

BIOLOGY AND PSYCHICAL RESEARCH

BY PROFESSOR A. C. HARDY, F.R.S.

It is easy, but not very illuminating, to say that one thinks it possible that psychical research may revolutionize the outlook of biologists and that if it does, it must in turn have a profound influence on human thought as a whole. I believe the time has come to consider how *likely* such a possibility may be and how the two branches of research may be brought nearer together.

I want particularly to discuss the biological significance of telepathy but not especially the kind of telepathy which has seemed to have been established by the card-guessing types of experiment. Having said this I must hasten to say that I have hitherto welcomed all the card-guessing experimental work which has been developed by Dr Rhine and his school at Duke University and extended by a large number of workers in different parts of the world. I welcomed it, with many others, because it appeared to give unassailable statistical proof of the reality of telepathy which actually we felt convinced about upon other, but less easily assessed, evidence. I confess, and again I am sure others have felt the same, that I was a little uneasy that the same kind of statistical tests appeared also to prove clairvoyance which seemed *a priori* much more difficult to accept.

I found it much easier to imagine one mind being in touch with another mind, than to conceive of a mind being able to know what kind of design is on the under-side of a card which has never yet been seen by any living being. Then came precognition which seemed equally difficult to imagine, but apparently equally proved by the same kind of statistics. More recently still has come psycho-kinesis, the alleged influence of the mind upon falling dice which is again seemingly established by the scoring of the same sort of degree of probability above a chance result as may be found in the other fields. Whilst I think many of us have felt uneasy at the similarity of the evidence for these four very different alleged phenomena, it has remained for Mr G. Spencer-Brown of Trinity College, Cambridge, to suggest the alternative and simpler hypothesis that all this experimental work in so-called telepathy, clairvoyance, precognition and psychokinesis, which depends upon obtaining results above chance, may be really a demonstration of some single and very different principle. He believes that it may be something no less fundamental or interesting—but not telepathy or these other curious things—something implicit in the

very nature and meaning of randomness itself. I must not say more about this now, because Mr Spencer-Brown, who has come to work with me in Oxford, is only just in the middle of testing his hypothesis ; it is entirely his own idea and he will explain it in due course if his results continue to support it. I mention it here because, knowing of this work, I want to say that, whether or not the results of the card-guessing experiments may be shown to be due to something quite different from telepathy, there is nevertheless to my mind quite sufficient evidence to prove the existence of a true form of telepathy which seems to me likely to be of considerable biological significance. In passing, let me say that if all this apparent card-guessing and dice-influencing work should in fact turn out to be something very different, it will not I believe have been a wasted effort ; it will have provided a wonderful mine of material for the study of this very remarkable new principle.

It will not be inappropriate in an article for this anniversary number of the *Proceedings* if I carry my readers back to consult the first two or three volumes of the series ; actually, however, I do so not for any reason of sentiment, but because I believe some of the work in telepathy recorded there is as important as anything that has been done since. I particularly want to refer to the series of experiments carried out at Liverpool by Mr Malcolm Guthrie, J.P., and Mr James Birchall, a Headmaster and Honorary Secretary of the Liverpool Literary and Philosophical Society, between April 1883 and July 1885, during which period 246 experiments were made. The first short report of the early experiments will be found in Volume I, pp. 263 to 283, with fuller accounts in Vol. II, pp. 24-42, and Vol. III, pp. 424-452. Also in Vol. II, pp. 189-200, Sir Oliver Lodge gives a separate account of some of the experiments carried out under his supervision. Sir William Herdman also took part in some of the later ones. These investigations are particularly important because in all the experiments either Mr Guthrie, Mr Birchall, Sir Oliver Lodge or other responsible persons acted as the agent, i.e. transmitter of the impression, so that unless we suspect all of these gentlemen of fraud (for they all at times got positive results) there can be no question of the successes being due to trickery by the use of some code to transmit the impression to the percipient.¹ So important is it to

¹ In some other telepathy investigations recorded in Volume I of the *Proceedings*, i.e. the Blackburn-Smith experiments witnessed by Gurney and Myers, it was always Blackburn who was the agent and Smith the percipient ; Gurney and Myers never formed a part in the experiments themselves and usually Blackburn and Smith were in contact during part of each experiment. They are experiments in a different category to the

make clear the quality of the Liverpool experiments that I will quote at some length from Sir Oliver Lodge's account :

Perhaps it may not be considered impertinent, since it bears on the question of responsibility and genuineness, if I state that Mr Guthrie holds an important position in Liverpool, being a Justice of the Peace, and an active member of the governing bodies of several public institutions, among others of the new University College; that he is a severe student of philosophy, and the author of several works bearing on the particular doctrines of Mr Herbert Spencer. I may also say that he is a relative of Professor Frederick Guthrie, and that he has exhibited in this experimental research such care and systematic vigilance as might perhaps have been expected on Mr Francis Galton's principles, and such as would, if properly directed, have placed him in a high rank of experimental philosophers. I may also remind you of what he himself has here said, viz., that he is a partner in the chief drapery establishment in Liverpool, and that it is among the employés of that large business that the two percipients hereafter referred to were accidentally discovered.

Let it be understood that the experiments are Mr Guthrie's, and that my connection with them is simply this : that after Mr Guthrie had laboriously carried out a long series of experiments and had published many of his results, he set about endeavouring to convince such students of science as he could lay his hands upon in Liverpool; and with this object he appealed to me, among others, to come and witness, and within limits modify, the experiments in such a way as would satisfy me of their genuineness and perfect good faith.

Yielding to his entreaty I consented, and have been, I suppose, at some dozen sittings; at first simply looking on so as to grasp the phenomena, but afterwards taking charge of the experiments—Mr Guthrie himself often not being present, though he was always within call in another room, ready to give advice and assistance when desired.

In this way I had every opportunity of examining and varying the minute conditions of the phenomena so as to satisfy myself of their genuine and objective character, in the same way as one is accustomed to satisfy oneself as to the truth and genuineness of any ordinary physical fact.

I did not feel at liberty to modify the experiments very largely, in other words to try essentially new ones, because that would have been interfering with Mr Guthrie's prerogative. I only regarded it as my business to satisfy myself as to the genuineness and authenticity of the phenomena already described by Mr Guthrie. If I had merely witnessed facts as a passive spectator I should most certainly not publicly report upon them. So long as one is bound to accept imposed Liverpool ones and are suspect because of the extraordinary sequel in 1911 when Blackburn published a so-called 'confession' of trickery which later turned out to be probably bogus. There is a full account of this affair in the *Journal* of the S.P.R., Vol. XV, pp. 115-132. The question of fraud is discussed later on p. 108.

ditions and merely witness what goes on, I have no confidence in my own penetration, and am perfectly sure that a conjurer could impose on me, possibly even to the extent of making me think that he was not imposing on me; but when one has control of the circumstances, can change them at will and arrange one's own experiments, one gradually acquires a belief in the phenomena observed quite comparable to that induced by the repetition of ordinary physical experiments.

I am particularly interested in the transmission of designs and am reproducing in miniature in Figs. 1 and 2 all those diagrams published in Vol. II of the *Proceedings*; they have all been accurately traced from these facsimiles and then reduced. The six diagrams, originals and reproductions, given in Fig. 1 were a

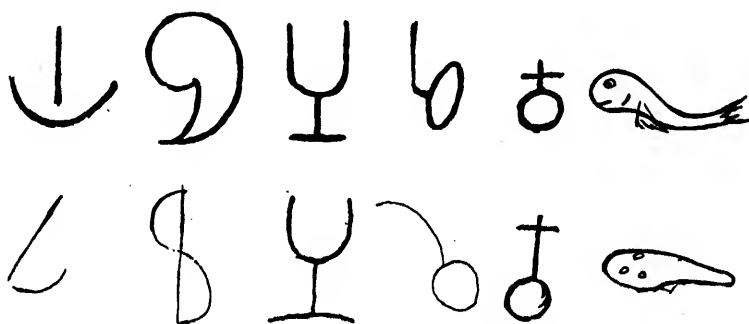


FIG. 1. A complete consecutive series of six drawings transmitted by telepathy from Mr Guthrie to Miss E. without contact during the Liverpool experiments. The original drawings are shown above and the reproductions by Miss E. below. When No. 6 was being transmitted Miss E. said almost directly, 'Are you thinking of the bottom of the sea, with shells and fishes?' and then, 'Is it a snail or a fish?'—then drew as above. (From *S.P.R. Proceedings*, Vol. II.)

complete and consecutive series at one sitting with Mr Guthrie acting as 'agent' and Miss E. as 'subject' (percipient), with no contact between them such as touching hands. Fig. 2 gives some selected successes from a number of other experiments. I will quote a brief note from the report showing how they were done, but the complete account should be studied.

The originals of the following diagrams were for the most part drawn in another room from that in which the 'subject' was placed. The few executed in the same room were drawn while the 'subject' was blindfolded, at a distance from her, and in such a way that the process would have been wholly invisible to her or anyone else, even had an attempt been made to observe it. During the process of transference, the 'agent' looked steadily and in perfect silence at the original drawing,

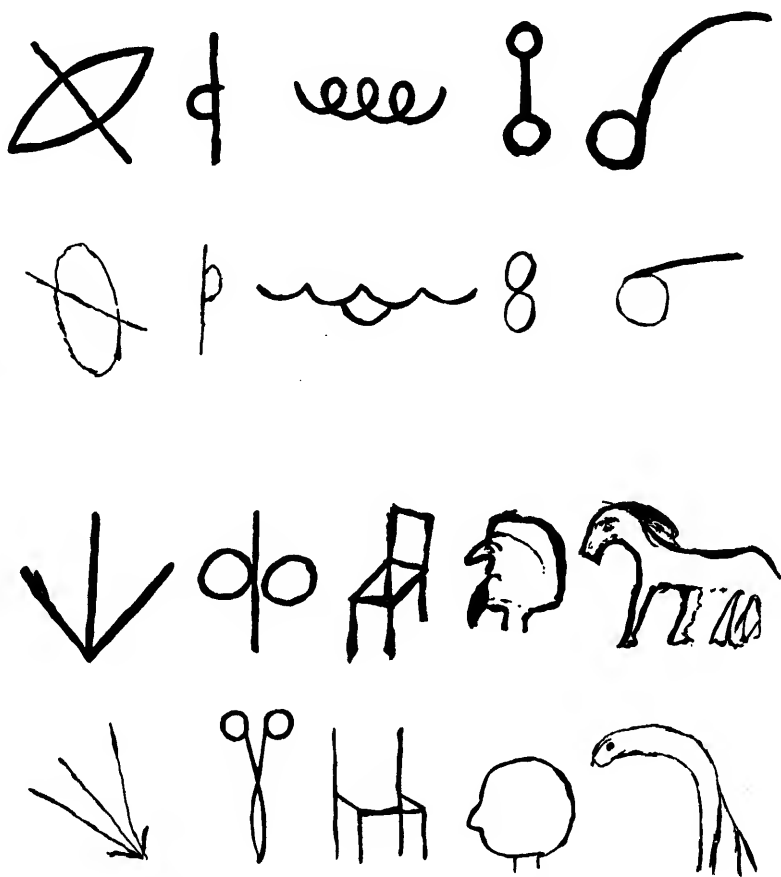


FIG. 2. Ten further examples of drawings transmitted by telepathy during the Liverpool experiments; originals above and reproductions below in each row. These are selected as some of the best examples. No. 1, Mr Gurney to Miss R., with half a minute contact before drawing was made; No. 2, Mr. Gurney to Miss R., without contact; Nos. 3 and 4, Mr Birchall to Miss R., without contact; No. 5, Mr Birchall to Miss E., without contact; No. 6, Mr Steel to Miss R., without contact; No. 7, Mr Steel and Miss E., contact before reproduction was made; No. 8, Mr Hughes and Miss E., contact before reproduction was made; Nos. 9 and 10, Mr Hughes and Miss E., no contact. Just before drawing No. 9, Miss E. said 'It is like a mask at a pantomime.' From *S.P.R. Proceedings*, Vol. II.

which was placed upon an intervening wooden stand ; the 'subject' sitting opposite to him, and behind the stand, blindfolded and quite still. The 'agent' ceased looking at the drawing, and the blindfolding was removed, only when the 'subject' professed herself ready to make the reproduction, which happened usually in times varying from half-a-minute to two or three minutes. Her position rendered it absolutely impossible that she should glimpse at the original. She could not have done so, in fact, without rising from her seat and advancing her head several feet ; and as she was almost in the same line of sight as her drawing, and so almost in the centre of the 'agent's' field of observation, the slightest approach to such a movement must have been instantly detected. The reproductions were made in perfect silence, and without the 'agent' even following the actual process with his eyes, though he was of course able to keep the 'subject' under the closest observation.

In a number of experiments *two* agents were used to look at the same drawing to be transmitted to the one percipient. On one occasion Sir Oliver Lodge made an interesting innovation which he recorded in a letter to *Nature* (Vol. XXX, p. 145, 1884). I quote from it as follows :

One evening last week—after two thinkers, or agents, had been several times successful in instilling the idea of some object or drawing, at which they were looking, into the mind of the blindfold person, or percipient—I brought into the room a double opaque sheet of thick paper with a square drawn on one side and a St. Andrew's cross or X on the other, and silently arranged it between the two agents so that each looked on one side without any notion of what was on the other. The percipient was not informed in any way that a novel modification was being made ; and, as usual, there was no contact of any sort or kind—a clear space of several feet existing between each of the three people. I thought that by this variation I should decide whether one of the two agents was more active than the other ; or, supposing them about equal whether two ideas in two separate minds could be fused into one by the percipient.

In a very short time the percipient made the following remarks, every one else being silent : 'The thing won't keep still.' 'I seem to see things moving about.' 'First I see a thing up there, and then one down there.' 'I can't see either distinctly.' The object was then hidden, and the percipient was told to take off the bandage and to draw the impression in her mind on a sheet of paper. She drew a square, and then said, 'There was the other thing as well,' and drew a cross inside the square from corner to corner, saying afterwards, 'I don't know what made me put it inside.'

Similar experiments to the Liverpool ones are also reported from Leeds, between a Mr J. W. Smith and his young sister, aged 13, in the *Proceedings S.P.R.*, Vol. II, p. 7 and pp. 207-216 ; I reproduce

eight of the drawings in Fig. 3. Sir William Barrett visited Leeds to examine the conditions under which they were made and reported favourably on them. Reference may also be made to

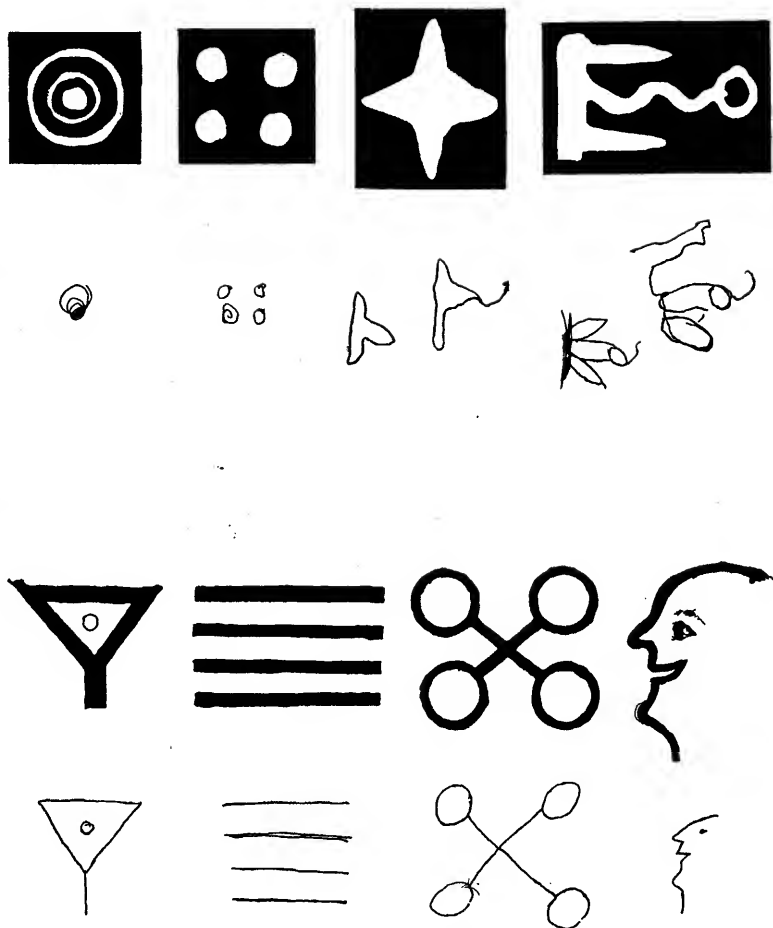


FIG. 3. A series of designs transmitted from Mr J. W. Smith of Leeds to his 13-year-old sister. Originals above and reproductions below in each row. The originals of Nos. 1 to 4 were drawn on a slate. From *S.P.R. Proceedings*, Vol. II.

experiments in Paris of MM. Schmoll and Mabire, *Proceedings* Vol. IV, pp. 324-337 and Vol. V, pp. 169-215; the number of successes in this series was limited—the 10 best results out of 104 drawings made are reproduced in Fig. 4.

These results are either fraud or something very wonderful. Some people indeed find them so wonderful that they cannot regard them as genuine. They are however really no more remarkable than the long series of results of a different kind which Dr Gilbert Murray, O.M., has obtained and to which he referred in his recent address. I hope the examples he described



FIG. 4. The ten most successful transmissions of drawings (out of 104) during the experiments made by MM. Schmolli and Mabire in Paris. Originals above and reproductions below in each row. From *S.P.R. Proceedings*, Vol. V.

will have caused those who were not already familiar with his earlier experiments to refer back to his former Presidential Address of 1915 and to the detailed studies of his results by Mrs A. W. Verrall in 1916 and Mrs Henry Sidgwick in 1924. Let me quote the opening two sentences of Mrs Sidgwick's report :

Professor Gilbert Murray's experiments in Thought-transference are

perhaps the most important ever brought to the notice of the Society, both on account of their frequently brilliant success and on account of the eminence of the experimenter. It is surprising, I think, that they have not attracted more general attention than, so far as I know, they have.

They are indeed worth very close attention. None can suppose that there is any conscious fraud here. Let me briefly remind readers of the method which, quoting from Dr Gilbert Murray's recent address, 'was always the same' ;

I was sent out of the drawing-room or to the end of the hall, the door or doors, of course, being shut. The others remained in the drawing-room : someone chose a subject, which was hastily written down, word for word. Then I was called in, and my words written down. I may add that, out of the first 505 cases, Mrs Verrall estimated the percentage as : Success, 33 per cent. ; Partial Success, 27.9 per cent ; Failure, 39 per cent. But it may be remarked that as evidence for the presence of some degree of telepathy most of the partial successes are quite as convincing as the complete successes : this would produce something like 60 per cent evidential and 40 per cent non-evidential.

Earlier in the address he says :

At one time, indeed, I was inclined to attribute the whole thing to subconscious auditory hyperaesthesia. I got almost no successes if the subject was not spoken, but only written down. Two or three successes and at least one error could be explained by my having heard or misheard a proper name, e.g. by confusing Judge Davies and the prophet David. But, apart from other difficulties in this hypothesis, there were some clear cases where I got a point or even a whole subject which had only been thought and not spoken.

Careful examination will, I think, lead one to agree with Mrs Sidgwick when she writes as follows regarding hyperaesthesia after considering many possible examples : 'In some cases the evidence against it seems, as we have seen, conclusive, and I feel sure that if hearing, however hyperaesthetic, has operated at all, it has done so rarely.'

The impressions transmitted to Dr Gilbert Murray are just as complicated and as often as not agree just as well with the original subject as do the drawings which we have just been discussing. For any reader who has not seen examples I will quote just one, taking the first to be given in Mrs Sidgwick's report :

In what follows remarks by the agent and contemporary notes are in round brackets ; additions by myself, to make things clear, in square brackets.

'I. *Subject.* MRS ARNOLD TOYNBEE (agent): " I think of the

beginning of a [story by] Dostoevsky where the dog of a poor old man [is] dying in a restaurant."

PROFESSOR MURRAY. "I think it's a thing in a book. I should think a Russian book. A very miserable old man, and I think he's doing something with a dead dog. [A] very unhappy one. I rather think it is in a restaurant and people are mocking, and then they are sorry and want to be kind. I am not sure." ("Nationality?") "No—I don't get their nationality. I have a feeling it is a sort of Gorki thing. I have a feeling that it is something Russian."

([Mrs. Toynbee]) had not said it but it was all true. Mr Murray had not read the book. It was a German restaurant, but Mr Murray had not felt that.)

I have myself been convinced of the reality of telepathy from two experiences I had many years ago. A report of them is of no value as scientific evidence because it may be argued that rather than accept them as telepathy it is easier to believe that I imagined the occurrences or unconsciously built up the examples from much slighter ones which might have been mere coincidence. Nevertheless, I think it may be of interest to record them because to me, they are as real and as important as any observation I have ever made in natural history. They took place during the First World War. For a time I was in a Cyclist Battalion stationed on the Lincolnshire coast where there lived a Mrs Wedgwood who was very kind in entertaining some of the officers of the regiment. Sir Oliver Lodge's book *Raymond* had just been published and after we had been discussing it she confessed that she herself had been an amateur medium. She was, I understood, the widow of a Mr Arthur Wedgwood who, with his brother Mr Hensleigh Wedgwood (a vice-president of our Society at its foundation), was much interested in spiritualism towards the end of last century. She then very occasionally, by holding objects, claimed to be able to 'see' and describe people she had never seen before. Once, by holding a letter she gave a reasonable but not very exact description of my mother who was then alive. I mention this without attaching much value to it only as an introduction to the two cases I consider so important, but in passing I may just say that her description of the path along which my mother and I had so often walked together was much more striking than that of my mother herself.

Mrs Wedgwood from earlier talks knew that my brother was an engineering student and that he was a prisoner-of-war in Germany. One Sunday afternoon I went up to her house to tea. With one or two others I had been trying some table-tilting séances with her—without getting anything but quite meaningless messages spelt out—and that afternoon after tea we sat down at the table. A moment or two after I had put my hands on the table

next to hers—I cannot remember now whether they touched hers or not—she suddenly said ‘Oh, I can see your brother in Germany quite clearly’. (I am not reporting her exact words, but as nearly as possible the gist of them.) ‘I can see him in a little room in his prison camp with a camp bed, he is sitting at a table drawing what I think must be some engineering plan ; on a large sheet of white paper I see him painting what seem to be squares and oblongs of red and blue.’ Actually she had described exactly what I had been doing myself all that afternoon and no one else knew I had been doing it. Our colonel had a great interest in military history and was giving the officers a series of lectures on Monday evenings on various campaigns. He knew I was quick at drawing and he had asked me to make a map for him to illustrate his next talk on the Franco-Prussian War. He didn’t know how I was going to do it ; it was only that afternoon that I had the idea of cutting out squares and oblongs of card painted red and blue to represent the various units of infantry, cavalry and artillery of the two sides so that he could move them about on pins to their different positions as the lecture proceeded. It was an obvious thing to do, but he had only asked me to prepare a large map of the area. I spent the greater part of the afternoon—in my rather bare room in my billet with a camp-bed in it—looking at the large white map and moving the red and blue cards about following a description of the campaign and making pencil marks where they should be at different stages. After I had finished I put the cards away, rolled up the map, and went straight off on my bicycle to tea with Mrs Wedgwood : I am absolutely certain that no one could have told her before I went what I had been doing. I would find it difficult to believe that the correspondence of her description of what she thought my brother was doing and what I myself had actually been doing all afternoon was mere coincidence ; with another case of almost the same kind I am convinced that coincidence cannot explain it.

The second case was a year later. I was now attached to the Royal Engineers as a camouflage officer and was attending a special course at the school set up in Kensington Gardens under Solomon J. Solomon, R.A. Mrs Wedgwood came to stay in London whilst I was there and I went out to dinner with her. The case is remarkably like the other one. That afternoon at the school we were doing experiments in dazzle effects. I had taken a large sheet of white cardboard and then painted it all over with a most vivid pink distemper. I was then going to cut it up into all sorts of shapes to use in our experiments, but I found it took much longer to dry than I had expected so that I had it in front of me and kept

looking at it to see if it was ready for some considerable time before I actually cut it up. Again I am quite certain that no one could have told Mrs Wedgwood what I had been doing, for no one at the camouflage school knew her or knew that I was going out to dinner with her. I had not sat down at the dinner table with her for more than a moment or two when she suddenly said 'Oh, what have you been doing? I see a large pink square on the table in front of you.'

There is no need to emphasise further how alike the two cases are; I will only add that I know I have a good visual memory and that colour and shape make a strong impression on me. It was not as if Mrs Wedgwood frequently made statements as to what she thought I, or people connected with me, had been doing and that these particular cases just described were the only two correct ones. The only other occasion I believe when she made such a statement to me was the one concerning my mother that I have already referred to. I cannot, as I have already said, claim that my account is scientifically good evidence; it cannot completely convince others. I have included it, however, because I hope it may, with the other cases I have given, help to induce biologists to feel that there is at any rate a case for a more thorough investigation of such matters. If telepathy is real—and from this experience I, myself, am convinced it must be—then it is likely to be of fundamental importance for biology.

Why is it that the philosophers seem to take more interest in these paranormal phenomena than do the biologists? Until very recently, with a few notable exceptions, scientists in general have tended to ignore—or perhaps one should say definitely *shun*—the results of psychological research. It is remarkable that our Society for Psychological Research, under the Presidency of some great personalities, should now have been in existence for 70 years, yet so few scientists until recently have thought it worth while to examine the contents of its *Proceedings* and *Journal* which together at present make over 80 volumes. The evidence for telepathy which I have quoted is but a tiny fraction of that to be found within their pages.

There are at least *four* major reasons why most scientists have an aversion for these matters. It is important that we should consider these points carefully. Some of my friends and colleagues are genuinely shocked that I should be interested in these things; they feel it is impossible to do so and still retain the spirit of science. While I do not share their views, I have a deep respect for the feelings which lie behind them and so I want to discuss them as sympathetically as possible.

The first and most obvious objection is of course that the experiments of psychical research cannot yet be repeated at will and this seems to place them outside science as we know it. Have not the laws of science been built up from observations and experiments which can be repeated anywhere by anyone provided he is working under similar conditions? Add so much hydrochloric acid to so much zinc and you will always get so much hydrogen liberated; a ray of light entering pure water at a particular angle will always be bent through the same angle wherever the experiment is performed—science has grown by the addition of such facts that can be verified by whoever may doubt them. Scientists are apt to say that they are not prepared to consider the findings of psychical research until they can be so repeated. There are, however, many facts of natural history which cannot always be demonstrated at will. Animals may usually behave in this or that fashion under these or those particular circumstances, but we cannot be certain that they always will. When this happens we assume that we have not got all the conditions exactly right or that there is something in the animal's psychology we do not yet understand; we don't, however, because of this deny the former observations on the animal's behaviour. Most of the repeatable experiments of science at present deal with the action of matter in the inorganic world or with the chemistry and physics within the animal body; we are only just beginning to understand the laws relating to the behaviour of whole animals under natural conditions. Because the results of psychical research cannot yet be repeated at will I do not myself feel that that is any reason for refusing to acknowledge that on a large number of occasions they have been demonstrated to occur.

There is so much of life which cannot yet be studied by repeatable experiment that it seems to me wrong that we should refuse to examine these paranormal events simply because they still lie more in the field of observation than in that of laboratory experiment. We may hope that in time we shall know much more about the factors which govern them and so eventually be able to reproduce them whenever we wish to; then we shall bring the observations from the domain of natural history into that of exact science.

Secondly comes the question of fraud. There has of course been a good deal of deception detected by psychical research among people who have claimed to have unusual gifts. This is certainly a very unpleasant side to the subject. The atmosphere of mystery which surrounds these reported phenomena excites the credulous and makes them the easy victims of the charlatans who delight in

pretending to have 'supernatural' gifts because it gives them a feeling of superiority and power over their fellow men. We must indeed respect the attitude of those scientists who say 'Now that fraud has been detected, not once, but again and again, I'm going to have nothing to do with it—to dabble in it is to damage the fair name of science.' Yet, of course, we all know that the great science of Chemistry sprang from the cradle of alchemy some of whose exponents were genuinely striving after the transmutation of metals and the elixir of life, while others were as rank impostors as any false medium or fortune teller of today. This new branch of knowledge which is now struggling to be born will one day, I believe, look back to this period as the chemists of today look back to their own history.

The third objection is one which requires a great deal of consideration ; it is, I think, the most important and the most difficult to deal with. The people who work at psychical research often seem to want a particular result. Surely, say the critics, this cannot be the scientific attitude ; such an outlook must make us suspicious of the validity of their findings. Are the experimenters not biased? Not that they willingly distort the evidence to support their case—but unconsciously may they not tend to disregard this, and give undue prominence to that, and so unwittingly arrive at false conclusions?

The man with the true scientific spirit, it may be said, should approach each experiment without the slightest desire to get one kind of result any more than another ; he should want only the truth whatever it may be. He does an experiment, let us say, to see if starch is or is not present at a particular time in a particular part of a plant ; he has no preference whatever as to what the result will be. He does another experiment to decide what kind of light, whether the red, green or blue part of the spectrum penetrates furthest into the sea ; he has no wish for it to be blue rather than green—he just wants to know which. We may easily have this unbiased scientific spirit (provided we have no pet hypothesis to defend) when we are dealing entirely with material things or with the physical and chemical reactions within the living body ; when, however, we come to investigate problems which may have a bearing upon our own relations with the universe—then I think we must honestly admit that such a strictly unbiased attitude is impossible. This does not mean that we must not investigate these problems ; it means we must proceed with much greater caution.

Again and again, biologists have claimed to have done experiments which they believed had demonstrated the inheritance of

acquired characters ; and again and again these experiments have been repeated by those who do not believe in such inheritance and they have not only failed to confirm them but have often shown up the fallacy which led to the false conclusion. Quite clearly many biologists have felt strongly that they could not tolerate a belief in the apparently ruthless mechanical control of life and evolution often implied in the theory of natural selection. They have felt by intuition that that could not be all and they have wanted with almost a religious fervour to show by experiment that there was some other principle at work : a principle which might mean that man's strivings after better things might in time effect an improvement in the future of the race. No wonder that people looking for particular results with such an emotional bias often thought they had found what they were looking for. We must rightly suspect such people not of fraud but of self-deception.

Some scientists in making these experiments have squarely faced the issue. I will quote from Professor William McDougall in his 'An experiment for testing of the hypothesis of Lamarck' in the *British Journal of Psychology*, Vol. 17, 1927, page 302.

I have every reason to believe that my assistants worked very faithfully and conscientiously ; and I was constantly in close touch with them, maintaining a close personal supervision of their work. In this connection it is necessary to avow that, during the course of the experiment, there grew up in all of us a keen interest in, I think I must in fairness say, a strong desire for, positive results. From the first it was obvious that a positive result would be more striking, would excite more interest in the biological world, than a negative one. And, when indications of a positive result began to appear, it was but human nature to desire that this result should appear as clearly cut and positive as possible. Further, on my own part, there was a feeling that a clear-cut positive result would go far to render tenable a theory of organic evolution, while a negative result would leave us in the Cimmerian darkness in which Neo-Darwinism finds itself.

I was conscious, therefore, of a strong bias in favour of a positive result ; and throughout I was consciously struggling against the temptation to condone or pass over any detail of procedure that might unduly favour a positive result. Such details are encountered at every point, more especially in the breeding of the animals. To have disguised from oneself this bias, to have pretended that we were superior to such human weakness, would have been dangerous in the extreme ; the only safeguards against its influence were the frank avowal of it and unremitting watchfulness against it. I can conceive of no task that could make greater demands upon the scientific honesty of the worker ; and it is in part this demand for unremitting watchfulness that renders the work peculiarly exhausting. I can only say that I believe we have succeeded

in standing upright ; and in fact, for myself, I am disposed to believe that I have leaned over backwards, as we say in America. Whether we have really succeeded in this, the most difficult part of our task, can only be proved when other workers shall have undertaken similar experiments. If our results are not valid, the flaw, which escapes our penetration hitherto, must, I think, be due to some subtle influence of this bias.

He was indeed right ; in spite of this caution, he too let a fallacy escape him and so came to false conclusions. His work has been repeated by Professor Crew of Edinburgh and others who have failed to get his positive results ; they were further able to show that among the rats used were distinct Mendelian strains which had different capacities for quick learning which McDougall had not looked for. It seemed likely that he (McDougall) had quite unconsciously been selecting races of rats which learnt more quickly, and so it was that he had appeared to demonstrate the inheritance of an acquired experience—subsequent generations appearing to master the problem of the escape from the tank more and more easily.

Now Professor Rhine who has done so much work on the so-called parapsychology at Duke University was McDougall's assistant and principal worker in this Lamarckian experiment. Is it conceivable that he and the others who have followed him in parapsychology have similarly, in their enthusiasm for the new work, overlooked some alternative explanation for the results they are appearing to get? I now believe from the work of Mr Spencer-Brown that this may indeed be a possibility. I think that this is more likely than the alternative that clairvoyance, precognition and psychokinesis are separate and real phenomena ; like Dr Murray, I am sceptical about these things. If it should turn out to be so, I shall not think less of Professor Rhine. It is his very energy and enthusiasm which has set all this work in motion—if what he thinks he has discovered should prove to be something very different—it will be his work which has led to the discovery of a new principle—just as the work of Joseph Priestley led to the real understanding of both combustion and animal respiration by Lavoisier. Priestley, of course, discovered oxygen without knowing it and demonstrated its properties of both making things burn more rapidly and making animals more active ; he believed in the soon-to-be-exploded phlogiston theory and called his great discovery dephlogisticated air. He had told Lavoisier of his experiments when he travelled to Paris with Lord Shelburne and it was Lavoisier who recognised the new gas as the secret of Priestley's work.

Don't let us be afraid of making mistakes—don't let us lose the opportunities of making discoveries because we don't like to risk making fools of ourselves. We must face the dangers of bias just as McDougall and Rhine did in their rat experiments and take every precaution we can against them. If we say science must only be concerned with experiments about whose results we have no emotional feeling—then we can never hope to come to a real understanding of living things. Psychical research does indeed appear to hold out a promise of results which may release us from a philosophy of materialism which an intuition seems to tell us is false. We may be interested in investigating the paranormal for this very reason; if so we would do well to admit it frankly and if possible seek the collaboration in our work of someone who holds quite the opposite view.

The fourth reason why some scientists will not examine the claims of experimenters in psychical research is because they believe them to be quite impossible and so a waste of time. Certainly telepathy does not appear to fit in within the framework of present-day science, but should we because of that refuse to investigate it? If we do, are we not just as those who condemned Galileo for his experiments and conclusions? As Professor H. H. Price says in the *Hibbert Journal* (Vol. 47, p. 109, 1949): 'Telepathy is something which ought not to happen at all, if the Materialistic theory were true. But it does happen. So there must be something seriously wrong with the Materialistic theory, however numerous and imposing the *normal* facts which support it may be.'

Our former President, Mr G. N. M. Tyrrell, whose recent death we so deeply deplore, has in the last few years put forward the view¹ that in the course of evolution the human mind has become adapted to its surroundings just as the body has and that 'common sense is impregnated with instinctive tendencies'. He believed that the implications of this are of great importance in the matter we have just been discussing: that the materialism of science and our reluctance to examine what appears to be contrary to its doctrines are the direct outcome of adaptive instinct. I find this view difficult to square with what history appears to tell us. Surely throughout the Middle Ages and up to the end of the 16th century western man had a sure faith in spiritual reality which was as strong as is ours of today in science. There was then no binding of the mind to material things; it was only too prone to believe in witchcraft and all manner of other nonsense in addition to having a more healthy spiritual outlook. Science in

¹ *The Hibbert Journal*, Vol. 49, p. 48, 1950, and his book *Homo Faber*, London 1951.

order to develop had to sweep away the encrustation of superstition ; it is partly a fear that the study of the paranormal may lead to a recurrence of a belief in 'occult' wonders that also leads many scientists to oppose psychical research.

Now I must come to the point and say just why I believe the recognition of telepathy as a reality will be so important for biology. I have already expressed my views on its possible significance in the development of evolution theory in an article in this Society's *Journal* (Vol. XXXV, pp. 225-238, 1950). I will not go over that ground again ; not only is it readily accessible to Members of the Society, but the hypothesis developed there is, I consider, of minor importance compared with the more fundamental relationships which telepathy may have to an understanding of living things. It is no use pretending that biology has gone halfway towards such an understanding when two at least of the prime characteristics of living as we experience it—consciousness and memory—are completely unexplained. There are some biologists of course who maintain that mind is an epiphenomenon of a mechanical brain and that memory is due to nerve impulses running along tracks which have been stimulated and in some way modified by earlier impressions from the sense organs ; the later impulses are thought to pick up the traces of the former ones much as a gramophone needle recalls some recorded song of long ago. The comparison of the brain, with its myriads of nerve cells and connections, to that of a vast man-made electric computing machine is striking and no doubt valid up to a point ; but can anyone really suppose that the experience of thought and memory are a physical part of the mechanism? The rapid succession of sounds as the gramophone needle runs its course or the flashing lines of varying light intensity sweeping over a television viewing screen are meaningless unless perceived and put together into a whole by something outside the mechanical system. As Dr Soal reminds us in his Frederic Myers Memorial Lecture of 1947, memory is at present really just as much a mystery as telepathy.

I call telepathy a mental miracle, but it is that only because to most people it is still an unfamiliar experience. To me the power which we exercise every day when we remember a past event is almost equally miraculous. That I should be able to recall how on a day of August 1933 I broke an ice-axe on the Blumlisalp glacier, how the next day I climbed the First with my friend Tom Jones, and how it rained incessantly on the three following days, and that I should be able to recall these events in their correct order in time is for me as inexplicable as any feat of telepathy. Yet so familiar are we with this astounding phenomenon of memory, that we never spare it a moment's thought.

The cardinal bearing of psychical research on biology centres about just this point : telepathy and allied phenomena make nonsense of a materialistic interpretation of life and its supposed epiphenomenalist explanation of mind ; any who are still doubtful of this statement should read the treatment of the subject by the philosophers Professor C. D. Broad (e.g. in *Philosophy*, XXIV, pp. 291-309, 1949), and Professor H. H. Price (e.g. in *The Hibbert Journal*, XLVII, pp. 105-113, 1949).

There are many biologists who are quite content to confine their investigations to the study of the physical and chemical interactions on or in the animal body—and no one will deny that this is a fascinating field of study for those with a mechanical turn of mind. It may be less satisfying however for those who have been convinced that physical mechanism (as we understand the term physical today) cannot be the whole of a living entity ; they may begin perhaps to realise that the physiology of today is not really touching the fundamental characteristics that make the living organisms different from a purely mechanical system. I expect physiology may go on exploring and unravelling the physico-chemical side of living processes *ad infinitum* and that in so doing it will continue to advance medical knowledge to the great benefit of mankind ; I think it possible however that *by itself* it will take us no nearer the real secret of living things.

Psychical research shows that there *is* a non-material—vitalistic, if you like—side to man as well as a mechanistic one. Few biologists can doubt that man is fundamentally one with the animal kingdom ; however highly developed he is in his mental and spiritual make up, we cannot believe that there has been any real break in the line of his evolution from more primitive creatures. Whether we shall ever be able to link together the two sides—the vital and the mechanical—we do not know ; we can only comfort ourselves with the hope that, if civilization lasts, science may stretch for thousands of years into the future where man's outlook on living things may be very different to that of our science of today which has but a few hundred years of childhood behind it.

The next greatest advance that can be made in science is, I believe, the clearing away of the old issue which for so long has split biology into two camps : is there, or is there not, some vital non-material element in living things? Everything else in biology is secondary to this ; its most important line of research must be to this end. We must at all costs hunt out more of those apparently rare people who have these telepathic gifts so much better developed than others ; we must get them to cooperate in the most

rigorously controlled experiments so that the evidence obtained may be built into biology just as securely as that of physiological research. Those who have followed psychical research know that these people are difficult to find. We must discover the laws governing these curious events. While they can probably be more easily studied in human subjects, we must not neglect the possibility of investigating similar phenomena which may sometimes be present among animals. Some may say that these curious people with telepathic gifts are so unusual and rare that the value of this phenomenon for biology is insignificant. Let me quote a parable from Professor Broad (*loc. cit.*):

Human beings have no special sensations in the presence of magnetic fields. Had it not been for the two very contingent facts that there are loadstones, and that the one element (iron) which is strongly susceptible to magnetic influence is fairly common on earth, the existence of magnetism might have remained unsuspected to this day. Even so, it was regarded as a kind of mysterious anomaly until its connection with electricity was discovered and we gained the power to produce strong magnetic fields at will. Yet, all this while, magnetic fields had existed, and had been producing effects, whenever and wherever electric currents were passing. Is it not possible that natural mediums might be comparable to loadstones; that paranormal influences are as pervasive as magnetism; and that we fail to recognize this only because our knowledge and control of them are at about the same level as were men's knowledge and control of magnetism when Gilbert wrote his treatise on the magnet?

If, by experiment, we do succeed in convincing the majority of scientists that telepathy—and so probably mind—is something outside physics and chemistry as we now understand them, how, it may be asked, is this going to advance Biology? I don't want to prophesy. I will only make suggestions. In spite of the advances of mechanistic theory in the last half century there are still some great unsolved problems concerning fundamental biological concepts apart from the obvious one of the body-mind relationship. I strongly suspect that some of these are associated in some way with this non-material side of life. These unusual people with telepathic gifts may well be presenting us with chinks in the material carapace of the living world through which we can probe bit by bit to find out a little more of what at present appears a mystery lying behind it. In the article in the Society's *Journal* to which I have already referred I have outlined what I believe to be the present generally held view regarding the mechanism of evolution—that based upon Darwinian selection at work upon an interacting complex of Mendelian genes; I then said that I accept

that position '—as far as it goes ; but surely no one can imagine it as anything like a final position. It is less than a hundred years since the publication of Darwin's masterpiece and only half a century since Mendel's laws were rediscovered. In this short time our conceptions of the nature of variation and inheritance have undergone radical changes ; we must look for and, in time, expect to find very much more.' I then went on to discuss a particular application which telepathy might have to evolution theory ; readers may refer to this if they wish, but actually I think it possible that it may touch the theory of evolution much more fundamentally than I then felt prepared to say.

It will be difficult in a very brief space to sketch an outline of these somewhat abstruse problems in terms that will not seem like technical jargon to the layman, but I will try. If, on the one hand, I fail in making myself clear, may I ask the general reader to skip onwards a paragraph or two? If, on the other hand, I make the biologists smile at my over-simplification, I ask them to forgive me in my attempt to balance between the two sides. The first point I want to make and explain concerns what in biology we call Homology. It is noteworthy that the term was first used by our great anatomist Richard Owen sixteen years before the publication of Darwin's *Origin of Species*. In his *Lectures on Invertebrate Animals* (London, 1843, pp. 374 and 379) he defines two terms: *Homologue*: 'the same organ in different animals under every variety of form and function' and *Analogue*: 'a part or organ in one animal which has the same function as another part or organ in a different animal'. This is really very simple when we explain it with diagrams, but before I do so I will just remark in passing that these definitions, when first made, were not intended to have any evolutionary meaning at all—Owen did not believe in evolution—they were intended to signify a *plan of creation*. In Fig. 5 we see a sketch of the skeletons of the fore limbs of several vertebrate animals—a man, a whale, a bird, and a bat. In each case we see a similar general arrangement of bones which correspond in the different animals: humerus (*h*), radius (*r*), ulna (*u*), carpals (*c*), metacarpals (*m*) and phalanges (*p*); throughout the vertebrates with limbs we see the *same* organs (in this case, these various bones) under a great variety of form and function, i.e. here shaped as a flipper and used for swimming, there shaped as a wing and used for flying, etc. These *same organs* Owen called homologous structures. Analogous structures need not detain us, as I am not going to be concerned with them, and one simple example will suffice: the limb of a vertebrate and that of an insect may serve the same function of walking but they are radically

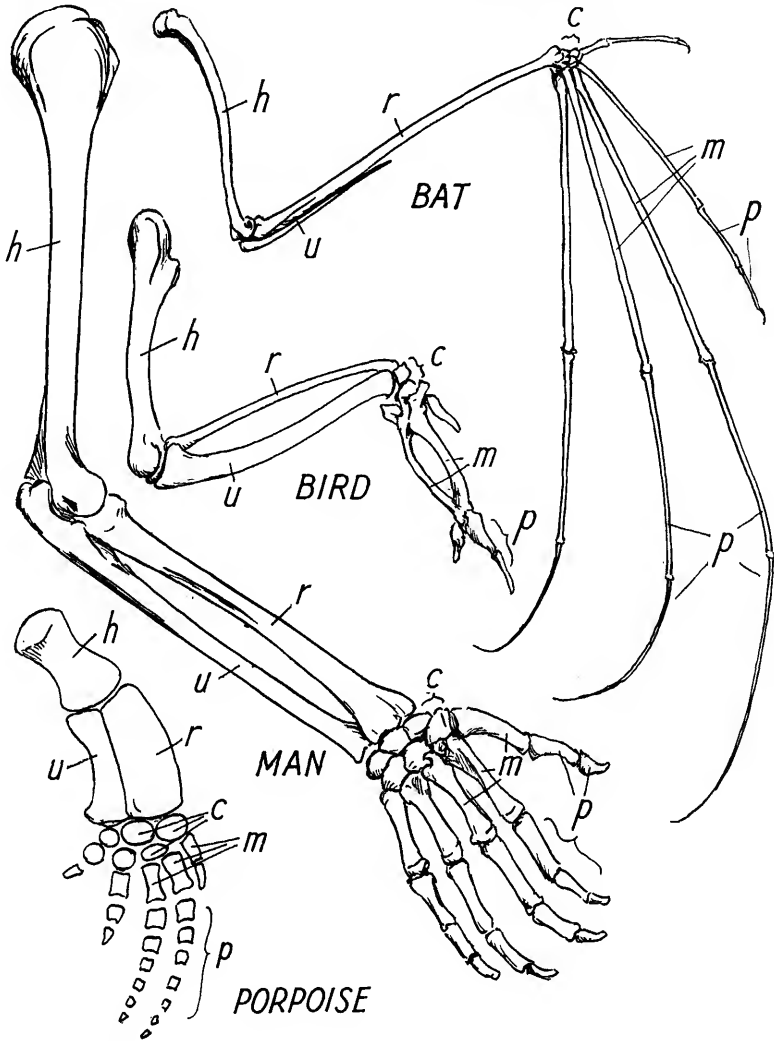


FIG. 5. A comparison of the homologous bones in the fore limbs of man, a small whale (porpoise), a bird and a bat: humerus (*h*), radius (*r*), ulna (*u*), carpals (*c*), metacarpals (*m*), phalanges (*p*).

different organs in structure and design ; the vertebrate limb has, of course, an internal skeleton of rod-like levers (the bones) to which the operating muscles are attached, whereas the insect has an external skeleton of plastic-like material made into jointed tubes

and worked by the muscles pulling from the inside. To return to the homologous structures—as soon as the evolution doctrine became accepted Owen's terms took on a new significance. Homologous structures were now defined as those derived from the same single structure in a common ancestor however much that structure may be modified by variation. All the humerus bones of the terrestrial vertebrates, for instance, are thought to be derived by modification over millions and millions of years from the bones of the primitive limb-like fins of the first fish-like amphibia that pioneered the conquest of the land from the water. In the same way, the hearts, the nerve cords, the eyes and so on are said to be homologous, derived by gradual modification from the original ancestral type. This seemed very simple at one time and is still spoken of by most zoologists as if it were; but the fact is that today the idea of homology is far from easy to understand.

It is a curious paradox that this concept of homology is absolutely fundamental to what we are talking about when we speak of evolution, yet in truth I believe we cannot explain it at all in terms of present-day biological theory except by assuming one postulate which seems to me to stretch speculative credulity far too far. This brings me to the more technical part. The bodily structure, colour, ranges of temperature toleration, etc. of an animal—in short, its whole form and physiology—appear to be governed by a vast complex of interacting Mendelian genes carried on certain bodies (the chromosomes) in all the cells of the body. The genes from time to time undergo changes which alter their effects (they are said to 'mutate'); it is the combined effects of the complex interactions of all the genes, which in conjunction with the influence of the environment, produce the form of the animal as we see it and as it is presented to the action of Darwinian selection. It might have been thought that homology of structure would be dependent upon the homology of similar kinds of genes handed on, but it is *not*. It is impossible to give the evidence here, but the groups of genes governing homologous structures may in the course of many generations be changed, not simply by their own mutation, but by the substitution of some genes for others, or by the addition or loss of genes, as can be shown in prolonged breeding experiments. *The concept of homology in terms of similar genes handed on from a common ancestor has broken down.* Perhaps homologous structures are always formed from the same corresponding set of *cells* in development? No, this also fails; the lenses of the vertebrate eyes must surely be regarded as homologous, yet in experiments on frogs and newts they may be formed from epidermal cells at all sorts of places on the body surface if

part of the eye (the optic cup) is grafted in below the skin. The optic cup is then said to act as an 'organizer'. It was thought then that homologous structures might be due to the handing on from ancestors of similar 'organizers'; this hypothesis, however, also collapses. For instance, in one species of frog (*Rana fusca*) the lens of the eye can only be induced by the presence of the optic cup; in another species (*Rana esculenta*) while it can be induced by the optic cup, it is also formed in its proper place if the optic cup is removed—formed apparently in relation to the developing whole animal.

For the present we appear to be forced into the position of saying that the only explanation of homology that the latest generally accepted views on evolution can offer is that *selection by the environment* is governing the maintenance of all the internal spatial relationships of the animal; i.e. all the multitude of homologous parts which make up complex creatures such as say a hedgehog, a chaffinch and a frog. We must recognise that within relatively short periods of time there is a good interchange of genes (gene flow as it is called) throughout the range of an interbreeding population and this helps to keep the race comparatively uniform; is it not however stretching the concept of *external* selection a bit far to suppose that it alone, by controlling the effects of an everchanging gene complex, is maintaining the stability of structure in a species over vast areas of different types of country—and over long periods of time? Can the whole complicated *internal* structure of our chaffinch, for instance, really be maintained—or rather slowly evolved—entirely under the influence of its multifarious *external* surroundings and nothing else? I could understand natural selection by the environment controlling the evolution of the whole intricate organ system if there were, associated with the homologous structures, some actual homologous units which varied and were handed on to be selected. But no, the homologous structures now appear to be governed by the *effects* of a whole multitude of units which are continually being reassorted. According to modern mechanistic biology the only 'plan' or 'specification' for the intricate homologous 'machinery'—for instance, the vertebrate, the arthropod or the molluscan plan—would seem to lie in the variable environment outside. To my way of thinking, such a conclusion is a *reductio ad absurdum*.

We must be careful not to underestimate the remarkable powers of natural selection. There is an almost infinite range of wonderful adaptations of animals—involving not only bodily structure, but instinctive behaviour as well—which could only, I believe, be produced by Darwinian selection acting in the environment *from*

outside the animal. I refer to the innumerable instances of animals obtaining protection from their enemies by all manner of different camouflage deceptions. Some, for instance, employ an exact imitation of some natural object such as a leaf or a twig ; others which habitually move against a variety of backgrounds, use the disruptive colour patterns found so valuable in war ; others again use some still more subtle principles known to the camouflage expert, but in addition nearly all combine these different devices with that other beautiful principle of counter-shading which under the overhead light of the sky makes the animal lose all appearance of solidity. In many cases the instinctive behaviour of the animal is adapted to match his disguise as when one animal mimics another in all the details of its colouring and form, and further imitates its typical behaviour, too, as when a spider may mimic an ant by lifting up and moving its front legs exactly like an ant's antennae. So also many of the non-mimetic camouflage schemes depend upon closely correlated behaviour patterns for their success. All these astonishing protective resemblances have meaning only if viewed *from outside—at some distance from the animal*. They could not possibly have been produced on Lamarckian lines—or any others involving internal factors—unless the animal could continually strive to modify itself in front of a looking glass! If these, which appear to be among the most elaborate of any adaptations, can only be accounted for in terms of outside selection, surely, it may be said, the whole interplay and balance of all the organism's internal anatomy and physiology can also be kept to the pattern which, as a whole, best fits the rigours of the environment. It sounds plausible at first sight, when looked at against a knowledge of this remarkable power of external selection, but will it bear closer examination? If we consider the amazing variety of external form of, for example, different vertebrate animals under all manner of types of surroundings and compare this with the remarkable constancy of form of the arrangement of the homologous internal structures over hundreds of millions of years—then I think we shall come to the conclusion that such an explanation is not a reasonable one. It is not of course the *adult* structures alone that we are concerned with ; we have to consider just as much the continuance in evolutionary time of the developmental processes which must be maintained along the same lines to produce the same homologous structures.

A theory which cannot explain 'homology' does not explain evolution at all. I think it likely that the complex of Mendelian genes and selection *may* account for the whole of the bodily evolution of our animals according to present accepted principles

if we add something more. What I am going to say is certainly speculative ; to me, however, it is a hypothesis no more unreasonable than the one we have just considered. It is put forward in the hope of starting a discussion ; its consideration and probable dismissal may serve to clear the ground in preparation for better ideas which, I believe, through the test of experiment, must in time show us some link between present-day biology and such phenomena as telepathy, hypnosis, memory and the mind-body relationship. I want to start such a discussion because I am convinced that a biology which fails to embrace these aspects of life is too narrow an abstraction to be worthy of twentieth-century man.

My idea is this : if once we can definitely establish that impressions of design, form and experience, such as those shown earlier in this paper, can occasionally be consciously transmitted by telepathy from one individual to another, is it not possible that there may be a general *subconscious* sharing of a form and behaviour design—a sort of psychic ‘blue-print’—between members of a species? If so, this telepathic ‘plan’ would be something like the subconscious racial memory of Samuel Butler, but it would be a racial experience of habit, form and development open subconsciously to all the members of the species, as in Whately Carington’s group mind. Now where it differs from the Butlerian idea is this : instead of the racial memory acting through Lamarckian use and disuse of parts of the body, it acts *by selection* ; external conditions being equal, those animals with gene complexes which allow a better incarnation of the specific plan will tend to survive rather than others whose gene complexes may produce somewhat faulty versions. There would be two, in a sense rival, selective forces always at work. One, the environmental one, would be tending to eliminate the animals whose gene complexes produce less efficient systems in *direct* relation to the external world ; the other, the internal one, would be leading indirectly to the elimination (also by the external world) of those varieties which fail because their gene complexes (or really their effects) have strayed too far from the species ‘design’, i.e. those which are weaker members because they are not sufficiently well genetically equipped to give a full expression of the species ‘plan of life’. Some people may perhaps doubt that there is a real difference in this rather subtle distinction, but I am sure there is. On the one hand, the environment selects those which are better equipped in direct relation to its various features : e.g. camouflage pattern against predators, strength of limb and claws against prey, etc. On the other, it is the species plan of life which sets a selecting standard, and those which do not come up to it in one way or

another are less efficient organisms and so tend to be eliminated in the struggle for existence. One would be an internal conserving selective force, explaining the secret of homology and the other a progressive selective force gradually modifying the race by picking out the individuals more suited to the changing environment with the passage of time. So it would be supposed that the 'racial plan'—'telepathically' linking all the members of the race—might gradually change as the population was modified by the external selection. If the concept of the 'racial plan' were true, then the old idea of a 'morph', 'form' or 'archetype' of the pre-evolutionary morphologists would take on a new meaning and some of the ideas of the German transcendentalists and our great Richard Owen would perhaps not seem quite so quaint as we have sometimes thought them.

The hypothesis I have just put forward will be seen to be a combination of the telepathic group mind postulated by Whateley Carington in his book *Telepathy* (1945)—particularly note his reference to spider behaviour on his p. 160—and something very like the idea of the so-called Organic Selection put forward by Baldwin¹ in America and Lloyd Morgan² in this country at the turn of the century. The idea behind Organic Selection is similar to that of Lamarck, that change of habit of an animal could bring about an evolutionary structural change; it is not however brought about as he supposed, but by the more frequent survival of those varieties which allow a fuller expression of the new behaviour. In my former article, which I have already referred to on p. 113, I discussed the possibility of a combination of this idea with that of Whateley Carington's Group Mind; I carried it, however, no further than the conception of new habits spreading in the population by telepathy and so bringing about evolutionary changes more quickly. Here I am suggesting something much more radical: a group-mind holding the whole plan of structural form and particularly development; it is seen to be closely parallel to Samuel Butler's racial memory. It was in fact *this idea* that I had in mind when I gave my address to the Zoology Section of the British Association at Newcastle in 1949; I ended my brief suggestion of it by saying 'If this flight of fancy ever proved to be a fact, it would be a wedding of the ideas of Darwin and Mendel on the one hand and of Lamarck and Samuel Butler on the other!'

I might have hesitated to put forward such a hypothesis if I had

¹ *The American Naturalist*, Vol. 30, pp. 441 and 536, 1896; see also his *Development and Evolution* (New York and London, 1902).

² C. Lloyd Morgan, *Animal Behaviour*, p. 115 (London 1900).

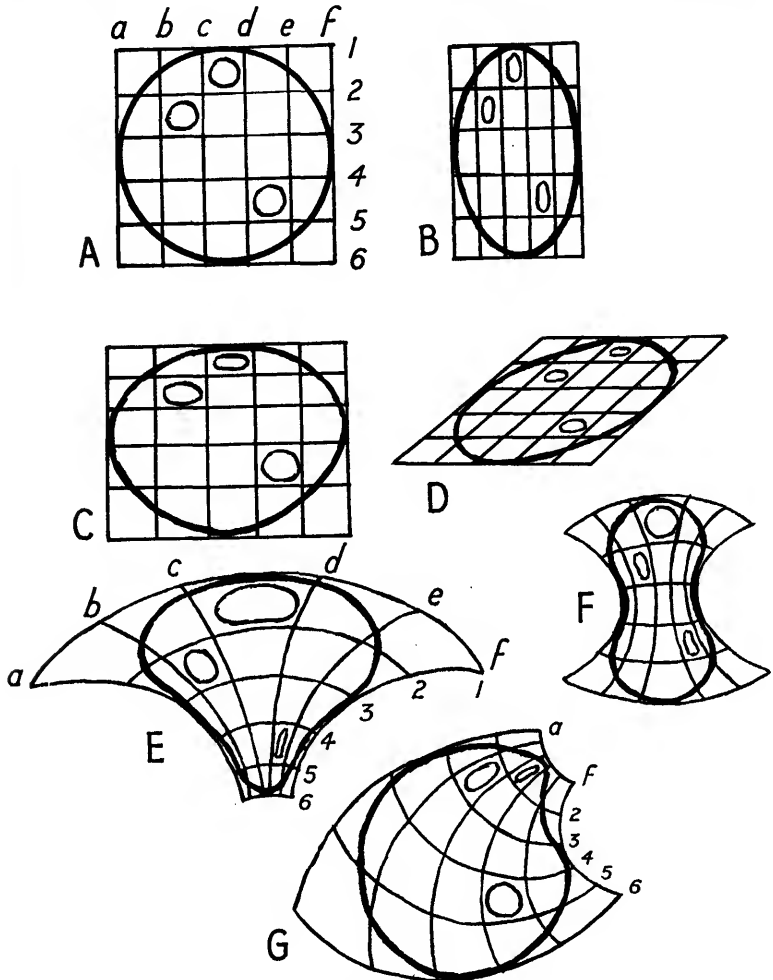


FIG. 6. Transformations of a circle (A) into various other shapes by replotting in relation to cartesian coordinates which are modified according to different mathematical formulae. Small circles added to show distortion of more detailed pattern.

thought it gave a possible explanation of the homology problem alone; there are in addition two other equally baffling puzzles which I believe might be solved by it as well. I must only refer to them very briefly. One is the theory of Transformations propounded by D'Arcy Thompson in his great book *On Growth and Form*, first published in 1917. If you draw a squared lattice over any

geometrical design, as in Fig. 6A, you can of course describe all the points on its curved lines in terms of measurements along these vertical and horizontal coordinates, as Descartes showed us long ago. Now, if you redraw the coordinates in different ways so that their relationships are varied according to definite mathematical laws—e.g. spaces between vertical lines half that of spaces between horizontal ones (6B), spaces between horizontal lines increasing according to a logarithmic scale (6C), or with the lines skewed, curved, etc. (6D-G)—then you may redraw your original design within each system so that the points on its curves again bear the same relations to the coordinates as before; if you do this you will find its shape modified in all manner of ways—each according to a particular mathematical equation. If we add circular spots to our original design, as in Fig. 6, we see their size and relationship also altered accordingly. Now D'Arcy Thompson found that if he placed such Cartesian coordinates over the shape of one animal and then examined the shapes of others belonging to the same zoological group, he usually found that it was possible to show that the form of one species could be transformed into that of another by such a relatively simple mathematical distortion of the plan. For example, in Figs. 7 and 8, redrawn from D'Arcy Thompson,

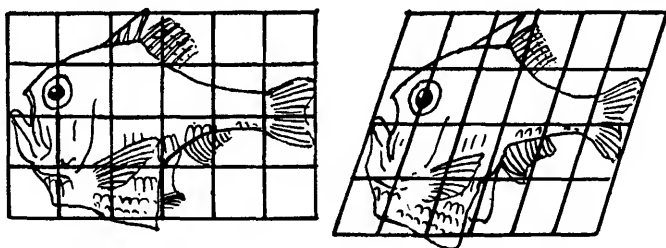


FIG. 7. Outlines of closely related species of deepwater fish: *Argyrolepecus olfersi* and *Sternoptyx diaphana*, the coordinates on the latter fish bear the same relations to its anatomy as those on *Argyrolepecus*. After D'Arcy Thompson.

we see in each case two different but closely allied species of fish, that on the right with a lattice drawn to have the same relation to the parts of the fish as has the lattice drawn over the one on the left. Again and again he shows such regular mathematical transformations within different groups.

Professor P. B. Medawar has more recently shown that one may get just the same type of transformation between foetus and adult in human development.¹ (Of course, it will be understood that

¹ 'Size, Shape and Age' in *Essays on Growth and Form* (Oxford 1945).

D'Arcy Thompson's transformations between adult and adult must be brought about by *changes in successive developments* during evolution.) Let us draw an outline of a human foetus, aged 5 months, and place it within a frame having equidistant horizontal lines drawn across it as in Fig. 9; now for comparison, let us

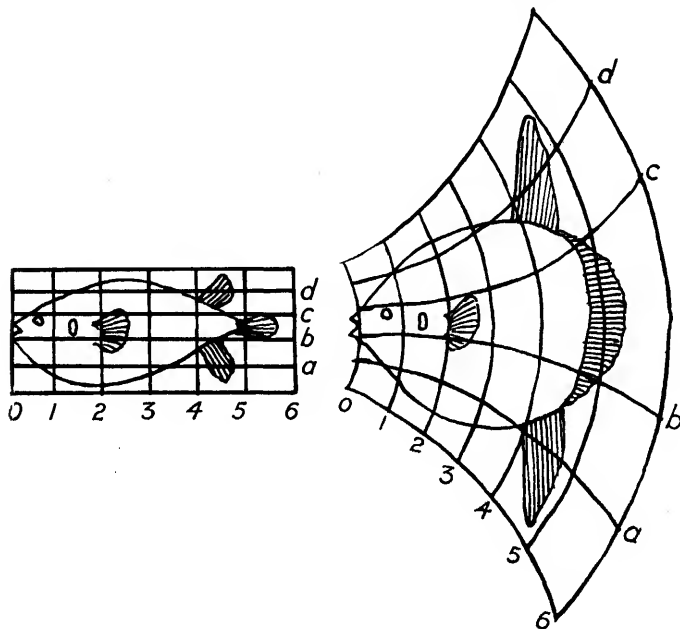


FIG. 8. Outlines of two closely related fish: *Diodon* and *Orthogoriscus*; the coordinates of the latter fish bear the same relations to its anatomy as those on *Diodon*. After D'Arcy Thompson.

draw to scale an adult human and draw similar horizontal lines at the same *anatomical levels* as those on the foetus. If we do this Medawar shows that we shall find that the lines on the adult occur further and further apart in a regular progression from head to toe. It is for all the world as if our drawing of the foetus had been made upon a rubber sheet which was thicker at the head end and tapered evenly towards the feet, and was then stretched so that the general form of the body now shows this regular increase in extension from the head downwards.

How are we to explain these remarkable transformations? It is customary to speak about varying differential growth rates, the regular diffusion of growth stimulating or inhibiting hormones and the like. I find it hard enough to imagine how the various organs

of different textures, complicated blood and nervous systems, etc., can by diffusing fluids, etc. all be distorted in just the right proportions (as are for example the circular spots in our diagrams in Fig. 6); I find it quite *impossible* to imagine how such a mathematical plan

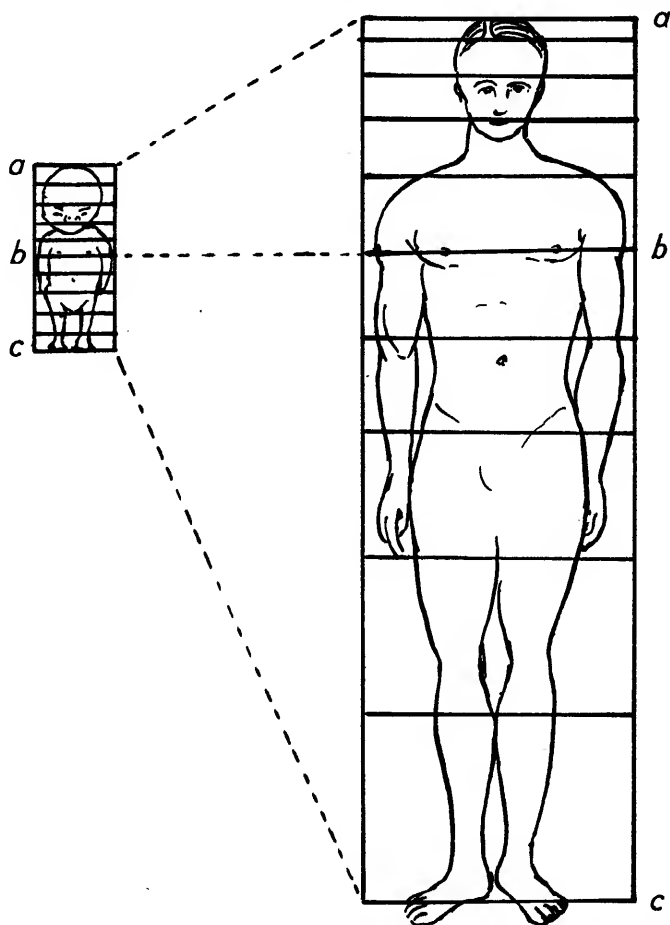


FIG. 9. Drawings of a 5-months-old foetus and an adult of 25 years. The horizontal lines on the adult are drawn in relation to the same anatomical points as are the equidistant lines drawn on the foetus. Redrawn from Professor Medawar's data.

of growth could have been evolved entirely under the selective influence of the very heterogeneous environment. It seems to me to have all the appearance of a definite mental conception like that of an artist or designer—a pattern outside the physical world—

which in some way has served as a templet or gauge for selective action ; it is suggested, as before, that it is this plan in the group mind which indirectly *selects* those gene complexes presenting its best expression. It is a species plan mirrored in each individual : a plan which in evolution may be stretched or warped in various ways but always as a *whole plan* is stretched and warped, and usually according to a relatively simple mathematical formula.

The analogy of the artist may be helpful. I do not doubt that the material body can be completely described in terms of physics and chemistry, any more than I doubt that the pigments and canvas of an old master can be similarly analysed ; I do, however, most emphatically doubt that physics and chemistry as we now understand them can by themselves tell us why the artist selects the particular paints he uses for a particular effect. Analogies are always dangerous, but it might not be carrying the idea too far to suggest that the hypothetical group-mind of a species is like *one* artist who is making thousands of reproductions of one picture—a miniature self-portrait, if you like—but each time with a slightly different paint box ; the paints are of course the genes. The chaffinch 'mind' is continually making chaffinch pictures each one slightly different because the available paints vary slightly in the different boxes. The finished painting is due to the combined effect of the many pigments mixed together and the extent of mixing is not the same, of course, over all the canvas ; the very picture emerges because in one place one pigment has more effect than in another : so also with the genes. The pictures themselves have the curious property of producing the new paint boxes for the future pictures ; in fact, the range of colours is duplicated in each box and when new boxes are to be produced two pictures are attracted to each other and they divide their colours so that the new paintboxes, to be made available for the artist to use again, have a fresh assortment of paints—half from one picture, half from the other. . . .

One might continue to be carried away by the comparison of chromosomes and the rows of paints in the box ; such fancies lead to folly if carried beyond the purpose of mere parable in general terms. I think the analogy is useful but may easily be misunderstood. I want to make it quite clear that I am not suggesting that the species-(artist)-mind is actually manipulating the genes (the paints) in development to produce each 'picture' anew ; although, of course, that *might* be found to be so. No, I think it likely that the genes are interacting according to chemical and physical laws during development, governed perhaps by a molecular 'code system' as Schrödinger has suggested in his exciting little book *What*

is Life? (1944); the particular code, however, which has fitness in relation both to the environment and to the expression of the way of life of the species-(artist)-mind—during development as well as in adult life—will tend to be preserved, selected, rather than others less well qualified. So by change of environment and perhaps the slow broadening of the experience of the 'artist mind', the pictures which are repeated again and again, slowly evolve from one style into another. In the general terms of the parable I think we may legitimately say that the biology of the last half-century has been engrossed in the study of the pigments and their interaction as they are mixed in the painting; in the coming half-century I think it possible that psychical research may show biologists—and the world at large—something of the artist as well.

The idea of the artist has been for a long time in my mind; I had almost flattered myself that I could call it my own, when I realized that I must have heard it before. I have been hunting back in my earlier reading: of course, it comes from Aristotle, and comes to me via my old friend Dr E. S. Russell to whom in my general outlook I know I owe so much. Here is a striking passage from his little book, *The Study of Living Things*, p. 61 (1924).

The living thing is not the clay moulded by the potter, nor the harp played upon by the musician. It is the clay modelling itself, or as Aristotle puts it in a beautiful figure, being moulded directly by Nature herself, without the aid of tools 'but, as it were, with her own hands'. Of human products, living things resemble indeed works of art rather than machines. A picture is not merely a collocation of pigments, a sonata a simultaneity and succession of sounds, a book a collection of little black marks on paper—though that is all they are in the eyes of physical science. There is an unanalysable unity in a work of art—the parts form an individualized whole or unity and have a meaning only in relation to the whole. So also in the living thing. As works of art are static organisms, so organisms are dynamic works of art. It follows that living things can, no more than works of art, be exhausted of their content by the analytic and superficial description offered by the physical sciences. They are the sounds and the shapes and the colours, but they are more, as works of art are more. They differ from works of art in being self-creative, or created by Nature as Artist.

The third problem which I think might be explained by the existence of such a 'group-mind' acting as a selective agent is the frequent appearance of a kind of momentum in evolutionary trends pointed out by those who have most experience in studying the sequence of past animals in the fossil record—and usually ignored, ridiculed or minimized by those who have *not* had such experience. I will give brief quotations from two leading palaeontologists each writing in a book called *Creation by Evolution*.

The late Dr F. A. Bather, the great authority on fossil echinoderms (starfish, sea-urchins, etc.) writes on p. 110 :

A drunken man staggers along, veering from one side of the road to the other, stumbling and stopping at random. His aim is not visible ; his course cannot be foretold. How different is the flight of an arrow towards its mark ; rising from the archer's bow, and then sinking in one unbroken curve till it pierces the bull's eye! This mathematical regularity is due to the momentum imparted by the bow and to the pull of gravity ; any deflections due to wind can be allowed for and calculated. Of like nature, and no less due to natural causes, is the regular change of an evolving series of animals.

And the late Dr A. Smith Woodward, equally eminent in vertebrate palaeontology, writes on p. 131 :

A student of fossils recognises that when any kind of animal shows a tendency to change in some particular part, the degree of this change increases in successive generations, especially if the change at first gives some advantage.

There is here perhaps the suggestion of some unit of life beyond that of the transient individuals of the species and one which develops as it goes along, building itself up from the unconsciously shared experience of all the members of the race.

If the hypothesis were true, that there is a 'species-mind'—or actually as many species-minds as there are species—playing such an important part in the process of evolution, is it not surprising, it may be asked, that we ourselves have so little experience of such an entity in our own race? Is there no more evidence than that of the few rare people who have telepathic gifts? Does it seem too presumptuous, or even blasphemous, to suggest that for us the Great Mind or Great Artist corresponding to what we have been suggesting in the animal kingdom, may not be at the heart of all the religions of the world—felt as the sense of the sacred, the sense of mystery or the sense of the communion with something in the Universe greater than the personality of the individual self? To state it thus, proceeding from a consideration of biological evolution toward this idea, it does perhaps strike one as sacrilege ; is not this however a prejudice due to our direction of travel? If we travelled in the other direction and stated the proposition in reverse, would it not seem rather different and much more acceptable to those who do not usually begin their thinking at the biological end? If we said, for instance, that the whole course of our past evolution from the very beginning had been under the unbroken guidance of our Great Creator, would not that seem less shocking? Yet it is really saying just the same thing. As I have said elsewhere, it would not surprise me to find that biology had

just as close a connection with theology as it has with physics and chemistry. The sense of the sacred is just as much a part of the natural history of man as is sex ; but it is a part which has not yet become the subject of science. That the psychologists are apt to link the emotions of religion and those of sex and parental love should not surprise us. The love of the Great Creator which links us with that mysterious source of power, so poetically described in the Psalms of David, may not really be so very far removed in its nature from the other love which is so essential a force in the whole process of organic evolutionary creation.

That fundamental problem of philosophy which Plato gave the world—the concept of the general ideas—may perhaps be found to have reality in the ideas in the Great Mind—ideas which have in fact evolved over long periods of time. I take the following quotation from Sir Richard Livingstone's brief introduction to the philosophy of Plato in *The Pageant of Greece*, p. 284 (1923) :

... his (Plato's) philosophy starts in a logical problem ; how, in the changing manifold world around us, are we to attain knowledge? How do we know that these pieces of wood of different shapes, which we call tables, are tables? In virtue of what do we call beautiful the countless different things to which we apply the word? The reply is : we have this knowledge in virtue of a general 'idea' of a table, and a general 'idea' of beauty ; these 'ideas' exist not on earth, or for our senses, but for our minds, and, to Plato's mind, have a higher reality than the fleeting objects of sense.

That is a standing problem of technical philosophy—the question what knowledge is. But in Plato it grows into something much wider. It raises the questions ; What is the Soul? What should men pursue—money, power, the various material things which we live among, or something else? Why do we take pleasure in beauty—a beautiful face, violets scattered along a hedge foot, the words of a great poet? What value have these beautiful things? These are real problems. . . . For Plato, Soul controls the world and is the supreme reality. It exists, entirely pure, as God ; but it is also present in all living things, more dominant in some than others, though in all mixed with and impeded by earthly elements.

All true religious experience points to the existence in all races of men of a feeling of being in touch with some greater power beyond their individual selves. The late Dr R. R. Marett when discussing primitive religion writes in *Head, Heart and Hands in Human Evolution* :

When it is a question of a more or less definitely religious rite of the primitive pattern, we should be wrong in assuming any consistent doctrine to underlie the performance. . . . It is a common fallacy to suppose that the savage has forgotten what it would be truer to say that he

never tried to understand. A play of images sufficiently forcible to arouse by diffused suggestion a conviction that the tribal luck is taking a turn in the required direction is the sum of his theology ; and yet the fact remains that a symbolism so gross and mixed can help the primitive man to feel more confident of himself—to enjoy the inward assurance that he is in touch with sources and powers of grace that can make him rise superior to the circumstances and chances of this mortal life.

It is particularly illuminating to compare that passage dealing with the religion of the savage with a paragraph in the postscript which William James added to his celebrated Gifford Lectures on *The Varieties of Religious Experience* (p. 523). Here he briefly explains his philosophical position in relation to religion. After stating that he cannot accept either popular Christianity or scholastic theism, he goes on to express his belief that communion with the 'Ideal' (or God) brings into the world 'a new force' which 'alters events in it'. He writes as follows :

If asked just where the differences in fact which are due to God's existence come in, I should have to say that in general I have no hypothesis to offer beyond what the phenomenon of 'prayerful communion', especially when certain kinds of incursion from the subconscious region take part in it, immediately suggests. The appearance is that in this phenomenon something ideal, which in one sense is part of ourselves and in another sense is not ourselves, actually exerts an influence, raises our centre of personal energy, and produces regenerative effects unattainable in other ways. If, then, there be a wider world of being than that of our everyday consciousness, if in it there be forces whose effects on us are intermittent, if one facilitating condition of the effects be the openness of the 'subliminal' door, we have the elements of a theory to which the phenomena of religious life lend plausibility. I am so impressed by the importance of these phenomena that I adopt the hypothesis which they so naturally suggest. At these places at least, I say, it would seem as though transmundane energies, God, if you will, produced immediate effects within the natural world to which the rest of our experience belongs.

I will take one more example of evidence of such a force working in man. Sir Frederick Bartlett in his Riddell Memorial Lectures for 1950, *Religion as Experience, Belief, Action*, writes regarding the power of religious action (p. 35) :

I confess that I cannot see how anybody who looks fairly at a reasonable sample of actions claiming a religious sanction can honestly refuse to admit that many of them could not occur, or at least that it is highly improbable that they would occur in the forms in which they do, if they were simply the terminal points of a psychological sequence, every item in which belonged to our own human day to day world. I am thinking not of the dramatic and extraordinary actions which people who write books about religion mostly seem to like to bring forward. They are

rare any way. I remember the ways of life of many unknown and humble people whom I have met and respected. It seems to me that these people have done, effectively and consistently, many things which all ordinary sources of evidence seem to set outside the range of unassisted humanity. When they say 'It is God working through me', I cannot see that I have either the right or the knowledge to reject their testimony.

These three passages which I have just quoted I regard as examples of much more, but perhaps not so concisely stated, evidence from the natural history of man which we cannot ignore when we are considering the possibility of extra-physical influences in the life of organisms in general. What we recognise as religious experience in ourselves may be the development of something similar in all animal life: the relation of the individual to the greater entity of the species-mind.¹ I am not saying that either the hypothetical species-mind or our higher Great Mind is simply the equivalent of the sum of all the individual subconscious minds; it may be so, or it may be they are very much more, linked in turn with a Higher Unit still—we have no means of knowing. I must not spread more into this field here—I have discussed the relations between biology and religion more fully in my Essex Hall Lecture of 1951.²

Whether or not there turns out to be a particle of truth in the hypothesis I have been developing here in relation to some of the remaining unsolved problems of evolution theory, I hope biologists may agree that there is at any rate a case for a thorough and big-scale investigation of psychic phenomena to see how these curious paranormal events link on with the rest of living processes

¹ It is impossible in a paper of this character to attempt to work out all the possible implications of such a hypothesis in full. The following points should however perhaps be mentioned. When applied to the Animal Kingdom it would appear to mean that the holding together of the species-mind would in some way be correlated with the interbreeding of the population. If two populations of one species become geographically separated from one another for very long periods of time and then come together again, it is usually found that there is now no longer an attraction between a male of one race and a female of the other. The two races no longer interbreed and become more and more distinct as their two 'pools' of available genes become in isolation more and more distinct. This is the origin of new species—there are now two in place of one: presumably there would now be two species-minds in place of one. It might be, if the hypothesis were true, the very isolation of the two parts of the species-mind over a period of time which now leads to the loss of affinity between them and so to the complete separation of the stocks, and thus to speciation.

² *Science and the Quest for God*, published by The Lindsey Press, 14 Gordon Square, London, W.C. 1.

as *surely they must*. I have confined myself to the consideration of telepathy alone ; there is the whole field of hypnosis as yet so little understood and recently brought so sharply into the biological field by the well-recorded case of a successful hypnotic treatment of congenital ichthyosis (Crocodile skin).¹ Regarding this case, Dr F. Ray Bettley writing in the *British Medical Journal* of November 1st, 1952, says 'It is surprising that it should respond to any kind of treatment ; that it should respond to hypnotic suggestion demands a revision of current concepts of the relation between mind and body'. It is no use going ahead developing biological theory while ignoring remarkable facts which do not in any way fit into it. We must do many more experiments with some of these rare people who have these paranormal faculties so much better developed than others : but we must investigate with the greatest possible caution. It must be possible by the use of soundproof chambers, duplicate cine-camera recording simultaneously from different angles, etc., to make absolutely certain that there is no possibility of fraud or subconscious hyperaesthesia. Far more difficult to counter is the danger I have already referred to, of the experimenter himself being biased and so coming to false conclusions ; the only safeguard for those who are emotionally interested, as I am myself, in the rejection of materialism, is for them to persuade someone who has the opposite point of view to join in investigating with them. Clearly I should be joined by an atheist. In spite of these difficulties and dangers, if the work could be on a big enough scale, it should be possible before long to begin to repeat effects and begin to establish some of the laws in this almost unknown field. An experiment of particular interest was that done by Sir Oliver Lodge in 1884 which I have already referred to on p. 101 ; if such a combination of the impressions from two separate agents upon one percipient could be frequently obtained, it should be possible by varying the conditions for one agent but keeping the other constant, to begin to make out some of the principles involved.

There are sure to be some who still think that we are making a lot of fuss about some very trivial abnormal events which they feel cannot really affect the general principles of Biology at all. Let me bring to my support the views of Professor H. H. Price which he gives at the end of the article in the *Hibbert Journal* to which I have already referred :

On the contrary, these queer facts are not at all trivial, and it is right

¹ *British Medical Journal*, August 23rd, 1952, and subsequent correspondence. (Reported in *Journal S.P.R.*, Vol. 36, p. 716 and Vol. 37, p. 25.)

to make the greatest possible fuss about them. Their very queerness is just what makes them so significant. We call them 'queer' just because they will not fit in with orthodox scientific ideas about the universe and man's place in it. If they show, as I think they do, that the Materialistic conception of human personality is untenable, and if they throw quite new light on the age-old conflict between the scientific and the religious outlooks, we shall have to conclude that Psychical Research is one of the most important branches of investigation which the human mind has ever undertaken.

Perhaps to end I may be allowed to quote one paragraph from the end of my recent Essex Hall Lecture :

The great institutes for scientific research having a bearing on man's bodily comfort—upon medical problems, direct and indirect, agriculture and fisheries, food, transport and so on—are dotted about the country, and are as symbolic of the present age as our glorious cathedrals and parish churches are symbolic of our spiritual past. If only one per cent of the money spent upon the physical and biological sciences could be spent upon investigations of religious experience and upon psychical research, it might not be long before a new age of faith dawned upon the world. It would, I believe, be a faith in a spiritual reality to match that of the Middle Ages ; one based not upon a belief in a miraculous interference with the course of nature, but upon a greatly widened scientific outlook.

Yes, to come back to my opening question, I believe psychical research if given sufficient support could revolutionize the outlook of biologists in a matter of twenty years or less—and so also that of the world at large.