

other than aptitudes they will be of limited value. This is especially true where the tests rule out the effect of unconscious forces in selection for the largest section of any unselected population. This is an omission for which the individual scientist and also science as a whole pay a high price.

If my answer to Dr. Humphreys has been critical, it is because it is distressing to me to have psychologists and psychiatrists take sides on this issue, instead of working together to contribute each from his own position. The psychiatrist should no longer have to hammer away at his psychological colleagues to induce them to include a consideration of unconscious forces. Actually this rarely is necessary now in our relationship to clinical psychologists. But the aptitude testers have lagged behind, caught up as they are with their fetishistic counting. Counting has its place, but counting alone can be one of the most sterile and obsessional and unproductive of all occupations. Finally, I would point out emphatically that the repeatedly stated purpose of my original papers was to urge that studies of this important

problem should be made. At no point do I suggest a program "designed to predict and control unconscious emotional forces." This misstatement is contained in Dr. Humphreys' final summarizing sentence. Instead I make a specific plea for a detailed investigation of a problem which I believe to be of critical importance for science and scientists, and for our whole culture.

I may add that I consider that the problems under discussion are equally important in every other field of education, as was stated recently in an article on "The Forgotten Man of Education" (*Harvard Alumni Bulletin*, February 6, 1954, 56, 349-353). The earlier articles in the *AMERICAN SCIENTIST* were limited to scientists, because of the uniquely important position of science in the world today. None of these problems is exclusively a problem of scientific education, but I know of no better place to begin a study of this issue than among ourselves. What I regret most about Dr. Humphreys' reply is the implication that there is nothing to study, and that bigger and better aptitude tests constitute the total answer.—L. S. KUBIE, M.D.

## THE PRESENT STATUS OF PARAPSYCHOLOGY<sup>1</sup>

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In these days, when you meet up with a parapsychologist, or with one of their fellow-travellers intent on defending them from the arrogant intolerance of the orthodox scientists, you are likely

<sup>1</sup> This comment is based upon and written after reading a new volume, *Modern Experiments in Telepathy*, by Soal and Bateman, published November, 1954 by the Yale University Press, New Haven [11], and after twenty-five years of active interest in the psychology of scientific controversy [1, 4] and a considerable interest in parapsychology because it is controversial, but my comments must depend on Soal and Bateman as my paradigm, for I am not an expert in this voluminous literature.

to be told: "Now that extrasensory perception is an established fact, parapsychology must devote itself to investigating the conditions under which the new phenomenon occurs." And so, with a sigh, you think back along the history of the accumulation of scientific fact. Is phlogiston a fact? Was it a fact? If it was and isn't, what stopped its being a fact? A new theory? Is a fact then a theory? Is scientific truth just current scientific theory? (Yes.) Is the atom, in this age of the atom, a fact? Is the indivisible atom a fact? Etymologically, yes; the word means something not

cut up. But actually, no. The atom is a theory and theories, although they represent reality, change with the *a posteriori*'s of observation and the *a priori*'s of human values.

And when you start thinking about new theories, new facts, new truths, you find yourself immersed in the history of controversies, for men, getting their egos tied up with their theories and their facts, fight one another for intellectual self-preservation, and clarity about the matter is often reserved only for their posterity which has no commitments to preserve. The controversies of the past seem silly to a posterity that understands them, that can say what men thought and why they thought it. *Tout comprendre, c'est tout pardonner*. Did Mesmer have animal magnetism? Mesmer thought he had. Respectable scientists thought he had not. The Académie des Sciences said not, said it then, and finally, almost a century later, reversed itself to say that hypnotism is a genuine phenomenon. Or was it that the theory and not the Académie had reversed itself? To the uncommitted student scientific controversy seems absurd and unnecessary, but actually it is not, for controversy is the inevitable symptom of that plus of emotional certainty and determination that gives men the patience to keep on with the monotony as such researches as Soal and Bateman have accomplished in parapsychology and the psychologists who criticize them have accomplished in their own fields. Research is fun for those who prosecute it, but only because they feel somewhat passionately about it. Passion may blind those whom it drives; nevertheless the rule of the history of science is that, after prejudices have fought each other, posterity benefits.

One of the contemporaneous fields of scientific disagreement is parapsychology or extra-sensory perception (ESP) or whatever you call the modern child of what thirty to sixty years ago went by the name of *psychic research*. There has long been an interest in what have been called supernatural psychic phenomena,

as the existence of an English and an American Society of Psychical Research attests. This field was regarded with suspicion from the beginning because the séances with mediums turned out so often to be fraudulent. Most scientific psychologists had little tolerance for it, and William James, who continued to keep an open mind toward the possibility of communication between persons, communication outside the ordinary channels of sense, found himself branded with guilt by association.

During the last twenty-five years scientific attention in this field has been centered on the perception that seems to occur without ordinary sensory mediation, research in which J. B. Rhine in America and S. G. Soal in England, with their enthusiastic associates, seem to have been the most important contributors. These patient researchers and others have kept at a problem, so slow in yielding positive fact that outsiders have had as much difficulty in understanding their persistence as Rhine and Soal have had in understanding the intolerance of the outsiders. Now, however, this small in-group of parapsychologists claim to have established the existence of ESP or *psi*, as the new kind of communication is called, and the scientific world has the right to ask just what they mean by this assertion. Such an enquiry is made easy by the recent publication by Soal and Bateman [11] of their *Modern Experiments in Telepathy*, a volume which reviews the history of telepathy and parapsychology to about 1930 and then continues on to the present with a detailed account of the researches of the authors in England and with casual reference to contemporaneous American work. The volume is not a complete handbook, but it is a complete case for the existence of telepathy and for some kinds of clairvoyance as the evidence exists in 1954.

There are a good many kinds of extra-sensory perception. There is *telepathy*, in which information in the mind of the percipient, a telepathically sensitive person, corresponds to information in

the mind of some agent, another person with whom the percipient is in rapport. There is *clairvoyance*, in which the percipient has information that corresponds to the information inherent in some objective situation, like the order of the cards in a pack, information which no one knows. Obviously, if the agent looks at the members of a set of cards furnished him by the experimenter, then you have, if a percipient guesses the cards correctly, either telepathy or clairvoyance, depending on whether the percipient was reading the agent's mind or the agent's cards. A great many of the Soal and Bateman experiments were of this sort.

Then there is *precognition*, which may be either telepathic or clairvoyant, according as the percipient calls off correctly the information that is going presently to be in the mind of the agent or the object that is going presently to be the target of attempted clairvoyance. Since this conception seems to imply that time can be reversible in respect of the cause-and-effect relation, the conventional scientist is likely to reject this suggestion with a snort of disgust, but let him not be hasty. There are many situations in which the future is found to determine the present. When the case is well analyzed, it may appear that the future is adequately represented by its past, which in turn appears to determine the present. In this way Hume's conception of a causality that pushes and cannot pull is preserved. In precognition no such analysis has yet been made.

In Rhine's laboratory at Duke University there is also *psycho-kinesis*, in which a sensitive subject is said to predict or determine a future event, as when the lie of a die is predicted before the throw is made. The difference between precognition and psycho-kinesis lies solely in the question as to whether there is any crucial movement after the prediction is made, but, since Soal and Bateman do not deal with psycho-kinesis, we can simplify our difficulties by leaving it out of this account.

One of the paradoxes of the psychic

research of the 1920's and earlier was that the psychic researchers were trying to prove the existence of the supernatural. That was, of course, impossible. You cannot prove a universal negative. If you got hold of a repeatable inexplicable case of communication, then you tried your best to explain it, that is to say, to find some familiar physical or physiological account of it. If you succeeded in that, you had failed in your main endeavor. The phenomenon was "normal." If you failed to find an explanation, then you had succeeded as well as you could ever hope to succeed in the main endeavor of establishing the existence of the "super-normal." This paradox puts a premium on stupidity. To lack the ingenuity to find a normal explanation was to support the negative, the supernatural hypothesis. We must not again fall into this trap.

The question as to whether phenomena are normal or supernatural is meaningless. The thing to do experimentally is to establish correlations between paired events. The reliability of such generalized observed correlations can be an initial hypothesis, subject to confirmation. Thus you get what may be called an empty correlation: *if A, then B*, when you still do not know "why" A causes B. For the ingenious investigator other hypotheses will spring up and be subject to experimental test. If the correlation is eventually filled in with a natural causal train of events, you have achieved a normal physical explanation. If it stays stuck at an action-at-a-distance, perhaps you would wish to call it supernatural. In that sense gravity is supernatural, and light became supernatural when we took the luminiferous ether away from it. Those positivistic physicists who think that the electron cannot have position and velocity at the same time, because you cannot observe both synchronously, certainly have something abnormal on their hands. The psychologist B. F. Skinner, noting the lawfulness of the correlations between stimulus and response in an organism for which we remain ignorant of the neural

or other connections between the two—an “empty correlation” for an “empty organism”—joins Newton in showing that action at a distance is not an unscientific concept [9, 10, 12].

My point here is that Soal and Bateman are not right when they suggest that physical explanations of parapsychological phenomena are a regression to the quaint materialism of Thomas Huxley. There are more mysteries in modern physics than parapsychology ever dreamed of. Did they hear J. Robert Oppenheimer's Reith Lectures on the BBC, now published as *Science and the Common Understanding* [7]? Have they read J. B. Conant's *Modern Science and Modern Man* [5], where the author denies that science seeks the nature of reality, affirming that its theories are not beliefs but policies (he is quoting J. J. Thomson in 1907) and ending with a chapter on science and spiritual values, a chapter that would let into science even Soal and Bateman's dualism of mind and body? Conant rejects old-fashioned materialism and new fashioned dialectical materialism in the interests of a modern physicalism, broad enough to accommodate anything that Soal and Bateman would want housed within science.

Soal and Bateman's book is a laboriously careful account of experiments and a discussion of the significance of the results. It is almost dispassionate, which makes it an excellent ground for unemotional consideration, even though its authors do let a few barbs show when they discuss the psychologists' criticisms of parapsychology. After giving the early history of parapsychology, they enter upon an account of their experiments with Basil Shackleton and Mrs. Stewart, a description and report that fills almost two-thirds of the book. Actually the book is deadly dull, because its authors have used an inductive descriptive style: they describe each experiment in detail, the setup, the precautions, the method, and then, as often as not, they end with the bare statement of the number of successes out

of a total and the notation that the result is therefore not significant.

Yet sometimes the results are significant, and, when at last they summarize four years of irregular work with Mrs. Stewart, they find themselves in possession of the data of 37,100 trials which yielded a score of 9410 direct hits, an excess of 1990 hits over the “chance” expectation, which would be one-fifth (for there were equal numbers of five kinds of cards in the pack guessed) of 37,100 or 7420. That gives a critical ratio of 25.8, one that runs off the probability tables but that they say is of the order of odds of  $10^{70}$  to 1 against the hits being “chance.” Of this more in a moment.

Mostly Soal and Bateman worked with telepathy without the exclusion of clairvoyance, but some of this work was with clairvoyance alone. With Shackleton precognition turned up: his guesses showed the greatest number of hits when compared with the card next ahead in the series, and, when the rate of guessing was increased, the precognition shifted to the card next but one ahead. Mrs. Stewart did not seem to show precognition, but, when they came to study the patterns of her guesses for intraserial effects, they found a significant negative precognition: she avoided guesses which would have been right for the next succeeding card more than “chance” would “allow.”

Now as to this business of the odds being so much against chance. Nearly all these experiments were done with cards, five cards of each of five kinds, thus packs of twenty-five—usually the Zener cards with geometrical forms on them or cards with five kinds of animals pictured on them. When you work in this way you are apt to take to your bosom the law of insufficient reason [3]. (“Equal distribution of ignorance” is the name I like for it, because that is what it is; when you cannot tell what the frequencies should be, you assume that they are equal.) Thus, with packs of this sort, you define the “chance” frequency as one-fifth of the total trials. You take the difference between

“chance” and the number of “hits” (correct guesses) and you divide it by the standard deviation, computed for  $\frac{1}{5}$  and the number of trials, to get the critical ratio. Then you look up in a probability table the odds against chance that belong to this critical ratio. The table that results from this procedure is like this:

Critical ratio	Odds against chance	Critical ratio	Odds against chance
1.0	2.15	4.5	147,190
2.0	21.	5.0	1,744,000
2.8	195.	6.0	500,000,000
3.5	2,149.	7.0	$10^{11}$
3.8	6,915	8.0	$10^{15}$
4.0	15,770	25.0	$10^{70}$

It is obvious that this use of the probability model constrains “chance” within a tight pocket. Soal and Bateman chose 200:1 as the odds against chance which they will accept as practically not chance, as significant for them, and here they would have the support of most of their psychologist-critics. When the frequency deviation is 2.8 times its standard deviation the odds are 195:1, so 2.8 is critical for the critical ratio. Double this observed deviation with everything else constant, and the odds increase to somewhere between 2 million and 500 million. A critical ratio of 3.3 gives odds of a thousand to one.

Suppose now you have a “brilliant” percipient, who remains able to guess unseen cards correctly for a long time, and suppose that for 1000 guesses he averages seven correct guesses out of every twenty-five, when five correct is “chance” expectation. That is what is meant by brilliant in this context, seven instead of five correct out of twenty-five. Seven out of twenty-five is 280 out of 1000, with 200 being “chance,” so the difference is  $280 - 200 = 80$ . The standard deviation is found to be 12.25 for 1000 trials, so the critical ratio is 6.3, and the odds against 7 in 25 being chance are about 500,000,000 to 1, a ratio which you can take as the odds for ESP. You see a “brilliant” performance in telepathy is

not so very striking after all. It is only 7 out of 25 instead of 5 out of 25. When people ask why these able percipients do not get rich by telepathing directors’ meetings and playing the stock market with their superior knowledge, they do not know how small an advantage the best available telepathy of the modern age provides.

This reviewer’s preference would be to get away altogether from the dubiety of the probability model, a model whose fit can never be tested empirically because every observed deviation from expectation still fits the model which tells you merely how improbable the deviation was [3]. It would be better to have scientific control rather than to depend on this faith that ignorance of bias requires the occurrence of certain frequencies, but is parapsychology yet ready to use this basic scientific principle? In a good experiment you would turn telepathy on and note the number of hits. Then you would turn it off—the control experiment—and note the number. If the difference were large enough to show that you are probably not in the two series dealing with the same populations of guesses, then you have ESP and also an indication of how surely you have it. But how do you turn telepathy on or off, controlling the independent variable? The best you can do is to take a good scorer and a poor scorer and compare them, or perhaps get a few guesses from each member of a huge sample of the population and use those frequencies as the norm. Yet not until we get away from this *a priori* conception of chance are we going to be able to write a good operational definition of telepathy.

One wonders how in experiments of this sort the percipient concentrates on the particular set of objects or thoughts intended. Why are not all sorts of casual information, irrelevant to the experiment, picked up? Telepathy is, however, aimed. If the cards to be guessed have pictures of animals on them, the percipient does not pick up a real horse outside in the street or even the horse on a

card in another adjacent pack. Soal and Bateman tried having two agents when the percipient thought there was only one, and the agents had packs of cards which agreed with each other at no point. The percipient picked one agent, the one she knew about. If she knew about both, she seemed to pick the one she liked best, but that is not certain. Attempts to get her to obtain part of her information from one agent and part from another did not work out well.

The guessing is automatic, mechanical, and without any conscious elaboration. Soal and Bateman like to say that the mechanism is unconscious. There is nothing voluntary that the agent or percipient does, except that the agent knows the cards in order and the percipient guesses without thought or deliberation. There is a similar kind of un-deliberative judgment that works well in psychophysics when sensory thresholds are being obtained. The stimuli are presented and the subject just listens to himself talk.

Good percipients are rare—the seven-out-of-twenty-five kind. Soal and Bateman have had only Shackleton and Mrs. Stewart at this level. They have no such luck with college students as Rhine does. Good percipients are also erratic. They go bad. Sometimes they get good again. You never know how long one is going to last. Not only can you not turn ESP on and off, but you never know when what you have will be turned off on you. Parapsychology lacks a satisfactory independent variable.

Is parapsychology science? That is the question about which contention waxes warm. And it is not a sensible question in any exact sense. One way of coming at a partial assessment of parapsychology's scientific status would be to ask: Can you formulate an operational definition of telepathy or of clairvoyance—always remembering, of course, Conant's [5] warning that "clear-cut operational definitions are never possible in the infancy of science." Let us take clairvoyance so as to get rid of the moot question as to whether general telepathy

is partly or even entirely clairvoyance and see what we can do.

*Clairvoyance is the agreement between the guesses of a person, called a percipient, and a set of objects that he is trying to guess correctly, guesses made when the ordinary channels of sense cannot be used and no one is privy to the order of the objects.* You might stop there, but this definition needs to include the notion of "chance" correspondence. So you will try to change the formula to say *the excess of agreement over or the decrement of agreement under the agreement in the control observations.* If parapsychology will not admit yet of control, then you must stick to *chance agreement* and define *chance*, but that surrender makes the new science very young indeed. Perhaps then you will want to say *an excess or decrement at least 2.8 times its standard deviation*, or you might choose some other critical value. This would be enough.

The other properties of clairvoyance are observed and do not belong in the definition. The chief difficulty lies in the vagueness of the control, but then you cannot expect clear-cut operational definitions in a very young science. There is nothing here about normal and abnormal. There is the unfortunate exclusion of ordinary channels of science, a negative which may always be discovered not to hold, just as gravity could be operationally defined as a force between bodies that are *not* materially connected. There need be no objection that the explanation suggests no causal continuity between the related terms; no such relation belongs in an operational definition, and the "empty correlation" differs little from action at a distance with Newton or B. F. Skinner [9, 10, 12]. Parapsychology is a very new science, and a great deal of labor has to be spent on a very small result—or so it seems to this reviewer—but it could be allowed to go its way and eventually to prove itself if that is to be history's verdict.

One presumption that seems to me to be a distinct defect in the thinking of Soal and Bateman is their addiction to

mind-body dualism. My own conviction is that a positivistic physicalism leads you into fewer errors, fewer vaguenesses, more rigor and more care than does a faith in the mystery of mentalism. Positivism is not a throw-back to nineteenth-century materialism. It is what physicists and psychologists have with effort been fighting their way through to in the present century, and in that progress I think American psychologists are ahead of the British—certainly far ahead of what Soal and Bateman seem to want in psychology.

Positivism does not deny consciousness or mental states; it reduces them to the operations by which they are observed. It denies only the unobservable mental states, and surely science has a right to claim that it cannot be asked to accept as data the unobservable. If Soal and Bateman want us to believe only in the existence of observable mental states, then they are physicalists, whether they know it or not, for I can provide the physical accounts of their defining observations. If they want privacy for their mental events, a privacy that cannot be published, then even I, who am arguing for tolerance, would rule their ineffables out of science—rule them out as the best way to keep the semantics of science univocal. American behavioristics have developed slowly out of dualism, pouring out the bath while hugging the baby, and I think the parapsychologists will benefit more by understanding this new way of describing conscious events than by regressing to a dualism that never worked scientifically. "Remember," I would say to them, "that science is a policy, not a picture of truth. It has got to work to be kept."

There can be no doubt that the public likes magic, that the inexplicable, which is never more than the as-yet-unexplained, has a popular appeal. The history of the belief in magic and the supernatural as a means of coming partially to grips with the adventitious threats of nature is as old as the history of thought. Apparently the Yale Univer-

sity Press does not know this fact when it says in the advance notice of the Soal and Bateman book: "If the evidence that some events in the external world can be perceived in a manner independent of ordinary channels of communication withstands critical examination, and if it appears that the extrasensory process has quite peculiar properties, particularly in relation to time, many mechanistic explanations that have proved so fruitful in modern science will require reconstruction." Nonsense, say I. All you have got yet for extrasensory perception is an observed difference between two frequencies, between hits and misses, and a great deal of ignorance as to what causes the difference. Ignorance does not overthrow old concepts. The "how" of this correlation may turn out to be given in terms of old concepts or new. There is nothing startling in the recognition of that fact. Nor is there any fact discovered here to make it seem that time works backward in the causal relation, any more than time always has been reversed when you observe a Humian correlation between the future and the present. Perhaps the future and the present are both determined by a common antecedent. Nor is there anything beyond the desire to believe in magic to make you expect that present ignorance will lead to a startling and unconventional conclusion. At any rate you need an acceptable theory of the nature of ESP before you can have any opinion on this matter. The burden of proof lies on the innovator and the demonstration of ignorance is no proof.

Am I fair? Just what has been discovered?

The new fact is an "empty correlation," one whose novelty could evaporate overnight if its defining conditions came suddenly to be specified more exactly. You have a target of thoughts or cards or both. A person guesses the items of the target in order. Mostly people guess near what is called "chance frequency" as fixed by probability theory. Sometimes an especially sensitive percipient turns up, one who does con-

sistently better than "chance" for a significant stretch of time, though his "ability" may break down suddenly. There is no good experimental control for these observations, but, if you rely on the probability model as most statisticians and psychologists do today, you can get a difference that seems small, yet is of the order of other scientific differences that are conventionally regarded as highly significant. And that is all you know now. There is no good evidence as to how this difference occurs, or why, or when, almost no knowledge of what it is about the sensitive percipient that makes him good. The new fact is a bit of knowledge embedded in a mass of ignorance. I can see nothing startling about it. Ignorance in science is commonplace.

If you go on from here, you try first to reify the newly discovered relation. You call it *telepathy* or *clairvoyance* or *ESP* or *psi*. You are thus creating a hypothetical construct that intervenes between the positive data of the target and the positive data of the guesses, something like the luminiferous ether. That is a convenient and proper scientific procedure, but reification at this stage tempts you to forget how little is known about the properties and conditions of ESP. An operational definition of ESP leaves you only with this difference and a number of negative conditions about there being no normal channel of communication. Nor can you repeat the experiments at will because you cannot order up a sensitive person and have him perform. You have only good performance as a definition of the good percipient, a circular definition. It is an awkward situation, but still you are within science. When at last the unpredictable splurge of correct guesses turns up, then you have got a psychological nova. You do not rule novae out of astronomy because you do not know their conditions, because you cannot predict them.

Trouble arises, however, when magic is imputed to the new concept. Now the scientific protection against magic is

positivism: never impute to a concept more properties than its operational definition allows. All we know about ESP is that it is a difference in frequencies. Soal and Bateman offer us a little magic when they imply that our ignorance of the nature of ESP should make us hospitable to the acceptance of some strange new principle that contradicts current physical law, when they urge that mind is not necessarily bound to the body, as K. S. Lashley's failure to establish the mind-brain correlations leaves possible (6).

Right here, I am convinced, lies the reason for all the heat of this controversy. The experimental method, which includes control, was invented and used by the natural philosophers because they mistrusted their own free inductive intuition. Such people are nowadays called scientists. Professionally they are humble, for they accept this constraint upon the free range of their imaginations. Personally scientists are egoists, for thus they get the drive that keeps them at research, but they fight the magic that is uncontrolled imagination. They sense magic in Soal and Bateman's addiction to dualism and they are angered when the public, preferring the magic of the free interpretation of ignorance to the solidity of observed fact, departs from positivistic safety to accept intuitive unobserved mystery. They are angered because uncontrolled intuition is what they as scientists are fighting, and they see in the way that parapsychology transcends its observed data a threat to what is basic in science. They are not angry, I think, because new scientific hypotheses are being proposed, but because they think the parapsychologists transcend their observations, finding mystery more exciting than fact. This anger does neither the scientists nor science any good, I should say, and it may well be diminished on both sides by understanding it.

Soal and Bateman have a little to say about why orthodox psychologists are so caustic in their criticisms of parapsychology: orthodoxy, they say, has to



defend what they call conventional physical materialism, of which the general validity is a faith and not a fact and so must be supported with invective since it cannot be proved. There is no doubt that such is often the response of scientific orthodoxy in any age to contradiction of its basic assumptions, but I think that Soal and Bateman did not go quite far enough. The issue in the case of parapsychology lies between observed fact and the intuition of magic, and, in generalist general, between positivism and intuitionism.

This argument on motivation can, moreover, work both ways. There have been times when I, for instance, have thought that the persistence of the parapsychologists needed explanation and should perhaps be made a special object of psychological study. Now, after reading Soal and Bateman, I am no longer so sure that the paradox of parapsychology, the motivation that sustains long and arduous research for seemingly small results, needs more explanation than it already has in the history of hundreds of partisan in-groups within scientific activity.

Take, for example, the in-group in which I was brought up—the Cornell school of introspective psychology formed around the self-confidence and erudition of E. B. Titchener [2]. We had a faith. Titchener recognized in himself a Messianic responsibility toward the future of scientific psychology. In the American scene we were more than in the minority; we were a very articulate, attention-compelling, though small in-

group, and the attacks from outside tended to solidify our determination to be true to what we refused to debase by giving it a special name—to be true to “psychology” and to “science,” we said. Where is that group now? In America almost nowhere. Philosophers and A. A. Roback [8] may be the only Americans of importance who would wish it back, though Soal and Bateman make me think there are more psychologists of this kind—dualists—in Great Britain. It is possible to think of Titchener’s life as wasted, devoted to the demonstration that a plausible scientific method would not work. Certainly the good things that Titchener left behind were not the ones he wanted most to bequeath. But that is the way in-groups are and the way that scientific work gets done.

I choose this instance, not because it failed whereas parapsychologists may succeed (if not as much as Newton, at least as much as Mesmer who may be said to have discovered hypnosis), but I choose this instance because it is the in-group that I know so well from the inside, a sincere minority in-group with a strong sense of mission, yet one that now takes on perspective in my mind as the times and I change. Titchener himself used to say, when his gospel seemed to gain little new acceptance: “Posterity is the judge. Let it decide.” Parapsychology seems to me to be a normal in-group phenomenon within that large body of activity that we call science. Of its importance in the developing scientific skein, posterity will be able to judge, and you cannot hurry history.

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BACK TO THE HOMERIC TRADITION<sup>1, 2, 3, 4</sup>

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We have been very much intrigued by Dr. Hedgpeth's article on soliciting reprints (*American Scientist*, 42, 497-499, 1954).

After careful analysis and thorough discussion of the problem, we came unanimously to the conclusion that Dr. Hedgpeth actually missed the point. We believe, in fact, that it is not the reprints that should be abolished,<sup>10</sup> but the articles themselves. And, together with the articles, naturally, also the journals.

We have not yet succeeded in pushing the investigation far enough to decide whether *all* written record, scientific and otherwise, should be abolished. Therefore, at the time of writing, we prefer to

take the more conservative point of view of abolishing only the scientific periodicals.

The advantages that would ensue from even such moderate a course are so obvious that a discussion should be entirely unnecessary. However, in consideration of the fact that some of the minds now devoted to Science seem to operate on a pre-logical rather than on a logical level, an analysis of our proposal may prove not entirely pointless.

The reasons that bring us to recommend the abolition of scientific articles are several. To begin with, these articles are confusing. This is proved by the lengthy polemics which so often arise and in which two or more scientists talk about the same things without being able to understand each other. Such confusion is inherent. Science is a wonderfully complex field and no part of it, however small, can be confined to a few ink marks on a sheet of paper. It requires wider spaces, deeper means of expression. Any effort to confine within the bounds of a few pages even such a simple topic as, for instance, "Thin Sectioning of Smaller Foraminifera"<sup>11</sup> will necessarily create the greatest confusion, which will last as long as the collections of the journal containing the article in question will last in the libraries of the world.

Second, scholars are too intent reading their own articles to pay any attention to the writings of other scholars.

<sup>1</sup> The title and some fundamental ideas were contributed by Dr. H. B. Craig<sup>5</sup> who, being currently involved in a number of lively polemics, refused to appear as co-author of the present article.

<sup>2</sup> Dr. C. A. Hutchison, Jr.,<sup>6</sup> editor of the *Journal of Chemical Physics*, offered many invaluable suggestions directed to deepen the thought and further the scope of the present article. Unfortunately, and for obvious reasons, he preferred not to appear as co-author.

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<sup>4</sup> Requests for reprints of this paper should be addressed to: Dr. J. W. Hedgpeth, Scripps Institution of Oceanography, La Jolla, Cal.

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<sup>10</sup> The fact that, actually, this is not proposed by Dr. Hedgpeth, has no bearing whatsoever on the following.

<sup>11</sup> EMILIANI, C. Notes on thin sectioning of smaller Foraminifera. *Jour. Paleont.*, 25, 531-532, 1950.