

Why “Unobservables” Cannot Save General Theory: A Reply to Mahoney*

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In James Mahoney’s article, “Revisiting General Theory in Historical Sociology,” published in *Social Forces*, volume 83, number 2 (pp. 459-90), certain foundational claims are made, including these, all of them knowingly inflammatory:

The aspect of a general theory that is general is its use of an abstract causal mechanism that exists outside space and time. (Mahoney 2004b:459)

A general theory is a postulate about a foundational cause that features two components: a causal agent and a causal mechanism. (Mahoney 2004b:459)

These mechanisms are empirically underspecified, exist outside specific spatial and temporal boundaries, and cannot be directly observed . . . they are original movers or “ultimate causes” . . . For example, instrumental rationality — the causal mechanism of rational choice theory — is an unobservable property devoid of precise empirical content and specific time/place referents . . . The positing of omnitemporal and unobserved mechanisms that serve as primitive causes has been discussed primarily in the natural sciences [In the penultimate version of the article, Mahoney’s supporting citations included the following in note 2: “A lesson from the natural sciences is that one should not evaluate general theories based on the empirical plausibility of their causal mechanisms” (Mahoney 2004a).], but the same practice applies to the social sciences. Taken together, a causal agent and a causal mechanism represent the “hard core” of a general theory, or that part of the theory that is shared by all scholars who use it. This hard core is not usually directly tested. . . . a general theory is not itself a testable hypothesis. (Mahoney 2004b:459–60)

Resources [in “power theory”] are potentially unobservable bundles of ideas and materials. (Mahoney 2004b:461)

When a general theory is used, the starting postulate assumes a given causal agent and mechanism. Since the causal mechanism refers to an abstract property,

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subsequent postulates are “bridging assumptions” that add empirical content to this mechanism (Hempel 1966:77-82). (Mahoney 2004b:464)

Insofar as one is committed to scientific realism (which is the case for most proponents of general theory, including mid-twentieth-century philosophers such as Karl Popper, Carl Hempel, and Ernest Nagel), one treats all postulates — even those that make reference to unobservable entities — *as reflecting an independent reality* (emphases added). (Mahoney 2004b:482)

Most tellingly, in a paragraph of the penultimate version of the article (dated March 10, 2004) which was deleted in the final iteration, perhaps in response to my already written critique of it, these remarks appeared in order to summarize the importance of “outcome explanation”:

Scholars using this strategy test the postulates because they contain the hypothesized explanation for the outcome. Notably, however, the initial postulate refers to an abstract causal mechanism that cannot itself be tested . . . In the end, nevertheless, the analyst must make a *leap of faith* in the existence of the causal mechanism, since this beginning postulate is never directly tested, and since the final proposition (i.e., the outcome of interest) is also not tested. (Mahoney 2004a:16, emphases added)

Thomas Aquinas would have appreciated these remarks, as would any of his brighter, scholastic colleagues in the thirteenth century. In fact, they might well have written them, since they were “realists” and not “nominalists” (even without the tutelage of the twentieth century “neorealists,” G.E. Moore, Bertrand Russell, Samuel Alexander, et al.). There is a touchingly medieval quality to the statements quoted above, and even though I cannot debate each point here, they do invite oblique examination.

Whenever I read an article of this general type, certain images come to mind, as if I had been asked to write a screenplay which might capture its unintended meaning. To wit: while their dedicated, officious assistants flutter about, I picture James Mason (playing Karl Popper) and Rod Serling (Carl Hempel) — both of whom serve as ceremonial godfathers for the article’s main thrust — gently lording over a summer conference at some *gemütliches* chalet in Austria around 1960. Darker-skinned guestworkers from Italy or Spain serve cocoa and biscotti to a rapt, appreciative crowd of well-groomed, mainly American and British graduate students. It is clear that all of them are tickled pink to have been invited because they are unified in their hearts by a single orchestrating hope, one that banishes all other, lesser desires from mind: to become Real Scientists; to master the Scientific Method, the Hypothetico-Deductive Model, to find the keys to the kingdom of Absolutely Falsifiable Near-Certainty along the lines that Hempel and Sir Karl had made abundantly clear in their writings over the preceding 30 years. Like Marx or Durkheim, they want to advance “scientific” knowledge; unlike them, they care more for

formal logic than for theorizing the messiness of human experience on its own terms.

Yet the only problem, and one which troubles them deeply when they are alone, is that they are not “real” scientists at all, not physicists or astro-geologists, nor even entomologists. Instead, they are sociologists and political scientists (dismissed from the true scientific brotherhood even by their supercilious colleagues, the post-Jevonian economists). And although their data sets are full of embarrassing holes, and while “agency,” “consciousness,” “collective behavior,” and all manner of other troubling characteristics of humankind interfere with their aspirations to “do real science” under the covering-law model of proof — their *explanandum* never quite living up to their *explanans* — they nevertheless put on the cheeriest faces of juvenile hope, adjust their Thinking Caps, and await Popper’s and Hempel’s directives on the *Autobahn* to the truths that only Big Science can deliver. To breathe the giddy air of the high Alps in such company is perhaps enough of a reward, even without learning precisely how to carry out social “science” when back at the sea-level homesite. Still, pursuing “deductive-nomological explanations” (Hempel 1966:51) is hard work, and they, NSF postdoctoral grants in hand, are up to the task. Fade to black. These cinematic imaginings aside, the article indeed poses significant questions concerning sociology’s heart-breaking quest to become a science, and on that basis alone deserves a response.

In view of this Alpine fantasy, perhaps I am not the right person to comment on this earnest piece, and should have passed it to someone equally keen on proving (yet again) that there is indeed, and contrary to experience, a “general theory” that could be fruitfully fitted to the social world of humans. Yet it does deal with some books I have read and thought about, in an area of work, “historical sociology” (particularly as practiced by Weber), with which I am not unfamiliar, so curiosity swept away my scruples. And the article is not without a certain value, even if not in the way the author intended. I have studied its arguments in order to find out if the author could actually demonstrate anything new or different or enlightening about the four major books and several articles examined. It holds, for example, that if one converts Rueschmeyer, Stephens and Stephens (1992), “formally speaking,” as the author puts it, into a formula (“using a straightforward additive rule for aggregation”) of $Y = \Sigma P w, r, m, b, \text{ and } l$, (where Y is the presence or absence of democracy, P = power, w = working class, r = rural peasants, m = middle class, b = bourgeoisie, and l = landed elites), and if one then works backward, supplying constants for each variable in some proportion that approximates the historical record of social changes in, say, Sweden and Italy, then, *mirabile dictu*, the “theory” “works.” (How well it would work in, say, Iraq today, is another matter entirely.)

Why exactly this “formalization” or “modeling” is held to improve on the book as it was published, or on social science as currently practiced, I cannot imagine — particularly since the burden of the book is to show that quantitative comparative research is *methodologically* inferior to historical case studies. What does occur

in plain view is the unseemly disembowelment and trivialization of a book in which the authors took considerable pains to “get it right” historically, and onto which are tacked a set of generalizations meant to illuminate or clarify the original discursive text. Similarly sacrificed on the altar of scientism are Skocpol’s famous dissertation (1979), Brustein’s analysis of Nazism, and Gerard Alexander’s consideration of Spanish democratization. All become grist for the “formal respecification” mill that is the *raison d’être* of the article. Of course, this sort of maneuver has been carried out before, with great sophistication, but with equally fruitless results. In 1971, the statistician Robert H. Somers subjected the very book that inspired Skocpol, Barrington Moore’s *Social Origins of Dictatorship and Democracy*, to an extended “formalization” (Somers 1971:357-420). With no inconsiderable subtlety, he tried to bring Moore’s best-selling historical monograph into the ken of statistical reasoning, beginning with this pivotal observation:

Moore is primarily a historian [?], and an analysis of his work offers a great challenge for one who views it from the relatively distant perspective of statistical and quantitative analysis. An assessment of the larger methodological issues that Moore’s study raises . . . leads to problems that are not easily solved by the conventional canons of quantitative methodology — problems that, indeed, raise questions about the very meaning of scientific work in this domain. . . It is quite possible that some of these problems have no solution. A work such as Moore’s remains, in many respects, art rather than science. While I have nothing against art, I believe there is merit in considering whether historical analysis, especially when it is intended to help man make more rational choices in the pursuit of desirable goals, cannot be made a basis for the development of knowledge that is more reliable, consensual, and systematic than artistic insights ordinarily are. (Somers 1971:358)

Without belaboring the obvious, and with the comfort of hindsight, perhaps it is enough to observe that Moore’s book has become absolutely canonical in comparative-historical sociology and political science, and its very “artfulness” is what has caused it to be read and emulated continuously since it was published nearly 40 years ago. The same cannot be said for Somers’s earnest, detailed reconstruction of Moore’s argument along positivist lines.

Sad to say, to document even minimally what I regard as the corrosive diminution of these important works in historical sociology would require far more pages than I have been allocated, so another tack must be taken if the article’s true character is to be revealed. This inadvertently demonstrates both the great charm and the great danger of reductive equations: they are at once curt, parsimonious, yet substantively empty and potentially distorting.

Rather than arguing over what is lost or gained by formalizing certain exemplars of historical sociology (e.g., the very historical details which give the books their strength), a quick appraisal of the author’s epistemological position might yield a larger payoff. This is because if the epistemology or “logic” of the favored approach is faulty, the edifice built upon it naturally caves in — a point with which the author

would surely concur. By way of beginning, consider this: Why is it that general theorists very seldom feel the need to disassemble the ordinary quantitatively inspired journal article — so-called “mainstream sociology” — to see if there are any ideas in such texts that could be restated in a style more in keeping with discursive theory, and therefore demystified? I ask this rhetorically, of course, since the paucity of “big ideas” in such publications is as much an accepted feature of everyday sociology as a lack of equations is standard within the typical theory piece. Not only do the creators of these contrasting scholarly styles, whose memberships rarely overlap, come to their work tables with entirely different tools, but the theorist usually lacks the statistical know-how required to decipher survey research pieces in expert fashion, just as their opposite numbers possess uncertain knowledge of theory proper, especially in its philosophical or historical dimensions. That this problem, if it truly is such, of bifurcated consciousnesses has been common to sociology since Comte (who, one recalls, was a mathematical prodigy) does not reduce its enduring importance to a discipline that continues hunting for a reliable epistemology, as well as the most plausible public persona. The troublesome “two cultures” that C.P. Snow immortalized 45 years ago, while perhaps minimized within some scientific arenas, lives on in sociology as vibrantly as ever. Yet the desire to create and defend “general theory” retains its appeal for a number of scholars on both sides of the aisle, quantitative and discursive. Considering all the well-known barriers to such a goal, one wonders why.

The immediate theoretical, as well as epistemological, impetus behind the article under examination seems to come from Kiser and Hechter (1991, 1998), and the half-dozen responses to them in ensuing years. Yet the drive to do “science” under the guidance of general axioms, from which subsidiary statements can be deduced, is much older — an elusive ideal inaugurated by Hobbes in 1651 and carried on by Locke, Turgot, Condorcet, Saint-Simon, Comte, and others, culminating in the statistical wizardry of Quetelet. Hobbes fired the first shot by claiming the “passions of men are commonly more potent than their reason” (*Leviathan*, chap. 19, para. 4), with the implied hope that destructive, animal irrationality could be curtailed among civilized people. The subsequent 350 years of world history gives little cause to believe that Hobbes was on the right theoretical track, yet he continues to be honored, even in the breach. The way to muzzle human animality, so he thought, was to understand it objectively, to theorize its essence, and then to sublimate or otherwise control it through reasonable government. If this seems worlds removed from works like, e.g., Kiser and Hechter or Coleman’s *Foundations of Social Theory*, the distance is an illusion, since the primary motivation behind works that emphasize “rational action” are all of a piece (in the same way that the *verstehende* tradition in German scholarship shares a distinct scholarly *Weltanschauung*). Their uniform goal, no matter how circumspectly expressed, is to attain “prediction and control,” the mantra of scientific thinking since Bacon. Doctor Faust would understand the appeal, and

also the costs. Note, too, that prediction and control have little to do, formally speaking, with “meaning and understanding,” the analogous pair of key concepts for the *verstehende* school peopled by Dilthey, Weber, Simmel, and others, such as Jacob Burckhardt, probably the most esteemed cultural historian of the nineteenth century. He lectured in 1869 as follows:

The study of any other branch of knowledge may begin with origins, but not that of history. After all, our historical pictures are, for the most part, pure constructions . . . Indeed, they are mere reflections of ourselves. . . . History is actually the most unscientific of all the sciences, although it communicates so much that is worth knowing. Clear-cut concepts belong to logic, not to history, where everything is in a state of flux, of perpetual transition and combination. Philosophical and historical ideas differ in essence and origin; the former must be as firm and exclusive as possible, the latter as fluid and open. (Burckhardt 1979:121–22, 35)

In today’s politics, as West once again confronts the Middle East in the so-called “clash of civilizations,” the urgent need to predict behaviors and then control or eliminate them has failed miserably precisely because the preliminary task of understanding cultural meanings was bypassed by ignorant functionaries spawned by *Realpolitik*.

Reconsider one fountainhead of this general attitude, *Research on the Propensity for Crime at Different Ages* (1833) by Adolphe Quetelet. The pertinent subsection is called “Concerning the Possibility of Establishing the Foundations of a Social Mechanics,” where Quetelet lays out a catechism for positivist research that could be inserted with few changes into today’s methods textbooks, and which embroiders upon the social physics tradition begun by Saint-Simon 30 years before. Quetelet sought “laws” of the *average* man as defined by national identity, calling for “studying him in a consistent way” in search of “certain forces which he has at his command.” He continues:

In admitting that these forces actually exist, as all observations appear to prove, I call them *disturbing forces* of man by analogy with the disturbing forces which scientists have considered in the system of the universe. One imagines that the effect which results from them act with such slowness that they could be called equally by analogy *secular disturbances*. The science which would have such a study as a goal would be a veritable *social mechanics*, which, no doubt, would present laws quite as admirable as the mechanics of inanimate objects. . . This way of looking at the social system has something positive [positivist] about it which must, at first, frighten certain minds. Some will see in it a tendency to materialism. Others, in interpreting my ideas badly, will find there an exaggerated pretension to aggrandize the domain of the exact sciences and to place the geometrician in an element which is not his own. They will reproach me for becoming involved in absurd speculations while being occupied with things which are not susceptible to being measured. (3-5)

Quetelet argues against these imagined counterpositions (mostly using the “power of the divinity” as his shield), despite his unconcealed admiration for “the geometrician” as a general scholarly type. Then he cuts to the chase:

After having seen the progress which the sciences have pursued in regard to universes, are we not able to try to pursue it in regard to men? Would it not be absurd to believe that, while all happens according to such admirable laws, the human species alone remains blindly neglected itself, and that it possesses no principles at all for conservation?

He is worried, though, about certain potential drawbacks to his plan for a social science that mimics physics:

Perhaps one will accord us the possibility of such an appreciation for the physical qualities of man which allow measurement directly, but how will it be proper to grasp them for moral and intellectual qualities? . . . Would not one laugh at the pretention of a geometrician who would maintain seriously that he has calculated that the genius of Homer to that of Virgil is as three is to two?

Once again, though, he brushes off his putative critics and defends his cognitive goals as do-able.

Despite its quaint flavor, this epistemological platform remains unchanged among those today who still wish to create a “social mechanics,” particularly the devotees of “rational action” or “rational choice” theories, since they feel they are by far the closest to a “real” science of human behavior — so long as one accepts without question their quiver of analytic assumptions. The longing among such people to achieve the Queteletian vision — for a social science that produces laws as reliable as those enunciated by Newton and Boyle — remains palpable in their endless programmatic statements, even if their rhetoric is now more practiced and guarded than was that of their many intellectual progenitors.

In exploring this dogged passion for causal understanding, let us leapfrog to a 1950 issue of *Ethics: An International Journal of Social, Political, and Legal Philosophy* (on whose editorial board sat John Dewey, Harold Lasswell, H. J. Paton, W. D. Ross, Ralph Barton Perry, and a dozen other academic stars). Herein a precocious Chicago graduate student, Paul Diesing, published “The Nature and Limitations of Economic Rationality,” which for clarity, scope, and general wisdom outstrips any similar statements one is likely to find nowadays. Sad to say, I cannot do justice to the article here, but a few excerpts are in order as we try to assess the meaning of “rationality” as currently celebrated by a number of scientific sociologists. The wonder, of course, is that reflections such as Diesing’s could have been current a half-century ago, yet have still not deterred those researchers whose Golden Fleece is a tracking of causality based on the fantasied rationality of social actors, whether individual or collective.

Diesing begins:

Economic rationality, or economizing, consists of the deliberate allocation of scarce means to alternative ends in such a way that the ends are maximized. There is

general agreement on the above definition, but disagreement on how it applies to reality. The disagreements concern two main issues. The first issue is whether economic rationality is primarily a description of how men do act or a standard of how they ought to act. . . . The argument begins with the utilitarian claim that the principle of utility is equally descriptive and normative, continues with theories which treat it as primarily descriptive, and concludes with a theory that is primarily normative. The second issue is whether the principle applies to all social action or to only one kind or one phase of action. This issue may seem to have been settled by the elaboration of noneconomic residual categories, as, for instance, by Pareto, which disproved the utilitarian claim that all action is economic. . . . The real problem is to discover a kind of rationality different from economic rationality. If there is no other kind, all noneconomic behavior is completely irrational, and therefore scientifically unknowable. . . . I shall show what happens when economic rationality is applied outside its proper limits. (12)

These are bold claims for a 27-year old graduate student, but the times were different then, for he had already served in the U.S. Army for three years during the War, and thus had seen “irrationality” in the flesh. Diesing also understood the underlying cultural roots of the rationality fetish:

Ordinary, rational, middle-class people could be counted on to economize; hence, if economic theory could not claim to describe the actions of all men, at least it described those of all good men. . . . hence, the practical corollary of this position is that irrationality, socialism, etc., ought to be abolished. The taking of this position was, and is still, common, as in the denunciation of backward colonial countries, the condemnation of laborers preferring security and ease to hard work and advancement, etc. Its similarity to the Protestant ethic as treated by Weber has been pointed out by Parsons.” (13)

Quite the same argument might be made about “rational choice theory” as posited in the article under consideration here: it partakes of an attitude that smacks of the politically reactionary, the culturally imperious, and what might overall be termed the “Popperian worldview” — or that of the very “nation of shopkeepers” which inspired so much of this thinking. Diesing evaluates the ideas of Alfred Marshall, Keynes, and Lionel Robbins, pointing out that “ethical criteria, of course, are also irrelevant” when issues of “economizing” are at stake. He shrewdly notes, almost as if anticipating today’s arguments, that “it is natural for someone concentrating on an analytic aspect of anything eventually to suppose that he is dealing with something concrete” (Diesing 1950:15). Philosophers of science, of course, love to distinguish between the “analytic” and the merely “empirical,” as if the former has nothing to do with the latter, as if defining the former could possibly arise in any way except after observing what constitutes the “merely empirical” (hence, mutable), and deciding, post facto, what categories might best illuminate a given phenomenon. There is considerable hocus-pocus to such positions, and Diesing was already aware of them 54 years ago.

He concludes with simple precision: “Economic laws do not describe how real people act, but how an economically rational man would act” (p. 16). This might serve well enough for a model-crazed economics that apes physics for all its worth, but within sociology — which, we might recall, concerns the lives of people *as lived* — such a theoretical angle leaves virtually everything important by the wayside. Dising takes his argument a step further: “Economic rationality, on the other hand, never appears directly but is imbedded in nonrational norms, such as thrift, enterprise, and efficiency . . . [it] never appears separately but always appears in containing norms . . . [It], then, is a rational a priori norm derived from the concept of allocating scarce means to alternative ends. It does not apply to concrete types of action. . . Its scope is limited by other norms, both rational . . . and irrational” (pp. 16–17). His final blast needs to be rebroadcast today: “Hence the standard of utility or any other unilateral means-end norm is inapplicable ultimately to a system of social relations. The system, has, so to speak, no purpose” (p. 21). In consequence, when “the scope of economic rationality is extended into the system of social relations,” it brings with it “destructive effects” (p. 22). Put another way, those scholars who opt for an analytic perspective based on rational action have traded the chance to understand human life at large for the nugatory comforts associated with small-scale precision — all predicated on a fictional (read: “analytic”) portrait of human action, in the marketplace or beyond it.

Speaking more precisely to the article at hand, there remain a battery of peculiar claims that require mention. For example, “The postulates themselves. . . are judged based on their capacity to yield empirically supported hypotheses, especially hypotheses that are counterintuitive vis-à-vis common sense or other theoretical expectations (Popper 1968).” The quixotic hunt for the counter-intuitive has always fascinated me, because of the allied notion that “real science” and “rigorous formal restatements of the merely anecdotal” ought above all to search high and low for cases which contradict normal experience and common knowledge. What’s the matter with intuitive appeal? Is it not the basis of all human knowledge except for that of those baleful scientists hoping to wow their peers with sharply contrarian wisdom concocted through ingenious experiments? Is there anything in Popper’s own work that is actually counterintuitive? If one knows the rudimentary outlines of epistemological and political conservatism, there is nothing surprising to the man, except for his lamentable misreading of Plato and Hegel.

The various subtleties, confusions, and elaborations that surround “functionalism” as realized by its most skillful proponents are missed in this article. Once again, one must ask: what does one gain exactly from “formalization” that was not already present in the original material? “The problem is simply that its practitioners do not formally specify their postulates and the lines of reasoning they use to deduce propositions. More fundamentally, the testable aspects of functionalist arguments often appear to be false.” Insisting that functionalism be measured “through the use of the strategies described here,” and then faulting it for failing to live up to these arbitrary standards, says very little for the utility of functionalist

reasoning. This is only a problem for those who demand of other authors fidelity to a model of inquiry which is alien to their original purposes. It is like blaming a fox for not being a hedgehog.

Because it generally abides by the standards of “hypothesis derivation” and “untestable postulates,” Brustein’s *The Logic of Evil* receives high praise; it is premised on this claim: “German voters were rational individuals.” This is hard even to write without breaking into demented laughter, something the author all but admits later on. With all the ink that’s been used in trying to figure out why those Germans who voted for Hitler did so, often *against* their own best economic interests — which is not unlike blacks or blue-collar workers voting for Reagan and Bush II, of course — Table 3 becomes a sad parody of “science” and “formal restatement.” In the end it comes to this: why are “testable propositions” (and one can leave aside for the time being specifying the exact mechanics of such “testing,” which may be much too cumbersome to perform) the Holy Grail? Who cares? In the case of Nazism, of all things historical, attempted formalizations such as this seem entirely beside the point. If one wants to understand why “decent Germans” voted for Hitler, one needs only to read the Versailles treaty, or the series of novels by Erich Maria Remarque that describe socioeconomic life in Germany between the wars. There is something to this particular exercise in hypothesis testing that verges on the morally repugnant.

Also, the article puts a lot of faith in Jasso’s special version of theorizing: “The ideal theory is a theory of the operation of a basic force,” writes Guillermina Jasso (1988:5).” An article that builds its edifice on such a tendentious claim risks crashing into disarray as the load of substantive materials it tries to accommodate becomes ever heavier. Who can say that an “ideal theory” is such a thing as Jasso imagines? First of all, the very notion of an “ideal theory” can be nothing more than a charming fantasy, and secondly, the best theories explain things in a persuasive and plausible way to experts in a given area. Other than that, theories have no obligations, and particularly none to please the frustrated physicists among us.

Finally, the discussions of neo-Darwinian theory, so-called, where the gene is king, and cultural theory (an extreme truncation of a huge panoply of scholarly fields), defined as that zone of life wherein “semiotic practices” predominate, leave too many essential questions unanswered to be really useful. However, one of the best paragraphs in the article just precedes the “Conclusion,” where the author admits that many cultural theorists “reject causal explanation as a goal,” and therefore produce work which is “incompatible with the pursuit of general theory.” With this realization, finally, I can wholeheartedly agree.

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