Committee I
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THE PROBLEM OF INTER-THEORETIC RELATIONS: A METHODOLOGICAL INTRODUCTION

by

Noretta Koertge
Professor of Philosophy of Science
Indiana University
Bloomington, Indiana

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Inter-Theoretic Relations*

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N. Koertge

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INTRODUCTION: It sometimes happens that a perfectly good word becomes so laden with controversy and so varied in connotations that one hardly dares use it. Examples of words which, in my experience, are almost guaranteed to polarize and confuse any heterogeneous audience include dialectic, feminist, genocide, and logic. I believe that the topic of many of the papers in our committee, reduction (or reductionism) is also a lightening-rod word. Some of the currents are positive, some negative--each emanates from a highly charged, more-or-less wispy conceptual cloud.

If (as Sir Karl, in his idealistic moments sometimes pretends) words didn't matter, the solution would be very simple--someone, say Nagel or the first speaker, would simply stipulate a definition for the troublesome word and we would all obligingly adopt it and stop speaking our private languages. But (as Sir Karl has also told us) words perform expressive and signalling functions as well as descriptive or argumentative ones.

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What I would first like to do is briefly describe (caricature?) a few of the diverse attitudes/programs which may be signalled when people talk about reduction. I want to then quickly move on to the question of the connection (if any) between a concern with reduction and a concern with criticism and the growth of knowledge. In the second part of my paper, I analyze some of the inter-theoretic relations among three distinctive approaches to the explanation of human behavior, those of Popper, Skinner and Marvin Harris.

PART I: Is Reduction the Most Important Inter-theoretic Relation?

The Classical Concept: To my surprise, the technical sense of reduction as used by philosophers of science does not appear in any of my dictionaries. The O.E.D. perhaps comes closest with "To bring into another language (I have reduced it into Englysshe--1484)" and "To bring a syllogism into a mood of the first figure."

I have been unable to determine when the term first became popular with scientists and philosophers. (I would welcome early examples from you.) But surely no one has expressed the idea more clearly than Bertrand Russell in the concluding chapter of Introduction to Mathematical Philosophy (although he does not use the term):

> "Mathematics and logic, historically speaking, have been entirely distinct studies. [However]... in fact, the two are one...[S]tarting with premises which would be universally admitted to belong to logic and arriving by deduction at results which as obviously belong to mathematics, we find there is no point at which a sharp line can be drawn, with logic to the left and mathematics to the right."(p.194)

There is certainly no hint in Russell's account that the reduced science (mathematics) is in any sense inferior to the primary science (logic). If anything, the evaluation is reversed: "They differ as boy and man: logic is the youth...and mathematics is the manhood of logic." (p.194) However, we will see that often the reduced science is considered to be diminished, either ontologically or epistemologically.

Positivistic Reductionism: The whole epistemological program of the logical positivists centered on the problem of reducing all of extant science to statements expressed in an epistemologically privileged primary language.

As the difficulty [impossibility] of this task became apparent, positivists compromised their program in various ways. First of all, they strengthened the primary language enormously, moving eventually from descriptions of Machian momentary sensations to observation languages which included rather esoteric terms such as pressure, sulfuric acid, and half-life.

But even with a beefed-up base, it was still impossible to produce adequate definitions of terms such as electromagnetic radiation, or ideal gas. And so again they retreated, this time by dropping the formal requirement that the concepts in the secondary science be strictly defined in terms of the primary vocabulary and admitting that the theorems in the secondary science could not all be strictly derived from the primary science. Thus, for example, we have Carnap's reduction sentences (which, despite their name, do not allow us to replace theoretical terms by observation terms) and Bohr's correspondence principle which openly admits that the secondary science holds only approximately in certain limiting cases.

What is generally not admitted is that these retreats totally vitiate the original epistemological program. If electrons cannot be defined in terms of sensations or pointer readings, then it no longer makes sense to claim that they are just a convenient conceptual shorthand for complicated arrays of sense experience. And if our best theories correct (i.e., are inconsistent with) our best low-level laws, then (as Popper has emphasized) one can hardly claim that the former are inductively supported by observational generalizations.

By clinging to the term reduction (while redefining and modifying it), latter-day positivists tend to obscure these very real changes in our understanding of both the structure and the growth of science.

Explanatory Reductionism: A very different reductionist program was initiated by the pre-Socratic philosophers. They wanted to reduce the wild fluctuating variety of experience to a few originative principles which behaved in a law-like fashion. Thus, the atomists wished to analyze the sweet taste of honey in terms of the roundness and smoothness of its constituent atoms. Plato called on astronomers to build up the complicated retrograde motion of planets from a combination of uniform circular motions. Galen tried to explain the wide variety of physiological and emotional states in terms of a shifting balance between only four humors.

But, whereas, Mach (and other positivists) took human sensations to be primary and tried to reduce everything else to them, the Greeks tried to explain our ordinary experience of the world in terms of conjectural primary starting points. Aristotle is very explicit about this pragmatic inversion--he stresses that what is better-known-to-us may not be the same as what is better-known-(primary)-in-Nature. (In a Lakatosian rational reconstruction of the history of philosophy, Aristotle would, of course, come well after Mach.)

And whereas the positivistic enterprise is vitiated if the attempted reduction to the epistemologically privileged primary science cannot be carried through, explanatory reductionism as a program emerges unscathed if our particular conjectural primary theory turns out to be false or incomplete--we simply revise it freely and try again. Or so one might expect from my account so far.

<u>Reduction and Metaphysics</u>: My deliberately naive account of explanatory reductionism left unanswered the question of where the explanatory concepts in the primary science proposals came from and why they were deemed attractive. As many non-positivistic historians and philosophers of science have stressed, most scientific conjectures originate in metaphysical world-views which are defended with philosophical arguments. Thus, Aristotle did not propose his four quality theory of matter just because it looked like a promising explanatory hypothesis. He also supported it with a variety of plausibility considerations. Likewise, for the Mechanical Philosophy of

Descartes, 19th C. Natur-philosophie, Ostwald's Energetics, and Popper's Rationality Principle Approach. The metaphysical context of our scientific proposals enriches our theorizing in many ways but it also means that there may be some motivation to gloss over reduction failures.

If magnetism cannot be reduced to complicated collisions of corpuscles, then Cartesianism is dead. If language learning cannot be explained in terms of Skinnerian re-enforcement schedules, then behaviorism is no longer a viable metaphysics of human behavior. If institutions have latent functions which benefit the group as a whole, but which have no direct discernible impact on individuals, then methodological individualism would be weakened.

On the other hand, opponents of the world-view of the primary science may be predisposed to over-estimate the difficulty of the proposed reduction. Although some so-called "anti-reductionists" are against scientific explanations of any kind (some phenomenologists seem to fall into this category), more often the opponents of reduction are really complaining about the limited explanatory resources of the prevailing mataphysics. (See, for example, the papers in Beyond Reductionism, the proceedings of a 1968 Alpbach Symposium on the Life Sciences.)

Reduction and Disciplinary "Trade Unions": Had Russell's logicist program succeeded, it of course would have been technically true (although misleading) to say that mathematics was "nothing but" logic. As Russell points out, in his system it is doubly erroneous to describe mathematics as the science of quantity or number. Leaving aside the traditional counter-examples of topology and projective geometry, after the reduction we see that even arithmetic is about sets, not about numbers as primary, unanalyable entities.

As we know from this example, the disciplinary and educational consequences of a reduction can be enormous. Not only must graduate students in mathematics now learn logic and set theory (which is prima facie a good thing), for a time at least, even elementary school education in mathematics was "logicized" (which was at best a mixed blessing). There is, of course, no a priori reason why the pedagogical order in which we approach a science should mirror the logical order of the subject. (As a first approximation, the historical order would be a better candidate.)

Practitioners of the secondary science often have reason to fear that their prestige and funding will also be reduced. My friends in zoology complain because their labs, library resources, and stockrooms are inferior to those of the molecular biologists. In chemistry, on the other hand, folks who synthesize and describe the properties of molecules may have less prestige than the atomic and nuclear chemists.

Again, there is no a priori reason why the secondary science should be less intellectually challenging or valuable than the primary science. And given the historical fact that there has never been a single successful reduction (in the classical Russellian sense), it is truly ironic and unfortunate that there should be so much historical basis for the paranoia of the scientists in the "secondary" field.

Methodological Reductionism: I have given a variety of reasons why reductionism may have unpleasant connotations: it is often associated with a positivistic epistemology, metaphysical chauvinism, or disciplinary imperialism. Furthermore, I have boldly conjectured (I invite the members of Committee I to provide refuting instances) that there has never been a successful case of reduction so far in the history of science.

Nevertheless, it could still be the case that trying to reduce one theory to another is the best way to make scientific progress. A teacher of mine used to point out that radical dualists are the people who should most welcome research into artificial intelligence and computer problem-solving. It is only by sincerely trying to simulate mind on a machine that we will discover what the differences are. And even though chemistry hasn't really been reduced to physics, still they are very intimately related sciences and it is only by trying to perform the reduction that these connections have been established. I have much sympathy with both of these arguments, but it is also important to point out that pursuing a reductionist program is not always the best methodology.

Let me illustrate this claim with a series of brief examples from the history of chemistry where progress was made either by ignoring the putative primary science (physics) or by developing theories which were inconsistent with the physics of the time.

A. First of all, it may not be at all clear at any given time which science is most likely to be primary. In Aristotle's system, the laws of motion were dependent on the chemical composition of bodies. Materials which were predominantly fire and air tended to go up. Conglomerates of the cold elements, earth and water, tended to go down. The natural motion of the celestial element (or quintessence) was circular. So in Aristotle's system, chemistry seemed to be at least as fundamental as physics.

Two thousand years later, in his textbook, Mendeleev speculated that the contemporary physical definition of mass might have to be replaced by a chemical one:

> "...in the future when chemistry shall have reached the age of mechanics..., some kind of compromise will be arrived at, and the quantity of a substance will be calculated in a manner quite different from the present one, although the conceptions of mass and atomic weight will be retained." (Principles of Chemistry, Vol. II. p.33)

Neither Aristotle's or Mendeleev's approaches survived in the long run, but both made important interim contributions.

B. Even when it is fairly clear which science is most likely to be primary, reductionist programs can be scientifically sterile. In a famous query to the Opticks, Newton laid down a reductionist program for chemistry--was it not true that matter was composed of hard little atoms which acted on each other via short-range forces? Given the success of Newtonian mechanics, it seemed reasonable to extend it to chemistry. Newton himself tried to reduce Boyle's Law (PV = const) to physics. He proposed a static model of gases in which adjacent particles repelled each other according to a 1/r force, but the force only acted on nearest neighbors. His hypothesis was completely ad hoc and untestable. There followed a hundred years of sterile reductionist model building. (See Thackray's account of the "nutshell" approach in Atoms and Powers. Also, Schofield's Materialism and Mechanism.)

Even experimental work based on the Newtonian paradigm was barren. Stephen Hales, in Vegetable Staticks, a work whose very title reflects his Newtonianism, performed destructive distillations of every imaginable natural substance and carefully weighed the products which he classified as tar, oils and ash. Alas, his quantitative work was all useless. Newtonian physiologists experienced similar failures.

There was much progress in chemistry during the 18th century, but it came not from the Newtonian program, but from chemists who were asking chemical questions and were proposing testable solutions to them which employed distinctively chemical concepts. I am thinking of Stahl's work on combustion and metallurgical reactions, the Swedish chemists' work on displacement reactions (e.g. iron displaces copper which displaces silver), Black and Priestly's discoveries of different "airs" (they are using a quasi-Aristotelian notion), etc.

Lavoisier, of course, revolutionized chemistry by the end of the 18th century with his oxygen theory of combustion. Since weighing products and reactants is such an important part of Lavoisier's methodology, it is sometimes claimed that Lavoisier's success comes from borrowing methods from physics. Well, this is just nonsense--we could just as well say that he borrowed the methods of bookkeepers or accountants.

The important thing to note is that Lavoisier's famous

Conservation Law is not just about the Conservation of Matter or Weight. He proposes that Elements are conserved. (On the Newtonian "nutshell" model, transmutation was possible.) Remember that for Lavoisier, elements can only be individuated by their chemical properties. There was no known way at that time of characterizing them in terms of physical parameters. The founding of modern chemistry came about, not by borrowing the concepts, theories, or methods from physics, but by treating problems in chemistry in an autonomous fashion.

C) In the 19th century, the relationship between chemistry and physics became much more intimate. Dalton characterized elements in terms of their atomic weights. It was very productive for organic chemists to visualize atoms in space--one recalls Kekule's benzene ring and Van't Hoff's tetrahedral carbon atom. The kinetic theory of gases at last provided a derivation of the various gas laws.

But lest we view this period as an unalloyed triumph of reductionist methodology, let me make three brief points about this era:

i) It was not the physics of Newton which chemists of the 19th century found especially useful. Rather, it was the theories of electrochemistry and thermodynamics which had been developed partly by chemists. Reduction certainly won't work if the primary science isn't rich enough.

ii) In working with molecular combinations of atoms, chemists found they had to introduce structural relationships which were quite different from anything encountered on the level of individual atoms. For example, Pasteur discovered left-handed and right-handed molecules whose biological properties are completely different. So we see that the secondary science may need to introduce structural or other macroproperties which, even if they can be formally defined in terms of the primary science, would never be dreamt of by scientists working only in the primary domain.

iii) Throughout this period, chemists kept using ideas which were formally inconsistent with the best theories available in physics. Consider valence. Why should carbon attract only four adjacent atoms and no more? This seemed contrary to everything physicists knew about forces. And even in the early period of Quantum Mechanics, physicists insisted on producing orbiting electron theories, such as the Bohr-Sommerfeld model, which may have accounted fairly well for the spectra of atoms but made absolutely no sense to chemists who knew that the electrons had to be localized if we were to understand valence and chemical bonding. Lewis and Langmuir proposed their static theory in full recognition that they were going against the best physics of their time.

G. N. Lewis says:

> "These two views [seem] to be quite incompatible, although it is the same atom that is being investigated by chemist and by physicist. If the electrons are to be regarded as taking an essential part in the process of binding atom to atom in the molecule, it [seems] impossible that they could be actuated by the simple laws of force, and traveling in the orbits required by the planetary theory. The permanence of atomic arrangements even in very complex molecules is one of the most striking of chemical phenomena. Isomers maintain their identity for years, often without the slightest appreciable transformation." (Valence and the Structure of

Atoms and Molecules, 1923, p.55.)

Eventually, of course, physicists extended and corrected the old quantum theory so that it could be applied to the phenomena Lewis describes. Once again we see that chemistry (and physics!) progressed because the chemists retaining a certain degree of autonomy.

From Reductionism to Inter-Theoretic Criticism: Agassi has argued that Boyle's Rule is dogmatic. (Boyle's Rule says that whenever theory and observation clash, it is always the theory which must give way.) In a similar vein, I have argued that reductionism, of either the epistemological, metaphysical or methodological variety is dogmatic. We can never know which science is primary and history tells us that whenever an inter-theoretic situation which vaguely approximates reduction occurs, the secondary science generally forces changes in the primary science at the same time that the primary science is correcting the lower-level laws.

What is essential for the growth of knowledge is not that we always attempt to reduce all sciences to one Ur-parent system, but that we constantly compare the results of various theoretical and metaphysical approaches. And when I speak of comparison, I do not mean the sort of pragmatic weighing of research program successes which Lakatos had in mind. I mean that we should actively look for logical inconsistencies between claims in disciplines which we may have assumed to be complementary, not competitors.

In my critique of reductionism, I praised G. N. Lewis for daring to develop a theory of chemical bonding which was inconsistent with the best physics of the time. But in so doing, I did not mean to laud (as would Feyerabend) the proliferation of pseudo-scientific proposals which contradict our best current science. First of all, Lewis' proposal had high empirical content--it was open to direct falsification. Secondly, both the chemists and the physicists were very aware of the inconsistency and made no attempt to cover it up. Instead, it was a pressing problem for all concerned.

This is not always the prevailing attitude. Sometimes, motivated in part by what I think is a legitimate reaction against reductionism, scientists tend to stiffle inter-theoretic criticism by dividing up the pie--"You do the external history of science, I'll do the internal...We do sociology of science so we needn't worry about the intellectual content...The best approach to crime is multidisciplinary, using economic, sociological, psychological and biological perspectives." Evolutionary biologists speak of proximate vs. ultimate explanations of inherited behavior as if they are quite independent. Psychologists distinguish various levels of behavioral analysis--micro, molar, etc.

But are the stories we're telling on each level or from each perspective, mutually consistent? If so, which one (or ones) is causally primary? Exactly how do they fit together? I would hate to see the imperialistic expansionism of the reductionist replaced by disciplinary Balkanization where inter-theory conflict is carefully avoided by playing different language games. Even worse is what we might call Californian methodology: "My theory's O.K.--your theory's O.K., and if our perspectives should happen to touch..."

Part II: Sacred Calves, Pigeon Responses, and the Rational Problem-Solving Approach.

In order to illustrate the benefits of actively comparing different theoretical approaches (and in an attempt to provoke and learn from the Popperian social scientists in Committee I!), I wish to juxtapose what I take to be Popper's general approach to the understanding of human behavior with B. F. Skinner's research program for psychology and Marvin Harris' attempts to explain cultural patterns in terms of their beneficial material consequences. I will proceed by first stressing the apparent conflict between these three views. On closer examination, however, it turns out that each of the three programs has positive aspects as well as obvious weaknesses. The paper ends with a call for a more sophisticated philosophical anthropology.

Popper's Approach to Social Science: Much of Popper's writing on social science is framed as a criticism of older political and social philosophies. However, in the course of arguing against unacceptable views, he does offer his own positive suggestions. These form the starting points for a distinctive approach to the understanding of human behavior. Other helpful hints can be found in his later works on evolutionary epistemology and World-3.

Let me list a few of these fruitful ideas: (I invite members of Committee I to criticize and improve upon this synthesis.)

1. Humans are problem-solving animals. (There is a formal similarity between the evolution of species through variation-and-selection and the trial-and-error methods we use in science and everyday life.)

2. Unlike other animals, humans can use language both to articulate theories and to criticize them. (This makes us much more efficient at problem-solving and permits us to solve more problems per lifetime.)

3. We inherit our ancestors' solutions to problems through World-1 transmission of genes and through World-3 transmission of ideas. These provide us with basic expectations which may be modified through experience.

4. The primary clue to understanding human behavior is the Rationality Principle (RP): People always act appropriately to their problem situations. To explain any behavior, no matter how bizarre, one describes the logic of the agent's situation <u>as s/he perceived it</u>.

5. Although the sense of <u>rational</u> which figures in the RP is a minimal one, Popper also at times recommends a stronger sense, at least as an heuristic. According to his Transference Principle (TP), what's true in logic is also true in psychology. Although the TP is invoked primarily in discussions of induction, it would presumably also warn us against too facile an adoption of ideological analyses. In logic our interest in a

proposition is irrelevant to its truth: perhaps the same is true for beliefs.

6. To understand the effects of a human action (as opposed to the reasons for it), we must look at the agent's overall objective problem-situation. Many of the bad things in society are the unintended consequences of our behavior, not the results of evil conspiracies.

7. Implicit in all the above is a methodological focus on the individual person (MI). Vague talk about class interests, the needs of society, or a pervasive Zeitgeist are at best shorthand statistical summaries of the goals, problems and theories of individual human beings. (British English furnishes a nice homey example of MI when one says "the Government are considering..." as contrasted with the American use of the singular tense.)

8. The ability of social science to predict the future is limited in a way which is qualitatively different from anything found in pure physical science (even those branches dealing with open systems evolving indeterministicly). Human activity is a result of World-3 problem-solving. Our problems and our best solutions are influenced by the theories of the world which we accept at the time. But our knowledge of the world is constantly changing. And we cannot predict today what will only first be thought of tomorrow. There is a kind of radical emergence in World-3 which I believe has no analogue in physical science. (Committee I members are invited to correct me.)

This concludes my attempt to condense several books and articles into an Eight-Fold Way, which I will dub the Rational

Problem-Solving Approach to Social Science. Now, let's look at two attempts to explain human behavior which either ignore or minimize the importance of the individual's beliefs about his or her problem situation. We'll start with Skinner.

Skinnerian Operant Conditioning: In Skinner's theory, one explains the present behavior of an animal by describing the history of rewards and punishments associated with similar behavior in the animal's past. Although Skinner has tried to extend his system to cover verbal behavior and intentional problem-solving, it clearly has the greatest appeal when applied either to lower animals or to cases in which higher animals are unaware of what they are doing. The success of animal trainers in getting animals to do tricks by a judicious schedule of rewards and punishments is well-known. Skinner claims that people's behavior can be modified in exactly the same way, especially if they don't know what you are doing. In his autobiography, Skinner claims to have modified Chomsky's hand gestures during a debate by smiling and nodding at appropriate times. Presumably, Chomsky did not know that he was gesticulating wildly and if he did, he certainly was unaware of the link to Skinner!

My purpose here is not to give a comprehensive evaluation of the success of Skinner's research program, but simply to contrast it with the Rational Problem-Solving Approach. We can roughly schematize Skinner's operant conditioning as happening in two big stages. (Each sub-process is numbered so it can be referred to later.)

Sequence at time I:

Organism O in stimulus situation S $\xrightarrow{(1)}$ (random exploration) O exhibits behaviors B, B', B", etc. $\xrightarrow{(2)}$ (causal process outside O) After B', O receives reinforcer R

(3) ↓ (causal process inside O)

O now in C, a changed neurophysiological state which cannot be observed directly.

Sequence at time 2:

O (in C) again in S $\xrightarrow{(4)}$ (propensity) O exhibits B' $\xrightarrow{(5)}$ (same as 2) O again receives R

As Skinner himself emphasizes, the parallels between his model and evolutionary theory are numerous. Consider process-1: Just as Darwinism gives no account of the causes of variation, so learning theory does not explain the repertoire of possible behaviors. In process-2, the response of the environment to the various behaviors (or genetic variations) is again governed by laws outside of our central theory. What both theories purport to do is explain the <u>increased</u> tendency for B' to occur in the second stage (or for an inherited trait to occur in the second generation).

In early Darwinian theory (before the discovery of chromosomes), the mode of transmission of the selected characteristic was totally unknown. Likewise, in Skinnerian theory. While I have alluded to a neuro-physiological change in process-3, Skinner himself would generally perfer to speak of the history of the organism without speculating on the details of the mechanism by which that experience is encoded.

Let us now contrast the Skinner scenario with a Rational Problem-Solving Approach. Whereas Skinner's model cannot even formally be applied unless the behavior in question is a repetition, the problem-solving model can--in principle--explain the very first occurrence of B. A rough sketch of such an explanation is as follows:

Individual I in Problem-Situation S	- - - → (thought process, may involve creativity)	I decides B may be the best way to get R	——→ (Causal, if reasons can be causes)	I does B

Because of its applicability to novel behavior, the Popperian model handles smoothly the cases of insightful behavior and learning by imitation which are troublesome for Skinner. One can, of course, add a second sequence in which individual I takes into account the actual outcome of the "experiment" of trying to get R through B. If B then becomes I's customary way of getting R in situation S, the observable differences between the Skinner account and the Popper account will be quite subtle.

Note that the RP approach cannot be applied if the individual is unaware of either S or B. For example, it cannot be used to explain why a teacher unknowingly begins to spend more time standing to the right of the lectern if the students conspire to smile and look more attentive when s/he happens to wander over there. Here Skinner's theory works best.

On the other hand, conditioning theory has difficulty explaining why the teacher scrupulously avoids the right side as soon as s/he is <u>informed</u> of what the students are doing. However, here the RP model works quite well. One is tempted to conclude that the two theories are complementary. However, I doubt if either Skinner or Popper would be pleased with this result. In particular, Skinner would probably try to account for the teacher's behavior after being told of the plot in terms of higher level rules (such as "Don't let people manipulate you") which have themselves been learned through operant conditioning. Popper, on the other hand, wants to extend his problem-solving approach to amoeba and sometimes seems to stress the importance of the objective problem situation. Both theories stress the importance of trial and error, both deal with individuals, both have some striking explanatory successes. Yet their conceptions of human nature are vastly different. Clearly a more comprehensive view which reconciles their differences needs to be worked out.

But let us now turn to a third approach, one which focuses on social patterns of behavior. Since Harris' work may be unfamiliar to some philosophers, I will introduce it by means of an example--the case of the poor little sacred calves.

<u>The Harris Explanation of Calf Mortality Rates</u>: I can think of no jollier introduction to anthropology than Marvin Harris' <u>Cultural</u>

Materialism. Whatever one may think of Harris' own research strategy, it is impossible not to take delight in his wicked attacks on common enemies, such as Marxists, sociobiologists, and functionalists. (The criticism of Levi-Strauss in a section called "The Raw, The Cooked, and The Half-baked" is especially wonderful.)

Very briefly, Harris' approach assumes that the basic problems which any society must solve are those of subsistence production and sexual reproduction. Furthermore, there are always problems of maintaining order and allocating resources. In analyzing any puzzling phenomenon, be it religious food taboos in New Guinea or the patterns of warfare in the Amazon, Harris' basic strategy is to see how the practice relates to cost-effective solutions to basic problems on what he calls the infra-structural level. In particular, he recommends that we take most seriously what people do; what people say about what they are doing (their theories of their problem-situations) is of little direct interest to Harris.

One of Harris' most debated analyses concerns the cost-effectiveness of the sacred cow taboo in India. In opposition to reformers who wanted to promote a beef-slaughter industry, Harris stressed the material benefits of the traditional system. I certainly do not wish to raise here this very complicated controversy over social policy. However, I would like to report one intriguing side issue which arose out of the extensive livestock censuses which were taken.

In most of India, oxen (male cattle) outnumber cows (females) by ratios ranging from 100 to 70 all the way down to 100 to 47 in the northern plains. (Cows, Pigs, Wars and Witches, pp.22-23.) The birth ratios are approximately equal, but fewer females reach maturity. However, in a certain district in Kerala, a state in southern India, the ratios are reversed. Female calves outnumber male calves by a ratio of 100 to 67. (Cultural Materialism, p.35.) When asked about the differential mortality rates, the Kerala farmers said that male calves were weaker, got sick more often, ate less. They all insisted that they would never deliberately shorten the life of any cattle and ardently affirmed the Hindu prohibition against doing so. (Cultural Materialism, p.32-33.)

How then are we to understand these skewed mortality rates? Is there a strange sex-linked disease which attacks males in Kerala and another one which debilitates females elsewhere?

Harris, of course, looks at the different material conditions in the districts concerned. In India, cattle provide dung (for fuel), milk, and traction (for plowing). In Kerala's agricultural system, there is little demand for plow oxen and the males obligingly die off. In other districts of India, however, breaking up the hard, thin soil is the top priority, so milk cows become less valuable, and as if by an invisible hand, up to twice as many female babies die.

Harris' proposal is certainly testable. One predicts that the introduction of tractors (keeping other variables such as the need for cows' milk constant) would tend to increase male bovicide. It also follows from his theory that given limited resources for the production of cattle, any alternative source of fuel, milk, and traction will place female calves in jeopardy because that is the easiest way to control population growth. And sure enough, according to Harris, in the cool plains of northern India where it is more feasible to produce water buffalo, the ratio of males to females reaches a maximum value. Yet this area, the headwaters of the sacred Ganges, is also a religious center for Hindus.

Harris' account seems to show that at least in this case, the agents' perceived problem situations and their more-or-less appropriate responses to them are quite irrelevant to the real explanation of their behavior.

<u>A Response from the Rational Problem-solving Theorist</u>: I think the common-sense reaction to Harris' story is also the one sanctioned by Popper's methodology: If all of these calves are being killed (no matter how subtle the means), don't the farmers actually know on some level what's going on? This example in no way eludes the Popperian approach if we hypothesize as follows:

Farmers in Kerala are in the following problem-situation: they have too many baby calves. Furthermore, milk cows are more valuable than draft animals. However, there is a religious taboo against killing cattle. The best solution to this multi-faceted problem is the following: Cleverly manipulate the situation so that male calves are disadvantaged. Then, if they should happen to die, no one (neither God nor the neighbors) can blame you--you didn't kill them. And in no case does it make sense to tell a visiting anthropologist what really goes on.

This hypothesis is also testable and, as a matter of fact, Harris himself reports some data which supports part of the proposed scenario. In Kerala, a couple of farmers under questioning "...suggested that the male calves ate less because they were not permitted to stay at the mother's teats for more than a few seconds" (Cultural Materialism, p.33), while in other cases "a triangular wooden yoke is placed about the unwanted calves necks so when they try to nurse they jab the cow's udder and get kicked to death." (Cows, p.23)

The Rationality Approach seems vindicated. By trying to fill in the details of the way the farmers perceive their problem situation, we learn more interesting things about them. In fact, we are now faced with a new question: How do the villagers reconcile their religion with their quite blatant bovicide? In solving this problem, we will probably learn a lot about villager theology, ethics, and maybe even their theories of animal husbandry.

However, a follower of Harris, it seems to me, might very well remonstrate at this point. Some social scientists are undoubtedly fascinated by the exploration of the webs of self-deception and mystification which people weave, but these thought systems are mostly ephemeral and without much real effect. Note that the villagers kill the calves despite the taboo. And their methods of killing them are so unsubtle that one can hardly find influence of the taboo in their choice. It's mainly their verbal behavior which is influenced by their belief system.

Furthermore, there are many other parallel cases where the people concerned either aren't acting deliberately or aren't even aware of the effects of their behavior. In such cases, Harris might argue, a rational problem-solving approach cannot work; yet the cultural materialist can offer the exact same sort of explanatory analysis.

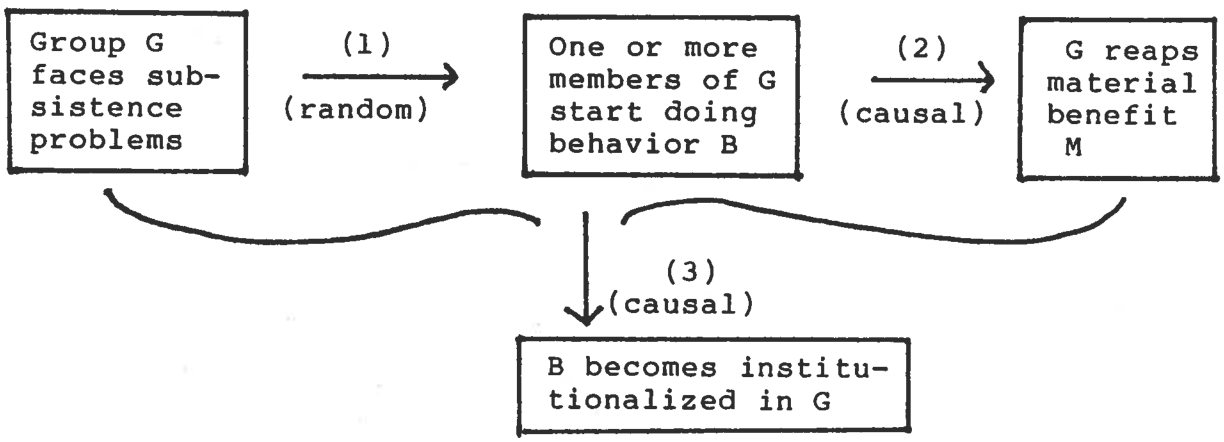
An example is the case where infants sleeping with their mother accidentally die. Demographic studies show that female child morbidity rates go up in castes or classes where dowries are high and nothing is to be gained by daughters marrying "down." The explanation does <u>not</u> assume that the infanticide is deliberate.

Harris, of course, is not the only social scientist to employ what Merton called <u>latent functional explanations</u>. One must not confuse what philosophers call<u>functional explanations</u> with the functionalism of a Parsons or Malinowski. Perhaps a better name for this form of explanation is Skinner's term, <u>selection by consequences</u>." When the people involved do not recognize the causal connection between a certain pattern of behavior and its beneficial effect, one speaks of a latent function.

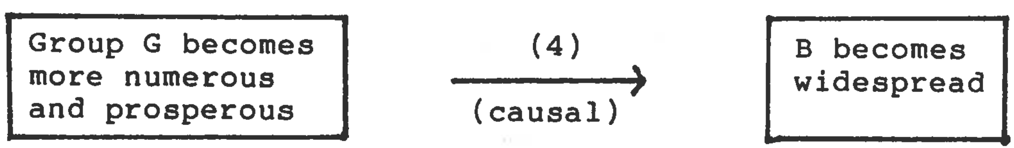
One can even try to provide a functionalist explanation of mythological systems by showing how they increase the material well-being of the folks who hold them. By definition, any <u>latent</u> functional explanation has nothing to do with intentional problem-solving or World-3. How then do they fit in with the Popperian approach described above? Let us look at this type of explanation in more detail. I will concentrate on Harris, but my proposed critique applies to all attempts to explain by latent functions.

An Analysis of Harris' Explanatory Schema: Not all of Harris' explanations involve group selection, but some do so I will present his model that way. It can easily be adapted to the case where a single individual makes the innovation which is then picked up by the entire group.

Sequence at time 1:



Sequence at time 2:



Let us now look in more detail at latent functional explanations. Harris' scheme is not limited to cases in which the members of G are unaware of the connection between behavior B and material outcome M, but in many of his most dramatic explanations, especially those of taboos, the beneficial consequences are not recognized.

Thus, the sacred cow taboo has the unintended and unrecognized consequence of preserving breeding stock of draft and milk animals during times of famine. The pork taboo in the Middle East at one time encouraged populations to remain nomadic. (Pigs, though wonderful to eat, would tie people to a very limited part of the countryside.) And the institutionalized violence and female infanticide of the Yanomamo contributes indirectly to the maintenance of their tapir supply. (The connections in this case are so complex that neither the Yanomamo nor most anthropologists can sort them out!)

In each example, it is argued that the group benefits from the puzzling institution and this fact is supposed to play a key role in the explanation of why the behavior originally became institutionalized. (Harris recognizes, of course, that the food practices of New York Jews or London Hindis require other explanations.)

Like Skinner, he does not provide a detailed mechanism for the transmission of the acquired behavior pattern, and like both Skinner and Darwin, he gives no story at all about how the behavior arose the first time. However, I wish to insist that we raise both the problem of origins and the problem of transmission. I want to argue that in some cases at least there is no plausible hypothesis (at least I can't think of any) which would permit these processes to occur in the way which they must for a latent functionalist account to work. My line of argument is similar to those critics of Darwin who argued that it was impossible that the eye could have arisen by natural selection because in its primitive stages it would have conferred no reproductive advantage. Those critics turned out to have been factually mistaken, but the <u>form</u> of their attack was appropriate.

I think my point is easiest to see in the case of Kongi. (I have used a simplified, somewhat artificial, example. Most of Harris' favorite examples are more complicated <u>but</u> I think the same objection can be raised there.) The Kongi domesticate sows, but keep no boars. Every spring their pigs go for a ritual run in the forest where they presumably mate with wild boars. The Kongi, however, know nothing about reproduction, swine or human, and they explain the ritual as offering the pigs to the gods, who luckily generally send them back. The latent functionalist would say that the practice (and indirectly, the myth) is to be explained in terms of the material benefits of having the sows fertilized.

Now, let's speculate about how the ritual got stamped in. Perhaps one spring by accident one woman's sow got loose in the forest. <u>We</u> know that the ten squealing pigs which arrived some weeks later were a direct result. But the woman doesn't know this. So why should she ever let the sow out again? It can't arise from Skinnerian conditioning for the reward does not follow closely on the bahavior. In fact, the short-term effects of her behavior are negative--she has to go chase the lost sow.

But let's hypothesize that purely by accident, on her way to find the sow she has a religious vision and finds an especially big mushroom and decides God likes playing with pigs once in a while. We have now explained why the woman will turn her sow out, but our explanation has nothing to do with the fertilization of the sow--with the latent function of the practice!

Let us now speculate on how the group comes to adopt the woman's belief. Perhaps they too turn out their pigs and also find heavenly mushrooms--in which case, again, the latent function is irrelevant. Or maybe they notice that the woman is particularly successful at pig husbandry and so they copy all her pig practices. But in this case, they do realize there issome causal connection between turning sows out and raising lots of baby piggies, although their theory is different from ours. It might go as follows:

> How can one be a successful pig farmer?
>
> You have to keep the gods happy.
>
> How do you keep the gods happy?
>
> Well, you let them play with the sows every Spring, you never kill a sow (only a boar), and you give them a food offering whenever you butcher a boar, you do pig dances every winter, etc. How do you know whether the gods are pleased? Well, if they are displeased, the crops fail, we have storms, and *there are no baby pigs in the spring* [my italics].

But in this scenario, the function is obviously not *completely* hidden from the people although their understanding of the connection is obviously not completely correct. Again, the best explanation of the practice would include the agents' theories about what's going on.

There are two other types of scenario, but I think they can be dismissed quickly. The first hypothesizes Darwinian a natural selection. Kongi who release their sows in the forest every spring can plausibly be said to increase their reproductive fitness because they have a more reliable food source. However, it seems unlikely that there is an inherited tendency to turn pigs loose. Also, biological evolution is much too slow a process to account for most cultural changes.

The second postulates cultural evolution through group selection Perhaps by sheer accident, plus the well-known human proclivity towards superstition, the Kongi as a group happen to adopt the institution of spring sow runs while other tribes who are competing with them don't. The Kongi prosper while their rivals are all killed off, chased away, or assimilated. There is nothing impossible about such a scenario, but I wonder how widely applicable it is. First of all, how often does it happen that rival groups differ on only one (or a small number of) cultural parameters? To the extent that the Kongi are victorious not only because of pig fertility, but also because they historically had the richest hunting grounds and the best water supply, our explanation is diluted:

> Why do the Kongi have ritual pig runs?
>
> Because they fell into doing it and it led to pig prosperity which led to their cultural supremacy in the region. (Of course other contributing factors were their rich lands and good water supply.)

However, the main objection I have to this scenario is that it provides no mechanism for subtle changes in the institution under study. How, for example, could we use this kind of gross cultural evolutionary scenario to explain the differences in behavior between farmers in Kerala and farmers in the

Himalayan plains? Must there be cultural conflict between rigidly-held ideologies in order for group selection to take place?

My argument can be summarized as follows. There seem to be only four ways in which the fact the B leads to M can cause B to become habitual:

1. One cognizes the connection (cf. Popper).

2. Skinnerian conditioning (works only if B leads quickly to M).

3. Darwinian selection (works only if B is genetically linked and M increases reproductive fitness; requires many generations).

4. Cultural evolution through group selection (works only if B is part of the group definition and M increases cultural survival; requires group-group competition.

In <u>latent</u> functional accounts, by definition the first mechanism (rational analysis of the effects of one's behavior) is ruled out. My guess, however, is that further probing of actual cases would reveal that the natives <u>do</u> theorize that there is some connection between B and M although their theory may differ from ours. Andin cases where Skinnerian conditioning, Darwinian inheritance, and cultural group selection are inoperative, without this cognitive mechanism, Harris' explanatory account won't work.

My conclusion is stronger than I anticipated: namely, that there are no latent functional explanations! (I am eager to get your reaction to this.) However, perhaps with hindsight, this conclusion is not so surprising and is to be welcomed. Maybe latent functions are too easy to find.

Consider the following somewhat parallel case: Many people love to collect examples of successful folk medicine, ranging from rauwolfia, now known to contain reserpine, to herbal teas some of which contain traces of useful drugs. It is sometimes claimed that although old-timers may have been in the dark about the exact nature of the beneficial effects of these home remedies, nevertheless, through trial and error they managed to select materials which by and large are efficacious. I am quite prepared to grant that folk medicinals are unlikely to be virulent poisons (though a surprising nunmber, e.g., penny royal are mildly dangerous). What I am skeptical about is the claim that the explanation of why these remedies were adopted is their latent medicinal function. My bet is that even in the cases where they are functional, the real explanation of their popularity lies in their soothing taste or the sheer blind force of tradition. Again, my reason is that when the good effects are subtle and not immediate, there exists no mechanism by which the practice can be selected.

CONCLUDING REMARKS

I have not attempted to give a complete analysis of the relationships between Popper's, Skinner's and Harris' approaches to the understanding of human behavior. But even our partial preliminary comparison illustrates some of the general methodological morals which I argued for in Part I.

It would not be productive to try to force any pair of these theories into a reduction relation--even an approximate one. Functionalism, operant conditioning, rational problem-solving--each have distinctive heuristic principles which guide our searches for explanation; each have paradigm cases in which they seem to work naturally. On the other hand, it would not be wise to try to divide human behavior into little intellectual fiefs. We must not simply say, "Let the Skinnerians have unthinking or habitual low-level behavior, and Popper have high-level decision making, and give Harris general cultural mores."

By actively comparing the three theories, we discover whether they overlap (in which case they must either be inconsistent or partially redundant). In particular, by raising a question external to Harris' research program, namely the problem of whether an institution whose primary function is latent could ever get started, we arrive at an interesting criticism of Harris' approach. Although he would like to exclude the beliefs of people from the explanation of their behavior, it seems that he cannot.

As to the rational problem-solving approach, in this paper I have presented the Eight-Fold Way as a research strategy for the explanation of all human behavior. Popper himself recognizes that it covers neither habitual or "automatic pilot" behavior (riding a bicycle without thinking about how to do it) nor irrational perseverative behavior (trying to open a door with a key we know doesn't fit). We should perhaps add to the list certain kinds of "blind" Skinnerian conditioning.

I also think there is an important line of thought in Popper's work which goes beyond the approach outlined above. I am thinking of his emphasis on <u>objective</u> problem situations when discussing the history of science and his stress on the objective existence of institutions. Perhaps further development along these lines would narrow the gap between him and Harris. In any case, comparing the two approaches seems to be a productive exercise.