure; in other words, that if a value of θ is chosen at random then the probability is 1 that the Fourier series converges to $f(\theta)$. *G.Ed.*]

* TITCHMARSH, E. C.: *The Riemann Zeta-Function:* Oxford, 1951.

Does the Fourier series of a function $f(\theta)$ whose pth power is integrable converge almost always when $1 < p < 2$: does the partial sum $s_n(\theta) \to f(\theta)$ for almost all values of θ?

Call the affirmative statement S. My idea is to prove that the answer is no: S is false.

I used for long, in lectures and teaching, the slogan that it can pay to find out what is the worst enemy of what you want to prove, and then induce him to change sides.* There is a beautiful example of this in the theorem about a function $F(z)$, regular in the infinite half-strip

$$-\tfrac{1}{2}\pi \leqslant x \leqslant \tfrac{1}{2}\pi, y \geqslant 0,$$

and satisfying $|F| \leqslant 1$ on the boundary. Is it true that $|F| \leqslant 1$ inside the strip? The function.

$$F_o(z) = \exp(e^{-iz} - 1)$$

shows that the answer is no. On the other hand, if we add the very mild 'general bounding condition' $|F| \leqslant K \exp(e^{(1-\epsilon)y})$, then the answer is yes. The position is that F is either bounded or violently unbounded. Now in the (very beautiful) proof of this, the 'enemy' $F_o(z)$ plays an essential role.

Let $n(\theta)$ be any function of θ in $(0, 2\pi)$ with integral† values satisfying $0 \leqslant n(\theta) \leqslant N$, where N is large. Let

$$g(t) = \frac{1}{2\pi}\int_{-\pi}^{\pi} \frac{\sin\{n(\theta)(\theta - t)\}}{\theta - t}\, d\theta, \quad \text{and}$$

$$J = \frac{1}{2\pi}\int_{0}^{2\pi} |g(t)|^q dt, \text{ where } q = p/(p-1).$$

Can we choose $n(\theta)$ in such a way that J is large (with N)?

I expect the answer is yes, and if the answer *is* yes, then we disprove, not precisely S, but something a very little stronger.

* [Contrary to a common opinion among laymen, this kind of vague analogical thinking is typical among first-class mathematicians. Mathematics is not 'sums'. *G. Ed.*]

† This is not actually necessary, but it simplifies the present account.

Now it is known that for s_n of the special form in which $n = 2^r$ the corresponding (weaker) form of S is true.* Correspondingly, if the $n(\theta)$ in J is restricted to be of the form $2^{r(\theta)}$ (with positive integral $r(\theta)$), then the new J is bounded by an A. [We have, therefore, a valuable principle for anyone trying to prove S true: if his argument would work as well for $2^{r(\theta)}$ as for $n(\theta)$ (and this is true of most arguments), then it is doomed.]

$2^{r(\theta)}$ is the enemy of 'S is false'. My idea (to prove S false) is to relate $n(\theta)$ somehow to '2^n'. ['2^n', figures in a number of important Fourier series problems, and my pupils know what I am suggesting if I say merely '2^n'.] The most obvious line is to take $N = 2^m$ (m large), and express $\theta/2\pi$ in the binary scale as $a_1 a_2 \ldots a_m$, stopping at the mth place; and then to make $n(\theta) = (b_1 \ldots b_m)2^m$ depend on the sequence $(a_1, a_2, \ldots, a_m)$ [with $2^{r(\theta)}$ the sequence has one b equal to 1, the rest being 0]. This makes $n(\theta)$ constant over the intervals $(2r\pi/N, 2(r+1)\pi/N)$, which is all to the good.

Zahorski has just contended† that he has settled the long outstanding problem of the case $p = 2$ in the affirmative; S is true when $p = 2$ (his complete proof is not yet available). For this case we have $q = 2$, and the boundedness of J (with $q = 2$) is exactly equivalent to S. It is equivalent, again, to the boundedness of

$$\int_0^{2\pi}\int_0^{2\pi} \frac{\sin\{n(\theta,\varphi)(\theta-\varphi)\}}{\theta-\varphi}\, d\theta d\varphi$$

where $n(\theta,\varphi)$ is the smaller of $n(\theta)$, $n(\varphi)$.

* Zygmund, A.: *Trigonometrical Series:* volume 2, page 231, Thm. 4.4, Cambridge, 1959.

† Zahorski, Z.: *C.R.*, volume 252, page 2366, 1961. [*Stop Press:* he has now withdrawn his claim.]

123 Jupiter's Satellites
by J. E. LITTLEWOOD

The mean periods of No. 3 and No. 1 are in the ratio 4·05 approximately. (The three mean motions satisfy, exactly, $n - 3n' + 2n'' = 0$, but this is a well-understood 'pendulum effect'.) There appears to be a number of other near commensurabilities in the periods of satellites, with evidence of more doubtful weight; but there is no doubt about this pair of Jupiter's.

Two questions arise: (i) Is the existing state (and in the absence of dissipative forces) stable, in the sense that it will last a long (but not necessarily infinite) time during which the degree of commensurability remains of the same order of magnitude? (ii) How did the satellites get to where they are?

For the first problem there is an analogy: the Lagrange equilateral triangle configuration (Sun, Jupiter, Planetoid) is stable in the sense that a displacement of order ε remains of that order over a time like $\exp(A\varepsilon^{-\frac{1}{4}})$.* (It is conceivable that similar methods, or again rather different unpublished ones found by Swinnerton-Dyer, might deal with the much more complicated new problem.)

I am, however, more concerned here with the second question. If we suppose the present state to be permanent, then it cannot, in the absence of dissipative forces, have been arrived at from a substantially different one; for by Poincaré's 'Recurrence Theorem' it would sooner or later have to return to approximately the original state. (There is a similar difficulty about how the Trojan [minor] planets got where they are, near the third corner of Lagrange's equilateral triangle.) The natural inference is that dissipation forces must have been acting, at least in the past. The problem is now to deal with this mathematically.

* LITTLEWOOD, J. E.: 'The Lagrange Configuration in Celestial Mechanics': *P.L.M.S.*, volume 36, pages 525–43, 1959. *See also P.L.M.S.*, volume 40, Addendum, 1960.

There is a class of second order differential equations in which 'damping' can be balanced by 'resonance' to produce a strictly stable solution. The analogy with the present problem is imperfect, as we shall see, but the facts are suggestive.

The solutions of the equation

$$\ddot{x} + \tfrac{1}{6} x^3 = 0$$

are periodic and of the form $x = X(c, t-t_0)$, where c is the amplitude, and the period

$$P = P(c) = 2 \int_0^c \frac{dx}{(c^4 - x^4)^{\frac{1}{2}}} = \frac{A,}{c}$$

takes all values from ∞ to 0 as c increases from 0. If we now add a forcing periodic term and a damping one, each with a small factor k, the resulting equation is

$$\ddot{x} + k\dot{x} + \tfrac{1}{6} x^3 = ak \sin t.$$

There is 'resonance' when $c = c_0$, the value for which the period $P(c_0) = 2\pi$, and it is the fact* that eqn. (1) has a strictly stable periodic solution of approximate amplitude c_0, to which all solutions with initial conditions not too different converge.

In the astronomical problem, of course, there cannot be, as there is for eqn. (1), a strictly stable solution; we must look for one in which there is 'momentary' stability at any one time, the parameters changing slowly under the friction.

A first approach would be to look for a simpler model, which yet has the essentials in it. One could abolish Jupiter and consider two particles on two concentric circular rings, a small attractive force between them, and small friction by the rings. This I think will be too rigid, and the radii (or one of them) must be allowed some sort of 'play'. Or again, abandoning the idea of similarity in the *picture*, try to invent simple abstract differential equations which have the essentials.

* CARTWRIGHT, M. L. and LITTLEWOOD, J. E.: 'On non-linear differential equations of the second order II': *Ann. of Math.*, volume 48, p. 27, pages 472–94, 1947. (The equation there is slightly different from that of the text.)

Appendix. Months of Receipt

All contributions were received in 1961, in the specified months. When using this list no veteran kudologist will forget that there is no *single* idea that is absolutely new under the sun. Often the authors themselves will have said similar things before, sometimes years before, at least in conversation.

1 November
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Subject Index*

* In this index, often only one page is mentioned for articles that discuss a given topic on more than one page.

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