

express in action certain value judgements. How, then, could a social policy not 'tend to support' the valuations that it seeks to carry out?

Again, every empirical theory presupposes the truth of some empirical propositions associated with it. In Taylor's example drawn from Lipset a basic claim is this: any complex society with peaceful constitutional change of elected officials by universal suffrage is one that requires group conflict of a contained and rule-guided sort. Now this claim may be false. But if we accept it as true, and also accept that the claim describes a defining feature of all democratic societies, does it follow, as Taylor suggests, that we must value democracies more highly than all other forms of government? Obviously not, for we may dislike and disapprove of societies with peaceful constitutional change, universal suffrage, and an absence of violent conflict between groups. So mere acceptance of this empirical claim does not in itself tend to support the value judgement that democracy is the good society. Of course, the truth of the claim does rule out many other claims, and in that way does narrow the range of choices from which we can draw our preference. But is this all that Taylor has in mind when arguing that theories in political science tend to support an associated value-position, and therefore such theories are not value-neutral? If so, then the same holds true of every empirical proposition—a thesis that has never been in previous dispute and is unlikely to be in the future. Proponents of the value-neutrality of science, whether natural or social, will be happy enough to welcome that outcome.

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Changing Order: Replication and Induction in Scientific Practice. By H. M. COLLINS. London, Beverly Hills, and New Delhi: Sage Publications, 1985. Pp. viii + 187. \$45.00 (cloth), \$18.95 (paper).

Harry Collins examines the moment when knowledge emerges within the scientific community: when phenomena come into shared human perception, when one person claims to see something and then others try to see it. As a number of observers of science have commented, replication is at the core of the empirical enterprise, for in replication the private chimera becomes the communal fact. Some, although not Collins, would even argue that replication offers the primary protection against error and fraud. Collins finds, rather, that experimentation is so embedded in forms of life that compelling experimental results are compelling only to those who have already entered into the form of life which generates the result.

As many have observed, replication is no simple thing, with every experiment repeated exactly an officially determined number of times, a scorecard published and a certificate of communal validation granted, giving license to operate as a fact on the highways of knowledge. At every point there is question. As Collins points out, only few experiments are subject to rigorous replication attempts. No replication can be exact; even an exact replication in itself would offer no strong confirmation, for it would generate the same artifacts as the original. Replication requires craft skill not communicated in the published account and best carried by individuals who have already competently performed the experiment. Yet there are no external criteria for competent execution of the trial, for the competence of the trial is only determined by getting the correct results, which is what the experiment set out to resolve. If two experimenters disagree over results, they accuse each other of incompetent

experimentation—that the other got the wrong results. But until third parties choose up which results they believe, they have no way of knowing whether any of the experiments are competently done. This conundrum Collins calls the experimenter's regress. As he says, 'Where there is disagreement about what counts as a competently performed experiment, the ensuing debate is coextensive with the debate about what the proper outcome of the experiment is. The closure of debate about the meaning of competence is the "discovery" or "non-discovery" of a new phenomenon' (p. 130).

Collins explores the phenomena of experimenter's regress and the structuring of debate around it through retelling of a number of his previously published case studies. An account of a physicists' difficulties in constructing functioning TEA (Transversely Excited Atmospheric pressure) lasers establishes the problems of replicating results within normal science (where no one questions what the results of the experiment should be). The main morals of the story are that tacit skill knowledge is necessary to make the experiment work and that the criterion for the experiment working is the production of expected results. An account of a dispute over experiments attempting to measure gravity waves examines the coextensiveness of debate over competence and debate over results. After consensus is achieved on what the results should be, the issue of which experiments were properly and improperly done is resolved. The competence debate vanishes, for the experiments that produced the wrong results were clearly improperly done. The last case Collins examines, that of replication of parapsychology experiments, is the radical case, where large gulfs between the forms of life embodied in the two different social networks carrying out the experiments allow no resolution over what proper results are. Competence debates between the two groups remain unresolved and serve to discount the results of the opposing group.

Because competence debates vanish so rapidly once communal agreement over the phenomena is reached, and thus the alternate forms of life evaporate, we are left with the impression of a stable empirically perceived universe. This vanishing act, Collins argues, keeps the evanescence of our accounts of nature hidden from all of us, except those scientists who have engaged in an intense replication controversy. Most of us live in a world where empirical results seem unproblematic and concrete in their meaning. Thus we create order.

Collins repeatedly uses the image of ships in bottles to describe our scientific knowledge. Our constructions of nature are artifices encapsulated in particular life forms. But the bottle and the artifice are usually invisible to us, because we do not see the artisan's craft. Exposing how certain perceived events became communally accepted as replicable facts, how new order is created around these new facts, helps us see the craft of natural fact making.

In an act of reflexive consistency, Collins sees the phenomena he is trying to establish in this book as also embedded in a form of life, a bottle—the Empirical Program of Relativism. The book is framed (as many books are) by theoretical chapters, which argue that order is a socially produced phenomenon, having little or nothing to do with the natural world that surrounds us. Although he does not discount the existence of nature, Collins argues we must view our accounts of nature as though they 'were about imaginary objects' (p. 16). Nature, to Collins, does not constrain what we properly can say about it, nor by extension what will be the replicable result of an experiment, nor what can be counted as a competent experiment. Thus he has no difficulty in constructing counterfactual histories of science, assuming the losing side in the debate won and all our experience of nature were adjusted accordingly.

The first chapter is a general undergraduate introduction (more properly indoctrination) into relativism and cultural systems. The position is urged, not argued, and the exposition assumes that relativism is mind-boggling news to long deluded victims of positivism. The second and sixth chapters are more subtly argued and more germane to the actual phenomena he examines in the substantive chapters. These two chapters have some interesting observations worth attending to, but their main thrust is that the phenomena he examines are not intelligible outside the frame of his theoretical bottle, and that within that frame they establish the validity of the bottle.

This reader, however, remains sceptical about how total and indissoluble the link between bottle and ship is. That goes both for his general claim and for my response to this particular book. That is, based on my reading of the text and familiarity with similar cases, I am quite willing for the time being to accept his account of events, the importance of tacit skill knowledge, the original construction of experiments within forms of life, and the connection between competence debates and phenomena debates. He here has identified potentially replicable phenomena. Yet I am not inclined to accept that phenomena cannot travel beyond the form of scientific life within which it was identified, nor that replicability does not depend on ambient nature. Ships, although built in protected environments, travel wider seas. Some float, some sink. Whether they follow their plotted courses depends on how well they master wind and wave.

My difficulty with his metaphor of ships and bottles is fundamental. A ship in a bottle is looked at from a distance, and not used. Once crafted by skilled hands, it sits on a shelf to be admired, or to gather dust. Such imagery leads to an encyclopedia view of knowledge. Once knowledge is created, it sits quietly in a book, to be looked up by the curious, to be praised by teachers, and to be forgotten by most of us. It is too special to enter into our everyday world and too frail for us to be constantly putting our clumsy hands on it. No doubt there are certain arcane phenomena, unrelated to other phenomena, unuseful, and uninteresting that are allowed to remain in the theoretical and experimental bottles in which they were first constructed.

But a large number of phenomena once they are brought into our view by experimental artisans begin rather active lives. We cannot leave the poor things alone. We want to use them for other kinds of experiments, to investigate secondary or different phenomena; we want to shoot them at other things, and shoot other things through them; we want to turn them into laboratory hardware; we want to forecast the weather with them, and build television sets. We don't just look at the ships, we sail them. If a phenomenon over a period of time proves less than robustly replicable under a great variety of natural conditions and theoretical assumptions, we will start to have questions. Yet if the phenomenon survives these rough seas we have good reason to accept the phenomenon, even if the bottle in which it was originally constructed has long ago been declared unsound and obsolete, and even though we now call the ship by a new name. Successful phenomena work in consonance with nature. They float. In this way we regularly cut through the experimenter's regress.

This is more than an ideological difference, that I don't happen to care for Collins' bottle even though I will most certainly use his ship (although by his reasoning I should have to throw out the ship with the bottle). I believe his particular bottle makes him see his ship much smaller and more isolated than it really is. In his model, scientific knowledge begins and ends with the identification of phenomena, and rests entirely within belief. Questions concerning the interaction of belief and experience are ruled out of order by the synonymy of

experience and belief. Questions concerning the continuity, elaboration, and transformation of phenomena are similarly off bounds because our perceptions of phenomena are contained within their separate bottles. And Collins creates hardships for people trying to construct bottles, by giving them no way to identify plausible arguments that stretch current beliefs without breaking an unacceptable amount of glass. All he can say is that it 'is hard work' (p. 149). Yet if he would allow his own observations out of his bottle, he might consider how plausible and compelling arguments make an ally of nature. And he might see how the construction of experiments and the structure of replication debates increase our stock of ordered experience of the natural world, even within our socially constructed orders.

An accounting of how science develops robustly useful knowledge fits within Collins' commitments expressed near the end of the book. In the postscript he decries the loss in confidence in the scientific enterprise as 'a disaster we cannot afford. For all its fallibility, science is the best institution for generating knowledge about the natural world we have' (p. 165). Unfortunately, nothing within his analysis gives guidance as to why science is any good for knowing about the natural world, let alone better than any other institution. Quite to the contrary, he argues for socially induced arbitrariness—an arbitrariness that gives an unsavory character of social conspiracy to all those who foster scientific culture (see, for example, p. 161). His bottle, as he constructs it, can only serve to encourage the loss of confidence he decries. Yet if he would set his ship to sail more broadly, let the phenomena he observes seek their potential power, I believe he can help us find the way to an account of how fallible human consciousness uses evanescent symbols to construct a guide for living in the world. What needs to be explained is not how we humans are fallible, self-interested, limited in vision, blinded by beliefs. That is an ancient story. What needs to be explained is how we have organized our frailties to gain a small mastery over the waves and winds that toss about here.

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Hegel's Philosophy of Spirit. Edited by PETER G. STILLMAN. Albany, N.Y.: SUNY Press, 1987. Pp. xii + 233. \$12.95 (paper, U.S.), \$39.50 (cloth, U.S.).

This volume contains the papers given at the Eighth Biennial Meeting of the Hegel Society of America (Russell Sage College, October 1984), together with the views of the commentators. The topic for that meeting was the third part of Hegel's *Encyclopedia*. The papers naturally vary quite a bit in style and approach, so that no reader could find them all equally interesting or illuminating. I shall try to indicate briefly what the principal thesis of each contributor was; and then comment upon most of them as constructively as I can. But I want to say, at once, that the general standard is very high, and that the editor is to be congratulated upon his achievement as programme chairman.

The opening paper by R. R. Williams on 'Hegel's Concept of *Geist*' emphasizes the intersubjective character of the category of 'Spirit'—which is at once absolutely primitive and absolutely comprehensive—in Hegel's thought. He traces and illuminates the role of 'recognition' in its genesis, insisting that *Geist* cannot be interpreted satisfactorily in terms of the Kantian 'transcendental-foundational model'. The issue is too complex to be settled in a short essay; but it is certainly wise to *begin* with the differences and the contrast. I find myself