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A Pragmatist Approach to Causality in Ethnography¹

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Drawing on early pragmatist theorizing, the authors propose three interrelated methodological activities for the construction of robust causal claims in ethnographic research. First, Charles S. Peirce's semiotic approach offers ethnographers a useful foundation for a mechanism-based approach to causality by tracing iterations of meaning-making-in-action. Second, taking advantage of the structure of Peirce's semiotics ethnographers can examine three forms of observed variation to distinguish regularly occurring causal sequences and temporally and spatially remote causal processes. Third, the authors emphasize that the standards to evaluate causal arguments—their plausibility and assessments of explanatory fit—are always made in relation to challenges provided within a disciplinary community of inquiry. The use-value of the pragmatic approach to causality is demonstrated with an explanation of the different reactions of parents and clinicians to positive newborn screening results.

Although most ethnographers, like other social scientists, routinely provide causal arguments, the criteria for identifying and spelling out causal pathways remain underspecified. Within ethnographic research in sociology,

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causal arguments are usually limited to variations of substantive themes that are embedded in different methodological traditions: many ethnographers trained in interactional traditions, methods of grounded theory, or analytic induction base causal claims on evidence of observed sequences of interactions in the field (see Glaser and Strauss 1967; Becker 1998; Katz 2001). Alternatively, ethnographers drawing from more structural literatures, such as the extended-case method, tend to assume general social forces external to their observations and then ask how action in the field shapes and is shaped by these causal forces (Wacquant 2002; Bourgois 2003; Burawoy 2009; Contreas 2012).

Whatever their provenance, all approaches to causality share common challenges: how to move from the messiness and abundance of empirical observations to a simplified causal explanation and how to demonstrate the influence of indirect, temporally and spatially removed, processes.² And while grounded theorists have focused on the former problem at the expense of being overly cautious of theorizing beyond the case, structural analyses often move too easily between observations and theorizing without specifying the processes that connect observations to larger social forces.

This article draws on pragmatist writings and processual approaches to causality to develop an alternative account of identifying and constructing causal explanations with ethnographic evidence. We propose three inter-related activities for identifying a causal explanation. *The first activity entails identifying a causal sequence based on meaning-making structures.* As a method of inquiry that rests on participation and close, detailed observation of people moving through their lives, ethnography has a unique ability to investigate unfolding moments of action. Building upon the semiotics of Charles Sanders Peirce, we argue that ethnographers should trace processes of meaning-making-in-action. The sequential process in which current meanings build further on previous meanings not only opens an analytical vantage point for the ethnographic study of causality but also allows ethnographers to systematically simplify messy, abundant empirical materials.

The second activity is to iteratively rework the proposed explanation through an examination of variation. A semiotic chain of action in a single observed instance does not necessarily constitute a generalizable causal explanation. To firm up the causal explanation, ethnographers are able to take advantage of the structure and temporal dimension of meaning making to generalize across observed variation and to recursively rework their proposed causal explanation. We suggest that ethnographers systematically examine

²These problems are especially acute in the social sciences, where the causes we present are never necessary, and seldom sufficient. As J. L. Mackie (1965) claims, this form of causality may be described as “INUS conditions,” which are situations in which causes are important, but no pattern in the data is necessary for an outcome, and no aspect of a given pattern is sufficient by itself (see also Mahoney 2008).

three forms of observed variation in order to buttress their causal claims. These forms of variation include differences among prototheoretically similar situations, or *data set variation*, variation in *meaning making over time*, and *intersituational variation*, where different kinds of actions across locales and interaction patterns are related to a common causal explanation. Checking for variations provides insight into the common resources and structural conditions. The result is a causal account that may include temporally and spatially removed processes without resorting to “invisible” social forces.

The third interlinked activity consists of engaging the proposed causal explanation within a broader intellectual community. Following pragmatist insights, we add that the usefulness of these forms of variation as evidence for causal explanations is always tied to an attempt to convince a “community of inquiry” that a particular account fits observations and that the proposed causal explanation is better than plausible alternatives. The power of a causal explanation is relative to alternative explanations that the ethnographer can expect various audiences to raise. The added value of a theory resides in the way it is taken up by others and makes a difference within communities of inquiry.

The intellectual contribution of our pragmatist approach to causality in ethnography is threefold. First, we ground causality in the construction of a temporal generalization anchored in actors’ observed meaning-making processes, capitalizing on both the methodological strength of ethnography and the strength of pragmatist semiotics. This approach does *not* imply a “first-person” approach to explanation, where we privilege actors’ reasoning. Instead, their *actions* (whether verbal, cognitive, or otherwise), as ethnographically observed, form the bedrock of analysis. Second, although past ethnographers have focused on forms of data set variation and causal development over time, our approach to causality emphasizes the role of *intersituational variation*—a form of variation common in ethnographic research but seldom explicitly theorized in ethnographic accounts of causality. Third, we emphasize that the plausibility of causal arguments and assessments of their explanatory fit are always made in relation to alternatives provided within a disciplinary community of inquiry. Unlike the tenets of grounded theory, the power of a causal account is thus inextricably tied to close familiarity with scholarship rather than being drawn only from evidence within a single study. And in contrast to some methodological logics, such as the extended case method, which is partial to neo-Marxist theories (Burawoy 2009), the criteria proposed by this pragmatist approach to causality are theoretically flexible, unifying a fragmented field around evaluative criteria.

We develop these pragmatist insights into the structure of causal explanations in two parts: we begin with a pragmatist account of meaning making, to show how such a definition of meaning making provides grounds for examining forms of ethnographic variation and explain the *plausibility* and

fit of a causal explanation. We then employ ethnographic data from a newborn screening study to show the difference that taking such a route provides. Starting from a puzzling interaction, we contrast the research subjects' causal explanations, as well as other possible causal alternatives provided by the sociological community of inquiry, with the ethnographer's construction of a causal argument that cuts across observations.

PRAGMATIST SEMIOTICS AS A MECHANISM-BASED APPROACH TO CAUSALITY

The philosopher of science Julian Reiss (2009) noted that various working definitions of "causality" exist in the social sciences, each compatible with particular forms of evidence and disciplinary aims. Thus, for example, econometricians tend to treat the notion of causality as synonymous with prediction, while for many "large-*N*" researchers causality is a matter of establishing a regularity over time.³ Because of ethnography's strength in capturing unfolding meaning-making processes (Emerson, Fretz, and Shaw 2011, p. 1), causal explanations based on ethnographic evidence are most amenable to *mechanism-based accounts*, which explain how a social phenomenon came into being or acts (see also Hedström and Ylikoski 2010).⁴

In an influential essay on mechanistic causality, Peter Machamer, Lindley Darden, and Carl F. Craver (2000, p. 3) define mechanisms as "entities and activities organized such that they are productive of regular changes from start or set-up to finish or termination conditions." Mechanism-based accounts assume that explanations can be decomposed into parts and thus specify generalizable processual links by showing how, continuously, an explanandum leads to an explanans. Importantly, mechanisms gain explanatory power when processes occurring on a lower order of aggregation can be shown to explain how something was produced on a higher level of aggregation or abstraction (see also Machamer et al. 2000, p. 13; Stinchcombe 1998, p. 267). Indeed, Gross (2009, p. 363) observes that "all work on social mechanisms assumes that mechanisms are the gears in some social machinery and thus stand in a relationship of lesser to greater vis-à-vis the causal effect they bring about."

³Reiss distinguishes between counterfactual accounts, regularity accounts, mechanistic accounts, and interventionist accounts. While we argue that ethnography is best positioned for a mechanistic account, principles of the counterfactuals approach, as we develop below in our discussion of variation (and, to a lesser extent, a regularity approach) complement the mechanism-based approach in important junctures.

⁴Note that *mechanism* is used here metaphorically to refer to potentially generalizable processes and goes beyond the narrow connotation of a machine-like regularity. This loose sense also seems to be closer to the way Merton (1968) treated mechanisms in his discussion of the self-fulfilling prophecy, one of the key references for mechanistic accounts in the social sciences.

The level of aggregation deemed low enough in a mechanism-based account depends on the kinds of problems a discipline tends to contend with. As Machamer et al. (2000) argued, every scientific discipline has a relative “bottoming out” of lowest-level components constituting the elements for mechanisms. Sociologists constructing mechanistic explanations have mostly relied upon rational choice theory and other individual-based theorizations as such “bottoming out” components (see, e.g., Elster 1989; Hedström 2005). But these individualistic reductions, as many have argued in different contexts, have their problems. Assuming essential and presocial human nature and social action is problematic: people may act reasonably, but reason itself is defined socially (see, e.g., Bourdieu 1997; Whitford 2002) and emerges interactionally (see Garfinkel 1967; Blumer 1969).

Drawing on pragmatism, Gross has made the case for moments of social action—problem solving in social practice—as alternative foundational processes for mechanism-based explanation in the social sciences. Gross rightly assumes that the kinds of explanations social scientists look for are necessarily grounded in human action and (often) reflexivity. Gross’s pragmatist account of social action leads him to define social mechanisms as “composed of chains or aggregations of actors confronting problem situations and mobilizing more or less habitual responses” (Gross 2009, p. 368). Centering attention on habitual problem solving appropriately shifts the unit of analysis in the mechanisms literature to the moment of action, along with its situational and socially emergent features, without making untenable assumptions regarding human nature and rationality.

The notion of “habitual problem solving,” however, has its limitations as the “bottoming out” level for a sociological, and especially ethnographic, approach to mechanisms. Whereas mechanisms need to provide an intelligible language through which causal processes take place, the focus on problem solving leaves quite a few questions unanswered: How exactly *do* people solve problems? What are the differences between habitual and unreflexive versus creative and novel problem solving? Gross emphasizes that meaning making is central to any pragmatist theory of action; in fact, he says that a “mechanism is interpretive all the way down” (Gross 2009, p. 369; see also Reed 2011). And yet, Gross’s focus on subjects’ habits, collective habits, and bundled repertoires of “habit sets” (Gross 2009, pp. 370–71) as forms of social and epistemic culture and resources for action bypasses the pragmatist-semiotic theory of meaning precisely where it might be of most use to describe how meaning is made.

Because ethnography has a first-row perspective on meaning-making-in-action and because of the deeply reflexive nature of the method (Hammersley and Atkinson 2007; Burawoy 2009), we argue that a pragmatist-semiotic account of meaning making may offer a more fruitful foundational building block of social mechanisms than habitual problem solving. We

therefore suggest that the pragmatist attempt to look for social building blocks from which to construct an intelligible processual account can be aided by what Charles Sanders Peirce, the founder of pragmatism, termed his “semeiotics.” Rather than beginning from individuals’ choices, or even with habitual problem solving, we argue that it is useful to start with the process of meaning making—whether in its most creative or its most habitual form. By focusing on the structure of meaning making we argue that ethnographers can provide precisely the kind of *intelligible* and *continuous* pragmatic account.⁵

Although Peirce’s theory of signs is notoriously technical, the basic logic of his semiotics is straightforward (see Liszka 1996; Short 2007). As a way to ground the logic of scientific inquiry, Peirce broke down different aspects of meaning-making-in-action. In contrast to the later division of the sign into the signified and the signifier (Saussure [1916] 1986), Peirce devised a threefold partition. He wrote: “I define a sign as anything which is so determined by something else, called its Object, and so determines an effect upon a person, which effect I call its interpretant, that the latter is thereby mediately determined by the former” (Peirce 1992, 2:478).

Meaning making thus consists of three interlinked parts: a sign, an object, and an interpretant. The first of these elements is the *sign*, which we can think of as the signifier in the same way that smoke signifies a fire or that a word signifies a concept or object. The sign does not exist on its own but is always in relationship to an object. It is the utterance, pointing finger, picture, or whatever vehicle actors use to represent an object in a certain way. The second related element is then the *object*, any entity about which a sign signifies, an actual thing “out there” or an idea in our head.

Peirce’s key insight, however, was that meaning making is not an abstract but a practical achievement. To capture this point, Peirce argued that every act of meaning making includes an *interpretant*—the effect of the sign-object through which any act of meaning making receives its practical definition. The interpretant is a reaction that the interpreter undergoes while

⁵ Of course, even such a processual approach to causality, as John Dewey stressed, is a logical “slicing” of a continuously changing and complex world, a temporal stream in which “cause” and “effect” may be abstracted, but can never be neatly separated (Dewey 1929, 1938; Whitford 2002). A simple $C \rightarrow E$ form of causal analysis is thus problematic when it leads us to forget that as a continuous stream, events are both unique and unbounded, so that each element pertaining to that stream acquires its meaning from its context. In the apt metaphor of the philosopher James Bennett (1980, p. 228), whereas a discrete-event view of causality would lead us to think of the concatenation of cause and effect like a dominoes game, in which each domino takes down another, Dewey’s view of causality is much more like mixing spices in a recipe where different ingredients partly define the taste (see also Katz 2012a). And just as spices in a recipe give rise to a particular flavor, the interaction between elements of a situation is consequential for action.

making sense of a sign. Simply put, a sign is not a part of an act of signification unless it has some kind of effect—an understanding, emotion, or action. Temporal movement is inherent in the action of meaning making, and signifying is thus always thrust into the future.

Peirce then made a second crucial move: the interpretant is always potentially the sign for another iteration of meaning making. Whether we are alone or whether we are enmeshed in interaction with others, each understanding or action throws us into another round of meaning making. This is obviously true in a conversation with others, when the speaking turn of one party becomes the sign the other party acts upon, but it is even true in a soliloquy, when each thoughts gives rise to the next. Thus, a useful way to conceptualize meaning making is to draw out the *semiotic chains* through which it is constituted. Peircean semiotics can be imagined as a spiral of meaning making, where the interpretant of one iteration of meaning making may become the sign for another.

This account of meaning-making-in-action is one that ethnographic mechanism-based explanations could find extremely useful. It is a processual approach, which works on a low level of aggregation and which compels the researcher to construct a continuous, and intelligible, causal account. Additionally, focusing on meaning making, we move from the problematic attempt to ground mechanisms in the characteristics of agents (e.g., rationally choosing ones) to the characteristics of meaning making in action; we provide the “how” of both explicit problem-solving and habitually embodied action. And, perhaps most important, we present an account that ethnographers are well positioned to be able to trace. Since ethnographers actually follow meaning-making-in-action, they are able to provide compelling semiotic accounts of iterations of meaning making (Short 2007).

FROM SEMIOTICS TO VARIATION

Peirce’s semiotics provides us with an analytical tool set to trace sequences of action at a foundational level. However, as philosophers have already observed, the focus on mechanisms does not solve the metaphysical problems of causality, and needs recourse to other forms of causal inference (see, e.g., Woodward 2002). Semiotic chains of action shift the scale of phenomena: instead of looking at phenomena at high levels of abstraction and aggregation, they magnify the resolution of the researchers’ analytic and descriptive lenses. But how can researchers convince their readers that the components of the process they emphasize are indeed the critical ones for understanding the phenomenon they wish to explain? And, if researchers want to make a potentially generalizable point, how can they distinguish incidental from regularly occurring causal processes?

Yet another challenge is to show how the different components of a mechanism work together in a processual way. Peirce's semiotics does not tell us why, in a specific case, meaning making operates as it does, only how to think of meaning making's constitutive parts. When people act in different situations, they obviously react to what is being overtly said and seen (the sign-object) but they are also influenced and take into account other, often more difficult to directly perceive, aspects that are explicitly or implicitly invoked within the situation—their histories, idiocultures (Fine 1979), ideological determinations, and so on—what pragmatists termed their “habits of thought and action” (Peirce 1992; see also Gross 2009; Kilpinen 2009). The effect of the sign-object in action, the interpretant, is thus always more than the sum of its overt “stimuli.” In essence, the challenge is still that of rendering an inherently “invisible” causal explanation visible. This is both because causality is never directly evidenced (Hume [1740] 1967), and as the interpretant is never directly formed by the sign-object but also by characteristics of the interpreter and the relationship between the actor and the situation.

We can, however, strengthen our mechanistic explanation to gain more confidence in a proposed causal account. Machamer et al. (2000, p. 13) highlighted that mechanisms produce “regular” changes. To figure out what constitutes an accidental or a regular aspect of a chain of causation, ethnographers look for similarities and differences among semiotic chains. Ethnographers systematically examine action across observations, arguing that an observation is “*a case of . . .*” a larger universe of observations that share a similar causal structure (see Becker and Ragin 1992).⁶ As each instance is seen as one piece of evidence for a generalizable semiotic chain, the very definition of the ethnographic object recursively shifts (see also Lakatos 1976; Katz 2001). This iterative process of redefinition provides insight into the workings of the proposed mechanism as it brings into purview salient resources and shared understandings in some cases but not in others. Ethnographers thus describe continuous and intelligible causal processes but also need to buttress the strength of their causal explanations by accounting for *variation* among different instances of a particular semiotic operation.

Variation, as many have observed, depends on the notion of a “set” (see, e.g., Goertz and Mahoney 2012). Only when a common question or characteristic already defines observations as comparable cases does it make sense to compare them in the first place. But the very architecture of a set is rooted in the researcher's theoretical assumptions. Here we develop three

⁶In that regard, mechanistic and regularity based notions of causality are complementary.

kinds of variation suggestive of causal explanations drawn from Peirce's semiotics: data set variation looks at multiple instances of semiotic gestalts, variation in time examines changes across a semiotic chain, and intersituational variation holds one aspect of the semiotic whole constant while examining its consequences in different cases.⁷

Data Set Variation

The first form of variation that is available to ethnographers, and the one that has received the most analytic attention, is what we term *data set variation*. Here, the ethnographer collects situations that seem—either proto-theoretically or because of previous research—to be instances of “the same thing.”

The broad semiotic set of sign-object-interpretant is assumed to be constant; we observe more of the same situation. Having defined the set of situations, ethnographers proceed to sift through their similarities and differences that lead to various consequences, double-fitting observations and causal explanations as they go—and often changing their assumptions about what is, or is not “similar” as they go along. Thus, although ethnography may be a “small *N* method” (Small 2009), any ethnography contains a large number of similar cases within the purview of the study. The logic of data set variation shares much with Mill's ([1843] 2002) methods of agreement and of difference to assess causality: interactions and situations are compared in order to see how specific differences in the situation lead to different outcomes.⁸ To the extent that ethnographers are interested in one kind of situation—be it “getting pissed off” while driving (Katz 1999), prison wardens typifying incoming prisoners as Black, White, or Hispanic (Goodman 2008), or how visibly disabled people manage interaction with “normals” (Davis 1961)—it is the data set variation among similar cases that is crucial for the construction of the causal claim.

Although ethnographers seldom formalize such variations, this form of causality construction shares much with counterfactual logic and Charles

⁷ While there may be other ways of addressing variation, we believe that these three forms account for variation used in contemporary ethnography. A multisited ethnography (see Marcus 1995), for example, is a combination of data set variation and intersituational variation; an ethnographic revisit (Burawoy 2003) is a special instance of layering data set variation and variation over time; a comparative ethnography is no longer a separate “kind” of ethnography because every ethnography has a comparative component, either internally based on variation among observations or by research design when comparing various research sites.

⁸ One critical difference is that Mill presumes an already constructed set while in ethnographic data analysis defining the relevant parameters of the set is a key analytical task.

Ragin's (1987, 2000) qualitative comparative analysis (QCA). In those instances, researchers compare observations following a subjunctive conditional, a *what if* logic. While eschewing counterfactual logic's emphasis on necessary conditions, ethnographers often "retrodict" from their observations, claiming that within a certain pattern of action, if X had been observed, then Y would have happened (see Katz 2001). And, in ethnography as in QCA, different patterns of meaning-making processes and compared and sufficient conditions are sought. Thus, while ethnographers are reluctant to assign a binary tag (Ragin 1987) or a numeric grade to an observation as in later iterations of QCA (Ragin 2000), they continuously ask, How similar is this situation to another situation in which a specific outcome was evident?

Variation over Time

A second form of variation relates to meaning-making instances *over time*. The ethnographer begins with the insight that meaning-making is an ongoing activity, where one iteration of meaning making influences how meaning is made next—where one interpretant becomes the sign of the next cycle. This is a form of variation particularly suited to the craft of ethnography. Ethnographers typically spend an extended time in the field and gather historical data alongside interactional observations. Ethnographers may spend years with the same people, observing how they make and remake sense of their worlds as they move within a specific social career (Becker 1952; Strauss 1993) or even across generations (see, e.g., Black 2010; Smith 2006).

To account for variation over time, the causal explanation must explain not only actions at a specific point of time but also transformations of meaning making. Like historical sociologists, ethnographers are thus interested in path dependency of trajectories (Mahoney 2000), in the social structure of turning points (Abbott 1997), and in tracing events over time (Mahoney 2012). Relying on an assessment of salient differences and agreements, they assess causality by examining how processes change over time and across observations.

Variation over time highlights the limits of basing causal claims solely on data set variation. Most ethnographic causal explanations simultaneously account for data set variation of situations at each point in time and for change over time. The resulting set of observations, however, is not a qualitative "panel study" in which data set variation in two points of time are compared. This is not only because ethnographers attempt to follow actors and meaning making more or less continuously, but also because the kinds of situations that they observe as time goes by change as well. For example,

the challenges that a social movement faces in its first months are different than those one year later when it must decide whether to institutionalize a specific identity (Blee 2012). Religious converts feel the presence of the divine differently, and in different situations, as they settle into their new religious lives (Tavory and Winchester 2012). Looking at action over longer temporal arcs thus pushes ethnographers to look at what may seem like “apples and oranges” to the quantitative methodologist.

Intersituational Variation

Intersituational variation constitutes the calling card of ethnographic research, a form of variation that is harder to construct in other methodologies. Here, the researcher collects actors’—and not necessarily the same actors’—actions in different settings and situations and shows that seemingly unrelated actions make sense as a single set under the researcher’s theoretical description. In other words, the researcher keeps one aspect of the meaning-making process constant to examine how different situations are refracted through—or transformed by—these semiotic aspects.

Thus, for example, researchers can follow an object across situations (see, e.g., Tsing 2005), or look at how actors’ habits of thought and action structure different situations they may encounter. As opposed to data set variation, the search for intersituational variation partly rests on the prototypical understanding that the situations the researcher collects into a set are *not* similar; and, as opposed to variation over time, the ethnographer also does not assume that meaning making in one situation structures meaning making in others. Rather, the situations are made comparable through the ways in which a shared characteristic affects a theoretically constructed causal explanation. Most paradigmatic urban ethnographies—from Whyte’s (1943) *Street Corner Society* to Liebow’s (1967) *Tally Corner* and Duneier’s (1999) *Sidewalk*—describe their protagonists as they navigate different kinds of situations.

A causal account emerges largely through the ways in which the ethnographer accounts for *different* interactional outcomes that arise in *different* situations. Thus, one of the most important observations in Liebow’s analysis of the fractured lives of inner-city black men is the ways in which they negotiated relationships differently with other men “on the corner” and with their female partners. The failed attempt to live up to a standard of masculinity gains explanatory breadth when we see the men bragging about their sexual prowess and manipulative attitude toward women among their peers and when we simultaneously witness their relationships fall apart because they feel they cannot provide for their female partners. Positing the attempts to generate a competent masculine persona in the

face of structural discrimination as his causal engine, Liebow can thus account not only for similarities among the actions of different people on the corner over time and employment situations, but also for how they make sense of their lives across situations, even if their actions may seem to be, at first, contradictory.⁹

Except for relatively straightforward cases—where, for example, class position (e.g., Bourdieu 1984) or personality structure (e.g., Stouffer 1955) are already supposed to affect a range of tastes or activities—ethnographic intersituational variation is difficult to formalize, as it groups heterogeneous situations. The ethnography-specific causal theorization is precisely what makes the different situations and observations comparable. If we argue, for example, that there is a distinct “code of the street” (Anderson 1999) that governs inner-city black men’s actions, we expect to see such a code in action, among other situations, in confrontations on the street, in job interviews, and in ways of seeing imaginary slights in mundane interactions. This does *not* mean, however, that men would behave similarly in these diverse situations, but that it is precisely the diversity of the forms and outcomes of interactions in different settings that the causal claim should explain. As opposed to the variation between obviously similar situations and actions, intersituational variation makes sense only under a specific causal description.

Defining and exploring variation is central to developing an ethnographic causal account. Through variation, ethnographers isolate analytically important elements in actors’ meaning-making process within their case. Ethnographers also come to recursively define the case by linking it through recurring elements—both discovering an account and justifying it. Still, even layering a mechanism-based semiotic approach and subjecting it to the tests of variation is insufficient for a convincing causal explanation. While semiotic specification and variation may increase our confidence in a causal claim, there are inevitably multiple causal explanations possible for configurations of observed variation. How can researchers sort these alternatives? The question, then, becomes how to evaluate the ethnographic

⁹This example also elucidates why some of the most compelling evidence for causal explanations in ethnography often comes from observations of unusual interactions, of situations that break down or interactions that go awry—whether or not they occur because of a direct manipulation by the researcher, because “shit happens,” or because of some structural reason that makes the ethnographers’ interlocutors prone to such situations (see Hughes 1945; Merton 1976). Often, these situations require actors to make their usually implicit forms of meaning making explicit, both for themselves and for others in the situation (see, e.g., Garfinkel 1967). But even if the taken for granted is *not* made explicit, these situations provide ethnographers with a new situation that they can examine, a new interpretant that can be linked to other meaning-making occasions.

causal account's explanatory fit and how to judge this account in relation to other plausible alternatives.

PLAUSIBILITY, FIT, AND USE

A causal explanation's *plausibility* refers to the extent to which there are alternative accounts that do a better job of accounting for observations. *Fit* reflects the extent to which a causal claim is backed up with the observations that the researcher presents. Questions of fit and plausibility may seem easily solved when causal explanation is established from variation in making-making processes within the research project. And yet, even in the most meticulous ethnography, both plausibility and fit cannot be incontrovertibly established. The problem of demarcating the realm of plausible alternatives is that there are, potentially, always an infinite number of such explanations. What, then, makes certain alternatives more plausible than those that were not accounted for? Similarly, the question of fit cannot be completely answered within the study: with enough intellectual gymnastics and the addition of qualifiers, we could probably fit any theory to any observation. How, then, does an ethnographer make a case that his or her causal account is plausible and that the fit between a causal explanation and observed forms of variation is convincing?

Although some alternative causal explanations can be easily rejected if we look at the study's variation, ethnographers cannot spend all their time discarding alternatives: the work would be endless, and the added value, it seems, very limited. Of course, many possible alternatives can be pushed aside through scientific boundary work (Gieryn 1983) or by relying upon demarcation criteria provided by philosophers of science (see, e.g., Popper 1963). Thus, ethnographers do not usually feel that they need to engage in psychoanalytic debates or to address explanations that can be neither falsified nor buttressed by their use of observed variation. Implementing such demarcation criteria, however, is seldom straightforward (see Collins and Pinch 1993; Fleck 1981; Kuhn 1962; Lakatos 1970, 1976). How, then, should ethnographers settle upon the plausible alternatives they must take into account?

Pragmatists Wright, Peirce, and Dewey offered a solution to this question with their observations about the *community of inquiry*. Thus, as Peirce wrote about philosophy, "We individually cannot reasonably hope to attain the ultimate philosophy which we pursue; we can only seek it, therefore, for the *community* of philosophers. Hence, if disciplined and candid minds carefully examine a theory and refuse to accept it, this ought to create doubts in the mind of the author of the theory" (Peirce 1992, 1:29; emphasis in original). In this view, scientific work is always conducted in relation to the work of others engaged in similar problems; other social sci-

entists whom we recognize as sharing the same disciplinary concerns. Thus, although the perfect community of inquiry is a regulative ideal that extends into the indefinite future, we can practically define the plausible alternatives the ethnographer needs to consider as *explanations the ethnographer can expect to be evoked by readers*. Scientific work becomes more credible as part of an ongoing democratic shared project, through its engagement with the work of others who have already persuasively winnowed down possible explanations in their own work.

The “community” in the community of inquiry is not necessarily a congenial effort to reach a consensus (Farrell 2001). Rather, much like a conversation, it is made of people’s lively engagement with each other to become a relevant frame of reference (see also Sennett 2012). To enter into such conversation, in turn, means that ethnographers have to be deeply immersed in the intellectual debates within their discipline and to know the scholarly corpus well enough to gauge possible alternative causal explanations for observations—ignorance of sociological theories and empirical debates weakens the ethnographer’s causal account.

This heuristic for identifying plausible alternative explanations involves an additional wrinkle in ethnographic work. Ethnographers are exposed not only to their disciplinary community of inquiry but also to the explanations constructed by actors in the field. Ethnographers’ interlocutors have their own explanations about why they act the way they do, and why others act as they do. These explanations matter: Peirce’s community of inquiry is inclusive “with no prior ring-fencing of what counts as the community” (Misak 2013, p. 37). Still, scholars can put too much trust in these explanations. Taking the hermeneutic position that ethnographers mainly tell “stories about stories” (Geertz 1973) has blurred the difference between local actors’ causal accounts and social science explanations. As we show below, while ethnographers need to consider these local explanations as alternative claims, they are not beholden to those causal accounts (Katz 2012*b*).

If a community of inquiry makes alternatives plausible, we must still ask what are the criteria for *fit*. As philosophers of science Thomas Kuhn (1962) and Imre Lakatos (1970, 1976) cogently argued, with the addition of enough clauses and subclauses, a theory can absorb most observed variation, even when it may seem in retrospect to clearly contradict the theory. Ethnographers constantly build up additional post hoc explanations around their favorite theories; rather than reject a hypothesis, they often end up either amending their theory (see also Becker 1998; Katz 2001; Burawoy 2009) or deciding that the observation was not part of the universe of cases covered by the explanation’s generalization.

“Fit,” then, is not a given. Rather, demonstrating fit is part of ethnographic causality construction. Of course, ethnographers construct their causal accounts and theorizations of the field through their involvement

with the data they have been collecting, and in this sense, they have an “unfair advantage” of already double-fitting their causality accounts to observations (Katz 2001). But although double-fitting of observations and explanations is crucial in any qualitative work, the outcome of such fitting often raises doubts. Thus, at one extreme, such double-fitting often results in “conceptual stretching,” that is, “vague and amorphous conceptualizations” (Sartori 1970, p. 1036) that fit observations simply because they are couched in extremely wide generalities. On the other side of the spectrum, such double-fitting may result in a continuous addition of clauses and sub-clauses to the theory, so that the theoretical framework covers the empirical materials so precisely that it may become suspect as a post-hoc rationalization of a specific set of observations. As with the question of plausibility, establishing fit is organized within the research process, but also as ethnographers present their work to their peers who may question the presumed links between explanation and evidence and ask about the full range of variation in the study. Causal explanation and evidence then mutually constitute each other: not only does an explanation fit the evidence but the explanation may also call for additional evidence going beyond the scope of the original research project.

Peirce is clear about the limits of a community of inquiry. The community of inquiry does not establish truths but yields the current best opinion about evidence (Peirce 1992, pp. 109–24; Dewey 1938). Pragmatism, in Peirce’s view, values scientific inquiry as an ongoing mode of sorting theories and concepts as true or false. Science, Peirce wrote (quoted in Misak 2013, p. 34), “is not standing upon the bedrock of fact. It is walking upon a bog, and can only say, this ground seems to hold for the present. Here I will stay till it begins to give way.” In his theory of truth, Peirce held that a true belief would withstand doubt if we were to inquire as far as we fruitfully could in the matter and also that truth was an ongoing concern for a community of inquirers rather than for an individual researcher (Misak 2013, p. 37). The truth of a causal explanation in ethnography is judged according to its ability to cover different forms of variation found in the study, but also in relation to other plausible explanations, and their respective “fit” to the same set of observations.

Recognizing that plausibility and fit of a causal claim are constructed in relation to a community of inquiry presents both intellectual advantages and possible dangers. Its advantages are clear: in order to establish a compelling causal explanation, ethnographers must be well-read in multiple academic literatures. Ignorance is not simply counterproductive, but makes it harder for ethnographers to convince their community of inquiry of their research. The danger remains of creating an intellectual “echo-chamber,” which reestablishes the academic truisms of its time by repeating the same sets of plausible theorizations.

While this danger is real, it is assuaged by three considerations. First, not anything goes. Plausibility and fit are constructed also in relation to variations in observations and there is thus an empirical check on causal explanations as long as ethnographers show readers how causal claims depend on empirical evidence. Second, ethnographers need to contend not only with their academic audiences, but also with lay audiences in the field who may also challenge the plausibility and fit of academic work but are not beholden by academic fads (Katz 2012*b*). Last, as in any sustained conversation, communities of inquiry have established communication challenges to critically engage with ethnographic work, rather than repeat conversational tropes and truisms. Engagement through peer review, book reviews (see, e.g., Du-neier 2004; Wacquant 2002), presentations at conferences, informal sharing of drafts, job talks, and other modes of receiving feedback has both a disciplining and a constructive component; they may bring relevant plausible alternative explanations to ethnographer's attention or call researchers on unwarranted causal claims.

The pragmatist legacy of our perspective on causality in ethnography goes beyond semiotics and variation to engage the *use* of scientific inquiry. Because it is still possible to create causal explanations that remain overly descriptive, rely upon unverifiable theoretical notions, or produce abstract truisms with little intellectual added value at stake, a critical pragmatic test of a causal explanation's value lies in the difference it makes in the way it shifts the habits of thought and action within the community of inquiry.¹⁰ "We need to look at the upshot of our concepts," Peirce argued, "in order to rightly apprehend them." In the realm of scientific inquiry, this means that causal accounts need to be generative—they need to help others in one's community of inquiry think through their own work. Thus, the construction of plausibility and fit in a social context allows us to see why overly abstract and overly specific causal accounts would be spurned if the author tries to provide a mechanistic-processual account—a causal claim is not only evaluated by its plausibility and fit, but also, ultimately, by its use. While ethnographers may try to anticipate the uses of their work, in the end the use value of inquiry is determined by the ways in which that work is taken up, contested, or ignored.

NEWBORN SCREENING AND CAUSAL EVIDENCE

The intellectual "cash value" (James [1907] 1981) of the pragmatist approach is best demonstrated by an example of the construction of a causal explanation in an ethnographic study. The case study examines the inter-

¹⁰There is an extensive literature on what pragmatists mean with "use" (e.g., Joas 1996). As Gross (2009, p. 367) notes, use value goes beyond utility maximization to cover a broad range of puzzle-solving situations (see also Whitford 2002).

actions between families and genetic teams following the expansion of a public health program of newborn screening in 2005. Within 48 hours after birth, a health care provider collects a blood spot through a prick to the newborn's heel. The blood sample is sent to a laboratory where technicians determine the concentration of specific chemical compounds within the blood. If the value lies outside a predetermined normal range, metabolic disease is a possibility, and the child's pediatrician orders a follow-up test. If the results still suggest disease, the family is referred to a regional clinical center for further follow-up testing and, if indicated, treatment. The purpose of newborn screening is to use early identification to prevent the onset of diseases.

Positive newborn screening results (i.e., the screen flags a disease) are rare. Between July 7, 2005, and April 30, 2009, the state of California screened 2,105,119 newborns. The program referred 4,580 newborns (or 0.22%) to a metabolic clinic for follow up. Of those, 754 infants were diagnosed with a true positive. Expanded newborn screening thus identified one true positive screen for every 2,778 births. Of the infants referred to a metabolic clinic, 3,334 were not confirmed to have a disorder (Feuchtbau, Dowray, and Lorey 2010).¹¹ While rare, the potentially devastating consequences of metabolic disorders render the results deeply meaningful for parents and clinicians.

The study focused on the implementation of expanded newborn screening (Timmermans and Buchbinder 2013). The analysis is based on close observations of clinical interactions between parents and the genetics team, which consisted of four medical geneticists, a nurse practitioner clinical coordinator, a dietitian, and a social worker. The ethnographers followed parents during clinic visits over a three-year period (October 2007–July 2010). In the metabolic genetics clinic, they audiotaped consultations between parents and the staff with a research team member present to observe the interaction and take ethnographic field notes. In addition, the researchers attended weekly staff meetings, consulted patient records, and interviewed families in the home and in the clinic. The families of 75 patients participated in the study. The ethnographers recorded a total of 193 patient visits, with one to twelve visits recorded per patient. In fourteen of the families, Spanish was the primary language. The project received IRB approval.

The Semiotics of a Puzzling Interaction

The choice of a field note launches the realm of possible explanations. Still, because we will be comparing this note with others when examining var-

¹¹ The actual false positive rate would be higher than this number, since many false positives are eliminated before they are referred to the specialty follow-up center.

iation, initial choices are not necessarily binding for a causal account. Here, we start with a puzzling interaction where the meaning of screening results is at stake.¹² In this particular situation, Stefan Timmermans entered the patient room with a geneticist, Dr. Silverman, who told him beforehand that the patient—four-month-old Michael—was picked up by newborn screening for a condition called Medium Chain Acyl-CoA Dehydrogenase Deficiency Disorder (MCADD) but that the screening result was likely a false positive. The geneticist planned to discharge the family from the clinic during the visit. Because most families hope for a healthy child, he expected the meeting to go smoothly. Instead, several interactional misfits occurred.

In the small patient room, the geneticist found a family of four. Strollers and diaper bags took up much of the space. The mother, Sarah, picked her talkative two-year-old daughter in her arms while the father, John, had Michael sleeping in his lap. After some short introductions, the geneticist mentioned the purpose of the visit. He prefaced the conversation with “You know that one of the down sides of [newborn screening] is that it causes great anxiety. False positives cause great anxiety. So, here’s what the original thing was: you had two abnormal metabolites.” Dr. Silverman explained that those elevated metabolites indicate two diseases, GA1 and MCADD. John interrupted: “What is GA1?” The physician replied: “Glutaric Acidemia-type 1. And that . . .” John interrupted again: “And so is this something new, that?” The geneticist attempted to clarify: “No, it was there right from day one of the newborn screening. Now that’s not a great condition to have. Although, the truth is that when we have remodel cases [i.e., retest], they may be noncases.” When the father protested that he never heard about GA1, the physician tried to reassure him that MCADD is their major concern. When he went over the follow-up testing results for MCADD, he concluded that Michael probably “is a carrier for MCADD. Do you know, probably one in forty people or one in thirty people are carriers? And it’s not a big deal.” Sarah, however, started to cry, sobbing “But it’s children who might have it.”

From both parties’ points of view, this interaction took several unexpected turns. The clinician anticipated dismissing the family from the clinic as likely false positive while the parents expected an update on follow-up test results that hopefully would give Michael a clean bill of health. The parents were surprised that Michael had screened positive for two conditions when they had been told only about one. The physician did not un-

¹² The choice of this incident is partly due to the drama of the tears and a reaction that seemed unexpected. At the same time, the initial choice already anticipates an academic payoff of explaining this kind of conflict. A causal explanation promises to engage scholars interested in the sociology of diagnosis, in genetic technologies, and in patient-doctor interaction.

derstand why the family reacted so emotionally to what should be good news—that Michael was likely a carrier for MCADD and not affected with the condition.

The components of the semiotic process are easily apparent in this interaction. The critical target of signification is the newborn screening results (the signs) which, according to the physician, indicate an out-of-range value for two conditions (the object). The reaction in the interpreters or the interpretant is the surprise, alarm, concern, and worry in the case of the parents and a belief in a likely false positive for the physician. The parents' reaction became a new sign leading to a new interpretant in the form of frustration for the physician. The physician reacted then with more explanation about the frequency of carriers in the population, which did not have the desired effect when the mother burst out in tears (another interpretant for her and simultaneously a sign for the physician).

This semiotic reduction of a messy interaction produces a descriptive account of a puzzle requiring explanation and some hints of the ways that cause and effect may be connected. The outcome requiring explanation in this semiotic sequence is how parents and clinicians react to positive newborn screening results and how clinicians and parents resolve disagreements regarding newborn screening results that affect how to act on the child's best interest. This outcome presumes an earlier process that begins with awareness about the screening results. The task for a mechanism-based explanation is to determine how the disclosure of test results leads to the diverging reactions of parents and clinicians. The semiotic process also suggests that the different positions and habits of thought and action of parents and clinicians may explain the divergence. We are, however, still a long way from a convincing explanation. We do not know, for example, whether this excerpt exemplifies a common set of misunderstandings. We also cannot identify a causal "culprit"—the semiotic lower level of aggregation presents the contours of the puzzle.

Variation in Response to Positive Newborn Screening Results

The ethnographic database of dozens of similar semiotic chains of parents meeting with clinicians to discuss positive screening results helps situate the *data set variation* of responses and discord about the screening results. There was no difference in beginnings: due to the institutional arrangement of calling parents into the clinic only when their child tested positive, all the semiotic chains originated from the initial disclosure of results. Parents, however, varied in how seriously they took these screening results. John and Sarah's reaction of shock, urgency, and danger was the dominant response in clinic visits. But there were some exceptions: not all parents took the possibility of hidden disease seriously. There was also variation among agree-

ments and disagreements between clinicians and parents. The situation in which parents responded to the newborn screening results as an emergency while clinicians tended to offer reassurance is common, yet for other cases parents did not seem to take the condition as seriously as clinicians.

The ethnographers were also in a position to consider *variation over time*. In the newborn screening study, the ethnographers followed families over multiple clinic visits, observing shifts in meaning making. The observations showed extensive temporal variation in how parents responded to the follow-up testing results: after initial concern, some parents tended to be reassured after genetic testing, others were reluctant to give up the metabolic diagnosis after follow-up testing should have exonerated their child, and in still other families the follow-up testing confirmed a disease and parents embarked on a journey of symptom prevention and disease management.

The final form of variation is *intersituational*: by observing families in the clinic and interviewing them in home settings, and by following clinicians both in the clinic and among peers, the ethnography covers interactions in multiple settings at different time periods. It was sometimes striking how families acted differently in the home from in the clinic, where they only partially shared how they responded to the possibility of disease with clinicians. Often families took many more preventive steps than they reported to their doctors, although some families mentioned the difficulties of following diets or adding food supplements. Clinicians, in turn, tended to be more ambivalent about the urgency of clinical interventions when meeting with colleagues than when speaking to parents.

This bird's-eye perspective on variation nestles John and Sarah's observations alongside others, extends the range of outcomes, and centers the semiotic puzzle. The causal explanation needs not only to account for how the disclosure of screening results most often lead to different reactions between parents and clinicians but also to agreements in some situations, and changes over time and across spaces.

From Variation to Mechanism

Once variation has been mapped across observations, we may look in greater detail at patterns of similarities and differences across clusters of parents and clinicians to construct an explanation for the diagnostic uncertainty that some parents of newborns with a positive screen experienced so vividly.¹³ The proposed ethnographic explanation accounting for the data set, temporal, and intersituational variation in the study highlights *parents*

¹³ Due to space constraints, we skip the actual working with the data to come to an explanation, but see (Timmermans and Tavory 2012) for methodological guidance.

and clinicians' reaction to the absence of an interpretive frame. When all parties agreed that the child was sick or healthy, clinicians and parents knew how to handle the situation and disagreements were rare. Clinicians and parents agreed that a child was either already symptomatic for a condition, the test results unambiguously confirmed the presence of disease, or the tests clearly established a false positive screen. In the former situations, the frame was one of disease while in the latter the child was healthy—both interpretive frames (Goffman 1974) that people know how to act upon and align. The interesting divergences and semiotic changes over time and over place occurred when parents and clinicians faced a situation in which a child is potentially ill, but shows *no* symptoms and *may never* have any symptom. This situation is uncharted territory. When there is no actionable frame for diseased-but-now-basically-healthy status, parents tended to fall back on the frame of the child as diseased while professionals gravitated either to a likely false positive understanding or to a “carrier” understanding.

The differences in meaning making between clinicians and parents stem partly from participants' incorporating *different sources of information*. Ironically, part of the reason that parents tended to embrace a disease frame was because clinicians initially impressed upon them the urgency and danger of metabolic disorders, which was further confirmed when parents checked on-line or print information. For clinicians, however, the information available online and in textbooks was no longer accurate. They shared experiences via list-serves and conference meetings with geneticists, and the as-yet unpublished consensus was that newborn screening revealed that metabolic disorders are different entities from what was known before. The diseases are more common, have greater variation in severity, and may require different treatment approaches. But, and this is key, clinicians did not know for sure that the infants picked up in the early days of expanded newborn screening are truly nothing to worry about. Their hunch was that some newborn screening patients required a new interpretive frame, but during the observed interactions they lacked the tools to construct a new interpretation. Still, they tended to be more reassuring than alarming for the patients with ambiguous results, especially in conversations with colleagues. The study thus captured a period of biomedical uncertainty in which existing epistemic frameworks no longer fit the signals from the screening program, and geneticists gravitated to a new framework while parents held onto the familiar frame of disease, initially provided to them by clinicians who were attempting to get parents to take the test results seriously.

This explanation of parents' buy-into the medical frame is further confirmed when we examine an exceptional “negative” case. A salient aspect of the observations was that almost all parents went along with geneticists in framing their child as potentially “sick,” although no symptoms were

apparent. An exception, however, was a non-Hispanic white middle-class father who suspected that the geneticists were experimenting on his child who, in his opinion, looked and behaved “normal.” He expressed anger with the geneticists for needlessly worrying him, and he refused to commit to follow-up testing. Resistance against medicalization is common in health social movements (Brown and Zavestoski 2004; Epstein 1996) but was absent, except for this father, in the newborn screening study. This exceptional reaction thus underscored how parents generally gravitated towards an illness frame.

The reaction to the presence or absence of a disease-frame explanation accounts not only for the variation between physician and parents’ discordance across the sample but also for variation over time and across situations. Some families experiencing biomedical uncertainty, including Sarah and John, resolved their initial discordance and agreed with the clinician after the genetic test results offered definitive proof that the child was disease free. Other families, however, held on to the disease frame even after clinicians exhausted follow-up testing. Why would parents continue to treat their infant as diseased when the clinician was willing to declare the child healthy? The reason was that in these cases there was *no authoritative test* to exonerate the uncertainty. Instead, clinicians based their conclusion on the time that had passed without the onset of symptoms. The parents, in contrast, ascribed the lack of onset to preventive measures they had undertaken to keep their child healthy. They resisted giving up on the disease frame exactly because the preventive measures seemed to be working. For clinicians, the preventive measures were unnecessary. Hence, continued discordance.

The proposed causal mechanism rests thus on the reaction of parents to the absence or presence of a culturally available disease frame when their child tests positively for metabolic disorders. When children are considered sick but completely asymptomatic, the causal process evolves based on the sources of information available from clinicians, the Internet and other media, and follow-up tests. With Gross (2009), we could consider these interpretive frames as habits, but by decomposing the semiotic sequences of the clinic interactions we are able to provide a more fine-grained causal account of the conditions under which habits of thought and action become relevant and emerge—as in the physicians’ growing realization that the infants picked up by screening differ from familiar patient populations.

Plausible Alternatives: The Community of Inquiry

In order to assess the causal claim they develop, ethnographers need to take into account the plausible alternatives available in their community of in-

quiry. In this case, one common sociological explanation is that concern about infants is specific to *social class* (Lutfey and Freese 2005; Link and Phelan 1995). Working-class and impoverished parents, for example, may face more pressing issues and may remain skeptical of medical information. They may also be more fatalistic and accepting of their children's condition (Keeley, Wright, and Condit 2009). Middle- and upper-class parents have so much invested in their children that the possibility of disability is deeply alarming and upsetting (Green 2007; Landsman 2005; Leiter et al. 2004). Such a hypothetical alternative explanation builds further on the literature on how parenting differs by socioeconomic status as a form of data set variation (Irwin and Elley 2011; Lareau 2003).

Genetic metabolic disorders testing affects poor and wealthy alike and is thus a good case to examine differentiation in response along class lines. If we look at the study sample of 75 families, we find two small clusters of five to seven families who were less concerned about a positive newborn screening result. These clusters, however, do not coincide with a defined socioeconomic group. A first group consisted of some of the Hispanic families. They all encountered a geneticist who spoke only elementary Spanish or relied on a translator during the interaction. The reaction of the Spanish families who met with a native Spanish-speaking geneticist, however, was more in line with the responses of the English-speaking families. This suggests, instead, that the first geneticist was unable to communicate the possible ramifications of the positive newborn screen to the families. Indeed, follow-up interviews with several of these families confirmed that they had only a sketchy understanding of newborn screening.

The second cluster of exceptions is a heterogeneous group of families who already faced serious challenges, such as parenting an older child with a disability or dire financial problems involving jobs and housing. These families prioritized other consuming challenges over a positive newborn screen with the *possibility* of disability in a newborn. Here, a class component is definitely present among those facing financial struggles, but there are many exceptions, including impoverished agricultural workers who decided not to return to work to take care of an affected child. Some of the families with an older child with a disability, on the other hand, were solidly middle class.

Rather than supporting a causal explanation grounded in class, these two clusters allow us to amend recursively the proposed processual account. Besides information sources and follow-up test results, parents' reactions to the presence or absence of a clear interpretive frame also depend on *resources* needed to act upon a positive newborn screen, including a physician who is able to communicate with parents and the absence of pressing concerns requiring immediate attention. Variation is thus a consequence of available resources only some of which are economic: the default pattern is

for parents to take newborn screening results seriously. Uncertainty deeply affects parents across the socioeconomic spectrum.

Plausible Alternatives: Actors' Own Explanations

From the perspective of some hermeneutic theories of meaning, the ethnographic causal claim may be alarming. Instead of constructing dense explanations from actors' "first-person" interpretations of the situation, the theoretical narrative begins with actors' *failure* to align their interpretations. According to the pragmatist perspective we develop here, however, as long as the ethnographer carefully attends to actors' semiotic chains, the researcher is *not* limited to the ways people interpret their predicament. Like the ethnographer, actors in the field attempt to understand their circumstances causally, very often in generalized terms. Indeed, if anything, the stakes are higher for actors—these are, after all, their lives. Yet, ethnographers are not beholden to these indigenous explanations as final arbiters of causality. While such explanations must be taken into account as causal possibilities and cannot be ignored, they may not hold up as generalizable *causal* claims. We show this point with an elaboration of the newborn screening field notes.

Michael's parents brought up their own explanation for their reaction to Dr. Silverman's announcement. When the ethnographers interviewed the mother at a later date, she explained that Michael was a very different child from his sister, especially in his eating habits. An MCADD metabolic crisis is preventable if parents feed their children regularly. If they become sick, they have few reserves to compensate for the lack of nutrition and may enter a fatal metabolic crisis. The clinic staff had impressed on Sarah and John to feed Michael at two-hour intervals, and Sarah found similar admonitions on-line.

With most infants frequent feeding is not an issue, but Michael refused to eat. His birth weight was six pounds nine ounces but it dropped to six pounds within a week. Sarah spent her days feeding Michael: "I was charting how many poops he took a day. How many wet diapers he had a day. How many ounces [of food] he had a day. And it was awful. . . . I can't even tell you. I think I have 12 different types of [bottle] nipples. . . . I couldn't enjoy him because every second I had, I woke up around the clock every two hours to feed him. . . . I kept my cell phone next to me, and I just set [the alarm] for every two hours." The parents visited the emergency room three times before Michael turned eight months, concerned that he was not eating enough. The consuming work of feeding their son also strained their marriage.

Sarah introduced a very different explanation that made her son's condition fit MCADD: her own brother died at seven months from "feeding

difficulties.” Her husband also lost an aunt at a young age. Her son’s diagnosis with a genetic metabolic condition rekindled possible biological continuities between relatives. In light of the family history, his feeding troubles may indicate a symptomatic manifestation of MCADD.¹⁴

Sarah’s explanation for the uncertainty that she experienced thus rested on the observation that Michael was different from his sister and on a family history of possibly fatal but undiagnosed metabolic disease. While Sarah formulated an explanation on which she acted (in the sense that it guided feeding Michael around the clock and led to emergency room visits), this does not mean that we have a social science explanation. At this point, we only have a single snapshot within a single case. In order to buttress, or reject, this causal explanation, we need to look at whether it holds beyond the specifics of this incident. As it happened, variation occurred over time, which led to a rejection of Sarah’s original causal explanation. Rather than dismissing the family from the clinic, Dr. Silverman agreed to conduct DNA analysis for the most common MCADD mutations. This molecular test could provide conclusive evidence of Michael’s carrier status. He decided to keep following the family until the test determined whether his hunches were correct. Michael’s molecular results came back negative. At that point, Michael was considered disease free, a result the parents accepted.

Subsequently, in a follow-up interview, Sarah revised her previous causal narrative: “We’ve just kind of chalked it up as he’s not a great eater.” Rather than a medical tie between Michael and her deceased brother, she now linked her son’s eating habits to her own picky eating: “You know, I am a small woman. I’m not even a hundred pounds. And I was a really poor eater, my Mom said, as well. So, we kind of just think he’s just not a good eater. And now I can relax a little bit because it’s not fatal if he doesn’t.” She also noticed his developmental milestones: “Michael was really good. He rolled over when he was five days old. Like his motor milestones, he met. Social milestones, he met. It was deceiving.” From a worrisome “bag of bones,” the genetic test results shifted Michael into a healthy, maybe even precocious, child with a difficult appetite.

Tracing Sarah’s meaning-making processes over time allows the researchers to reject its *causal* implications. If we were to generalize Sarah’s account as a social science explanation, we would assume that parents’ reaction to being told that their child is a carrier of a possible genetic illness is primarily caused by personal histories of undiagnosed metabolic illnesses, comparisons between children, and observable behavior congruent with symptoms. This account seems straightforward until we examine variation over time. While Sarah understands her situation through her

¹⁴ Note that clinicians would disagree with this assessment: for them, feeding difficulties do not indicate MCADD but put Michael at higher risk for metabolic complications if he had MCADD.

personal history and her comparison between children, once her son is pronounced false positive (and thus healthy) she does not simply abandon her past, but rather invokes a new past with a new morale: she now understands her son's eating "problems" as that of a picky, small, and precocious child taking after his mother. This, also, is a personal history, albeit a completely different one.

Following semiotic chains and using them to construct axes of variation thus solves a methodological-cum-ethical conundrum—Can ethnographers produce arguments that “work behind the backs” of their subjects? Do researchers' explanations draw on actors' “first person” experience, or do they construct “third person” narratives that are detached from actors' understandings of their situation? Our pragmatist approach implies that ethnographers can and sometimes should construct causal claims that seem very different from those the actors hold, but that these should meet a high evidentiary threshold for rejecting actors' explanations. This threshold is met when researchers collect data and analyze them in systematic fashion for agreements and differences across cases. Ethnographers cannot simply assume, *a priori*, that they know better than their subjects do. Rather, the onus of rejecting actors' causal claims depends on an explanation's ability to account for variation.

Our proposed causal explanation rests mostly upon directly observable interactions, but the elements of the causal claim are not strictly interactional or observational. They include not readily apparent elements such as material and cognitive resources, which, in turn, are linked through aggregations of meaning making to processes distant in time and place. We can further examine causes behind causes when we investigate how the observed puzzling interactions and varying responses were historically produced. For example, the initial shock that parents experienced when they were informed of the screening results was affected by the process by which newborn screening was institutionalized. Parents in the study were usually unaware that their infant had been screened because in all but the Washington, D.C., area they are not asked to consent to the procedure. Realizing the lack of informed consent may lead the ethnographer to conduct historical research into the origins of the screening program and detail how the program escaped the application of bioethical principles. Historical and structural forces are then not postulated but linked through additional inquiry. As in research subjects' explanations, these increasingly remote links need to meet a high evidentiary standard to be incorporated in the mechanism-based account.

Delineating semiotic components and then checking for variation within and across cases leads to a causal claim. Ethnographers must then further refine the proposed explanation in light of other plausible alternatives in the academic literature and accounts offered by research subjects. In this sense,

the community of inquiry directly informs the construction of causal claims. When the causal claim is made public, the community of inquiry yet again takes an active role. The public explanation becomes itself a “sign” leading to a communal “interpretant.” As with all interpretants the sign is insufficient to predict the interpretation; the actions of readers and interlocutors will influence the claim’s reception. The consequence of the causal explanation resides in how it serves as a reference point for subsequent local meaning making and ethnographic work—the causal claim is predicated upon its usefulness for other actors in their ongoing attempts to “puzzle out” (Winship 2006) their world.

CONCLUSION

What practical difference does it make to construct a causal argument in ethnography? As in everyday life, an ethnographic causal claim provides an explanatory generalization of a temporal flow of action that renders past, current, and future events meaningful. Taking our cue from the work of pragmatist philosopher Charles S. Peirce, we argue that establishing causality in ethnographic research includes three intertwined analytic activities. The first, based on a processual mechanism-based approach, requires researchers to trace the iterations of meaning-making-in-action through which the proposed explanans is connected to the explanandum. Focusing on the structure of meaning making is a particularly compelling “bottoming out” process for explanation in ethnography, making the most of ethnographers’ ability to trace iterations of action over time. Such a level of explanation, we argued, is also theoretically compelling, as it requires us to think of action as a basic unit of analysis. Rather than focusing on the properties of actors as the explanatory framework, the making of meaning—both in its most reflexive and its most habitual form—requires us to focus on the relationship between a sign, an object, and their effect in the making of meaning.

Decomposing action into semiotic sequences is insufficient to produce a robust causal account. In order to further establish whether the aspects of meaning making the researcher highlighted as a causal mechanism are indeed the most relevant ones, and to explore the unfolding process, the ethnographer then must examine axes of variation. Based on the properties of Peirce’s semiotics, we propose three modes of variation—*data set variation*, *variation over time*, and *intersituational variation*. In all its forms, such a view of variation provides insight into the regularity and unfolding of the process and thus strengthens our confidence in our processual causal claim.

We then note that these two activities are always constructed, assessed, and reworked in relationship to a community of inquiry—both the academic audiences and the lay actors who are engaged with the same prob-

lem. While the community of inquiry extends into the indefinite future and provides a regulative scientific ideal, ethnographers establish the plausibility and fit of their causal claims by accounting for, and rejecting, the alternatives that they can expect others in the community to raise. Of course, ethnographers can never reject all plausible alternatives. At the end of the research, some plausible alternatives may emerge outside the scope of the study that would have, perhaps, called for a radically different research design. As in any other form of scientific work, the ethnographers' responsibility is not to construct a hermetically sealed explanation. Rather, they are accountable to the construction of a continuous, intelligible account, the variations of meaning making within their case, and for the plausible alternatives that they should have foreseen were they attentive to the literature. The alternatives that emerge beyond that point are provocations for further research, the next step in the ongoing conversation sustaining a community of inquiry.

Moreover, being enmeshed in a community of inquiry provides heuristics for the contours of a successful causal explanation. Since a causal claim, like all scientific constructs, is finally assessed by its use, explanations that are too specific or that are couched in generalities that can capture any and all phenomena are less useful as causal accounts. Ethnographers thus do not have the final word: their work advances a research-based explanation generalizing patterns of meaning making to be taken up or ignored by the community of inquiry. This three-pronged pragmatist approach to the construction of causal accounts in ethnography provides both methodological guidelines for the construction of causal accounts and evaluative criteria for assessing such causal claims.

How does this pragmatist account of causality compare to widely used forms of theorization in ethnography? While ethnographic causal claims begin with situation-specific acts of meaning making, the ethnographer can highlight aspects of a causal claim that are invisible within the observed interactions and remain outside the grasp of individuals going through interactions. In this sense, our pragmatist approach differs in critical respects from that of grounded theorists. Their accumulated written record shows a reluctance to move beyond a close conceptualization of observations (see Timmermans and Tavory 2007). While we obviously agree that causal claims must be grounded in observations, the proposed causal pathway may extend beyond observations to include distant cultural and structural elements, as long as such extensions are carefully traced. Moreover, the pragmatist position we develop here emphasizes that a crucial moment in establishing causality is precisely "grounded" in theoretical debates in one's community of inquiry. Leaving the community of inquiry to the end of the research project, as classic grounded theory proposed (see, e.g., Glaser 1992), risks reinventing well-known themes based on a single study and weakens its causal claims.

Our focus on the extension of causal arguments both empirically and theoretically is much more akin to the original formulation of the extended case method in anthropology (see, e.g., Gluckman [1961] 2006). But unlike the extended case method, especially in its contemporary American reformulation (e.g., Burawoy 2009), a pragmatist approach does not privilege a move from observed social processes to invisible social structures based on pre-defined theoretical assumptions. The requirement for invoking nonobservable processes as causal actors is that a tightly linked semiotic chain can be reconstructed. And, moreover, the beginning point is not necessarily a “favorite theory” (Burawoy 1998, p. 16) but rather the interplay of a range of plausible alternatives and the mechanism-based semiotic evidence and its variations. To return to the newborn screening study, the shock of a positive newborn screening result can be partly traced back to the lack of informed consent, which, in turn, can be traced back to path dependent processes beginning with the initial screening program. It was thus only after a causal explanation has been specified—the reaction to lack of common frame for understanding a new form of possible disease—that the historical conditions that led to its emergence become causally relevant. To do otherwise is to invoke theory as *deus ex machina*.

By providing standards of evidence and opening up new avenues for research, a pragmatist approach to causality provides a way out of an overly debated tension in the ethnographic literature between interactional “inductive” approaches and more structural “deductive” ones. Much of the discussion on the subject relates to the differences between the kinds of causal and broader theoretical narratives these approaches construct and their great indebtedness to specific theories. A pragmatist approach to causality transcends such theory-methods packages. Inspiration for ethnographic causal claims can come from the actors own causal accounts, from the interactional sequences evidenced in the field, from established categories and from a theorization of macro forces; these can be accounts that actors’ in the field are well aware of and act upon, explanations they only partially grasp, or even explanations to which they are completely oblivious. But whatever their source, the evidence for such causal claims in ethnographic work should be sought in iterations of meaning making and its variations.

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