

**WHY CURRENT STUDIES OF HUMAN CAPACITIES
CAN NEVER BE SCIENTIFIC**

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1. Introduction

Ever since the human sciences (anthropology, economics, linguistics, political science, psychology, and sociology) rather slowly and tentatively separated themselves from philosophy they have had to explain why the break was never clean, why they have a massive amount of data but little theoretical understanding, in short, why they have not found their own secure way of doing science as the natural sciences have. A recent discussion of this subject has taken the form of a three way debate in which Charles Taylor defends a clear distinction between the kind of interpretive understanding we can expect in the sciences of man and the kind of theoretical explanation we have achieved in the sciences of nature; Richard Rorty claims there is no essential difference between the *Geistes* and *Naturewissenschaften*, only between normal and non-normal science; and I hold that the human sciences cannot be normal because they must but cannot account for their own conditions of possibility.¹

After reading Michel Foucault, I now think that my attempt to distinguish human from natural science on the basis of the different way their background practices are internal to each fails to show any essential differences. It shows at most certain limitations on what Foucault calls the sciences of *man*, which are defined in such a way as to make their conditions of possibility internal to them, but not on the scientific study of *human beings*. Yet I do think that the strikingly different status of the natural and human sciences cannot be an accident. So after showing why Taylor, as understood by Rorty, and the ethnomethodologists as understood by me, fail to make their case that the human sciences cannot be like the natural sciences, I will use some of their insights plus Heidegger's account of the essential structure of the everyday world and of theory to try once more to locate the difference. I will argue that, although one cannot *prove* that the human sciences can never be normal sciences, one can give arguments which lead one to expect them to fall short of being normal in just the way they do. I will then suggest a way of studying human beings which can be rigorous and repeatable, in short disciplined, without being a theoretical science.

2. The Argument That the Sciences of Man Cannot Be Predictive and Stable Because They Deal Essentially With Interpretation

Natural science (especially physical science) is normally **stable** and relatively **cumulative**. Normally, sciences like physics have a generally agreed upon paradigm of how to do research. Occasionally a crisis comes along in which there is disagreement as to how to account for anomalies and about what counts as evidence and valid arguments. Crisis continues until the anomalies are removed by some new scheme which gains agreement, re-establishing normal science. On this view, the natural sciences are not as cumulative as once believed, but there is still progress.

The human sciences, on the other hand, have never been stable or cumulative. They suffer from what Foucault calls "essential instability."² These "dubious disciplines," to use Foucault's pejorative phrase, do not progress through revolutions like physical science, but merely go through episodes in which certain fads tend to dominate until some competing fad gets most of the researchers onto its bandwagon. One style of research gives way to another not because the new theory

¹ *The Review of Metaphysics*, Sept. 1980, pp. 3 - 55.

² Michel Foucault, *The Order of Things*, Vintage, 1973, p. 348.

explains certain anomalies the old theory failed to explain, but because researchers have become bored and discouraged with the old one. The new style allows everyone to forget the old questions by introducing challenging new methods and problems. Thus the human sciences are in a state of constant factionalism and reorganization, but this is not a crisis -- a period of competing paradigms between periods of normal science. It is a pre-paradigm state. As a result, the human sciences are not even normally stable nor are they even relatively cumulative.

In his article, "Interpretation and the Sciences of Man,"³ Charles Taylor attempts to show that there is an essential difference between the subject matter of the natural and social sciences, which explains why the social sciences cannot make reliable predictions and so cannot become normal. According to Taylor the essential difference between the natural and the social sciences is that the natural sciences study mere objects, whereas the human sciences study self-interpreting beings and so have to take their object's self-interpretation into account. As Mark Okrent puts it succinctly: "For this position the human sciences are radically distinct from the natural sciences. Their 'object' (what is investigated) is understanding, not things, their aim is understanding, not explanation, and their method is interpretative, not empirical."⁴

To take Taylor's example, recent political scientists assume that the political domain is essentially restricted to the adjudication of individual preferences. This definition of politics makes possible theories based on polls which determine preferences. But this is just one possible interpretation of politics which can be challenged by others, and can change with social self-interpretations. The point is *not* that the actual techniques used to collect data can be criticized as not sufficiently controlled, etc. (such a critique could also be appropriate in physics), nor that the assumptions guiding the collection of data might be false and therefore lead to false data (this can happen in any science since all data are theory laden). Taylor's example is meant to show, rather, that *interpretation* is involved in determining what are to count as political facts. Such disputes over what constitutes what has been called the regional ontology of a discipline cannot be settled in an objective way. Yet on Taylor's view they are internal to the human sciences. Taylor claims that similar conflicts of interpretation concerning the domain of investigation need not occur in the natural sciences, since the objects they study are not self-interpreting.

Rorty points out in response to Taylor that conflicts of interpretation are not restricted to, nor inevitable in, the human sciences. Although at present what counts, for example, as political is constantly disputed within political science, while the physicists take for granted some shared notion of what counts as nature -- and leave the question of how to interpret the paradoxes of quantum reality to the philosophers -- this has not always been the case. There is no in-principle reason why it might not come to pass that all political scientists would once again come to agree on the ontology of the political domain as they did in the classical age, and physicists disagree again as they once did over quantum theory. Then, the background interpretations and practices in the human sciences which define their domains, at present a constant issue within the disciplines themselves, would be taken for granted by all practitioners, while the background interpretations of the physicists would again become problematic.

According to Rorty, then, the difference Taylor finds between the *Geistes* and *Naturewissenschaften*, viz. that in the former the background interpretation establishing the regional ontology is

³ Charles Taylor, "Interpretation and the Sciences of Man" in *Interpretive Social Science*, ed. Paul Rabinow and William M. Sullivan, U.C. Press, 1979.

⁴ Mark Okrent, "Hermeneutics, Transcendental Philosophy, and Social Science," *Inquiry*, 1984 (forthcoming).

an issue for the discipline while in the later it is not, is actually the difference between both pre-paradigm and revolutionary science, on the one hand, and normal science on the other. Rorty concludes that the only important distinction between kinds of sciences is the distinction between normal and revolutionary science -- a distinction which cuts orthogonally across the old *Geistes/Naturewissenschaften* boundaries.

The line that Taylor is describing is not the line between the human and the non-human but between that portion of the field of inquiry where we feel rather uncertain that we have the right vocabulary at hand and that portion where we feel certain that we do. This *does*, at the moment, roughly coincide with the distinction between the fields of the *Geistes-* and the *Naturewissenschaften*. But this coincidence may be *mere* coincidence.⁵

Thus Rorty holds that *both* the natural and human sciences are capable of periods of relative predictive power, stability and cumulative development, as well as periods of confusion and failure. While, at present, the human sciences are unable to find an agreed upon vocabulary, this fact has nothing to do with the essential nature of man or of science. It is just an accident of the current scene. Taylor, on the other hand, insists that the success of modern science shows that we have finally got the right vocabulary for the natural world, and that when we see that the natural world is essentially meaningless and the human world is essentially meaningful and self-interpreting, we will see why we can never find the right vocabulary in the human sciences.

I am not convinced by either party. Rorty is right that in a successful science of human behavior human self-interpretation would turn out to be irrelevant, just as the sociobiologists, for example, hope. Still I agree with Taylor that the instability of the human sciences is no mere coincidence, but has something to do with the essential characteristics of their subject matter. There must be something about human beings and about the nature of theory which explains why these particular disciplines have never become normal. I will try to defend what I take to be the truth in Taylor's position in Part IV, but first we must consider an interesting recent approach to the human sciences which argues that the problems of the human sciences are not merely accidental as Rorty claims nor are they due, as Taylor thinks, to the ontological status of their subject matter. Rather, the problems of the human sciences arise from the way their subject matter, man, has been defined.

In *The Order of Things: An Archaeology of the Human Sciences*, Michel Foucault argues that the human sciences "are doomed to an essential instability," because of the way they have been set up. As Foucault puts it:

What explains the difficulty of the 'human sciences,' their precariousness, their uncertainty as sciences, their dangerous familiarity with philosophy ... is not, as is often stated, the extreme density of their object; it is not the metaphysical status or the ineradicable transcendence of this man they speak of, but rather the complexity of the epistemological configuration in which they find themselves placed...⁶

According to Foucault the human sciences involve a unique human self-interpretation, which reaches its fullest expression in Kant. They interpret their domain of investigation, man, as a transcendental/empirical double -- a meaning giver who constitutes the world and determines what counts as objects, and yet is an object in the world like any other. This conception of man makes

⁵ Richard Rorty, *Philosophy and the Mirror of Nature*, Princeton University Press, 1979, p. 352.

⁶ Michel Foucault, *The Order of Things*, Vintage, 1979, p. 348.

human self-interpretation essential to an understanding of human beings while at the same time stipulating that human beings are meaningless objects amenable to the sort of theory characteristic of the natural sciences.

Philosophers from Kant to Donald Davidson have accepted this self-definition and argued that man can be viewed under two aspects, i.e. that one can give both a total intentionalistic and a total physicalistic account of all human activities. This view runs into problems concerning free will, but even if this double aspect theory could be made to work, the sciences of man which operated within this metaphysical perspective would be abnormal. Each aspect is supposed to yield a complete description without reference to the other, so in any discipline in the human sciences one would expect an interpretive and a materialist school to confront each other, each claiming to have the right vocabulary in which there was no place for the vocabulary of the other. If the materialistic side could complete its research program and produce a predictive theory, it would no doubt carry the day, but since it cannot, for reasons Foucault does not try to explain, but which I will develop in Section IV, the sciences of man as defined by Kant inevitably exhibit the “wearisome” form noted by Foucault. An interpretative social scientist like Taylor will claim that there is something about human subjects or meaningful practices which make them essentially different from objects, and so intrinsically incapable of being brought under objective theory, while the other side will argue that human activity is identical with physical motion and so must fall under some sort of objective laws. Each side then supports its own approach by convincingly showing the inadequacy and incompleteness of the other.

3. The Argument That the Sciences of Man Cannot Be Scientific Because They Must Yet Cannot Have a Theoretical Account of Their Own Background Practices

Foucault’s approach allows us to understand the force and the fallaciousness of what seems to me the only interesting argument other than Taylor’s that the human sciences can never be scientific, viz. the ethnomethodologists’ claim that the background skills presupposed by social scientists (not the self-interpretations of the human beings they study) are internal to the human sciences but cannot be treated theoretically.

To understand this challenge to objective social science, we must first distinguish ontological background practices which establish a domain of reality to be investigated, from those ontic practices which produce the specific data studied by a discipline. The practices which determined what counts as language, history, the political, etc., embody an ontology, and establish a general domain. To take Taylor’s example: American social practices contain an interpretation of political life as negotiation among private individuals each with his or her own preferences, while Japanese social practices define the political as an attempt to find a consensus among those whose primary loyalty is to the social group.

Given a particular domain of reality there are other practices which we will call *ontic*. These ontic practices enable the practitioners in a discipline to collect data by picking out and isolating specific facts within an already established domain. Such skills enable coroners, poll takers, linguists, psychologists, etc., to produce the data social scientists attempt to relate by laws. Thus we can distinguish the *constitutive* or *ontological* background practices of society which interest Taylor from the *ontic* background practices presupposed by the scientists themselves which interest ethnomethodologists.

Ontological practices involve an interpretation of what counts as a domain of facts -- an interpretation which is subject to challenge by a counter-interpretation and can result in a scientific revolution or a pre-paradigm change of bandwagon. *Ontic* practices, however, do not involve an interpretation in the same sense. They merely determine what are the relevant details within an already taken for granted domain. To take suicide statistics as an example, what is at stake when Garfinkel criticizes Durkheim's use of suicide statistics is not a conflict of interpretations as to what counts as a suicide, but a question of what specific considerations are relevant to determining a suicide. The point of the interpretive social scientists is simply that suicides are not given as brute facts but must be produced by a set of specific practices which produce specific facts. To control for these practices, ethnomethodologists want to find the rules which enable the expert, in this case the coroner, to produce his statistics.

Garfinkel has recently discovered that not only sociologists but also physicists, astronomers, etc., have elaborate shared skills which enable them to produce objective data. Garfinkel thinks that these background practices should be a part of physics just as the sociologists' background practices are a part of sociology. Unfortunately, natural scientists turn out to have what Garfinkel considers a reprehensible "amnesia" concerning their background skills when they write up their results in publishable articles. I think that Garfinkel misunderstands this so-called amnesia. What he has discovered is that scientists in a normal science have a double skill. They have a first order skill for producing decontextualized data by using instruments which select only context-independent features such as color, weight, rate of diffusion, etc., for only such decontextualized facts can fill in the variables over which scientific laws range. But, normal scientists also have learned a further skill by which they eliminate all reference to taken-for-granted, shared first order skills from their final reports. Thus a description of the practices by which everyday objects get worked over into bare physical facts is external to normal natural science. Descriptions of such skills appear and should appear in the journals of the ethnomethodologists, not in *The Physical Review*.

The ethnomethodologists would prefer to see the physicists themselves reporting on their background skills, but this is not their main point. Their essential claim is that the human sciences *must* include an account of their own background practices, not just because otherwise they might produce faulty data -- that is true in any science -- but because, as I once put it when I held this view, the practices of social scientists are an essential part of human activity and so cannot be omitted from human science. Thus the question inevitably arises: Can the human sciences provide a scientific theory of their own background practices? At this point, the interpretive social scientists claim, objective social science breaks down. If background skills could be captured in strict rules then we could have objective social science, e.g. "cognitive" sociology, but skills cannot be objectified. They must be taught by apprenticeship and can at best be formulated in *ceteris paribus* rules whose everything-else-being-equal clause presupposes a shared background of practices which cannot be further specified in rules.

I think the claim that skills cannot be treated theoretically is correct and very important, but this fact does not have the consequences the interpretive social scientists suppose. But since I wish to draw on this point later, I will sketch here the phenomenological considerations which lead me to the conclusion that, however skilled behavior is produced by the nervous system, it is not the rule-governed organization of elementary movements or actions as theories such as behaviorism and cognitivism require.

Skills may, and, indeed, usually do begin as rules for manipulating context-free elements. This is the element of truth in cognitivism. Thus a chess beginner must follow strict rules relating such features as center control, material balance, etc. After one begins to understand a domain, however,

one sees meaningful aspects not context-free features. Thus a more experienced chess player sees context-dependent aspects like unbalanced pawn structure or weakness on the king side. A further stage of proficiency is achieved when, after a great deal of experience, one is able to see a situation as having a certain significance tending towards a certain outcome and certain aspects of the situation stand out as salient in relation to that end. Finally, after even more experience the current situation is perceived as similar to another already experienced one and one sees immediately what is required. The chessmaster, for example, sees the issues in a position almost immediately, and the right move just pops into his head. *There is no reason to suppose the beginner's features and rules, or any other features and rules, play any role in this mastery.*⁷

If there is no reason to suppose and strong reasons to doubt that the skills which make possible the isolation of features and rules can be understood in terms of features and rules, and if the social sciences must have a theory of their own background practices, then theoretical social science cannot be achieved.

I hope this argument sounds convincing, since I was until recently convinced. Foucault, however, shows both why it is convincing and also why it does not have the significance one might suppose. In *The Order of Things*, he points out that the demand that each human science produce a theoretical account of that science's foundations is a natural outcome of trying to have a science of *man*. Since man is understood as both the meaningful activity which determines what counts as facts, and at the same time an object totally subject to theoretical investigation, the sciences of man in trying to subsume every aspect of man must keep trying to objectify the background practices which provide their objective data.

[T]he human sciences ... find themselves treating as their object what is in fact their condition of possibility. They are always animated, therefore, by a sort of transcendental mobility ... They proceed from that which is given to representation to that which renders representation possible⁸

But these problems are only problems for a science that studies a peculiar interpretation of human beings as a special sort of transcendental/empirical double Foucault calls *man*. In spite of Taylor's and Garfinkel's arguments it would seem that neither ontological nor ontic background practices put an in-principle barrier in the path of the scientific study of *human beings*. Only those sciences in the grip of the transcendental/empirical double are compelled either to objectify everything, even the background practices which make them possible, or else to give up theory and turn to interpretation. In *The Order of Things*, Foucault, like Rorty, holds that the instability of the sciences of *man* is in no way essential to, or even relevant to, the scientific study of *human beings*. Just as Rorty thinks that current cognitive science might well be on the right track, at the time he wrote his critique of the human sciences Foucault felt that a science of human beings was being achieved by "structuralist" formalizations in the works of Lacan, Chomsky and Lévi-Strauss.

Still, the structuralist sciences and cognitivism have fared no better than their predecessors, so it seems that the contrast between the success of the natural science and the failure of the sciences which study human beings was not simply the result of a transient historical configuration. Granted the inevitable failure of the sciences of *man*, we must still ask: What are the obstacles in the way of

⁷ One can, of course, recall the rules one once used and act on them again, but then one's behavior will be halting and clumsy just as it was when one mastered the rules as an advanced beginner. For a more detailed account of the stages of skill acquisition and the implications of this account for cognitive science, cf. *Putting Computers in Their Place*, Hubert and Stuart Dreyfus, Morrow Books, forthcoming.

⁸ *Ibid.*, p.364

any theoretical study of human capacities?⁹

4 The Argument That There Cannot Be a Theory of Human Capacities Based on Elements Abstracted From the Everyday Context of Human Activity

Rorty admits that the human sciences, construed as *Geisteswissenschaften* or sciences of *man*, have been in trouble from Dilthey to behaviorism, and Foucault explains why, but neither can explain why cognitivism and structuralism (which are supposed to have superseded hermeneutics, behaviorism and man in general) are no more predictive and so no more normal than their predecessors. I will now argue that this non-normality is inevitable given the relation of theory to human activity. Inevitable difficulties arise not from the definition of man but from the fact that the current sciences of human beings imitate natural science in seeking a theory that predicts events in the everyday world using context-free features abstracted from that world.

First I will have to define what I mean by theory and by science. Theorizing is a special form of intellectual activity, discovered by Socrates and refined by the philosophical tradition. Ideal theory has four essential characteristics, never fully achieved, but approached to varying degrees. The first two are introduced by Socrates. (1) *Explicitness*. A theory does not require interpretation but applies to the phenomena by virtue of features which can be recognized “automatically.” (2) *Abstractedness*. A theory must not require reference to particular examples. In the *Euthyphro* Socrates presupposes these two requirements when he asks the prophet, Euthyphro, for a definition of piety, which is completely explicit and applicable by anyone, angrily rejecting Euthyphro’s appeal to examples and his own special intuition. Descartes and Kant complete the Socratic discovery of theory by extending Socrates’ requirements. (3) *Discreteness*. The classical age added analysis into elements. A theory must be stated in terms of context-free elements -- elements which make no reference to a context of human interests, traditions, institutions, etc. (4) The Enlightenment added *systematicity*. A theory must be a new whole in which decontextualized elements, (attributes, features, factors, etc.) are related to each other by rules or laws.

Plato clairvoyantly expressed all four characteristics in the myth of the cave: the theorist must remove himself and his object of knowledge from the everyday perceptual and social world, in order to see the elements, in this case the ideas. Freed from all context, the elements form a system of their own -- all ideas are organized by the idea of the Good. Plato saw that while everyday accounts are implicit, concrete, local, and partial, theories are explicit, abstract, universal, and have their own total intelligibility.

Plato put the natural sciences of his day on only the third level of his divided line since they were not indubitable and rationally grounded. Still, the theoretical explanation of nature developed by modern natural science while contingent is nonetheless the most successful version of the sort of theory Plato envisaged. Indeed, by relinquishing indubitable and grounded knowledge, the modern natural sciences have been able to add a fifth characteristic of ideal theory. It is this characteristic and its advantages which is sought in vain by the social sciences.

(5) *Prediction and completeness*. The description of the domain investigated must be complete, i.e. it must specify all the relevant types of changes of elements and their effects. In the

⁹ We will have to call such a science a science of human *capacities* since the idea of a science of human *behavior* is as much a reflection of the transcendental/empirical double as is a science of human *actions*.

natural sciences this completeness permits precise prediction. The human sciences seek similar predictive power. The behaviorist seeks a complete description of human action in terms of elements of behavior and proposes covering laws to predict the occurrence of particular behavioral events; the cognitivist assumes human beings are devices whose functional components are in discrete internal states and seeks to explain this device's capacities by analyzing the capacities of its components and the rule-like relations holding between their states. Both approaches aim, in the ideal limit, at attaining the sort of complete explanatory account which allows one to predict precisely the effects of alterations in an object's internal and external environment.

The problem for the sciences which would study human capacities as their object is that they must define an appropriate level of investigation. They cannot seek to predict all the motions of human bodies. For example, they do not wish to predict people's coughs or the trajectory of their falls. Rather, they must pick out as their object that aspect of everyday human activity which is described in everyday terms as speaking, political action, social interaction, etc. Then this everyday activity under its everyday description must be predicted following the model of natural science, i.e. by subsuming elements abstracted from these everyday events under laws or rules. This is, indeed, the only available approach for the science of human capacities, but, I will try to show that such a project must fail because (1) the features necessary for a theory of everyday capacities cannot be totally abstracted from the everyday background practices, and, (2) as I have already argued, these background practices cannot themselves be analyzed in terms of elements related by rules.

In the natural sciences decontextualized elements recontextualized by covering laws are all that is needed to make predictions. The elements and laws are meaningless, and so are the phenomena they are meant to explain. Natural science, according to Heidegger is: "[A] subspecies [of knowledge] which has the legitimate task of grasping the present-at-hand [decontextualized elements] in its essential unintelligibility."¹⁰ Just as physical science predicts and explains everyday motion in terms of meaningless, context-independent properties such as mass and motion which can be abstracted from the everyday world, so a theoretical science of human beings would have to abstract meaningless, context-free features or attributes from everyday context-dependent meaningful activity, and then predict and explain this everyday human activity in terms of formal relations between these elements. So, to take up Foucault's favorite examples, Chomsky looks for syntactical elements and formal transformational rules to account for everyday judgments of grammaticality, and Lévi-Strauss abstracts objects used in exchanges to formalize certain aspects of everyday social interactions.

The reason this approach does not work is that it attempts to predict and explain everyday activities whose constitutive objects and events are picked out in pragmatic contexts, by means of *decontextualized* features alone. Precisely the contextual understanding in terms of which human beings pick out the everyday objects and events whose regularities theory attempts to predict, is left out in the decontextualization necessary for theory. Thus what human beings pick out as relevant objects and events may not coincide with those elements over which the theory ranges. For this reason, predictions, though often correct, are not reliable, since these predictions work only as long as the elements picked out and related by theory happen to coincide with what the human beings falling under the theory pick out and relate in their everyday activities.

My thesis can be illustrated by the sort of difficulties that confront Lévi-Strauss's structuralist theory of gift exchange. Pierre Bourdieu, in *Towards a Theory of Practice*, argues that Lévi-

¹⁰ Martin Heidegger, *Being and Time*, Harper and Row, 1962, p. 194.

Strauss's formal, reversible rules for the exchange of gifts -- abstracted as they are from everyday gift giving -- cannot account for and predict actual exchanges. His point is not that theory leaves out subjective, so-called phenomenological, qualities of gift exchange. That would not be a valid objection. The natural sciences legitimately abstract from context-relative and subject-relative properties as long as the resulting abstractions serve as the basis for predictive laws. Bourdieu's point is that Lévi-Strauss's abstraction of the pure objects exchanged leaves out something essential since the exchange would not take place at all if it was perceived as simply a reversible operation.

The difference and delay which the [theoretical] model obliterates must be brought into the model not, as Lévi-Strauss suggests, out of a "phenomenological" desire to restore the subjective experience of the practice of the exchange, but because ... the *interval* between gift and counter-gift is what allows a pattern of exchange that is always liable to strike the observer and also the participants as *reversible*, i.e. both forced and interested, to be experienced as irreversible.¹¹

But this is still not the whole point, since the scientist need not care how the exchange is perceived, or that it must be misperceived if it is to take place at all. The real problem arises because the meaningful context does more than provide a necessary cover-up of the formal skeleton of the transaction. The tempo of the event actually *determines what counts as a gift*.

In every society it may be observed that, if it is not to constitute an insult, the counter-gift must be *deferred* and *different*, because the immediate return of an exactly identical object clearly amounts to a refusal.¹²

Thus predictions based only on formal principles fail in those cases in which what formally counts as a gift in the theory is rejected because it is reciprocated too soon or too late to count as a gift in everyday practice. Bourdieu makes this point (although he does not seem to realize its importance):

It is all a question of style, which means in this case timing and choice of occasion, for the *same act* -- giving, giving in return, offering one's services, paying a visit, etc. -- *can have completely different meanings at different times*.¹³

What cannot be accounted for is the meaning or interpretation of the event. Thus what is at issue is not the *social scientist's* ontological interpretation of what counts as as social interaction emphasized by Taylor, nor the ethnomethodologist's ontic skill in picking out a particular type of social event, but rather it is the *societies' interpretation* of what counts as the sort of event in question. *The meaning of the situation plays an essential role in determining what counts as an event, and it is precisely this contextual meaning that theory must ignore.*

The theorist may be able to predict that *if* an action is accepted as an exchange it will have a certain consequence, but he will not be able to predict which events will be taken as exchanges. The rules for what counts as refusals, insults, etc. if they play any role at all, are *ceterus paribus* rules which presuppose the skills of living and acting in a particular culture, not the strict rules required for prediction. If one tries nonetheless to formulate predictive laws relating abstractly defined exchanges, the laws will be at best approximate. Prediction will not only fail across changes in human self-interpretation, as Taylor contends, but prediction will fail every time the situation is

¹¹ Pierre Bourdieu, *Outline of a Theory of Practice*, Cambridge University Press, 1977, pp. 5,6.

¹² *Ibid.*, p. 5. My italics.

¹³ *Ibid.*, p. 6. My italics.

abnormal since in such cases only background skills and *ceterus paribus* rules, not strict rules relating abstract features, can determine what counts as the facts.

Philosophers of science differ on what counts as an explanation, but one thing seems clear; if all the factors which the theory takes into account can remain the same, yet the resulting behavior vary, such an account is not an adequate explanation of the behavior in question. But such variation is inevitable in any area where features such as what counts as a gift depend on background practices not integrated into the theory. The problem is not that the social theorists do not have a theory of how they (the theorists) determine what counts as a gift -- the fact that they have no such theory is no more damaging than that physicists do not have a theory of how they classify bubble chamber photographs. The problem is that the social theorists do not and cannot have a theory of how the people they are studying determine what counts as a gift. The problem is that the social theorists' way of defining a gift, since it must be abstracted from the everyday world, will not coincide with the pragmatic way the social situation defines a gift. There is no corresponding problem for bubble chambers, since bubble chambers do not classify their own bubble-tracks.

The natural way to cope with this problem is for the social scientist to abandon the attempt to find what one might call first-order interpretation-free features and to settle for second-order features such as the participants judgment whether what was just exchanged counted as a gift. Such judgments or, more generally, scores on questionnaires, would be treated as context-free facts or features and then related by theory to explain and predict other objectively determined judgments or test scores. But if the social theorist wants precise and reliable predictions this "solution" must be only provisional. As Taylor points out, as long as practices determine, in a way which in principle falls outside the theory, what counts as the elements over which the theory ranges, the theory is at the mercy of possible changes in the practices which might in effect redefine the features and thereby undermine the predictive power of the theory.

Consequently, all theories must aim at objectively determining the judgments on which the theory is based, at finding a level of analysis divorced from context effects. Chomsky, for example, introduces the distinction between competence and performance, thereby replacing actual judgments of grammaticality by ideal judgments. And Lévi-Strauss has declared that structuralist rules are not subject to the vagaries of human contextual judgments since they reflect universal structures innate in the human brain. This saves the theories by making them empirically untestable. But since a research program's general acceptability requires that it demonstrate its capacity to make reliable predictions, theories idealized so as to be immune to prediction and to failure cannot serve as the basis of a stable, normal science.¹⁴ Moreover, pragmatic limitations on the possibility of reliable prediction are not anomalies which one can hope to eliminate by extending the theoretical approach. There is simply a limit to the predictive possibilities of a theory of everyday capacities.

¹⁴ Economics seems to be an exception here. It accepts the current understanding of money, property, etc. and seeks laws relating these socially defined elements. But its laws are only as stable as these social definitions. Charles Taylor makes this point in his article, "Social Theory as Practice":

Economics can aspire to the status of a science, and sometimes appear to approach it, because there has developed a culture in which a certain form of rationality is a ... dominant value. And even now, it fails often because this rationality cannot be a precise enough guide. What is the rational response to galloping inflation? Economics is uncertain where we ordinary agents are perplexed.

In the face of such a problem economists like linguists turn to a high-powered formalism divorced from reality but also divorced from predictive power. Oddly, this has not resulted in as much pre-paradigm factionalism in economics as one finds in linguistics.

John Searle has claimed that formal, causal accounts in the social sciences must fail because intentional mental states play a causal role in human behavior and so must be taken account of in any science of human capacities.¹⁵ But this does not seem to me to be the right place to look for the essential limit on prediction. Much of human behavior could and does take place as ongoing skillful coping without the participants having any mental states (i.e. beliefs, desires, intentions, etc.) at all. To repeat my point, what is crucial is that what human beings pick out as specific sorts of objects depends on background skills which are not rule-governed, and so what counts as a certain sort of object is not rule-governed either. Thus prediction will fail because an object or event which has the defining features which, according to the theory specify a type of object or event, will, in some contexts, not count as an instance of that type. Imprecision in the study of human capacities is inevitable because what counts as an everyday fact depends on a background of meanings and skills which is explicitly excluded by the decontextualization required by theory.

This is the deep reason behind Foucault's historical observation that the sciences of man are driven to try to objectify their own conditions of possibility. What drives the human science to try to objectify their background practices is not merely a particular historical structure defining what counts as their object as Foucault holds, but the necessary condition that any predictive science bring all the relevant factors into its theory. That is why would-be sciences, such as structuralism and cognitivism that no longer study human beings as subject/object, or transcendental/empirical doubles are still caught in the necessity and impossibility of giving a theoretical account of the pragmatic conditions which determine in each situation what will count as the sort of events their theory seeks to predict.

To deal with this problem it would be natural to try to explain the pragmatic background itself in terms of features abstracted from it. Thus one can attempt to treat the whole meaningful context as a belief system, i.e. as a system describable by strict rules. Rorty's optimism concerning Cognitive Science in spite of his pragmatism, must stem from his confidence that some such development is possible. And, indeed, if the problem were really a problem of background *knowledge* then there would be hope for the sciences of human capacities as we now understand them. But Heidegger's description of everydayness in *Being and Time* and my description of skills suggests that there is a level of pragmatic activity which cannot be understood as *knowledge* at all. In some cases these social skills may have been acquired by consciously following rules defined over elements but, like any skill which has reached a level of mastery, our ability to cope with everyday things and situations cannot be analyzed in terms of the elements which went into its acquisition. Yet no one has found any other elements or is even trying to find them. The pragmatic background and its practical kind of understanding is the source of the problems confronting a theory of everyday capacities, and recent attempts by AI researchers to analyze everyday understanding in terms of elements and rules, face the same insoluble problems concerning meaning and skill which confront any theory of a specific human capacity, only at a higher or more general level.

If everyday human behavior cannot be predicted by laws and rules relating elements abstracted from that behavior, we can understand why the new structuralist and cognitivist sciences of human capacities have turned out to be as unstable and abnormal as were the old behaviorist sciences of man. All such theories must either refuse to make predictions or else confront repeated exceptions. Given the lack of any solid successes, other approaches inevitably arise in the discipline which offer competing types of systematic accounts. Such competing types of account do not agree on method,

¹⁵ John Searle, "Intentionality and its Place in Nature," *Dialectica* (forthcoming).

evidence, or even on what are the problems. We then have neither normal science nor even revolutionary science, but just the sort of pre-paradigmatic instability characteristic of the social sciences from their inception to the present.

5 The Logical Possibility of a Theory Based on Other Features than Those Abstracted from the Everyday Context

It follows from the above considerations that the right vocabulary for what were once called the human sciences would have to be a vocabulary which picked out entirely different features than those abstracted from our everyday activity -- features or attributes which would remain invariant in different pragmatic situations and across cultural revolutions. Just what such features would be, no one can say. And for this very reason there is no way to prove that such hypothetical features do not exist. All agree that if there were such features and we were able to find them by some sort of luck or divination, then the study of human capacities could, in-principle, be closed, predictive, and normal. Entertaining such logical possibilities has little use, however, except to keep someone like Taylor or myself from arguing that, *in principle*, the study of human behavior can never be scientific. For even if there can be no in-principle argument against the possibility of a scientific theory of human capacities, it does not follow that the lack of such a theory may be mere coincidence. Even less can one conclude that current cognitive science may, at last, be on the right track.

Taylor and Rorty, because each seems to think at times he has proven more than he has or could, fail to appreciate each other's points. Taylor sometimes argues that human self-interpretation plays a causal role in human behavior so we cannot predict across changes in self-interpretation. At other times Taylor is more consistent. He admits that in principle one might have a science of human beings under some other description. He then argues that to formulate a theory of human activities in other terms than those abstracted from everyday activities would be to deny the reality of human action as we understand it. Thus what Rorty calls finding the right vocabulary could not be finding the right way to explain and predict what the human sciences are now trying to explain and predict; rather it would have to be finding a way of explaining and predicting something else.

Rorty's optimism concerning cognitive science shows that he too thinks his arguments prove more than they can. He sometimes seems to think that he has shown that since there is in principle no argument against a science of human capacities under *some* description, then any science of human capacities, even the sort arrived at by cognitivists and structuralists, might work. That is, he seems mistakenly to conclude from his correct point that in principle there could be a successful science of human capacities, that there cannot possibly be an argument that the human sciences as currently understood and practiced cannot succeed. Nonetheless, the argument I have presented does lead us to conclude that all current human sciences, even recent forms such as cognitivism, since they are committed to using features obtained by abstracting from pragmatic contexts to predict everyday events in everyday pragmatic contexts, will necessarily run into difficulties which will drive them to try to formalize the everyday common sense background of human affairs. A project which will fail since background skills cannot be understood in terms of the features they enable us to pick out. (Whether the background skills can be analyzed in terms of some other features is not in question since we have no access to such features and they are not sought by current cognitive science.)

6. Conclusion

To clarify this complex debate I will separate the issues into three distinct questions:

(1) Can there be a science of *man*? Here Foucault is right. There can be no stable science of an entity which as meaning giver is the condition of its own objectification. No science can objectify the skills which make it possible. But this only shows we should abandon the Kantian definition of man.

(2) Can there be a science of human activities? Here Taylor, as modified by me, is right. No, if one tries to follow the model of natural science. That is if one seeks a theory of human capacities by abstracting context-free features from everyday contexts and then trying to predict everyday human activities using rules or laws relating these features.

(3) Can there be a science of human capacities using other features than those used in everyday practice? Here Rorty is right. In principle such a theory is possible. But, one must add that since we have no precedent for such a theory, no reason to believe the abstract features it would require exist, and no way to find them if they did, this abstract philosophical point casts no light on the past, present or future difficulties facing the social sciences.

7. Epilogue

If it is true that a theory of human capacities using features abstracted from the everyday world, is, in principle, impossible, what can social scientists do? One thing they can do is go on as they have been, trying to abstract feature so as to develop closed predictive theories, pushing each new theory to the limit until it fails to predict some phenomenon it is supposed to cover, and then jumping on the next new bandwagon to come along. Some useful data accumulates in this way, although not very much. Or they could try to find meaningless, context-free features unrelated to the sort of features people sometimes notice, in terms of which to explain something like human capacities. But where would they look? How would they begin?

Another possibility is to develop a disciplined study of human beings which does not seek to be a *theory*, but still seeks to be a systematic account of everyday activities. One such possibility is the study of the role of prototypes in various areas of human activity. Kuhn has shown that the background practices of the natural sciences are organized by exemplars, whose role can be studied by the historian of science. Clifford Geertz and Foucault organize their interpretations of cultures around specific paradigms such as the cock fights in Bali or European prisons and confessional practices which focus general characteristics of the background practices. It may be a coincidence, but I think it is not, that some linguists are now studying the prototypes which show up in the grammars of all languages. For example, by studying transitivity in many language, linguists have been able to show that in a prototypical transitive sentence the subject is an agent, the object is inanimate, and the relation is one of transmission of causal power. On this basis one can predict that a transitive sentence like "John desires a change," would be relatively rare in the world's languages and harder to learn. This turns out to be true.¹⁶ Prototype study is also a very lively branch of current psychology. Research has shown that the study of prototypes in history of science, anthropology, linguistics, and

¹⁶ Paul J. Hopper and Sandra A. Thompson, "Transitivity in Grammar and Discourse," *Language*, Volume 56, Number 2 (1980).

psychology can lead to illuminating results and even to repeatable experiments.

Yet the function of prototypes cannot be captured in a theory. A scientific paradigm Kuhn has pointed out, is neither rationalized nor rationalizable. An activity is recognized as good science by being recognized as similar to an exemplar, without it being similar with respect to any abstractable identical features. Thus paradigms or exemplars cannot be treated as abstract types; rules cannot replace the typical case. More generally, human beings tend to agree in their judgments of how similar an object or event is to a prototypical case, without being able to explain their judgment in terms of context-free features as required by cognitivist theory. Indeed, as Eleanor Rosch has noted, what counts as a relevant feature seems to follow from judgments of similarity rather than to be presupposed by them.¹⁷ This would, indeed, make theory impossible.

One could, of course, postulate some sort of highly complex features of prototypes abstracted by the mind in ways unknown to the human beings doing the abstracting. But this is not how cognitive science now proceeds. Rather an army of investigators have been searching for the sort of abstract features which human beings do on occasion actually use, in an unsuccessful attempt to use such features in a theory which would explain judgments of similarity in terms of recognition of identical features. Meanwhile, prototype studies are producing interesting, repeatable, unified accounts of human capacities in perception, knowledge, language, etc. These are not explanatory theories in the strong sense laid down by Plato, Descartes and Kant, which have succeeded so impressively in chemistry and physics, but such accounts of what are called prototypicality effects are as scientific as some areas of biology such as current accounts of evolution. After 2000 years it seems clear we must give credit to Socrates and Plato for the vision of theory which has flourished in the natural sciences, but in the human sciences it might turn out that Euthyphro, who kept trying to give Socrates paradigm cases rather than abstract rules, was a true prophet after all.

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¹⁷ Eleanor Rosch, "Principles of Categorization," in *Cognition and Categorization*, ed. Eleanor Rosch and Barbara B. Lloyd, Erlbaum, 1978, p.42.