



## The Full Hempel

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## THE FULL HEMPEL<sup>1</sup>

THE LOGIC OF HISTORICAL EXPLANATION. BY Clayton Roberts. University Park, Pennsylvania: The Pennsylvania State University Press, 1996. Pp. 320.

The British film comedy *The Full Monty* tracks the efforts of several Englishmen left unemployed by changing industrial circumstances to redeem their fallen economic and social status by becoming male strippers. The film cleverly builds on the irony of men attempting to escape their (socially, economically, sexually) degraded state via an activity usually associated with the social, economic, and sexual degradation of women. At the point when these men finally take the stage before a laughing, cheering, encouraging (screen) audience of women and men alike, the theater audience too has become engaged in hoping the men succeed in their quixotic endeavor to overcome their marginal physical and economic existences. It is not the promise of “the full Monty”—“going all the way”—that holds each audience in sway. Rather, it is just because the men are so clearly *de trop* that their efforts to prove otherwise amuse.

In retrospect, the misplaced efforts chronicled in the movie recall the response of some historians to Hempel’s proposal in “The Function of General Laws in History,” namely, to try to fit the deductive-nomological (D-N or “covering-law”) model to historical practice. The parallel is, of course, that one would be hard pressed to find another case where a philosophical model of explanation was as *prima facie* ill-suited to the discipline to which it was to apply as was the D-N model of explanation to the practice of history. For what the Hempelian account demanded of historical explanation represented neither a reasonable abstraction from nor distillation of existing historical practice. The reason that philosophical theory here so poorly suited disciplinary practice was recognized from the start; theory demanded historical explanations provide an analogue to laws in the natural sciences, laws not currently or foreseeably available to the target audience.

But why imagine that the Hempelian route was the road to the redemption of historical inquiry? The attraction of the model to historians resided in the fact that historians, having seemingly lost their place in the academic equivalent of a basic industry—as practitioners of a genuine science—were offered scientific status were they to display the “full Hempel.” By aping the form of “scientific” explanation, historians might hope to reclaim inclusion within the realm of true science, their subject’s lack of suitability for what the format seemingly demands notwithstanding.

1. I would like to thank Jack Nelson, Lynn Hankinson Nelson, and Piers Rawling for helpful comments on an earlier draft of this paper.

But if an event—the attempt to mate Hempelian theory and historians’ practices in order to breed a science of history—was farcical the first time around, what is one to say when philosophical history repeats itself? This is the question generated by Clayton Roberts’s *The Logic of Historical Explanation*. For Roberts announces at the outset of his book “that the covering-law model, when applied to . . . micro-events (that is, to the discrete events leading up to those wars and revolutions) is valid, useful, indeed indispensable” (viii). Roberts believes that the positivists’ effort to map the bounds of cognitive significance lives on, and in roughly the form Hempel gave it over a half century ago. So once again historians are called upon to gyrate and display “the full Hempel.”

Yet, as I discuss below, Roberts fails to establish *any* of his central claims—the relevance of the covering-law model to historical events, the existence of laws useful for historians’ purposes, his much needed distinction between micro- and macro-events, and the existence of anything plausibly qualifying as a “logic of colligation.” The only interesting question provoked by Roberts’s book is: what can we learn from this failure? Is there reason still to quest for “the logic of historical explanation”?

Fear motivates, but different fears motivate differently. Richard Rorty, in a characteristically astute assessment of what anti-relativists fear, puts the matter this way:

Relativism is the view that every belief on a certain topic, or perhaps about any topic, is as good as every other. No one holds this view. Except for the occasional cooperative freshman, one cannot find anybody who says that two incompatible opinions on an important topic are equally good. The philosophers who get called “relativists” are those who say that the grounds for choosing between such opinions are less algorithmic than had been thought. . . . So the real issue is not between people who think one view is as good as another and people who do not. It is between those who think our culture, or purpose, or intuitions cannot be supported except conversationally, and people who still hope for other sorts of support.<sup>2</sup>

Fear of relativism motivates a search for a way to legitimate a faith that rational inquiry is fated to produce, if not truth, at least a consensus built on methodological certainties. Indeed, Roberts numbers among those made indignant by the suggestion that the cognoscenti do not speak with one voice regarding the tenets of rational inquiry. “The objectivity of history lies in the fact that historians do widely agree upon . . . canons of evidence and rules of logic” (x). What motivates Roberts to embrace his variant of the positivist project is the desire to make this demarcation criterion explicit, and so establish that there is a way of differentiating “the real thing” from pretenders.<sup>3</sup>

2. Richard Rorty, “Pragmatism, Relativism, Irrationalism,” in R. Rorty, *The Consequences of Pragmatism* (Minneapolis, 1982), 166-167.

3. While one might be understandably indignant or otherwise put off by the postmodern fashion in general and in history in particular, this outrage has not (yet, anyway) produced particularly cogent responses. See, for example, my review essay of Murray G. Murphey, *The Foundations of Historical Knowledge, History and Theory* 34 (1995), 231-244.

What is to give teeth to Roberts's "passionate belief" that "historical research is not hopelessly subjective, that historical truth is not irremediably relative" (x) is precisely this contention that histories are judged "by canons of evidence that are widely, if not universally, accepted" as well as "by rules of logic that are widely, if not universally, accepted" (x). Yet, given the claim that the relevant "canons of evidence" and "rules of logic" are "widely, if not universally accepted," it seems ironic that Roberts must set himself the task of specifying what these are.<sup>4</sup> Moreover, a sophisticated relativist need not be put off by characterizing "objectivity" as what is "widely accepted" inasmuch as what historians "widely agree upon" may itself change.<sup>5</sup>

As I read Roberts, he intends to have us view historians as rich in a store of implicit knowledge regarding the nature of explanation. This tacit logic has two key components, each component corresponding to a distinction between types of objects of explanation. On the account Roberts constructs, the explananda can be one of two event-types—either what he terms a "micro-event" or a "macro-event" (for example, wars and revolutions versus "the discrete events leading up to them").<sup>6</sup>

A division of explanatory labor for the historical cases corresponds to this distinction between macro- as opposed to micro-events. When the explananda are micro-events, they are explained by the covering-law model. Roberts supplements his D-N fervor by conjoining it with his variation on another concept familiar from philosophy of history of an earlier vintage. On his account, the explanation of macro-events rests on an explication of the "logic of colligation."

The debate over the covering-law model in the 1970s proved sterile because the positivists persisted in applying it to macro-events and for that reason ignored the explanatory power gained by tracing the course of events leading up to the macro-event—a process I have chosen to call colligation. It likewise proved sterile because the humanists, though they appreciated the explanatory power of tracing, persisted in denying that in order to trace one had to subsume these lesser events under laws governing the occurrence of like events—a process I have chosen to call correlation. Colligation and correlation are the warp and the woof from which historians weave their explanations. (viii)

The positivists were wrong in thinking that covering laws could be adequate to macro-correlation; their humanist opponents were wrong in thinking that cover-

4. Although Roberts is widely read in the literature surrounding the debates in the analytic philosophical tradition, striking by its absence is any mention of important historical or historically grounded accounts of the quest for objectivity which reach a very different conclusion from the one that Roberts promotes, for example, Peter Novick's *That Noble Dream: The "Objectivity Question" and the American Historical Profession* (Cambridge, Eng., 1988) or Geoffrey Hawthorn, *Plausible Worlds* (Cambridge, Eng., 1991).

5. See, for example, *That Noble Dream*. Novick is neither cited nor discussed by Roberts. Kuhn is mentioned once, in passing, and apparently not seen as relevant to the issues of objectivity in history. Hayden White is dismissed in a footnote in which Roberts asserts, leaning on the authority of David Carr, that "human experience is already structured narratively" (37, fn. 54).

6. For reasons rehearsed below, it is unclear whether or not for Roberts these event-types have to be absolutely or relatively characterizable (that is, whether they are like "large" or "small" used relative to some comparison class, or more in the manner of, say, locations given in the belief that space-time coordinates are absolute). If the latter, no criteria are provided by which to mark one event-type from the other.

ing-law explanation had no role to play in how a historian links some types of sequences of events together.

The task Roberts sets himself is to demarcate the proper sphere of each type of explanation and to unpack “the logic of colligation,” a task philosophers have left undone—“no philosopher” he asserts “has discussed” this logic (viii). Roughly, his position is as follows. While there is no “covering law” that, together with antecedent conditions, itself explains, for example, a revolution, still a causal chain exists that determines why things turned out as they did. For, Roberts maintains, a causal explanation exists for each subevent which contributes to the character of the event to be explained. The process by which to link subevents together so as to display the causal structure of the larger event Roberts terms “colligation.” But “colligation” is itself a form of causal explanation, since each aspect of it is in turn explained by appeal to covering laws. So, for example, there is no covering law by which to explain the Reformation, but for each causally significant subevent leading to the Reformation, such as Luther’s nailing the 95 Theses to the church door, the Church’s selling of pardons, and so on, there is a formal Hempelian explanation waiting in the wings. In what follows, I trace through Roberts’s effort to specify the salient features of the fundamentally different explananda he posits, the explanatory “logic” said to be appropriate to each, and the relation of the explanation of subevents to the macro-event to be explained.

Reviewing some of the history surrounding the Hempelian model will prove helpful in assessing Roberts’s account of explanation, for it will remind us both of what made the model attractive and what its shortcomings now are taken to be. I assume familiarity with Hempel’s basic deductive-nomological account. My concern is not so much with its specifics, but why it exercised the hold it did on philosophical and historiographical imaginations.

The philosophical attractions of Hempel’s famous model are readily specifiable. It explicitly promoted as the generic defining feature of explanation the very intuitively plausible notion that to account for the way something is is to show why it is a particular of a general type. Laws and law-like generalizations do the chief explanatory work. The *explanandum* is the instantiation of a law or law-like generalization under the appropriate antecedent conditions. The empirical challenge, for whatever it is one wants to explain, is to identify the general case of which it is an instance.

The intellectual glue that cements the *explanans* to the *explanandum* is that of formal logic. That is, the model situates and assimilates its components within the well-rehearsed and understood format of deductive argument. This explanation-as-argument model both explicates the notion of explanation in terms of one that is better understood (formal argument), and assimilates the core of the explanatory relation to one—the relation of an instantiation to its “covering” law—that satisfies a pre-analytic sense of what it is to explain. Additionally, this model unifies the notions of explanation and prediction. The D-N model held out

the promise of conceptual unification and conceptual clarification, a model applicable to any discipline worthy of the name “science.”

The problem, of course, is that what the model, in either its purely deductive or statistical forms, stipulates as necessary and sufficient conditions for explanation turn out to provide conditions that are neither necessary nor sufficient.<sup>7</sup> Like its soul-mate, the verifiability criterion of meaning, the model’s claim to attention lay with the clarity and unity its formal features promised. In this respect, positivism offered a method to cut through all forms of obfuscation, at least with regard to what could be deemed a meaningful claim and what not. But positivists lived by the sword and died by the sword. The formal failings of the models were their undoing.

In the case of the D-N model, the manifold shortcomings of the model *qua* formal model are evinced through a series of counterexamples developed and closely examined over the last several decades as responses to Hempel’s account. The counterexamples function to undercut not just the model’s claims to formal adequacy, but also to intuitive plausibility. What many of the classic counterexamples make manifest is that the model accommodates cases—the flagpole’s shadow as explanation of its height, the falling barometer as explanation of the storm, the red shift as explanation of the movement of stars—where the accommodated accounts have no *explanatory* merit.

But the problems do not end with the fact that the Hempelian model provides neither necessary nor sufficient conditions for explanation and its failure to generate a plausible account of how the *explanans* “connects” to the *explanandum*. Problematic also is what it is for a statement to be a “law” or a “law-like” generalization. For while the model demands that some statement fill this role, I am uncertain whether anything would count today as the “received view” on the topic of what marks a statement as a law. As Hempel comes to acknowledge, the meaning of the terms such as “law” or “explanation” adverts to larger theoretical contexts. For whatever it is to be a “law of nature” such statements do not stand free of wider theoretical commitments. Paraphrasing Frege, only in the context of theories do laws explain.

It is worth recalling, in other words, that the theory-dependence of laws can be argued for independently of appeals to the history of science and debates regarding the incommensurability of successive scientific theories. The crucial element here, rather, is the holistic relation of statements such as laws to other beliefs. Indeed, Hempel himself famously makes just such an argument, based on his analysis of the logical structure of the meaning of scientific statements.

[W]hat is sweepingly referred to as “the (cognitive) meaning” of a given scientific hypothesis cannot be adequately characterized in terms of potential observational evidence

7. Clear and thorough presentations of the many counterexamples and difficulties besetting Hempel’s models are to be found in Richard Miller’s *Fact and Method* (Princeton, 1987), chapter 1 and Wesley Salmon’s Whiggish but magisterial essay, “Four Decades of Scientific Explanation,” in *Scientific Explanation: Minnesota Studies in the Philosophy of Science*, ed. P. Kitcher and W. Salmon (Minneapolis, 1989), volume XIII.

alone, nor can it be specified for the hypothesis taken in isolation. In order to understand “the meaning” of a hypothesis within an empiricist language, we have to know not merely what observation sentences it entails alone or in conjunction with subsidiary hypotheses, but also what other, non-observational, empirical sentences are entailed by it. . . . In other words, the cognitive meaning of a statement in an empiricist language is reflected in the totality of its logical relationships to all other statements in that language and not to the observation sentences alone.<sup>8</sup>

A fundamental goal of the Hempelian account was to make explanations both identifiable and (at least partially) evaluable by virtue of their form alone. It fails dramatically on both counts. Form does not prove to be a necessary or sufficient guide to explicating in what the explanatory relationship consists. Moreover, the chief ingredients in Hempel’s formal recipe—laws—turn out to have their meaning dependent on—identifiable only within—many variously interrelated commitments and beliefs. Thus, presuppositions central to motivating the positivist (or any) explicatory project—by its form alone you can identify something as, for example, an explanation, and a broader notion of analytic equivalencies—fall by the wayside. The relevant notions of “explanation” and “meaning” turn out to be contextually dependent.

The concepts Hempel links as deeply interconnected aspects of scientific explanation—law, prediction, argument—turn out not to fit happily together. To the extent that philosophers still pursue the ideal of a model of explanation, the proposals rely increasingly on specification of a context of explanation (for example, van Fraassen’s “pragmatic” account) or on sets of historically evolving patterns of explanation (such as Kitcher’s views on “explanatory unification”).<sup>9</sup>

The deductive-nomological model, not unlike the verifiability criterion of meaning, remains an interesting and important intellectual artifact, one from which much may be learned by studying why it is mistaken. But its appeal now should be only to the unwary. Salmon dates the effective demise of the Hempelian model to the late 1970s. By that point, Salmon observes, Hempel had been forced to make so many concessions regarding his account that “he rendered the I-S [inductive-statistical] model indefensible.”<sup>10</sup> Salmon concludes on the same page, “The key doctrine of the received view—that explanations are arguments—stood on shaky ground.” It is ground the model has never left.

The foregoing is a very brief recounting of a story that is long, well, and widely known.<sup>11</sup> But, it appears, those indifferent to philosophical history are con-

8. Carl Hempel, “Problems and Changes in the Empiricist Criterion of Meaning,” reprinted in *Classics of Analytic Philosophy*, ed. Robert Ammerman (Indianapolis, 1990), 227. Hempel’s essay originally appeared in 1950, the same year as Quine’s “Two Dogmas of Empiricism.”

9. For representative positions, see, for example, essays in the Kitcher and Salmon volume cited above, but especially those by Kitcher and Salmon. Skepticism about the merits of prospects of a model of explanation are powerfully voiced, for different sorts of reasons, in Miller, *Fact and Method*, and John Dupré, *The Disorder of Things: Metaphysical Foundations of the Disunity of Science* (Cambridge, Mass., 1993).

10. Salmon, “Four Decades,” 116. As it turns out, this is precisely the form of the Hempelian model on which Roberts relies.

11. Indeed, one of the better standard accounts of the shortcomings of the Hempelian model—Richard Miller’s book cited above—is cited too by Roberts (though not with regard to Miller’s discussion of Hempel).

demned to repeat it. Certainly the single most striking feature of Roberts's book is that there is no real acknowledgment or careful discussion of the formal failings of the Hempelian model. The issues raised in this regard are pressing since they go to the reasons why the model should have any credibility as an account of explanation, and not just to its credibility as an account of explanation in history.

I have outlined what I take to be the most fundamental problem with the appeal Roberts makes to the utility of the covering-law model. But others are closely related. For example, what is a law or law-like generalization for purposes of historical explanation? Roberts's answer here is stark and bold: truisms suffice. Moreover, what Roberts chooses to call a "law" has the following characteristics: laws are probable, not invariable; parochial, not universal; empirical, not analytical; predictive, as well as explanatory; inductive, not deductive; implicit, not explicit.<sup>12</sup> I am not aware of a philosopher of science who would hold, for example, that laws are parochial, not universal. This is not some quirk of philosophers, of course. For the problem is that the meaning of the term "law" becomes unclear once one, as Roberts is content to, contextualizes and historicizes it. The distinction between a "law" and a "passing phase" becomes indistinguishable on this account.

Roberts's repeated claim is that without covering laws, nothing in a narrative would connect. Without appeal to a covering law in some form or other, there is no bridging what are taken as the causal sequences in a narrative (52). "In order to connect the discrete steps in an explanatory narrative, covering laws are necessary" (54; see also 66).<sup>13</sup> The wisdom of a historian consists in the fact that, having read well and wisely, that historian knows the relevant dispositions, truisms, or related factors that are the springs of action for the people being written about.

"Laws" for Roberts may be peculiar to the era under study, but they nonetheless are key to specifying what causes are. In this respect, Roberts criterion for explanation is *stronger* than Hempel's inasmuch as Hempel did not require that the laws cited for purposes of explanation be specifically causal ones. For Roberts, however, causal "covering laws" provide the explanatory unity to the events narrated.

The historian who declares that the levying of ship money caused discontent need not state the generalization that Englishmen in the seventeenth century resented paying taxes, especially taxes to which they had not consented in parliament. . . . But the fact that historians need not make explicit the covering laws they employ does not excuse them from the necessity of making sure those laws are true. Indeed, the value of their histories depends as much on the truth of the covering laws they employ as on the truth of the assertion that the causal events in question occurred. (88)

But consider here one of Roberts's own examples: that the anger and fear manifested by members of the House of Commons in January 1642 was caused by

12. This list is given on page 55.

13. In the "Glossary" that Roberts appends to his book, he defines neither the term "cause" nor "law."



Charles I's attempt to arrest the Five Members. But just why does the intelligibility of this "explanation" require postulating an underlying law?

The claim that a law must be lurking in the background in order for there to be an explanation is a bit of philosophical dogma Roberts uncritically assimilates. For example, it is sometimes the case that what calls for explaining, with regard to human action, is someone's failure to act as expectations led others to anticipate. There is no reason to think that anticipated dispositions to behave were any the less subject to failure in other ages than in our own. Moreover, for certain types of actions—my going to the store because we're out of milk—no generalization seems necessary or relevant to the adequacy of the explanation. If reasons can be causes (and historians can hardly do without this assumption), then what is needed for purposes of explanation is an account of the "reasonableness" of reasons, not the citing of a law.<sup>14</sup>

Roberts himself is aware of this, sometimes appealing to truisms as the bases for explanation, sometimes noting that what needs explaining is the singularity of the event. It is unclear, given the laundry list of attributes Roberts assigns to the notion of a historical law noted above, exactly what sorts of statements could not possibly be laws.<sup>15</sup> In any case, by explicitly historicizing the relevant generalization, he makes his appeals to the notion of a law otiose. His laws are not recognizably such by any criterion.

In general, a fundamental lack of clarity regarding notions central to his account pervades Roberts's exposition. For he not only gives no hint as to how he would repair or clarify standing problems with the conceptual apparatus on which he relies—counterexamples to the D-N model, puzzles about what a law is—but also he provides no workable specification of notions he introduces and which are to be wedded to the D-N model.

Key here are his accounts of micro- and macro- events, and the related notions of micro- and macro- correlation. Each is said by Roberts to have a distinctive logic of explanation. Yet he provides no more of an account of what a "macro-event" is other than to describe it as "complex" (10). Examples include "impeachments, revolutions, the rise of parliaments, death rates, and changes in belief" (10). The notion of a micro-event is likewise instanced but not explicitly

14. Roberts is aware of this familiar Davidsonian point, but dismisses it for the following reason: "Historians must, whenever they have no knowledge of the dispositional traits of an agent, fall back on calculations concerning what any and all rational persons would do in a certain situation" (182). How to fill out this notion of a universal rationality is one of the many questions Roberts leaves unanswered.

15. For example, the philosophical literature is rife with examples of cases where *low probability* events might be cited as causes by way of explanation. One famous example concerns the fact that only people with syphilis can develop paresis, although a low percentage of syphilitics actually do. So, it is true that the cause of someone's getting paresis is that they had untreated syphilis even though the causally relevant factor does not entail that most people with this condition do develop paresis. More generally, this example and ones like it point to the fact that one cannot assign some prior probability limit to a relationship for that relationship to count as causal or explanatory. It does not take much imagination to see that one could seize on other attributes listed by Roberts, for example, that laws are parochial, and show that they allow in so many different sorts of cases so as to erase any sense of a meaningful distinction between laws and non-laws.

defined; examples include “births, marriages, divorces, abortions, murders, voting, illness, and death” (12).

The failure of the covering-law model to be adequate for the explanation of the former is laid to two causes. First, events such as revolutions cannot be captured by laws because there are no “typical” cases. Complex events are, if not *sui generis*, at least not instances of some regular type. There is nothing essential to a complex event such as a revolution *qua* revolution that would make it the appropriate object of a law.<sup>16</sup> Second, and sufficient unto the purposes of Roberts’s argument, is his observation that “complex events cannot be explained by subsuming them under a covering law; the laws are simply not there” (13). The point here is critical. The difference in explanatory kinds rests on the contention that events of a certain (unspecified) degree of complexity do not allow of typification; less complex events are, however, identifiable as events of a certain type-kind, namely, a type-kind that exhibits (at least some) regularities *qua* type. The unstated link is that events of the higher degree of complexity are compounded from those of the lesser degree, but not in a way that the regularities found at the lower levels determine or provide regularities at the higher level.

Roberts offers no argument for this distinction; he is content merely to assert it. In addition, he provides no clue regarding what distinguishes one type of event from the other. If the distinction is contingent on having knowledge of how the microstructure determines macrostructure, then the notion of colligation is just a place-holder until that time in which the requisite covering law is known. If, however, the distinction is meant as a principled one, Roberts’s position is deeply puzzling. For why should there fail to be laws of sufficient complexity to cover any event?<sup>17</sup> If the answer here is that kinds of historical events (such as revolutions) are not “natural kinds,” and so not subject to laws of nature, then his whole account unravels. For what reason is there to think that most events in human social history are natural kinds? But if laws apply to social kinds as well, he gives no principled reason to insist that only some social kinds but not others can be explained by appeal to laws.

However, the most basic puzzle I find regarding Roberts’s use of colligation is his claim that colligated events can be known to be causally linked even in the absence of our knowledge of laws determining the macro-event. Put another way, Roberts’s contention is that the relevant causal subunits of a macro-event such as a revolution can be identified and causally analyzed despite the fact that we have no general laws regarding what conditions entail such macro-events.

This brings us to the vexed question of the relation between the notions of colligation and causation, especially as Roberts uses them. Roberts attributes the

16. Roberts does not argue for this so much as simply assert it.

17. Trivially, it would seem to be the case that if, as Roberts concedes, subevents are subject to covering laws, then why shouldn’t the macro-event be “explained” by some conjunction of these laws? The various “logics” of colligation Roberts purports to specify in chapter 6 would affect only how these conjunctions are formed. Unless the macro-event has some emergent property (a point for which Roberts never argues or asserts), I fail to see why conjoining the laws of the relevant parts would fail to entail the macro-event, given the parallel conjunction of actual conditions.

following “confusion” to Walsh’s use of “colligation.” Specifically, he discerns a confusion between, on the one hand, colligating by means of placing particular acts as related to a certain general type, such as certain actions as connected by being part of a strike or a game as opposed to, on the other hand, colligating particulars as part of a causal chain, and so as properly part of an explanation (18). Roberts’s distinction here between colligation understood as a classificatory operation and understood as a means of tracing connections among events is critical to his entire explanatory strategy. For, Roberts announces, the “path I have chosen to follow” is the one that distinguishes colligation as “a tool for tracing a complex causal process, such as the sequence of events that leads to strikes, revolts, wars, or the scoring of a goal in a soccer match” (19; see also 20).

Hume taught that causal connections are not “directly observed” or an “obvious” inference from what is observed. Moreover, whether one imagines that connections are imputed as a result of “custom and habit” or necessarily imposed by our cognitive faculties in the act of understanding (as Kant thought) does not matter here. The point is that, baldly put, Roberts thinks Hume and Kant had it wrong. We never should have made Kant’s “Copernican turn.” Moreover, Hume had it wrong both with regard to simple types of causal inferences (one billiard ball striking the other as the cause of its motion) and relatively sophisticated ones (why turning the key of my car starts the engine). Hume got it wrong because he thought that we had to stop at the noting of correlations; correlation, that is, could never lead us to discover the “necessary” connection in which causation consists. Hume was right to think that the observation of constant conjunction was bedrock in the analysis of causality; he was wrong to find anything lacking in this.

Roberts distinguishes among correlation, causation, and colligation. The distinction is this. Events are correlated just in case they are noted as successive. What separates correlation from causation proper is that we know enough to appreciate that the succession was non-accidental. Relations are apprehended in a colligatory manner just in case the fine structure of events is known to be such that, at any step in the sequence, we can say why the procession of events from *a* to *b* to *c* and so on was as it was and not some other way.

For the vast majority of phenomena, for the phenomena studied in chemistry, biology, psychology, sociology, history and most of physics, it is quite meaningful to speak of cause, since one can trace the sequence of events, repeated again and again, that explains the existence of a correlation. This can be done either by direct observation or by theoretical models constructed from observed data by means of rules of correspondence. (28)

In other words, regularity of succession by itself does not satisfy us that we have discovered causes. We need first to discover the “inner workings.” Put succinctly, colligation turns correlation into causation. (29)

What is needed to separate cause from epiphenomenon is a more microscopic examination of all the causal links involved, an examination that will show that the falling air pressure, not the falling barometer, caused the storm. In short, what is needed is colligation. (30)

Colligation charts the causal path from subevents to a particular outcome. When one cannot find the cause, such as in a case where requisite knowledge of the micro-structure is lacking, one “hypothesizes” as it were what the colligatory structure might be. In this respect, Roberts uses “cause” ambiguously to refer both to connections demonstrably known (that is, where the relevant covering laws are in hand) and to those cases where the relation is taken to be causal but no covering law exists.

Events stand related to one another, Roberts claims, not as parts to whole, but as steps in a causal sequence. “It would be better to conclude that a subevent is related to an event by the fact that it contributed to the change in the state of affairs that the event is said to have caused. . . . Contributing to that change is the common denominator that holds the subevents together, as part of what may be called an aggregate event” (107-108). One “explains” in Roberts’s sense of the term by “unpacking” large events—revolutions, for instance—into their causal constituents—how it came to pass that the French were moved to behead their king and a great many others as well.

*But colligation, in the sense that Roberts employs the term, presupposes some prior grasp of causal connections.* To see this, one need only scan his examples of types of colligation. For example, consider his list of seven subevents that constitute the “colligatory chain” in the explanation of the English Civil War. Each statement explicitly contains the term “because” or some surrogate, such as “led to.” Roberts is explicit about this: “each link in the [colligatory] chain is made up of the initial conditions plus the covering law that connects these initial conditions to the events to be explained” (109). Colligation presupposes causal explanation, and causal explanation presupposes laws.

Roberts critically confuses his exposition by insisting, at other points, that colligation helps identify genuine cases of causal relations: “colligation explains why correlation exists, that it transforms correlation into causation” (133). That is, Roberts emends the account quoted a paragraph above by claiming that the causal idiom explicitly incorporated there is subordinated, in the actual process of writing history, to tracing steps between events leading to the larger events in question. So where the first step in a “colligatory chain” may be “1. The introduction of the Service Book caused the Bishops’ Wars” (108), and this in turn parses “because” in terms of the full Hempel, Roberts then insists that this statement in turn is understood via the logic of “microcolligation” by means of which it is analyzed in yet finer detail.

But if there is to be some explanatorily useful contrast between “microcolligation” and the Hempelian presuppositions otherwise at work, then some alternative analysis of “because” is needed, one that specifically analyzes this process he terms “microcolligation” and shows that “microcolligation” is not being used synonymously with “cause.” Otherwise, there is no gain in clarification regarding the meaning of the concepts in question, much less any gain in understanding by being told that one is useful for the purpose of explicating the other.

Yet a non-circular account of this relationship is precisely what Roberts fails to supply. To see this, one need simply note any of the statements Roberts provides as instances of microcolligation, such as “1. The introduction of the Service Book led to the riot in St. Giles” (126). The problematic idiom is, of course, “led to.” Roberts uses “led to” in almost all of the examples he offers of characterizations of microcolligation. (He even uses the term “caused” in one case.) But what cognitive faculty reveals that one event “led to” another? Its happening repeatedly? Roberts himself offers only the unhelpful analogies of finding an actual physical connection or instances of individual agency. And even if one accepted Roberts’s remarkable contention that causal relations can be directly perceived, such immaculate perception is of no use for explicitly historical cases.

The basic lack of clarity regarding the attributes distinguishing correlation, causation, and colligation is further manifest in Roberts’s discussion of the relation between what he terms subevents, the events serving in the explanans of his “logics of colligation” for an explanandum event. Consider the following example (building on one of Roberts’s own at 107). If the explanandum event is, say, the defeat of Napoleon, an event possibly cited by way of explanation is the Battle of Waterloo. The latter, let us suppose, is causally responsible for the former. But what then of the various “subevents” such as skirmishes that might have been part of that battle? How are these factored into an explanation of Napoleon’s defeat? After all, for want of a nail . . .

The fact that an event is often composed of subevents raises the problem of the relation of the subevents to the principal event. . . . Events do not cause the subevents, neither do the subevents cause the event. There may well have been many skirmishes at the Battle of Waterloo that did not contribute to Wellington’s final victory but are regarded as part of the Battle of Waterloo. . . . It would be better to conclude that a subevent is related to an event by that fact that it contributed to the change in the state of affairs that the event is said to have caused. Thus any event that contributed to the replacement of the Catholic faith with the Protestant may be regarded as a subevent of the Protestant Reformation. Contributing to that change is the common denominator that holds the subevents together, as part of what may be called an aggregate event. (107-108)

I find it difficult to make sense here of what Roberts is saying. For while the “logics of colligation” (there is more than one) supposedly chart a causal path showing how subevents aggregate to bring about some macro-event, this very charting process relies upon a type of causal (namely, counterfactual) knowledge Roberts concedes we do not possess.<sup>18</sup> His suggestion that, for example, there “may well have been many skirmishes of the Battle of Waterloo that did not contribute to Wellington’s final victory but are regarded as part of the Battle” presupposes a possession of nonexistent counterfactual knowledge—even if such-

18. Roberts is explicit that we have counterfactual knowledge only when we have covering laws (81). And, *ex hypothesi*, there are no known covering laws for macro-events. Hence, one cannot say what alteration in subevents would necessarily alter those aspects of the macro-event and its outcomes that interest us.

and-such a skirmish had not occurred, the outcome of the battle would have remained unchanged.

Consequently, there is no determinate way within Roberts's own preferred schema of explanation of saying, for example, which skirmishes were irrelevant to the outcome of the battle and which not.<sup>19</sup> By the very criterion Roberts offers, then, there is no sorting of subevents into those that are causally relevant and those that are not. Without this sorting, there are no "logics of colligation."

It is not surprising then that Roberts never figures out how "cause" and "colligation" fit together, for "colligation," as he uses the term, is to display the non-accidental connections. But Roberts has no independent account of non-accidental connection to offer, and so chants the term "colligation" in mantric fashion in the hope it will levitate him over the gaps in his argument. For he ends up both using the notion of colligation to define causal relations and appealing to causality to distinguish what is colligated and what is merely correlated. Thus he winds up denying, by definition, that macro-events fall under a covering law and yet makes the colligatory relation of micro to macro-event dependent on counterfactual knowledge, knowledge which presupposes that we possess the relevant covering law. Yet without the alleged counterfactual backing for his claims that events are colligated in a particular manner, he is left without a reason for his assertion that subevents cited are causally linked to the explanandum.

The confusion is further compounded by Roberts's quick and easy way with the notion of a law, a notion rendered vacuous by his apparent willingness to call any parochial generalization, however limited and contingent, a "law" if doing so suits his purposes. But so labeling generalizations begs rather than clarifies the question of what a law is.

He puts himself in this position because for "the full Hempel," he needs the nomological—laws. But he has no laws. So colligation becomes a type of place holder concept. But this can't disguise the fact that all talk of "colligation" is simply an effort to paper over the absence of what is required for genuinely Hempelian explanations by Roberts's own lights.<sup>20</sup>

An interesting and important question Roberts's book forcefully raises just because it is so bad concerns his felt need to specify a "logic of explanation." The

19. Finally, I am not sure that events do not cause subevents. Borrowing a point famous from Danto, the fact that certain actions by Luther and others led to the Reformation causes, retrospectively, these actions to be subevents in the Protestant Reformation. But the acts by Luther *et al.* only become subevents of this sort *after the fact*. Later history, in this respect, causes earlier events to be describable as events of a certain type where this description could not be given of the events at the time they happened. Indeed, something may only retrospectively appear as an event at all, for example, the accident that led to the discovery of the vulcanization of rubber.

20. Roberts discussion on pages 157-159 (see also 258) of the "laws" discovered by historians is, as he more or less acknowledges, a list of truisms that appear correct for behavior in a particular time and place. While underlining the vexed question of what Roberts can possibly mean as a "law," it also runs against his explicit advice that in some cases the causal factor is identified by its "abnormality" or "uniqueness" (what he calls "abnormalism" in chapters 5 and 6). Indeed, Roberts vacillates between identifying causal relations in terms of unique factors and in terms of common or repeated factors (253).

need arises for Roberts, recall, because he seeks to defend the objectivity of history. This defense, in turn, is to rest on revealing that historians provide causal knowledge, knowledge backed by laws that are empirically testable. But, as I hinted at the outset, philosophical considerations (such as underdetermination and holism) and historical ones (such as Novick rehearses) provide reasons for skepticism regarding the sanctity of accepted standards, questions which Roberts never addresses, much less resolves.

Roberts appeals to the D-N model because he imagines that it is criterial of scientific explanation. But Roberts's project is best regarded as a symptom of the disease for which it professes to be the cure. The disease is scientism, and its cause is the belief that there is a special method—the “scientific method”—by which to demarcate real knowledge makers from the rest. Those suffering from the disease imagine that a cure lies in adhering to a rigorous methodological regimen, one shown to certify results to rational people everywhere. But no method has been shown to confer such status.

The real issue is that, so far as we have been able to discover, there is nothing to demarcate. That is, so far as anyone has been able to discern, forms of inquiry do not, upon examination, fall into natural epistemological kinds. As I have argued elsewhere,<sup>21</sup> the very notion of science took its contemporary meaning predicated on the idea that some logical or methodological features existed that would mark off certain forms of inquiry as quintessentially rational or as definitive of the proper fashion by which to interrogate nature. In the waning days of this century, the moral appears to be that this assumption is just mistaken.

Moreover, the cogency of the notion of the unity of science can be challenged not only on methodological grounds, but on metaphysical ones as well.<sup>22</sup> Whatever separates history rightly done from the rest, it cannot be that one group participates in the scientific scheme of things and the other does not, for there is no “scientific scheme of things” in which to participate. There remains no good reason to motivate a continued search for a way to carve inquiry at the epistemic joints.

J. L. Austin famously observes that appeal to ordinary language may not be the last word in analysis and argument, but it is (or should be) the first. Likewise, I would suggest, concerns to separate what is good from what is not in historical analysis should start with those very works historians take to be paradigmatic for their enterprise. Like it or not, there is no point of cosmic exile (as Quine famously noted). Contra Roberts, the place that debates about historical explanation ought to begin is with historical practice, and preferably those instances that his-

21. “Will the Real Scientists Please Stand Up? Dead Ends and Live Issues in the Explanation of Scientific Knowledge,” *Studies in History and Philosophy of Science* 27 (1996), 813-838, and my review essay of Murray G. Murphey, *The Foundations of Historical Knowledge*.

22. For an example of the former, see Alexander Rosenberg, *Instrumental Biology or The Disunity of Science* (Chicago, 1994); for the latter case, see Dupré, *The Disorder of Things*. See also *The Disunity of Science*, ed. P. Galison and D. Stump (Stanford, 1996).

torians take to be paradigmatic for framing historical explanation. Even if no one has, philosophically, the last word, discussion of current practices stands as the only plausible place that helpful conversation can start.<sup>23</sup>

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23. For a more sophisticated account than that which Roberts provides of the possible role of laws in historical explanation, and a considered critique of these efforts, see the special double issue of the journal *Theory and Society* 26 (April/June 1997) devoted to the topic: "New directions in formalization and historical analysis." An example of what I would deem "helpful" reflection on methodological issues is A. D. Moses's "Structure and Agency in the Holocaust," *History and Theory* 37 (1998), 194-219. While I do not endorse the specifics of Moses's account, it raises the sorts of issues which might possibly advance and clarify disputes over the nature of historical explanation.