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The craft of understanding the mind: why it cannot be a science[1]

El arte de comprender la mente: porqué no puede ser una ciencia

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Abstract

Contemporary psychology has a number of fundamental muddles about basic concepts that have resulted in misdirection of the discipline, and therefore retarded its development. One of these is the misunderstanding of the concept of 'mind' which has led to misdirected efforts to build a science of 'the mind.' The word mind, however, is not a scientific term. When we ordinarily talk of mind, the phenomena to which we refer are not themselves amenable to scientific investigation; rather, they *arise from* phenomena that can be scientifically analyzed. In the present century, this issue has been addressed and effectively resolved by philosophers such as Wittgenstein, Ryle, Austin, and Quine. However, recent developments in the philosophy of 'mind' and of 'consciousness' by, for example, Sears, Dennett, and others, which purport to provide a basis for the recently growing 'cognitive science,' seem to proceed with no cognizance of the earlier insights and consequently they have again confused the matter. On the other hand, the behaviorist approaches in psychology have equally been

1. Invited paper presented in the symposium, "Behaviorism, science, and philosophy: critical assessments", University of Guadalajara, Mexico, February 22, 1996, subsequently to be published in a volume edited by the present author and Emilio Ribes-Inesta. This paper is dedicated to my colleague and friend of old, T.R. Miles, from whom I learned about the matters discussed here although he is not, of course, responsible for the particular views expressed. Beginning in 1963 we discussed these issues at length as he and I worked to establish a new department of psychology at the University College of North Wales. In retrospect I appreciate these discussions more than I did then, as I do his seemingly endless patience. Correspondence should be sent to the author at Department of Psychology, Auburn University, Alabama 36849-5214, USA. E-mail: harzepe@mail.auburn.edu

muddled in statements concerning the concept of mind. These issues and their resolution are discussed in this paper.

Key words: mind, philosophy of science, behaviorism, cognitive science, conceptual analysis, category mistake, conceptual fallacy.

Resumen

La psicología contemporánea tiene una serie de confusiones fundamentales respecto de los conceptos básicos, lo que ha resultado en una dirección incorrecta de la disciplina y, por consiguiente, ha retardado su desarrollo. Uno de estos es la mala compresión del concepto de 'mente' que ha conducido a esfuerzos mal dirigidos para construir una ciencia de 'la mente'. Sin embargo, la palabra mente no es un término científico. Cuando hablamos ordinariamente de la mente, los fenómenos a los que nos referimos no son susceptibles de investigación científica; más bien, surgen de fenómenos que pueden ser analizados científicamente. En este siglo, el problema ha sido tratado y resuelto con efectividad por filósofos como Wittgenstein, Ryle, Austin y Quine. Sin embargo, los desarrollos recientes en la filosofía de la 'mente' y de la 'conciencia' realizados, por ejemplo, por Sears, Dennett y otros, que intentan procurar una base para la recientemente creciente 'ciencia cognoscitiva', parecen proceder sin conocimiento de ideas previas y, en consecuencia, han confundido otra vez la situación. Estos problemas y su posible solución se examinan en este trabajo.

Palabras clave: mente, filosofía de la ciencia, conductismo, ciencia cognoscitiva, análisis conceptual, error categorial, falacia conceptual

I. On rules, conventions, and doctrines

There are a number of rules that, in the course of this century, have become established in psychology as unquestionable conventions. That terms should be 'operationally defined,' that anthropomorphism is a bad thing, and that the principle of parsimony applies in every case, are examples of these. Some others, for example that research *must* proceed from theory, and that a hypothesis *must* precede every experiment, similarly dominated psychology for a time, but fortunately in due course they vaned.

Rules, whether in scholarly work or in daily life, are helpful devices. They short-circuit certain chores and make it possible that processes and procedures

that previously have been worked out do not have to be worked out again from scratch. They can be dangerous, however, if they are followed blindly. History of sciences shows us that often significant discoveries arise from deliberate questioning or disregard of the prevailing conventions of a given discipline.

In the Psychology of the present century, unquestioned compliance with a given set of rules has combined with yet another unfortunate practice; namely, the blind adherence of scholars to a particular doctrine, and its defense with the calculated neglect of all other possibilities. Even now Psychology is cluttered with 'ism's of many sorts. There are the Cognitivists, the Freudians, the Jungians, the Skinnerians, the Radical Behaviorists, -the two do not always seem to be the same- the Humanistic Psychologists, and so on *ad nauseam*. .

The unfortunate consequences of this combination are many. Just one such consequence is that otherwise well-informed and rational psychologists, some of them quite eminent in their areas, come up with statements that are patently absurd to any rational person, and embarrassing to more than a few of their own colleagues. Amongst such statements are "I do not know what 'mind' is" and its variations such as "there is no such thing as 'mind.'"

There are many such hopeless muddles, and they do not promise well for the discipline of Psychology. They are, however, so rampant and so wide-reaching in their effects that dealing generally with all of them calls for a separate work, beyond the brief of the present paper. Here I discuss only one of these, namely the issues related to the concept of 'mind,' and how we may be able to avoid the past muddles about that important concept.

II. Human nature, scientific knowledge, and the mind

A great Shakespearean scholar began his treatise on 'King Lear' as follows: 'For insight into human nature we go to great writers." (Danby, 1949) So we do.

We go to other sources, too: to music and drama, to art, to history —the accounts of sacrifices endured, intrigues engineered, treacheries committed, and loyalties sustained. And, above all, we go to our own personal experiences. It is, after all, quite astonishing how much we apparently know about each other: so much that we are willing and able to from life-long friendships, commit to sharing our life with another individual, decide in whom we can have enduring trust and in whom not, and sadly, whom to hate and whom to avoid. We are, of course, sometimes wrong in these judgements. We renege on our life-long commitments, change our minds about people we have known, and regret

previous dislikes. However, if the reader of these lines were to pause and reflect how often he or she has been right about others and how often wrong, he or she will find far more rights than wrongs. On the whole, but with some distressing exceptions, we are quite good at judging others and living our social lives with reasonable success.

On the other hand, there are some sources to which we would not think of going for insight into human nature. It is hard to imagine any reasonable person seeking this kind of insight in studies of the effects of schedules of reinforcement, in research into information chunking, and in theories of short-term memory. Now, this is not, of course, because such studies are deficient in some fundamental way, and not because they cannot be trusted. They are perfectly sound and important sources of certain sorts of knowledge, namely, scientific knowledge. Scientific knowledge is not, however, the only sort of knowledge, and in many human situations it is not even relevant knowledge. Just as the young woman interested in the man who declares his love for her would not think of starting to take Anatomy and Physiology courses so as to understand him better, we would not go to scientific research into human action so as to understand ourselves and others.

The word 'mind' is not a scientific term. Its natural home is ordinary language, and, as it is always the case with words and phrases of ordinary language, its usage has undergone changes across the ages. One of the usages that has emerged in this way, mostly in rarefied circles of scholarship and speculative thinking, but also in general speech, is as *'the* mind.' This kind of shift in language almost always carries with it the danger that some will be misled by it, and set out in search of the entity thus named.[2] Sometimes such a search will take the form of scientific inquiry, as it has done in the case of 'the mind.'

The idea of a science of mind entails an enormous *category mistake*.[3] In other words 'the mind' does not belong to the category of things that can be scientifically studied. This is not, of course, to say that we cannot sensibly talk about the mind, inquire into what we mean on those occasions when we naturally refer to

2. Some other examples are "personality", "memory", "intelligence" and many others. Some psychologists have set out to establish the location of these, misled into belief in the existence of entities thus named, by the existence of the nouns in the language. I use the term 'existence' here, in the same sense as one might refer to the existence of two cats in my house, or trees outside the house. Personality, etc. do not, of course, 'exist' in that sense; but there are other senses in which they can be said to exist. None of these, however, imply a location.
3. This concept, 'category mistake' is, of course, due to Gilbert Ryle (1949). It has been a significant means of sorting out some of the more trenchant muddles of conceptual discussion.

the mind, and, indeed, conduct scientific inquiries into some of the questions that may arise in this connection. It does, however, mean that if we set out in scientific search of *the mind,* we will find ourselves in a maze of mirrors where there is no exit, and the only escape is through where we came in.

III. Prospects of a science of *the mind*

These arguments show not that we must now give up all hopes of a science of the phenomena from which our concept of the mind arises, but that the conceptual foundations on which many of the past attempts have sought to build a science of the mind have been faulty. To carry the analogy a step further, it is possible to put up quite impressive structures on faulty foundations but they will be impermanent, awaiting unpredictable but certain-to-occur collapse. That is what has happened in the cases of the past attempts in Psychology to build a science of the mind. In the contemporary scene, the so-called cognitivist psychology appears like an impressive structure, together with the associated searches to find philosophical underpinnings for it, but it is, sadly, yet another misdirected, impermanent creation of the tradition that has become entrenched in Psychology. Psychologists who profess 'cognitive psychology' are much given to talk of a 'cognitive revolution' that is said to have taken place some five or so decades ago. But revolutions are not good things in science. Science is a cumulative body of knowledge, always building on existing information and moving beyond. Even the theory of relativity was not thought to be a cause of revolution, resulting in the discarding of the Physics of past centuries painstakingly built by great scholars from Aristotle to Galileo to Newton and beyond. The theory of relativity rose on their shoulders. And no one thought of calling the influence of the theory of evolution upon Biology and related sciences a revolution. Boasting of revolutions may serve political purposes, and it may assist the career ambitions of some; it will contribute nothing positive, however, to science.

Unfortunately, there is nothing in this discussion about cognitive psychology to give comfort to its ardent critics, the convinced devotees of a behaviorist perspective in psychology. In recent decades parts of the behaviorist approach, too, has fallen short of open-minded scholarship. Assertions of the kind discussed above, such as 'I don't know what mind is' come quite often from behaviorist psychologists of our times. Even worse, behaviorist psychologists, apparently unaware of the importance for Psychology of concepts such as 'intelligence," love,' 'happiness,' and 'personality,' —there is a large number of

them— have painted themselves into a conceptually sterile corner. Rather than seeking to understand the phenomena that give rise to such concepts they claim, with no little disdain, that they do not study such things. Instead, they use an esoteric jargon, and discard with perfunctory regard -and yet with, apparently, some degree of self-satisfaction- phenomena that others find interesting. The copious publications on, for example, concurrent schedules have been impressive in their precision and detail, but also in the apparent limitation of their implications. One troublesome aspect of this sort of otherwise admirable, careful behavioral research is that the question, 'what does it all mean?' has rarely been asked in a broad context, and when asked, it has gone unanswered in any significant way. There are theories of 'matching,' theories of 'stimulus-equivalence,' theories of 'autoshaping,' and so on, but there are no attempts to relate the phenomena treated by these large number of seemingly unrelated minor 'theories,' in order to arrive at a broad working picture of what has been discovered so far. In, for example, the so-called radical behaviorist work, the first person to make an attempt of such a broad kind was B.F. Skinner —*Walden Two* was published in 1948, and *Science and Human Behavior*, in 1953— and he is, to date, also the last person.[4]

The researches of the cognitivists, the discoveries of the behaviorists, the conceptual reflections of the philosophers of science, have all to be put together to give, one would hope, at least the early emergence of a picture as will happen at a point when a portion of the pieces of a jig-saw puzzle are put together.

IV. What Psychology studies

That Psychology studies behavior is not a recommendation; and it is not the doctrinaire assertion of a behaviorist psychologist: It is observation of a simple fact.

Psychologists interested in 'memory' study words, numbers, etc. uttered or selected by their subjects from an array presented, those interested in 'chronometric explorations of the mind'[5] study the button-presses of their subjects in temporal relation to stimuli presented to them, and those who have been interested in exploring the mind through the introspective method have studied

4. It may reasonably be asked 'Has nothing new and significant emerged since that time, and if it has, was Skinner a prophet to see it in advance and to treat it in his works?'
5. An attention-getting but misleading title which grossly exaggerates both its content and its achievement.

the statements of their subjects in relation to what they (the subjects) were asked to do. Yet, the question, 'what does Psychology study?' has led to astonishing amount of argumentation, some of it going beyond the bounds of learned debate into acrimony unbecoming to scholars. The difficulty arises from a more fundamental confusion than considered in these arguments. An important distinction needs to be seen between two overlapping but different usages of the verb, 'to study.' First, there is studying a phenomenon by observing it, recording it, carrying out analytical procedures on the collected data, and the like; second, there is studying a phenomenon by deriving implications about its characteristics from what has been observed, recorded, etc. When the behaviorist psychologist claims to study behavior, he or she is focussing on the first kind of study; when the cognitivist psychologist claims to study, say, memory, he or she is focusing on what is to be derived from what has been observed. In fact both, unavoidably, observe and record some set of phenomena; they differ on what they make of their observations.

Consider a simple example. Two promising high school students are engaged, in their school laboratory, in observing and recording the light refractions produced by water droplets of different densities, impurities, etc. One says he is studying the rainbow since the rainbow in part results from refraction of light by water droplets; the other says he is studying what happens to light when it passes through water droplets. Which one is right? What is to be made of any ensuing argument? Of course, both students are correct; they differ in their emphasis, and in neither case does being correct necessitate the other being incorrect. Yet, this is the sort of argument that has troubled Psychology for almost a century, sustained by theoretical allegiances, and cloaked in apparently learned but in fact empty language with esoteric terms of little meaning.

Here we have, once again, a major category mistake. But this time it is an *inverted category mistake.* Gilbert Ryle noted the category mistake where unlike concepts and observations were erroneously treated as if they belonged to the same category. In the present case we have two activities, (I) observing phenomena and (ii) deriving implications from them, treated as if they did not belong to the same category. Yet, they are parts of the same activity which we may term, say, 'scientific inquiry .' In fact behaviorists observe phenomena *and* speculate on what such observations imply, and so do cognitivists, too, differing only in their emphasis. It is astonishing that overlooking this fact has dogged Psychology, and, for that matter, the other social sciences for so many decades.

V. Philosophies and fallacies of a science of consciousness

In the present century, some of the developments in Philosophy of Science and Epistemology have produced significant insights that bear on the fundamental conceptual problems of Psychology. Wittgenstein, Ryle, Austin, Ayer, Quine,[6] and others, the group known as the Oxford philosophers, the Vienna group, have provided pertinent insights the full importance of which still remains to be appreciated in Psychology. On the other hand, unfortunately, the recent and strenuously debated contributions in Philosophy specifically on subjects that might be expected to relate most closely to Psychology, have further confused the same issues. These are the discussions on the prospects of a science of 'the mind' and 'consciousness,'[7] stimulated by, and in turn providing a philosophical basis for, the recent developments of 'cognitive sciences.'

As it may reasonably be expected, these are complex, sophisticated discussions, not readily amenable to brief summarizing. At the cost of gross oversimplification, however, they may be put in two main groups, representing two opposing positions as to the possibility of a science of consciousness. One entails the fundamental assertion that consciousness is the *same* phenomenon as the phenomena of the nervous system, as studied by, for example, neuroscience, computer models, artificial intelligence, etc. (See, e.g., Dennett, 1994). The second holds a view that a science of consciousness is possible, but there is more to it than the phenomena of the nervous system. Various writers included in this group then go their own different ways, describing and prescribing how this 'more' is scientifically to be discovered.[8] (See, e.g., Searle, 1980).

Unfortunately, however, all of these philosophical positions are untenable for two main reasons. First, in these writings, in common with all of the extensive literature that has grown under the head, cognitive science, terms such as 'mind,' 'consciousness,' 'awareness,' 'memory,' and the like are bandied about

6. This is a brief and an almost random list of names of great philosophers whose contributions have been particularly relevant to Psychology For a fuller account of the contributions of those named and not named here, the interested reader is referred to, for example, Ayer's book, 'Philosophy in the twentieth century.'
7. For the purposes of present discussion the words 'mind,' and 'consciousness,' as used by the philosophers in question, are taken to be names referring to the same set of phenomena. It is possible, of course, to point to many significant differences of these terms that may and should be noted in other contexts. In the remainder of this section, the term 'consciousness' will be used for both.
8. In this discussion I have selected two authors as representatives of the two groups; Searle, because his work, in my opinion, his is the best there is, and Dennett's because his book has attracted some general interest. The interested reader is referred to, for example, the fascinating books I list in the references, by Crick, and Penrose.

unquestioned and unexamined. Reading these one would be forgiven to assume that Wittgenstein *et al.* had never written, or that Dennett, Searle, and others had never read them. The former, of course, is not, and the latter cannot be true, however, since the philosophers in question are distinguished scholars. It is as if there is an unstated compact amongst them and other scholars of the cognitive science they wish to promote, to ignore the cautions of Wittgenstein, Ryle, Austin, and others. Wittgenstein wrote 'Say what you like as long as it does not stop you from seeing how things are...And when you have seen this there is plenty that you will not say.' (1953, section 79). It seems that Wittgenstein was too optimistic in the second part of this statement. Second, the arguments in both groups are rendered untenable by three distinct fallacies that permeate them. These I shall term (i) *the empirical assumption fallacy,* (ii) *the implicit assumption fallacy,* and (iii) *the location fallacy.*

The empirical assumption fallacy occurs where an empirical outcome is assumed without the empirical data. Such assumptions are generally said to be intuitive. They are where the writer takes as a given 'fact' what can only be confirmed by *systematic* empirical evidence, in the absence of any such evidence. Assumptions of that kind are persuasive because they tend to be in accord with the existing knowledge of their times. It is not difficult to imagine, for example, how convincing an argument would have been before the discovery of the microscope, that no serious damage can possibly be caused to healthy people by living things so small that no one has ever seen them; or that five million discrete objects cannot be contained in a tiny drop of blood[9] (the number of red blood cells per cubic millimeter of blood). Searle's (1980) much discussed 'Chinese room argument' is of this kind. Stated briefly, Searle asserts that *if* (i) he does not speak Chinese, (ii) he is solitary in a room, (iii) he has a supply of Chinese symbols, (iv) he has a rule-book in English,[10] (v) he is passed a series of Chinese symbols from outside, and (vi) his task is to select, in compliance with his rule-book, appropriate responses from amongst his supply of Chinese symbols and to pass them out, *then* he would do this successfully without understanding a word of Chinese. In this Searle is mistaken. The conditions he describes are not —and the available evidence shows cannot be— static; rather, they are progressive. Regardless of whether he wishes to do so, under these conditions Searle would gradually learn Chinese, and how well he does so would depend

9. These arguments were actually used in scientific debates of their time.
10. In one place Searle says 'dictionary' in English, although that cannot be so. Given a dictionary one would, of course, learn. What he has in mind is a book of rules in English, showing only what Chinese symbols go what Chinese symbols. I thank Robert Arrington for bringing this point to my attention.

on how long the process continues. The presence of the rule book in English is helpful; however, he would learn Chinese even without it, albeit more slowly, on the basis of the internal consistencies of the procedure. One needs only think of the deciphering of the Rosetta stone to see this possibility. Moreover, ironically, Searle has described the very conditions under which an infant learns language. That is, the conditions under which the infant (i) has no language, —a condition more stringent than the one Searle describes where the individual already has English, (ii) is exposed to correspondences between two sets of events: sounds and occurrences in the environment, and (iii) the equivalent of Searle's rule book, that is, people in the child's environment guiding him on the correct correspondence between the units of those two sets. (In the Chinese room these units are, of course, the Chinese symbols.) Now, Searle may object that there is more than this in the child's language-learning environment. The issue at hand is, however, that the onus is on Searle empirically to show, rather than merely assuming, that first-language learning is *dependent* on these 'more' things. My guess is that the interactions of the two sets of occurrences would lead to the learning of the language, although the process would be considerably slower without the more things.

The point here is not to refute the arguments Searle seeks to support through his Chinese room argument, but rather to illustrate the empirical assumption fallacy that permeates the literature.

The *implicit assumption fallacy* occurs where an assertion is conditionally taken as given, 'for the sake of argument,' but as the argument proceeds the conditionality of one or more premisses on which the argument rested is overlooked. Sometimes, the conditionality is hidden from the outset. A particularly bothersome from of this is ubiquitous in all of the literature under discussion. This literature sets out to discover how brain processes *cause* consciousness. Searle states this in numerous places, reiterating the question as 'how exactly do neurobiological processes in the brain *cause* consciousness?' (1995a, p.60, my italics) and 'how brain processes *cause* —or even could *cause*— our conscious experiences?' (1995b, p.54, my italics) and so on. Of course, that brain processes cause consciousness is an unexamined, assumption. It is an assumption which serves as a foundation-stone of the cognitive sciences. It does not, however, stand up to scrutiny. To assert such a causal relation is like asserting, for example, that Shakespeare's play is (on a given occasion) caused by the television set. In each of the cases just one of the participants in a complex phenomenon is being singled out as the cause of the others. Even more importantly, in both cases *the* consciousness which is the object of study is left out of the

phenomenon considered. In the television example, the television produces rapidly moving dots on its screen and waves in the air. These become Shakespeare's play as part of the consciousness of the television watcher; that is, the phenomenon is associated with the observer and not amongst the observed phenomena. By the same token, when brain processes are scientifically observed, consciousness is with the observer, that is, the scientist, and not amongst the neurobiological processes observed. I shall return to this point at the conclusion of the paper.

The *location fallacy* is the assumption that because we talk of phenomena or processes these, in every case, must have locations. Dennett's belief that neurological processes *are* consciousness is an example: where these processes are located, so is consciousness. There are, however, many things we talk about and know quite well that do not occupy space. Psychologists, for example, have shown great interest in understanding entities such as 'marriage,' 'friendship,' 'red' etc. None of these, however, is to be found in any location; marriage is not located in married persons, and friendship is not located in friends, and, even more tellingly, the color red is not located on the red object.

VI. Understanding the mind

None of these arguments implies, of course, that there cannot be sciences of neurobiological processes, and of actions of people, and, for that matter, of the interactions of these two broad sets of phenomena. They do not, however, add up to 'consciousness' by any reckoning. To use the word 'consciousness' is simply to distort its natural usage, and consequently to promise more than can be delivered. We can and do have remarkable insights into our own consciousness and that of others, and sometimes we are mistaken about such matters. Understanding ourselves and others is a craft that we learn by experience, and it is bound to remain a craft for two reasons. First, what we understand is in a continuous state of flux *as seen by us*, and does not have any property of constancy about which scientific statements can be made and tested. This is a debatable point, open to objection, but the second, the more important is not. It is that in any observational situation, whether casual or scientific, consciousness to be observed is a set of phenomena bound to the observer and not located in the observed. Consider, for example, that we have succeeded in implanting fine electrodes in an area of the brain of our subject, and connected to electrodes, through a complex of transducers, amplifiers etc. to a television set. We

now get our subject to read a passage, to answer questions, to look at pictures, and so on, and we see on our television screen the changes brought about by these activities. Are we now seeing the activities of our subject's consciousness? The answer has to be 'no,' because the consciousness in which we are interested has now moved to the observer of the television picture, that is, ourselves The television picture is simply presenting to the scientist some electronic transformation of the neurological events picked up by the electrodes. In short, consciousness is an ever-receding concept, always remaining with the observer and thus eluding scientific capture. Just as we will never get hold of the pot of gold at the end of the rainbow, we will never *scientifically* ensnare consciousness. A quest to that end merely misdirects neurobiological and behavioral sciences.

Behavior scientists would do well to study behavior, neurobiologists, to study neurobiological phenomena, and philosophers of science, to study conceptual errors, problems involved in interpreting scientific data, and the language used in the formulation of scientific theories.[11] It is best to leave appreciation of consciousness and the mind to the poet, the painter, the musician, and the insightful creator of characters in great plays and stories.

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11. In case this is misunderstood, let me add that I do not advocate confining 'specialists' to their areas, their area being determined by some extraneously formulated criteria. In my view, anyone conducting sound work in, say, biochemistry is, *at that time,* a biochemist and, at another time, a behavior scientist if then engaged in studying human behavior. What is important is how well-informed that person is in the area of the investigation. Pigeon-holing people by their diplomas has been a harmful practice of our times, merely an unattractive aspect of turf-protection. Good scholars move in an out of the confines of 'their areas,' in natural pursuit of their investigations.

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