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The Human Sciences: Obstacles and Opportunities

Alexander Rosenberg

My paper has two aims: to outline a diagnosis of the ills of social science, and to argue for the attractiveness of sociobiology as an alternative to conventional social science. Both of these aims can be attained by reflecting on the same considerations. Indeed, since sociobiology is not yet a discipline to be respected for any actual explanatory successes and predictive strengths, one of the strongest arguments on its behalf would be to show that it is free from the ills of conventional social science. What are these ills? Their symptoms are the failure of social science since its inception (at least as early as Plato and Thucydides) to have produced anything like the laws which natural science has produced. It is not just that there is nothing in social science approaching Newtonian physics, or the chemistry of the elements, or even the generalizations of biology: Social science has not even produced the earliest step in a sequence of improvable generalizations. It has no history of a succession of generalizations each deeper in its explanatory power and more precise in its predictive strength than its predecessor. An adequate explanation of why this is so turns out to be a good argument for sociobiology, and I shall try to give both the explanation and the argument.

There are two widespread objections to my diagnosis. The first is that social science is not or ought not to be the search for causal regularities, and the second is that in one or another discipline, say economics, laws or their first approximation have already been found. The first objection supposes that the aim of social science is to provide emotionally satisfying intelligibility, and not predictively useful knowledge. Such useful knowledge can be attained only by research that uncovers laws or their improvable first approximations. The social science disciplines must uncover the causal determinants or underlying factors that generate the human behavior they purport to explain. Identifying these factors, and discovering the relations that hold between them, is just what the
search for laws or their approximations is all about. The difference between those who demand predictively useful knowledge from social science and those who seek subjective understanding or comprehension without improvable control over human behavior is not a mere matter of taste, a difference between the practically minded and a literary or humane approach. It is an epistemological difference: The former hold that putative explanations do not count as knowledge unless they can be tested against subsequent experiences, experiences not merely compatible with the explanation’s contents, but whose occurrence and character we are correctly led to expect by the explanation. Their opponents hold that such standards for knowledge are only appropriate for the natural sciences. In what follows I assume that this epistemological double standard is no more justifiable than are double standards in ethics and moral philosophy. To the second objection, that laws have already been discovered by one or another of the social sciences, I can only reply that in every case these alleged laws have not been improved in explanatory systematization, predictive power and precision, or clarity of expression even when, as in economics, they have been the common coin of their disciplines for two hundred years.

An earlier explanation and its limitations

Most empiricists have considered the absence of laws of human behavior to be in principle temporary and have identified two obstacles to discovering laws or their precursors: First, the sheer complexity of the subject matter: Human beings and human groups are such frightfully complicated systems that a scientific understanding of them is not to be expected until long after we have acquired such understanding of their simpler biological and physical constituents. Since our scientific theories about the chemistry and biology of the subsystems that compose human beings are far from complete, it may seem reasonable to counsel patience, to place the empiricist’s embarrassment about the absence of laws in proper perspective, and to await the outcome of further research by able social scientists. Second, there are moral and social impediments to undertaking experiments on human subjects under conditions closed to all interference with the phenomena under study. Treating people as persons is incompatible with treating them as subjects of an experimental protocol, as the work of researchers both malign and benign has revealed. Since we cannot do the sorts of experiments we would in principle want to do on human beings, we cannot expect to acquire easily or perhaps at all the laws that govern their behavior.

The trouble with this explanation is that it hinges on a dubious premise. It presumes that if the complexity of a subject matter and its resistance to experiment increase, the rate of progress in the scientific understanding of the subject matter cannot also increase. But the history of natural science shows this to be plainly false. Over the long run, and in the recent short run, the subject matter of natural science has increased in complexity and in resistance to experiment at accelerating rates, and yet progress in systematically explaining and reliably controlling these phenomena has accelerated even more. Since complexity and experimental recalcitrance have not impeded progress in natural science, they cannot be the sole explanation for the failure of social science. Fully to explain the failure of social science to produce laws requires showing why such features constitute obstacles in these
disciplines when they do not in others. The full explanation must show that the failure reflects a matter of fact about the subject matter of social science and not a logical obstacle to the very possibility of scientific knowledge of human behavior. For aside from the fact that no empirical social scientists will actually be detained by the impossibility proof* which such an explanation would constitute, the result would only be more and harder questions to answer: Viz., what is it about human beings that logically or metaphysically exempts their behavior from scientific systematization? What difference in kind is there between sentient creatures on the one hand and matter in motion on the other which results in this impossibility? Taking these questions seriously is less attractive than accepting the lame explanation that appeals to nothing more than complexity and experimental recalcitrance.

In fact, the full explanation of the failure of social science will have to identify a false belief whose deleterious effects on progress is limited to these disciplines. Of course the belief cannot be repudiated at the cost of excluding the logical possibility of nomological social science—based on genuine scientific laws and not mere accidental generalizations—altogether, but it nonetheless must be central among our beliefs and highly ramified in its influence on research strategy. For only such a belief can explain both why all and only the social sciences are equally unlikely to hit upon improvable generalizations, and why no one has yet detected this erroneous belief and repudiated it.

**Generalizations, classifications, and the organization of knowledge in the natural sciences**

The social sciences are not the only disciplines ever to have lacked general statements that successively approximate to nomological status. Behind each of the so-called mature natural sciences there stands a predecessor which reflects the same history of frustration the social sciences evince. In each of these disciplines the chief cause of fruitlessness was the failure to "carve nature at the joints": Each of the natural sciences organized the items in its domain into classes that were causally heterogeneous. That is, the classificatory labels used did not bring together items which shared a manageably small set of causal determinants and consequents, and so none of the items could be made the subject of causal regularities. Here is a simple illustration of the obstacles erected by a classification that fails to "carve nature at the joints." Suppose we use the term 'fish' to designate any aquatic creature. This designation is not far from ordinary usage, even among the biologically sophisticated. If now we enquire after the method of respiration of fish by examining what we can catch with a hook and worm, we may conclude that fish respire through the use of gills. But once we discover whales and porpoises we shall have to revise the generalization to include lungs: Fish respire by the use of gills or lungs. On the discovery of jellyfish, we shall have to make a further emendation: Fish respire through the use of gills or lungs or by osmotic diffusion. The discovery of squid, of various bottom dwelling shellfish, of starfish and sea cucumbers, and so on, will complicate the generalization further, as

*An impossibility proof has the conclusion "x is impossible." An impossibility proof is a proof of impossibility.*
will that of microorganisms properly designated as aquatic creatures. The result will be a useless generalization about a vast disjunction of alternative respiratory mechanisms, a generalization incapable of systematic explanation from a small number of more theoretical laws, and powerless to enable us to predict the breathing mechanism of the next creature we pull from the sea. This generalization, even if completed by an exhaustive study of all types of aquatic creatures, will be no law. The culprit is obvious: The term 'fish,' when defined as 'aquatic creature,' does not designate a simple kind of organism, but a very heterogeneous collection of different types of organisms, with different properties, that behave in accordance with different causal regularities. This causal heterogeneity is characteristic of a category that does not "carve nature at the joints." But if we substitute for 'aquatic creature' some more restricted definition like 'scaly aquatic egg-laying bony vertebrate' we will be able to formulate a generalization about how fish breathe that is not riddled with exceptions, that can itself be explained by appeal to a manageable biological theory, and that will enable us to make correct predictions about how the next such creature we discover breathes. And though no doubt not strictly true because of obscure exceptions like the lungfish, our generalization seems improvable in the direction of greater precision and accuracy.

Each of the revolutions in the natural sciences started with the overthrow of such causally heterogeneous categories in favor of a system of types of items, properties, objects, events, and states that did turn out to bear manageable, regular, causal relations to one another. When this happens the sciences have uncovered their appropriate natural kinds: the kind terms that are causally homogeneous, that do "carve nature at the joints," and that therefore figure in improvable regularities and ultimately in laws. Some obvious examples come readily to mind: The Newtonian revolution, for example, repudiated the distinction between terrestrial and celestial motion in favor of a distinction among the natural kinds of rest and accelerated motion. Modern chemistry starts with the replacement of phlogiston by oxygen. Atomic theory owes its real beginnings to the periodic table of the elements, which finally brought the objects of chemistry into a system of types related to each other in ways eventually explained by microphysical principles. Deciding on the natural kinds in the domain of a discipline is inseparable from discovering the first approximations to laws in that discipline, because a natural kind just is a kind that falls under causal regularities. Thus, the discovery of improvable regularities goes hand in hand with the elaboration of a descriptive vocabulary for expressing them, and the suitability of that vocabulary to the tasks of the discovery is as much a factual matter as a theoretical one. Deciding on a kind vocabulary for a scientific theory is not merely a terminological matter—conventional, empirically neutral, and made on considerations of convenience alone. A distinct and useful science must embody manageable generalizations, qualifications, computational intricacy, and sheer numbers; and such laws cannot be made if the descriptive vocabulary of the subject is a jury-rigged, gerrymandered patchwork.

The contingent fact that scientists got the natural kinds of their disciplines wrong helps to explain the failures of physics, chemistry, and biology before their scientific revolutions in the seventeenth, eighteenth, and nineteenth centuries, respectively. The failures of the social
sciences are to be explained in the same way, by finding fault with their descriptive classes and categories, their conceptual scheme. Such fault has been found many times before by social scientists ready to provide the correct set of natural kinds with which to describe human behavior. Innovators advocating neologisms are nothing new to the long-suffering social sciences. The trouble with their proposals is that they give no reason independent of their neologicist alternative for the failure of the conventional vocabulary. Nor do they obviate such reasons by providing for actual generalizations any more improvable than the ones they are meant to replace.

Like others, I locate the failure of social science in its categorical and conceptual commitments. These commitments reflect shared false belief about the natural kinds into which human behavior and its determinants fall. Their falsity can be shown by reflecting on contemporary biology and its conceptual foundations. Such an argument will be independent of any issue controversial in the social sciences or their philosophy. Moreover, it has an important positive upshot for what the natural kinds of human behavior really are.

**Toward a new explanation of the failure of the social sciences:**

**Five premises**

My argument begins with five premises, which I shall state and then defend.

1. The social sciences are all resolutely intentional: They classify the behavior to be explained as various sorts of actions, and the variables that explain it as various sorts and combinations of desires and beliefs.

2. All definitions of intentional predicates must eventually resort to their exemplification by Homo sapiens. Intentional description of the states or behavior of nonhumans is either metaphorical or rests on analogies between such behavior and paradigmatic human intentionality.

3. *Homo sapiens* constitutes a biological species. (This is the least controversial of my premises.)

4. The coherence of biological theory, and in particular the theory of natural selection, dictates that species names are not kind terms like 'planet' or 'oxygen,' but are names of spatiotemporally restricted, particular objects, like 'the earth' or 'the Eiffel Tower' or 'the family Medici'—an object, albeit a scattered one, just as a species is scattered.

5. But no spatiotemporally restricted particulars can figure in general laws. Only "qualitive" predicates may be so used. At best, names can figure in accidental regularities that explain nothing.

From these five premises it follows that:

6. *Homo sapiens* names a spatiotemporally restricted, particular, though scattered, object, just as 'the family Medici' does.

And that:

7. There are no laws about *Homo sapiens*. There are no laws about causal relations between intentional states or between intentional states and behavior, however that behavior is described.

In other words, there are no laws in the social sciences; nor any prospect of them so long as we persist in describing their subject matter.
in terms that are spatiotemporally restricted, that do not pick out natural kinds, that do not carve the nature of human behavior at the joints.

**Premise (1): The social sciences are intentional**

Premise (1), that the social sciences are resolutely intentional, will go largely unchallenged. The social sciences are not concerned with the topography of human behavior, with the spatiotemporal displacement of the human body: They are interested in bodily motions under descriptions that show the displacement to be explainable in terms of desire and belief.

Although they do not always use these terms, the determinants of human behavior social scientists search for are always cognates or variants of desire and belief. For example, economics appeals to expectations, i.e., beliefs about the future, and preferences, i.e., relative judgments of desirability. Social psychology may explain prejudice as an attitude caused by the combination of beliefs and desires—that is, desire to avoid or even harm someone falsely believed to be inferior or dangerous. Cultural anthropology explains a ritual in terms of meaning for its participants; and meaning, with its roots in language, is perhaps the most intentional notion of all. Even macrosocial theories make implicit appeal to intentional variables. For example, macroeconomic theories trade on implicit attributions of the desire for money balances, and beliefs about interest rates which affect these balances. Similarly, even so antipsychological a theory as Durkheim's account of suicide rates in terms of the strength of forces of social integration rests on the identification and recognition of those forces by the individuals whose suicides make up the statistical rates he purports to explain.\(^1\)

It is important to note that the use of the term 'intentional' here connotes more than just purposeful, or goal-directed behavior. Intentional behavior is indeed goal-directed or, as philosophers have called it, "teleological." But it is more than that, and philosophers employ the term 'intentional' to label that further property of human action and its determinants—desire and belief—which distinguishes them from other goal-directed processes like photosynthesis, or the feedback regulation of the mammalian iris, or the imprinting behavior of a duckling.

An intentional state has **content**, it is "directed" toward a proposition or an object; it is "about" something. When an agent has a belief, there is a proposition he believes; he stands in a certain relation to that proposition: the relation of belief. And the difference between two beliefs is in the propositions they "contain." Similarly, when an agent has a certain desire, there is a proposition he desires to be true, typically a proposition to the effect that some object or state of affairs is attained or obtained. Philosophers express this special character of mental states, their "aboutness," by the label "intentionality." The intentionality of psychological states like belief and desire consists in their having content, being directed at ways things are, or might be.

**Premise (2): Intentionality in the social sciences is an attribute restricted to one species, Homo sapiens**

Premise (2) of my argument is philosophically the most controversial and the hardest to expound in a way that combines plausibility with

1. There are some theories in behavioral science, and some macrosocial theories that are not committed to the intentionality of behavior. But they are fewer than one might think. About the only clear case of such a theory is Skinnerian operant or instrumental learning theory. This theory is teleological, but it does avoid appeals to the content, directedness, or representational aspects of psychological states or the behavior they explain. Accordingly, it is exempt from my criticism, and its chief result, the law of effect, may well turn out to be an improbable approximation to a law. On the other hand, if the Skinnerian research program is judged a failure, my argument will not explain the failure. Macrosocial theories as diverse as econometric models and Durkheimian hypotheses of social integration are sometimes supposed to be utterly divorced from hypotheses about the intentions of individual agents. While they might be logically independent of any particular intentions attributed by underlying theory to human beings, to the extent that they have any microeconomic or social-psychological foundations, these theories are firmly committed to treating human behavior as intentional action.
2. This attitude is best illustrated in a passage by Norman Malcolm: "Since it has nothing like a human face and body, it makes no sense to say of a tree, or an electronic computer, that it is looking or pointing or fetching something (of course one could always invent a sense for such expressions) . . . . Things which do not have the human form, or anything like it, nor merely do not but cannot satisfy the criteria for thinking, I am trying to bring out part of what Wittgenstein meant when he said 'we only say of a human being and what is like one that it think,' and 'The human body is the best picture of the human soul.'" ("Knowledge of Other Minds" in Knowledge and Certainty [Englewood Cliffs, N.J.: Prentice Hall, 1963]), pp. 135-36.

Intentional states represent expectations as well as facts. Expectations may represent our attitudes about facts, but they are not factual.

Intentional states are specific. Unlike generalizations, they do not permit substitutions of terms.

Intentional states have a property which distinguishes them from all other states, and in particular from merely physical, or even goal-directed states of nonintentional teleological systems, plants, thermostats, organs of the body, or lower forms of biological life. It is a property which hinges on the fact that they "contain" or are "directed" to propositions about the way things are or could be. The property they have is that when we change the descriptions of the states of affairs they "contain" in ways that seem innocuous, we turn true attributions of belief into false ones. For instance, consider the presumably true statements that (D) Oedipus desires to marry Jocasta, and (B) Oedipus believes that Jocasta is the Queen. These two statements report relations of desire and belief between Oedipus and the propositions, respectively, that Oedipus marries Jocasta and that Jocasta is the Queen. These two statements are the contents of Oedipus's intentional states. But Jocasta is not only the Queen, she is also Oedipus's mother, so anything true of the Queen or of Jocasta is true of Oedipus's mother, and we should be able to refer to Jocasta indifferently as the Queen and as Oedipus's mother. But when we refer to her as Oedipus's mother in describing Oedipus's desires and beliefs we produce the presumably false statements that (D') Oedipus desires to marry his mother, and (B') Oedipus believes that his mother is the Queen.

By making innocent substitutions of terms that refer to the very same objects we have turned truths into falsehoods. But this is something we simply cannot do to any expressions that report nonintentional relations, such as those characteristic of physics, chemistry, or biology. I may refer to the earth and the moon in any of an infinite number of ways, and the statement that their gravitational attraction varies as the inverse of the square of the distance between them will be true no matter
how I refer to them. Any statement about the physical or chemical properties and relations of the Hope diamond will remain true no matter how I refer to it, as the largest diamond in the world, as the stone that brings bad luck to its owners, as the only gem mined in Katanga in March of 1914, and so on. Similarly, the goal of photosynthesis is the production of polysaccharides, and this statement will remain true no matter how I refer to it, as starch, as the substance with the chemical formula $(\text{CH}_2\text{O})_n$, as the chief constituent of papier-mâché, as the cause of cellulite, or as the chief ingredient of my favorite junk food. But such substitutions are not permissible in intentional statements for they may, and often do, change a true statement about an agent’s beliefs or desires into a false one.

This special logical feature of intentional states has important consequences for the ways we identify and distinguish them. We can in fact do so only by reference to the propositions they contain, to the states of affairs they represent. To see this consider again the statement that $(B^\prime)$ Oedipus believes that his mother is the Queen. The only temptation we have to suppose that $(B^\prime)$ is true is that at the end of *Oedipus Rex* Oedipus does come to believe that Jocasta, the Queen, is his mother. In consequence he plucks his eyes out. But this belief at the end of the play is a different belief from that which Oedipus has in the middle of the action. His belief that Jocasta is the Queen, $(B)$, is a different belief from $(B^\prime)$. The difference in the beliefs is based on the difference in the contents of the beliefs. It is not based on the facts of the matter, which have not changed throughout the play. It is not the case that at some point in the play it was false that Oedipus’s mother was the Queen and that later it became true. Accordingly, we cannot identify Oedipus’s beliefs by reference to the facts about Jocasta. It was not on these facts that his beliefs depended, and it is not by appeal to them that we can identify his beliefs or trace the changes in them. Much the same can be said about his desires. Jocasta was the object of his desire, but were he to have been asked whether his mother was the object of his desire, he would have rejected the suggestion violently. Yet Jocasta was his mother. Accordingly, his desire cannot be identified or disclosed by any examination of the actual person at whom it was “aimed,” but only by appeal to its content, a proposition about that person. This is even clearer in the case of unattainable desires: Ponce de León desired to reach the fountain of youth. But we cannot find out what he desired by any inspection of the fountain of youth or his actual physically describable relation to it for there is no such thing as the fountain of youth. We can only identify his desire by its content.

If the identity of an intentional state is determined by the proposition it contains and if beliefs can be false and desires unattained, we cannot decide what beliefs or desires an agent has by finding out whether any proposition about the world independent of his intentions is true or false. Of course we can and do discover peoples’ intentional states—by asking them—but this method is itself intentional. Their responses to our questions will only be counted as replies, as the utterance of meaningful speech, as actions, if we assume that they are sincere and that they understand the meaning of the language with which we put our questions; that is, if we assume that they desire to answer truthfully.

Unlike physical states, intentional states are relative. They may alter even though their related facts do not change.

A scrutiny of its objects alone cannot identify a belief or a desire.

Therefore, a knowledge of facts independent of intentions cannot reveal the truth or falsity of intentional states, or of the human behavior motivated by intentional states.
Therefore, it is only minimally useful to attempt to identify the causes of action in terms of its effects in action. And believe that the utterances they emit will attain this desire. In other words, our normal means of identifying the causes of actions are in terms of their effects, the actions themselves; and the identification of an event, a movement of the body, as an action presupposes the attribution to it of intentional causes: A bodily movement is an action only if it is caused by intentional states, a desire and/or a belief. Now, there is nothing improper about this intentional circle so far as our everyday nonscientific purposes are concerned. But social science seeks to sharpen its explanations and predictions of human behavior beyond commonsense levels of accuracy. Therefore, it must break out of this intentional circle: We must find a way of identifying movements as actions without assuming that the movements were caused by intentional states; we must find a way of identifying intentional states without appeal to their effects in action. The reason is simple and can be illustrated by considering the explanation and prediction of how alcohol thermometers work.

To explain why the column of alcohol in a thermometer rises I note that the substance it is in contact with has become hotter. This increase in temperature causes an expansion of the alcohol in the closed tube of the thermometer, and thus a rise in its level. But to ascertain that the substance has become hotter I require a thermometer. If I employ an alcohol thermometer to establish this initial fact, my explanation will be open to the criticism that it presumes what it sets out to explain. More seriously, without an alternative means of measuring temperature, say a mercury thermometer, a bimetallic bar, or a gas diffusion thermometer, I will be unable to test, correct, and improve my measurements of heat by the use of an alcohol thermometer and so will be unable to make improving predictions about the effects of changes in temperature, effects like changes in state, or electrical resistance, and so on. And without alternative measures of temperature I will be unable to relate any regularities about the relation between heat and linear expansion in a closed tube to the rest of thermodynamics. Indeed, the rest of thermodynamics would have been undiscoverable without alternative means of measuring what an alcohol thermometer measures by linear expansion in a closed tube.

The situation with respect to the intentional variables of social science is identical and has been long recognized. The recognition that intentional variables need nonintentional anchors if an intentional science is to improve beyond the level of commonsense "folk psychology" has long been discussed as the "problem of other minds." The suggestion that when detached from behavior the intentional variables are cognitively empty, theoretically barren, or methodologically suspect, that we have no good grounds to attribute such states to others, is the real force of philosophical skepticism about our knowledge of other minds. The need to anchor the intentional in the nonintentional spawned behaviorism. Unfortunately, behaviorism, even of the most sophisticated sort, cannot provide the connections to the nonintentional that are required. No matter what the setting, no mere movement of the body can reveal a desire unless we already know a person's relevant beliefs; no mere movement of his body can reveal a belief unless we already know his relevant desires. So any definition of a desire will have to mention beliefs, and any definition of a belief will have to mention
desires. Definitions free of such intentional residues will not correctly identify paradigmatic beliefs, desires, and actions that folk psychology can identify. But such nonintentional definitions are just what behaviorism demands.

Behaviorism's failure should come as no surprise. It is foreordained in the fact that intentional statements have the logical property of forbidding otherwise innocent substitutions, while any statement about mere movement permits all such substitution without provoking a change in truth or falsity. Therefore any nonintentional definition of the intentional was bound to leave something crucial out. The only alternative to behaviorism as a way of anchoring mental states to nonintentional ones is a neuroscientific reduction: the idea that we should be able to find the brain states that constitute states of belief and desire. Although locating such brain states will be of no practical use in the identification of intentional states, it will provide a theoretically possible way to check our ordinary attributions, and perhaps even to improve them in artificial, clinical situations. In this respect, a neurophysiological approach to mental states would be like a highly complicated and inconvenient thermometer which cannot replace our ordinary ones in any practically important context, but which provides theoretical reassurance that we can in principle test and correct the alcohol thermometer's accuracy. Unfortunately, neuroscience has revealed no brain states that march in lockstep together with mental states. At the level of detail on which neuroscience currently operates, there are no changes in the brain or parts of the brain that seem to vary at all systematically with changes in the mental states that experimental subjects report. Even if someone were to report the required parallelism, the gulf between nonintentional brain states which can be described in any of an indefinite number of ways and intentional mental states for which there are only privileged descriptions will remain to be explained.

The upshot is that there is no way to break out of the intentional circle of terms we employ to describe and explain human action and its causes. Both in ordinary life and in social science the variables we cite to explain behavior are understood by ostension, by pointing to paradigm cases of such behavior by human beings in circumstances that make their behavior intelligible from our own individual perspectives. And when we explain the behavior of monkeys, Martians, and machines as the outcome of intentional processes, we make such intentional attributions to them on analogy with the paradigm of human actions, the behavior of Homo sapiens. This conclusion is no criticism of such claims, nor does it preclude our someday giving new, nonintentional behavioral definitions for such expressions as desire, belief, and their cognates. Such definitions would allow us to make attributions of belief, say, to nonhumans, on nonanalogical grounds. But it would remain to be seen whether, so defined, 'belief' picked out a natural kind; that is, was scientifically fruitful. What is more, the use of such definitions for our mental states would be tantamount to surrendering the belief that humans engage in actions, as opposed to mere movements, that we are agents, as opposed to mere patients of change. Such redefinition would change the subject matter of social science from human action to the topography of the displacement of human bodies. In short,
any attempt to circumvent premise (2) by redefining intentional states runs afoul of premise (1), the claim that the social sciences are all resolutely intentional. This premise, coupled with the special logical peculiarities of intentional statements, commits us firmly to the conclusion that the definitions of intentional states must resort to their exemplification by *Homo sapiens*.³

**Premises (3) and (4): Homo sapiens is a species, but species names are not kind terms; they are the names of restricted, particular objects.**

Premise (3) of my argument needs little discussion. *Homo sapiens* is a biological species, like *Felis domesticus*, *Canis familiaris*, and *Didus ineptus* (the Dodo bird). But Premise (4) constitutes one of the most important contemporary conceptual revisions in biology and its philosophy. I shall outline three of the reasons that biologists and philosophers have increasingly come to view particular species as spatiotemporally restricted particulars. If I can convince the reader to embrace this view the crux of my argument will have been established.

One indication that species terms do not name classes or kinds or organisms is the demise of *essentialism* in the biologist’s notion of a species. Essentialism is the hypothesis implicit in biological classification that there is a small set of properties distinctive of all the members of a species, a set of morphological features, or perhaps a genetic feature (like number or kinds of chromosomes or genes) that causally explains why they are its members. If this view is correct, species are natural kinds, for there exists a generalization about the universal manifestation of all these properties by all members of the species. The trouble with the hypothesis of essentialism is that there just does not seem to be any such set of explanatory traits common and peculiar to the members of any species. Developments in evolutionary and genetic theory have shown that variation, even among the most central traits, is not only possible but also predominant. There is no single property which can be deemed natural, normal, or typical, and variation around a statistical mean is not to be construed as the result of disturbances and perturbations deflecting members of a species away from the mean.⁴ Genetics has done away with the contrast between the “specimen” and the “sport.” But this denial of essentialism is tantamount to the claim that species are not natural kinds. If they are kinds, they must be of the sort which, like the ordinary term “fish,” fail to carve nature at the joints.

But species are not kinds at all, either natural or artificial. What they are has been explained by a second, independent development in biological theory. Over fifteen years ago Michael Ghiselin reached the conclusion that species can only be particular objects, albeit scattered ones. His conclusion is coming to be accepted by the most prominent biologists of our time. Ghiselin argued that the theory of evolution and the behavior of species require them to be such individual scattered objects. Organisms are not the *instances* of a species; they are its constituents, its parts. As Ghiselin puts it: “Species are like galaxies and *Homo sapiens* is like the Milky Way.”⁵ The general concept of ‘species’ is the concept of a kind, a type, but its instances, *Homo sapiens*, *Didus ineptus*, *Cygnus olor*, are not themselves further subtypes, the way triangle is a subtype of plane polygon. Species terms name par-

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ticular things; just as ‘the Eiffel Tower’ names a tower, and not a type of tower. It is what species do which makes them individual, spatiotemporally distributed objects. The most obvious thing they do, according to the theory of natural selection, is change: Species evolve, whereas kinds do not change, as Plato argued.

The actual number of items designated by a kind term, like a chemical element name, can change from empty to indefinitely large and back again to empty. But the Dodo bird, *Didus ineptus*, is not a kind with a temporarily empty membership. It is extinct, something no one would say about an element (such as element 109, which has no instances at the moment). The species *Didus ineptus* went out of existence. Should a new population of birds arise, wholly indistinguishable from the Dodo bird, no biologist would consider them instances of the species *Didus ineptus*: Since the original species is extinct, the branch of the phylogenetic tree on which it was a node ended with its extinction. The new population would have to be on another branch, would be the product of a different course of evolution, and therefore would be a different species. In addition, the lines of descent whose evolution is explained by the theory of evolution manifest the properties of things and not of kinds. Not only do they come into existence and go out of it, but they divide and they merge, just as individual organisms do. Just as binary fission makes two organisms out of one, geographical isolation can divide the line of descent that constitutes a species and keep the two branches separate long enough so that, despite their similarities, they will constitute two species, not one. Similarly, two species may merge and create a third. The whole evolutionary phenomenon of speciation is most clearly intelligible on this view of species as individuals.

There is another set of arguments for this view of species as individuals: Only by so viewing them can we correctly understand the hierarchy of laws and theories in biology. Philosophers have leveled three claims against biology: that it has no laws, or that its laws are far gappy and never likely to approach the universality and integration of physical laws, or that the leading explanatory theory of biology, the theory of natural selection, has no predictive content and may perhaps be a grand tautology. This litany of charges rests on the assumption that particular species must figure in biological laws at some level of generality or other. But if this assumption is wrong, these charges are wrong; if species name individuals, the most we can expect is to find laws about all species. According to premise 5, no spatiotemporally particulars can figure in general laws.

The philosopher’s assertion that biology has no laws rests on the supposition that such laws must at least include generalizations about all members of a given species. The accompanying complaint that biology can have no laws because its rough generalizations about groups or kinds of species cannot be improved or upgraded to exceptionlessness rests on the supposition that species are natural kinds. Both suppositions are false. Since species are not kinds, natural or otherwise, there can no more be laws about species than there can be laws mentioning the Eiffel Tower or the *Mona Lisa* or the family Medici or groups composed of them. The laws of biology begin not at the level of particular species, or even at the level of groups of species. They begin at the level of all species, and species are restricted particular objects, generalizations based on Homo sapiens cannot be successfully predictive.

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Since Homo sapiens is a species, and species are restricted particular objects, generalizations based on Homo sapiens cannot be successfully predictive.
Intentionality, tied to human behavior, is irredeemably particularistic; it can never form the basis for generalizations about human behavior. Insofar as the social sciences are based on intentionality, they are doomed to fail in their attempt to formulate laws.

Species. At this level, statements about species will be deducible theorems of the fundamental laws of evolution. On the other hand, because the theory of evolution is a set of general laws, it cannot make claims about particular individuals, say species evolving on a particular planet during a particular time. The theory of evolution must never be confused with the history of evolution on this planet. It must be able to explain this evolution, provided we add enough details about the starting point and the environmental forces. But the theory itself must describe a mechanism that can operate across the universe if it is to constitute a body of natural laws. Most attacks on the theory of natural selection as unfalsifiable or bereft of predictive content rest on the mistaken demand that this universal theory have consequences for particular facts about particular species. Once we recognize that species are individuals, the theoretical character of evolutionary theory, the real hierarchy of findings, generalizations, laws, and theories, becomes clear. Indeed the character of biology as a natural science, parallel in every feature of generality to physics or chemistry, becomes clear.

Preliminary conclusions: To be successfully predictive, generalizations must begin at the level of natural kinds

The last steps in my argument are easy to expound. If every species is an individual, spatiotemporally restricted, scattered object, then the term *Homo sapiens* can no more find its way into nomological generalizations than the term 'Mona Lisa' can. Laws have a kind of generality lacked by statements about particular objects; they cannot refer to particular objects, places, or times if they are to retain their explanatory power. And so there can be no laws about *Homo sapiens*, nor any laws about properties distinctive of *Homo sapiens*. But this means we can expect no laws about actions, beliefs, desires, or any of their cognates—preference, expectation, fear, anxiety, hope, want, dislike, or any other intentional term. For all such terms are tied conceptually to the notion of *Homo sapiens*. They are tied to it because intentionality is anchored to paradigmatic human behavior. Thus, it will not do to say that there may be laws about intentional systems, including humans, certain higher primates, computers, possible extraterrestrials, and so on, on the ground that laws about intentional systems in general do not mention any particular species. For regardless of how widely intentionality may be attributed, it is always grounded on an anthropomorphic analogy, on comparison to how *Homo sapiens* performs.

In search of natural kinds for the social sciences

Now at last we have an explanation of why the social sciences have failed to find improvable generalizations. It does social scientists the justice they deserve. For if it is correct, nothing in their methods or their beliefs can be impugned as inferior to those of natural scientists. Their error has been to believe that the natural kinds which describe human behavior, and its causes, are intentional. This is a belief which no one has seriously questioned since attempts to explain human behavior began before Plato. (Indeed Plato's *Phaedo* ridicules the sug-
gestion that the explanation of human action might not be intentional.) But since intentionality is a spatiotemporally restricted property, it can figure in no laws, and attempts to frame and improve generalizations expressed in this idiom were doomed to failure from the outset.

We can now explain why social science has not really progressed since Plato, while natural science has at an accelerating rate, in spite of the growing complexity and experimental recalcitrance of its subject matter. In the natural sciences, the chief obstacle to advance was the fact that the natural kinds of each science were not marked out by ordinary language. Advance in natural science came through a growing awareness that things are not to be divided up according to our ordinary conceptual scheme no matter how suitable it is for human intercourse. But in the social sciences we have always thought we knew what the appropriate kind terms were for describing human behavior. If the intentional categories in which we characterize human behavior and its determinants were indeed natural kinds, social science would have been faced with an inestimably easier task than the natural sciences. For we would have possessed the natural kinds of human behavior since time immemorial, in contrast with physicists, who acquired their natural kinds, or something like them, only four hundred years ago. But in the past four centuries, physics has progressed so far that the failure of social science has become the first item on the agenda of the philosopher of social science. For given the potential conceptual advantage of the social sciences, the contrast between the rates of progress in these two disciplines becomes even more staggering than it otherwise seems.

All these mysteries disappear if the problem is just that social scientists have firmly held to a false belief about the natural kinds into which human behavior falls, a belief so widespread, and so well entrenched, that it is almost impossible to surrender it. The view that our behavior constitutes action and that its causes are to be sought in the joint operation of desires and beliefs is not only at the foundations of all the social sciences, it is also part and parcel of the view we take of ourselves as agents, with responsibilities, obligations, opportunities, rights, and all the other features that make us more than just creatures of biological interest. For purposes of a social science with nomological potential we must surrender this conception of ourselves as agents. (For all other purposes, of course, we may continue to employ it.)

If the intentional vocabulary with which we have hitherto described human behavior and its determinants is the wrong one for any attempt to uncover improvable generalizations in the social sciences, what is the correct one? The question, What is the right typology of natural kinds of human behavior? is an empirical one, not philosophical. For, as I have said, deciding on the right descriptive network is tantamount to discovering the improvable generalizations on which scientific success rests. Still, there is more to be said than merely warning social scientists off the sterile ground of intentional descriptions. And this is where sociobiology comes in. It is our best bet in the search for the natural kinds of human behavior.

It is our best bet because it is both the safest bet in the long run, and the one most likely to pay off with interesting and improvable results in the short run. The search for laws of human behavior is in effect the search for the narrowest natural kinds under which that behavior
Biology's natural kinds are the narrowest ones that we know subsume human behavior.

can be subsumed. We are of course familiar with some of the natural kinds that subsume human behavior: the categories and kind terms of physics and chemistry. But these kinds are far too broad. They include much behavior that is not even organic, let alone mammalian or human. The known laws in which the natural kinds of physics figure tell us nothing about human behavior. An attempt to frame laws distinctive of human behavior in the kind vocabulary of physics would be foolish.

Among the natural kinds we already know of, that is, among the kind terms of physics, chemistry, and biology, the narrowest natural kinds under which human behavior is subsumed are those of biology. These biological kinds include the concepts of ecology, population biology, and the theory of evolution, as well as the vocabulary of functional biology and especially the neurosciences. These are the narrowest natural kinds that we know human behavior to be subsumed under.

Of course, there may be narrower ones, but we do not at present know of any. And since the only other candidates for such kinds are the intentional kinds of conventional social science, we can say with confidence that there are no natural kinds of human behavior narrower than those of evolutionary biology and the neurosciences. Even if there are natural kinds that are narrower in their relevance for human behavior than those of current life science, they will have to be not just consistent with, but also coherently interconnected with the evolutionary and neuroscientific kinds. For it is a feature of the natural kinds of all the sciences that they are actively related to one another in a network of laws and theories. Therefore our best guide to the shape which the narrowest kind terms for human behavior will take is the conceptual space allowed for them in the theories to which they must be the most closely connected: those of biology.

Sociobiology is nothing more or less than the application of this conclusion to the attempt to understand human behavior.

Sociobiology and the possibility of generalizing about human behavior

Sociobiology is a very new subject, one which is far from having proved its legitimacy through uncontestable explanatory and predictive success. For a long time to come much of its rationale will be negative: it will be recognition of the failure of conventional social science that attracts social scientists to sociobiology. But because its natural kinds will be those of biology, not the intentional kinds of superseded human (i.e., social) science, there will lurk the suspicion that sociobiology can tell us little of what social scientists want to know. Much recent work in sociobiology has been directed at allaying this suspicion. But these attempts to show the relevance of sociobiology to the traditional agenda of concerns in the conventional social sciences involve a considerable risk. They jeopardize the very features that provide sociobiology's advantage over the older disciplines: its freedom from the snares of intentional description.

These risks are aptly illustrated in the standards of success for sociobiology established by one of its prominent exponents. In *Genes, Mind and Culture*, the most well-known sociobiologist, E. O. Wilson, and his coauthor, Charles Lumsden, set out the following criteria for
the success of sociobiology:

First, it must derive rigorous propositions that are the unexplained axioms of other theories in the social sciences. Second, we require that it achieve a level of predictiveness and testability greater than that provided by other modes of explanation, or at least that it subsume the exact phenomenological models of disciplines such as economics and anthropology so as to make the underlying assumptions identical. Finally, it must suggest new questions and problems, as well as identifying previously unknown parameters and laws to be woven into a network of verifiable explanations from genes through mind to culture. Yet sociobiology cannot be expected to meet the first and second of these three demands if it is to meet the third.

The first condition Wilson and Lumsden lay out requires in effect that whatever underived laws are fundamental to the current social sciences be shown to be derived laws of sociobiology, that is, that they be propositions related to principles of evolutionary biology in roughly the way the Pythagorean theorem is related to the five postulates of Euclidean geometry. But if as I suggest there is nothing worthy of the name "underived law" in conventional social science, any theory which implies the claims sometimes identified as the laws of economics, or the general statements of social psychology, politics, or anthropology, could only be saddled with their defects, defects that make sociobiology seem attractive just because it is free of them. Additionally, any demand that sociobiology explain as its consequences the analytical schemes of disciplines like structural anthropology that deny any interest in causal laws deprives sociobiology of any chance to discover general laws. Finally, and most crucial, all the conventional human sciences are intentional, either explicitly in the case of economics, or implicitly in the case of functionalist sociology. Showing how the "unexplained axioms" of these disciplines exemplify the leading ideas of a biological approach to human behavior would require the very entrenchment of intentionality that I have argued is impossible in a science of human behavior. If intentional kinds are not natural, it cannot be required as a necessary condition of the adequacy of sociobiology that it show them to be natural.

The second criterion Lumsden and Wilson set for sociobiology is that of matching the explanatory and predictive powers of conventional social science. They require that their theory attain increased power, "or at least that it subsume the exact phenomenological models of [conventional social sciences] so as to make the underlying assumptions identical." But since these powers have proved to be negligible, any theory can satisfy the second stricture on explanatory and predictive power. And sociobiology cannot subsume theories in social science if it hopes to transcend them.

Sociobiological theory may stand a good chance of meeting the third of Lumsden and Wilson's demands: the requirement of scientific fertility. But only on the condition that its proponents forgo the first and second requirements for success. Their theory may well present new problems; it may well generate hypotheses about the parameters and the laws governing human behavior. But it cannot do
Sociobiology’s major challenge is to discover nonintentional surrogates for the intentional questions of conventional social science.

so unless it remains rigorously biological (and therefore nonintentional) in its conceptual machinery. But remaining rigorously biological will prevent sociobiological theory from answering many of the questions about actual human behavior and its meaning that are the stock in trade of conventional social science.

Questions about human behavior and its meaning have a life of their own, independent of the answers proffered by the social scientist. They are questions we ask in ordinary life about the causes and effects of our own and others’ intentional actions, past, present, and future; they are also questions we strive to answer in order to gain or improve our ability to predict the forces controlling human welfare for the purposes of public and private policy.

Now if such questions are intentional, and if they do not have nonintentional surrogates, the fact that sociobiology must be silent about answers to them poses arresting consequences: Either the study of human behavior must be recognized as a theoretical activity without implications for serious practical problems of social life, its effectiveness and improvement; or there is no real hope of acquiring an understanding of individual and social welfare fine-grained enough to enable us to understand and improve social life. In the former case, sociobiology leaves a vacuum that an intentional approach to human behavior will continue to attempt to fill, no matter how firmly it is stigmatized as scientifically barren. In the latter case we must reconcile ourselves to very narrow restrictions on the degree to which we can apply our knowledge to any reliably expectable improvements in social arrangements whatsoever. The choice we may face is roughly between permanent frustration and overwhelming pessimism.

From a purely theoretical stance it is easy to let the chips fall where they may. One could conclude from the fact that there are no improvable regularities about human behavior, save those of neurology and population biology, that there is no scientific basis on which to arrange improvements and forestall deterioration in social life. But this pessimism is simply too unattractive to withstand more optimistic visions of our powers to understand and improve social life. The opponents of sociobiology have attempted to tar it with the brush of such pessimism, attributing to its exponents a commitment to “biological determinism,” according to which social life, as we currently describe it, like all other biological phenomena, is fixed and unchangeable by human intervention no matter how theoretically informed. This is a view which sociobiologists do not embrace; indeed, if I am correct, it is one they cannot embrace since they are committed to agnosticism about the understanding of human behavior intermediate between the individual brain’s effects on the body and the forces of natural selection on the human population.

If sociobiology is to survive as a subject of more than theoretical interest, and if it is to displace the intentional sciences as a repository for knowledge that claims to be socially useful, it will have to show that, for all its neutrality about human action, it has socially significant findings to report about the aggregation of individual actions into biological populations. It will have to find nonintentional surrogates...
for our current agenda of questions about the forces which affect human welfare for better or for worse. This is the sternest test that sociobiology will have to pass. But it is not one that sociobiology can be expected to meet immediately. Instead, the theory must be given a chance to develop in directions dictated by the tactics of scientific research, and not practical application. For unless it is allowed to develop in this purely theoretical way, it can hold out no more hope in explanation or application than the disciplines which we would have it supplant.  

8 I am deeply indebted to Jonathan Bennett for extended comments on and considerable improvements in the form of this essay.