

FORUM: HISTORICAL EXPLANATION

4.

THREE DOGMAS (MORE OR LESS) OF EXPLANATION¹

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ABSTRACT

What ought to count as an explanation? Such normative questions—what “ought to be” the case?—typically mark the domain that those with a type of philosophical aspiration call their own. Debates in the philosophy of history have for too long been marred by bad advice from just such aspirants. The recurrent suggestion has been that historians have a particular need for a theory of explanation since they seem to have none of their own. But neither the study of the natural sciences nor the study of narrative compels or even makes plausible the view that it will be possible to adduce the norms of explanation, either in history or elsewhere, in advance of identifying theories that explain. I readily concede to Stueber, Carr, and Følrand the use of a certain vocabulary when speaking of others. But it is one thing to point to a pervasive habit of explaining behavior in certain terms. It is quite another to document that these explanations have any value as explanations.

What apart from habit or philosophical dogma establishes any of their proposals as explanatory? Explanation by invoking the myth of the shared should be replaced by explanations that have empirical content.

What ought to count as an explanation? Such normative questions—what “ought to be” the case?—typically mark the domain that those with a type of philosophical aspiration call their own. Debates in the philosophy of history have for too long been marred by bad advice from just such aspirants. The recurrent suggestion has been that historians have a particular need for a theory of explanation since they seem to have none of their own. But neither the study of the natural sciences nor the study of narrative compels or even makes plausible the view that it will be possible to adduce the norms of explanation, either in history or elsewhere, in advance of identifying theories that explain. Better, as David Carr wisely suggests, that any such normative proscriptions be given on the basis of how well the proffered accounts serves the purposes of inquiry:

Perhaps the conclusion to be drawn is that it belongs to the spirit of inquiry to be skeptical of common-sense and easy explanations, both in general and in particular . . . But I would maintain, in good pragmatist fashion, that conceptual frameworks are meant to serve inquiry and not the other way around. In other words, skepticism works both ways, and should

1. I would like to thank Mark Risjord and my three co-symposiasts—David Carr, Tor Egil Følrand, and Karsten Stueber—for their many helpful comments on an earlier draft of this paper. These thanks should not be construed as implying their agreement with or responsibility for the views expressed here.

apply equally to all unquestioned and unargued theoretical commitments, whether they be common-sense or scientific-reductionist in character. (Carr, 28-29)²

Conversely, when what recommends an explanatory form consists only of its fealty to philosophical mandates, this should elicit worries about how it connects with actual inquiry. Just this “spirit of inquiry” guides my assessment of the accounts here on offer. The concern to adhere to a pragmatic and skeptical spirit can be distilled into the following advice: Avoid theories of explanation; seek only theories that explain.

Keep in mind that explanation ought to abet *empirical* inquiry. A *sine qua non* of this is that such explanations prove corrigible. Otherwise, proposals to view the very form of explanation as a contingent result of empirical inquiry would be surreptitiously replaced by an *a priori* judgment masquerading as an empirical discovery. Such an outcome should hardly count as an improvement over models of explanation now discarded.³

No synoptic account of rationality, science, and related notions such as explanation like those that positivism provided, presently waits in the wings. In addition, discussions of the structure and logic of narrative notwithstanding, formalism in a theory of explanation ought to serve a particular evaluative purpose, namely, to aid in deciding the correctness of explanation. Hempel and Popper modeled explanations on deductive argument forms since these make failures of explanation as perspicuous as those of deductive arguments. The now well-known counter-examples to the formalisms of Hempel and Popper demonstrated how they either included cases that ought to have been excluded or excluded ones that should have been included. In this regard, studies of narrative structure fare even worse. No account of narrative structure links considerations of narrative structure and those of *empirical* adequacy. Ironically, then, formal accounts of narrative structure also fail as evaluatory mechanisms for narrative explanations, albeit for reasons other than those involved in the formal failings of the positivist models.

The essays under consideration all go awry in ignoring the lessons suggested by the sad history of discussions of historical explanation. For Carr, Følrand, and Stueber do not concern themselves with the relative merits of actual historical explanations. Rather, each essay promotes an *a priori* and so unempirical theory of explanation.⁴ Consequently, all prove idle as guides to evaluating actual inquiry.

2. All parenthetical page references to Carr, Stueber, and Følrand are to their articles in this forum: David Carr, “Narrative Explanation and Its Malcontents,” *History and Theory* 47 (February 2008), 19-30; Karsten R. Stueber, “Reasons, Generalization, Empathy, and Narratives: The Epistemic Structure of Action Explanation,” *History and Theory* 47 (February 2008), 31-43; and Tor Egil Følrand, “Mentality as a Social Emergent: Can the *Zeitgeist* Have Explanatory Power,” *History and Theory* 47 (February 2008), 44-56.

3. Although I do not argue the issue here, a conception of explanation as demarcated by a particular logical form only reflects how the idea of a unified science continues to exercise influence as an unacknowledged assumption in these debates. Better, I suggest, that such forms be distilled from and refined in terms of what aids inquiry. Norms may guide inquiry, but they also result from it. Norms, too, only incarnate contingent results.

4. In terming each of the three accounts “*a priori*” and “unempirical,” I mean to draw attention to how each writes as if explicating some fixed notion of explanation. As note 3 indicates, I find no basis for this distinctively philosophical presupposition that conceptual explication represents an appropriate method for clarifying explanation. For in each case I find the explanations employed less clear or more problematic than the concept being explicated.

In particular, the papers under consideration in this forum share a strong confidence in certain pretheoretic accounts of explanation. These pretheoretic aspects emerge as a shared faith in what “everybody knows” as genuinely explanatory. The appeals take slightly different forms: “common-sense” or “folk psychology” or a “*Zeitgeist*.” Further, the shared dogma has each relying on an unspecified collective wisdom to underwrite the favored schema of explanation—action, narrative, or collective. *But what sharers supposedly share proves to be not just unverified but empirically unverifiable.* Because of their commitment to this dogma of what everyone knows or shares, the authors promulgate just the “common-sense and easy” approaches that Carr rightly warns against.

I owe to Louis Mink the observation that common sense rests on dead theories.⁵ I take this to be closely related to claims regarding what does or does not constitute a “common-sense” explanation and what does or does not instantiate some “natural” form of understanding. All involve an appeal to a type of self-evidence, to a truistic status that no reasonable person would (or should) contest. But surely the time has passed when philosophical presuppositions about shared meaning or shared content should suffice to certify the legitimacy of some particular form of explanation.

The key problem resides in the fact that the so-called truisms do not deserve the epistemic status attributed to them. Appeals to a shared something prove dogmatic because actual research contests the seemingly widely accepted assumption in the philosophical literature that the philosophically uninitiated use, for example, propositional attitudes in anything like a clearly shared and systematic way. Research does not support claims that there exists cross-culturally a shared stock of similarly used terms identifying the so-called propositional attitudes. Propagation of “folk psychology” as an explanatory theory remains an activity rooted primarily in philosophical armchairs.

Moreover, the claims made on behalf of folk psychology and common sense cast an unpleasant aura of *ad hominem* argument over the proceedings, inasmuch as any criticism immediately suffices to convict its author of various forms of ineptitude—linguistic, cultural, conceptual. For who would deny what all the folk know? But unsubstantiated invocations of folk wisdom or common sense should elicit concern that what does the work of argument has the status of mere dogma that provides no real substance to empirical knowledge claims.⁶ The purported explainer—what the folk know—stands in desperate need of substantiation and clarification.

Curiously, each of the essays claims on behalf of folk wisdom or common sense something not quite the same as the others, so whether adversions to what all the “folk” know represents three articles of philosophical faith or just one cannot be

5. Mink, of course, similarly derides appeals to what “everybody knows” (with regard to Universal History) in “Narrative Form as a Cognitive Instrument,” in Louis O. Mink, *Historical Understanding*, ed. Brian Fay, Eugene Golob, and Richard T. Vann (Ithaca, NY: Cornell University Press, 1987), 182-203.

6. For work that puts various folk notions under much needed empirical/experimental lenses, see Jonathan Livengood and Edouard Machery, “The Folk Probably Don’t Think What You Think They Think: Experiments on Causation by Absence,” *Midwest Studies in Philosophy* 31 (September 2007), 107-127; Edouard Machery, “The Folk Concept of Intentional Action: Philosophical and Experimental Issues,” forthcoming in *Mind & Language*; and Joshua M. Weinberg, Shaun Nichols, and Stephen Stich, “Normativity and Epistemic Intuitions,” *Philosophical Topics* 29 (2001), 429-460.

determined. Følrand conjures up images of histories as approximations of “ideal explanatory texts,” as if inquirers already know what this ideal includes and what not. Stueber urges a reliance on “reenactive empathy,” but the very idea of an empathetic schema cannot be made intelligible, and his claims made on behalf of his empathetic approach prove untested and untestable. Carr seemingly occupies a middle ground, claiming on the one hand that his notion of narrative accords with a type of generic common sense about explanation, yet on the other incorporating an explicitly metaphysical claim regarding the homology of narrative structure and human action. Yet his homology claim, although fundamental to his argument, proves to be without any empirical content. As with the others, he relies on a dogma that seeks to explain by “discovering” what we must share. But however many dogmas there may be, the appeal to such shared wisdom serves only to immunize the claims about explanation in question against all possible empirical disconfirmation.

Tor Egil Følrand borrows freely from an account of explanation found in the work of Peter Railton. Følrand appreciates Railton’s ecumenism without fully taking into account the larger vision that constrains and rationalizes it. For Følrand neglects to take seriously what makes the text in question ideal, much less explanatory, when considering how he looks to exploit Railton’s notion of an “ideal explanatory text.” As he said in an earlier essay in this journal:

By accepting explanatory accounts without laws Railton demands less for an explanation to be valid than does Hempel. On the other hand, he also demands *more* of explanations than Hempel does, since the ideal explanatory text contains all due-to relations relevant to the explanandum. . . . In Railton’s view, scientific explanation seeks to establish comprehensive *theories* which provide us with some conception “of what the organizing principles of the world are . . . which we must grasp in order to know the *how* or *why* of things.” In a sentence that indicates why he regards his account as nomothetic Railton declares that “the main characteristic of a developed and mature science is the presence of a theory that enables us to fit a variety of phenomena . . . under general principles.”⁷

The relevant sense of an explanatory “ideal” to which Railton appeals presupposes a theoretical backing in a natural scientific theory. This informs as to why Railton’s account can be ecumenical regarding the form and content of an explanation. His ecumenism remains coherent (and not a mere pastiche) because it is restrained within a more general theoretical account. The background theory determines the domain of objects or events to be explained and the licit modes of explanation.

What to count as a “due-to” relation for a particular case, as well as whether that case contributes to some comprehensive ideal, can be adjudicated only against this background theory. So here’s the rub. There simply does not exist even a rough analog in history to how, for example, physics functions in the natural sciences. If Følrand wished to explicitly endorse, say, Marxism, then appeal to an “ideal explanatory text” would have some content. One might disagree, but the theoretical arena within which debate takes place would at least have been established. But Følrand offers no such theory. Absent an account that does the actual

7. Tor Egil Følrand, “The Ideal Explanatory Text in History: A Plea for Ecumenism,” *History and Theory* 43 (October 2004), 326.

work of legitimating alleged connections, appeal to Railton's ecumenism proves idle since the relevant notion of an "ideal" is contentless.

Førland recommends using collective facts and he looks for the "added explanatory value." In so doing, the analysis he provides does not make this case. First, he attempts to underwrite his claim about the applicability of Railton's approach to historical cases with an *a priori* assertion about how human psychology operates: "Although agents act according to the situation—and they act differently depending on how they construe it—we should still see them as acting on the basis of the practical syllogisms of belief and desire that are basic in rationalizing action explanations, and within structures that constrain the scope of possible actions" (Førland, 52).⁸ It may be analytic that one cannot act outside of constraints (otherwise, they wouldn't be constraints); but what research shows or even could demonstrate that agents "still act on the basis of the practical syllogisms"? The role that practical syllogisms play in deliberations now and in the past on which Førland insists and relies can never be empirically established. Terming practical syllogisms "basic" and baldly asserting their timeless persistence indicates how Førland conceives of explanation. But it reveals nothing about the content of explanation *per se*, or that explanation has a content *per se*. Such a *a priori* psychologizing constitutes only an unempirical dogma of empirical explanation.

Second, he writes as if once he has shown that collective beliefs are logically possible, then he has done all that needs doing to establish the "added value" of such beliefs. For note how his value-added case for collective facts actually goes:

1) "It seems to me that there is room in the ideal explanatory text for practical syllogisms with plural subjects that have beliefs and pro-attitudes" (Førland, 52);

2) The "*Zeitgeist* [of the 1960s]," should be construed as "a social emergent that is realized by a multitude of thoughts embodied in texts, music, pictures, and so on. As shorthand, to suggest the direction of prevailing intellectual winds in a period, it can be useful and explanatorily economical" (Førland, 53);

3) "Let us allow that the sixties radicals were a social group whose worldview and values were represented by the 'spirit of the sixties.' This would make that spirit causally relevant to the degree that participant agents of the New Left *qua* members of that plural subject suspended their personal beliefs on some matters to make their views accord with the group belief" (Førland, 54);

4) "In that respect the *Zeitgeist* consolidated and harmonized the beliefs of the members of the group not by any action of its own . . . but by being regarded by participant agents as containing the proper worldview and values of sixties radicals, who pooled their wills in order to be part of the 'sixties generation' or 'the movement'" (Førland, 54-55).

Step (1) claims that there exists a "logical space" for plural subjects, but since no one has any idea what constitutes an "ideal" here, this claim cannot be refuted. Step (2) inserts a particular plural subject in the space that (1) deems logically possible, but again, since only a logical space need be created for the desired entity, there can be no possible empirical disconfirmation of it. Step (3) simply asserts in the first sentence what can be assumed *possible* according to (1) and (2); its sec-

8. This clearly echoes the rationale Stueber offers, though without Stueber's general tub-thumping for the merits of folk psychology as imagined by philosophers.

ond sentence represents the consequent of a conditional, the antecedent of which was conveniently just assumed. The “argument” offers no more than a series of assertions and hypotheticals. It moves from the logical possibility of representing a group in a certain way to the claim that so representing them “explains” the group action. At no point does any empirical consideration enter in. Everything needed to make the argument work rests on maintaining the mere logical possibility of the plural subject. Once the shared spirit of the sixties has been assumed into existence, the explanation of group behavior can be readily generated.

On Førland’s account, a collective notion such as a *Zeitgeist* could be explanatory if construed as a reference to a plural subject. As a possible example, Førland suggests that although “the New Left had no clearly defined membership does not mean it should not be regarded as a plural subject; the question is whether its participants saw themselves as being part of a ‘we’ with a jointly accepted view or jointly ready for action. *If the answer is yes . . .* the ‘sixties spirit’ may have an explanatory function, and not only as shorthand” (Førland, 54, emphasis mine). But this leaves us with just a conditional, as Førland’s formulation makes explicit. *Absolutely nothing in the account argues for the explanatory efficacy or necessity of the hypothesized antecedent.* Moreover, and more importantly, nothing indicates what would count by way of evidence for the truth of the antecedent.

The issue here does not concern whether one ought to be a methodological individualist. Rather, the question to be answered concerns whether appeal to a *Zeitgeist* enhances explanation. The *Zeitgeist* constitutes, as Førland admits, an unbounded, unspecified entity. Moreover, there exists no “physics of the *Zeitgeist*,” no theoretical account of what it can and cannot do, whom it can or cannot affect. The puzzle of how individuals come together to work as a group has been replaced by the mystery of how a *Zeitgeist* manages this trick. Since no one knows how a *Zeitgeist* works, appeal to it provides no information. *Nothing (literally) has been explained.* This represents “added value” only if the goal were to make explanations irrefutable because impervious to anything that could be adduced empirically.

Carr shares with his co-symposiasts the *a priori* view that a type of “common-sense” or “folk” psychology successfully bears the explanatory burden. Unlike Stueber or Førland, however, he identifies the structure of action not with the practical syllogism, but with narrative form. “This rather obvious fact suggests that the narrative mode is very close in form to the structure of action itself, from the agent’s point of view” (Carr, 20). The symposiasts all subscribe to a belief in a symmetry between the structure of explanation, the nature of self-understanding, and the explanation provided from a third-person point of view. Whether one labels this “folk psychology” or a “practical syllogism” or “narrative structure,” all assume *a priori* that action explanations partake of some “natural” or “self-evident” format, one that people share; this fact accounts for the appropriateness of the form of explanation each favors.

Yet perhaps that three learned theorists of the explanation of action all take different forms to be self-evidently “natural” or “common-sense” should give pause. Just as Førland confidently proclaims the ubiquity across time of the practical syllogism, Carr with equal confidence asserts that “the narrative mode is very close

in form to the structure of action itself.” But what could possibly count as revealing “the structure of action itself”? Since “action” has for some time now been a term of philosophical art, the question might be taken as conceptual, not empirical. But the paradox of analysis suggests that it would be no easier to demonstrate the rightness of a conceptual analysis than it would be to determine what evidence could settle the issue. Whether analytic or synthetic, there exists as yet no way of ascertaining what answers to “the structure of action itself.” If all agents share in this, in what do they share?

Carr’s “defense” of narrative rejects the view that he attributes to theorists such as Hayden White to the effect that narrative structure reflects only “an autonomous mental or cultural realm that has no roots or connections beyond itself” (Carr, 29). But this misleads, for what Carr actually believes the theorists he criticizes overlook concerns the homology of structure between narrative and action. “Storytelling obeys rules that are imbedded in action itself It is because of this closeness of structure between human action and narrative that we can genuinely be said to explain an action by telling a story about it” (Carr, 29). But of what does the “sameness” consist for the asserted homology? At one point, Carr suggests it is homologous to an agent’s self-understanding—similar to the very story the agent might tell. Yet he goes on to state that the explanatory narrative may in fact deviate substantially from a first-person account. “The explanatory story . . . may be very different in many respects from the initial agent’s story” (Carr, 29). Nonetheless, Carr insists, what they share is “sameness of form” (for example, Carr, 29). But according to his previous remarks, the homology—the sameness of form on which his explanatory account depends—consisted in the form of “the action itself.” But if the stories deviate substantially in substance, in what does the homology now consist? For nothing appears to answer to “the structure of action itself”: there exist only the various tellings about it. But if behavior can be differently described, claims to sameness and so homologies ring hollow, for there exists no action *per se* against which to assess the claims Carr makes about it.

What has structure involves the stories told about what a person did. But an action, by the usual definition, consists of a bit of behavior and a belief/desire component. Change the belief/desire component, you change the action. But if this is so, what connects the narrative structure with something “beyond itself”? On Carr’s own account, narrative explanation “is located on a continuum of repeatedly revised explanations, understandings, and interpretations that is part of life itself” (Carr, 30). But then the difference between an imposition of structure and the discovery of one has disappeared.

The homology claim dogmatically insists that human understanding of action must reflect something beyond its articulated structure—the so-called “structure of action in itself.” But nothing answers to that on which the dogma insists—something prenarratively given and shared as part of common understanding that narrative then represents. For Carr’s analysis to work he would need to show how one can make sense of the “structure-of-action-in-itself” in order to establish that common sense had a hold on some *thing*. But on Carr’s own account, actions are an artifact of the stories told about them. In no other area of empirical inquiry

would a homology claim be allowed to pass when one of the relata cannot be identified or examined except through what is supposedly distinct from it.

Nothing in Carr's argument establishes narrative as a mode of explanation. Indeed, as the preceding discussion indicates, his exposition works only to assimilate it to the very "common-sense and easy explanations" against which he cautions. In addition, his survey of how the notion of narrative has been viewed both historically and within the context of contemporary theorists demonstrates that no consensus exists either that narrative constitutes a form of explanation or, if it does, in virtue of what it explains.⁹

Indeed, despite his ostensible emphasis on narrative, the dogma of common-sense explanation—what all the folk know and how they think—pervades Carr's account. For he legitimates his emphasis on narrative by the status he assigns it as the way in which people think about their acts. "[T]he narrative mode is very close in form to the structure of action itself, from the agent's point of view" (Carr, 20). This repeats the assertion already noted in Følrand and echoed by Stueber regarding the "natural" or "normal" or "common-sensical" account of action explanations.¹⁰ But once one moves beyond philosophers who take the dogmas of their discipline too much for granted, evidence for these philosophical assertions proves lacking.

Følrand worries about dogmatic methodological individualism. Unfortunately, his efforts to escape it lead only to a different but no less dogmatic view of how people must think and the group spirits that move them. Likewise, Carr would like to use the very ordinariness of narrative explanations as he understands them as a dam against a rising tide of reductionist explanations of action. This motivates his well-taken skepticism about endorsing in advance of inquiry some one form of explanation:

Perhaps the conclusion to be drawn is that it belongs to the spirit of inquiry to be skeptical of common-sense and easy explanations, both in general and in particular; but discarding a mode of explanation simply because it does not fit an *a priori* ontological mold is not truly scientific. Thus if we depart from a common-sense mode of explanation, such

9. Carr appropriately distinguishes two questions—one concerning what factors make an explanation psychologically satisfying as distinguished from those that make a theory explanatory. However, he supplies exactly the same answer to both questions. With regard to why a narrative explanation might be taken as psychologically satisfying, he suggests that this results from the way it assimilates the problematic to the familiar: "the narrative explanation is satisfying precisely because it never strays far from ordinary discourse. . . . [I]ts proximity in form and style to our day-to-day dealing in human situations lends it an air of familiarity that we may find comforting" (Carr, 21-22). But then he goes on to assert that the "familiarity of the context of narrative helps answer our second question: how does narrative explain?" (Carr, 22). Like Stueber, he insists that narratives cannot explain actions by appeal to any old generalization. They must be generalizations that serve as "appropriate" motives for action. What constitutes the test for appropriateness? Carr ties this to his understanding of agency. In particular, any form of reductionism must be excluded from the explanation of action. Stueber, more cautious, simply informs us that "reenactive empathy" will do the requisite sorting of relevant generalizations for us.

10. As Carr writes later, "A second thing that stands out about this explanation is that it is perfectly in line with everyday discourse and 'common sense.' These are slippery terms, but I mean by them to say that the explanation reflects the way we talk about our own actions and those of others as we deal in the ordinary way with the world around us" (Carr, 21). The scope of "we" here needs to be established and its actual vernacular use determined, not assumed.

as narrative explanation, in favor of another model, we had better have good reasons for doing so. (Carr, 28)

But wait. How does the “thus” work its way into this passage? The conclusion to be drawn from Carr’s own remarks, I would have thought, would consist of a caution distilled into the advice noted at the outset: Avoid theories of explanation; seek only theories that explain.¹¹

Stueber’s lapse into dogmatism about what “everyone shares” begins with his insistent use of a group consciousness that has no demonstrated empirical basis, as when he writes repeatedly of “*our* empathetic ability to reenact another person’s reasons” (Stueber, 34) or “*our* basic mode of understanding another person’s reasons through empathic reenactment” (Stueber, 34, emphases added). Ironically, Stueber concedes the very point noted in this complaint, namely, that there exists no general theory linking reasons and actions.¹² But he attempts to make a virtue of this necessity:

Yet as Jane Heal in particular has pointed out, it is very unlikely that we possess any general theory that allows us to decide which of the myriad beliefs we and other people have are relevant to consider in a particular situation. Our only option is to use our own cognitive capacities and to put ourselves imaginatively in their shoes in order to grasp their thoughts as their reasons. (Stueber, 36)

So, on Stueber’s own view, folk psychology does *not* provide a theory—a determinate inferential link—connecting reasons and actions. Fortunately for us, “our” empathetic abilities bridge this gap.

The process of linking beliefs and behavior must place the beliefs in question into a large epistemic context: “Suffice it to say that thoughts can function as reasons only in the context of other relevant thoughts an agent has” (Stueber, 36). Stueber’s recourse here involves the claim that empathy comes in to permit us to recognize what counts as a good or appropriate reason, one that accords with “our ordinary concept of causation” (Stueber, 37, note 16). But how exactly does empathy or common sense enter into the story? The vexed point in Stueber’s account concerns just the alleged difference between having a reason and having a reason that folk psychology informs us provides a good or sufficient reason for action. This leads to the following paradox: If “sufficient” means “logically sufficient,” a need for empathy remains undemonstrated; if it means “works for me,” empathy proves unexplanatory.

11. In correspondence, Carr expresses a worry that I have in all three cases set up straw men, specifically in working a supposed contrast between folk and scientific psychology. Only the latter would be explanatory, he reads me as maintaining. Thus I appear no less dogmatic than those more sympathetic to folk psychologizing as explanatory. But I do not take this alleged contrast to be my complaint. What I ask for but do not find is some evidence that the sort of explanations offered do in fact explain. Various paradigms of explanation exist. In this piece I mention two—Daniel Goldhagen’s and Christopher Browning’s. Both ground their explanations in patterns established independently of their own imaginative reconstruction of events. I remain open to any explanatory account. I only insist that what makes a narrative explanatory must be established empirically, not “conceptually,” whatever that might be thought to mean.

12. “Understanding another person’s reasons does not consist of an inferential procedure involving the use of a folk-psychological theory and its relevant generalizations” (Stueber, 35-36).

This paradoxical result develops in the following way. Empathy has a role because of a supposed inferential gap that must be bridged between all available reasons and actions. Various reasons, including those imbedded in various folk-psychological generalizations, might offer themselves as possible explanations of an action. But which of these explains the act? According to Stueber, a need for empathy arises precisely in order to discriminate which among the possible reasons the actual reason might be: “reenactive empathy is required in order to determine whether another person’s thoughts can also be his or her reasons for acting” (Stueber, 39). But how can this happen? On the one hand, Stueber maintains that “One needs only to admit that the information necessary and sufficient for showing that a folk-psychological explanation is epistemically sanctioned contains such generalizations” (Stueber, 38). The generalization must necessarily be *folk-psychological* in order to be an explanation of *action*; yet, in order to be an *explanation* of action, the specific action must be an instance of folk-psychological *generalization*. Since the generalizations that concern Stueber have the logical form of conditionals, the only apparent epistemic sanction to be satisfied would be whether or not the antecedent conditions have been fulfilled. This would deductively entail the consequent, and that would be as strong an epistemic sanction as any philosopher could hope to have. Given a generalization but absent the antecedent conditions, nothing sanctions deeming a generalization explanatory for a specific event.

Yet on the other hand, Stueber also insists that the “information that is necessary and sufficient for justifying an action explanation has to be rich enough in order to enable us to recognize the mental states mentioned in the explanation as an agent’s reasons for his actions” (Stueber, 39). These he explicitly relativizes to specific agents (39). But then what could possibly be the role of generalization here, folk-psychological or otherwise? In addition, what could an appeal to empathy possibly add? If the explanation of action can be fleshed out to show how a generalization applies, the usual logic works. If the logic cannot be applied, how could empathy help? Given a generalization of the usual logical form and information satisfying the antecedent conditions, explanation results. Absent a generalization, how does explanation take place? Either logic does the work needed or nothing gets explained.

The structure of explanation that Stueber wants to employ has many similarities to the structure Følrand suggests. But instead of an action explanation *qua* practical syllogism that belongs to an ideal explanatory text *à la* Railton, Stueber imbeds his account in Woodward’s model of explanation. Woodward’s model requires invariant generalizations sufficiently robust to support counterfactuals. Again, this model draws its typical examples from the natural sciences. How well does it work for the sort of historical cases to which Stueber would apply it? One complication is that the model relativizes the invariance to a domain. Stueber has already committed himself to the view that the relevance of a folk-psychological generalization must be determined relative to the individual to whom one applies it. So it now looks like Stueber requires domains specific to each individual. But what of larger groups, for example, nationalities? Here something other than toy examples would be needed. For any interesting explanatory controversy in

history, such as Goldhagen's and Browning's competing explanations of perpetrator behavior during the Holocaust, debate concerns precisely what domain explains—generalizations specific to the long-term history of Germany (Goldhagen), or relative to short-term conditioning of people in particular situations (Browning). Goldhagen, in fact, claims that the inability to empathize with perpetrators indicates that the explanation must be very culture-specific; Browning maintains that in like situations one would most likely act as the perpetrators did. Stueber's format offers no insight into where or how to apply empathy, or how to know if it has been rightly applied. Appeal to empathy to reveal what people do or do not share proves quite empty. "Empathy" seems only to name those prejudices about what to count as reasons with which one starts, not anything inquiry could possibly discover.

Stueber's model, like Følrand's and Carr's, makes the claimed necessary condition of action explanation simply analytic, and so empirically irrefutable. Since all explanation of actions will be *post hoc*, they will be impervious to refutation inasmuch as there could be no testing. Like Carr's appeal to the "structure of action in itself" as the objective reality represented by a correct narrative, Stueber's invocation of empathetically identifying the agent's point of view leaves one with no clue how to separate a point of view that makes sense from another's perspective and "the point of view of the agent." What in reality answers to that description? Empathy as Stueber imagines it relies on a contrast, but the very idea of an agent's point of view misses the precise point on which Stueber himself insists. There exists no other view until it can be made part of ours, and once made part of ours, in what sense was there "another point of view"? Stueber unwittingly embraces the paradox against which Donald Davidson warned: "we can understand others as people like ourselves who act for reasons" (Stueber, 40).

Actions that cry out for explanation belong to the class that falls outside what Stueber demarcates as the "proper explanatory domain of folk psychology." Perpetrator behavior represents a particularly dramatic instance of this sort of action. The central claim for Stueber's "engaged" conception of folk-psychological explanation has it "that thoughts can function as reasons only in the context of other relevant thoughts an agent has" (Stueber, 36). Appreciating the context allows another to empathize in Stueber's sense inasmuch as one recognizes another's "thoughts as thoughts that could be potentially our reasons if we were in their situation and shared their other beliefs, desires, and normative commitments" (Stueber, 36). Stueber's approach employs truisms such as these to generate a paradox: Historical agents cannot be understood in *our* terms because of their different temporal and cultural setting, but historical agents must be understood in terms of what *we* consider "good" reasons to act. Empathy supposedly bridges the divide between what cannot be our reasons and what must be, but Stueber nicely articulates what he takes to be the answer—and what I take to be the problem—when he writes:

We understand an agent's thoughts as reasons because we understand them, with the help of reenactive empathy, as in some sense comprehensible responses to the situation that the agent faced, and thus his acts as responsive to the dilemmas or problems that he thought he was facing and for which he thought his behavior constituted a solution. (Stueber, 41)

Against the background, however, of what Stueber has to say about the role of generalizations and the domain of inquiry, the very idea of the agent's point of view becomes this Carr-like *Ding-an-sich*, the unempirical legitimator of claims to explanatory adequacy. But since the only route to this "what is not us" lies through our own sense-making ways, any claim to having "reenacted" becomes an uncontested dogma of explanation, a product of the way a theory of explanation insists issues must be framed. Nothing in this account furthers inquiry. Its legitimacy consists solely and totally in its portrayal of what it more plausibly may be thought of as creating—the Other's point of view.

Folk psychology consists primarily in a loose collection of terms, the so-called propositional attitudes. Stueber, however, consistently writes as if there were a canonically recognized folk psychology, some collection of agreed-upon generalizations robust enough to explain behavior generally and across an interestingly wide swath of times and peoples. I do not myself know where these generalizations have been written, and Stueber does not tell us.

Let me distinguish between the claim that (i) there is no empirical evidence for use of folk-psychological terminology in attempting to explain other people's behavior, and (ii) there is no empirical evidence for those attempts to be genuinely explanatory.¹³ It is the second claim that concerns me here. For the nub of my charge is that all three "explanatory" accounts just represent philosophical dogmas rooted in the claim that practical syllogisms offer any explanatory purchase. For this I am aware of not a shred of empirical justification. The practical syllogism is an artifact of philosophers' attempts to rationalize action. Whether it has any explanatory purchase would have to be demonstrated, and I find it very difficult to even imagine what sort of experiments would validate the claim that the practical syllogism can do anything more than present to philosophers the appearance of rationalizing actions. The tendency or ability to project mental states to others strikes me as entirely beside the point. The point is to provide compelling evidence that people overwhelmingly act because of reasons, and that they typically frame these reasons as a practical syllogism. But rationalizing action is one thing; explaining it quite another. The symposiasts feel entitled to infer from the former to the latter. I want to know by what license.

It is one thing to point to a pervasive habit of explaining behavior in certain terms. It is quite another to document that these explanations have any value as explanations. I readily concede to Stueber, Carr, and Følrand the use of a certain vocabulary when speaking of others. I have caught myself doing so on many occasions. But what apart from habit recommends this vocabulary to any science as explanatory? Explanation by invoking the myth of the shared needs to be replaced by explanations that have empirical content.

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13. I owe this way of putting the distinction to Stueber. He made me aware that my argument seemed to vacillate between the two points, and that (ii) was the main point at issue. I remain uncertain about (i) as well, but it is not the critical point in this context.