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# Theory and Contrastive Explanation in Ethnography

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## Paul Lichterman[1] and Isaac Ariail Reed[2]

## Abstract

We propose three interlinked ways that theory helps researchers build causal claims from ethnographic research. First, theory guides the casing and re-casing of a topic of study. Second, theoretical work helps craft a clear causal question via the construction of a contrast space of the topic of investigation. Third, the researcher uses theory to identify social mechanisms that condense causal accounts. We show how each step can accommodate the everyday meanings typically central to ethnographic research's contributions. This tripartite role for theory thus preserves ethnography's traditionally recognized strength in interpretive validity, while realizing ethnography's potential for offering causal, and partly generalizable, accounts that can engage the wider sociological discipline. The discussion brings ethnographic research into conversation with recent debates on the role of mechanisms, comparative and counterfactual thinking in causal accounts. We illustrate and defend our argument for theory in ethnography with an extensive analysis of a contemporary ethnographic monograph along with briefer attention to parallel uses of theory in two other ethnographic studies.

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## Introduction

In this article, we consider how ethnographers can use theory to construct causal explanations and connect them to lines of sociological inquiry that may rest on a variety of methodologies. We do this by specifying three incision points where theory can be used by ethnographers to build toward causal claims. First, theory guides the casing of a topic of study. Second, building on the ongoing process of casing and re-casing, theory helps the ethnographer define and develop an answer to a clear causal question for the study. Third, the researcher uses theory to identify social mechanisms that focus and condense causal accounts. Our account of these incision points is designed to build upon, rather than bracket, the interpretive work associated with much sociological ethnography. Thus, our argument about theory's role in ethnography draws upon a larger movement in sociology to transcend the long-standing distinction between sociology that seeks ''understanding'' and that pursues ''explanation'' (Reed 2011).[1]

While our discussion builds on prominent existing accounts of causal thinking in ethnographic research discussed subsequently, we propose a distinctive set of rationales for integrating theoretical claims making into ethnographically based causal arguments. We offer ethnographers a map of the ways theory can help integrate interpretive and explanatory aims into one argument, even within a study that may also have other purposes. For practitioners of other methods, our account shows how theoretical assumptions train the choice of appropriate comparison cases, and counterfactual reasoning about causes, in subtle as well as explicit ways, thus connecting ethnographic research to advances in comparative–historical methods.

Recent debates on comparative–historical, and more broadly, ''qualitative'' methodology consider the place of social mechanisms, necessary and sufficient conditions, set theory, and comparative and counterfactual thinking in historical sociology and comparative political science (e.g., Hedstrom and Swedberg 1998; Goertz and Mahoney 2012; Gross 2009; Mahoney 2007, 2012; Ragin 2000). Over the past decade and a half, these new debates on causality have included focused statements on the uses of small-N or case-based research (Goertz and Mahoney 2012; Ragin 2000, 2009) and broader

inquiries into the social, historical, or disciplinary contexts (Steinmetz 2005) that define what counts as sociological, causal knowledge to begin with. Contributors to these recent discussions (especially Goertz and Mahoney 2012) already point out that qualitative studies often are concerned with explanation, just as quantitative ones typically are. It is the conceptual language of causality that differs: Speaking schematically, for Goertz and Mahoney, a ''qualitative'' culture of causal analysis tends to seek out necessary or sufficient causes of historical events (see also Goertz and Starr 2003), while a ''quantitative'' culture of causal analysis is exemplified by regression analysis and the measurement of average effects. Though Goertz and Mahoney (2012:5) recognize that interpretive work is important in sociology too, it is not part of what they call the ''qualitative culture'' of causal analysis. Their text codes interpretive work as noncausal and does not focus on it.

While entering an ethnographic perspective into these discussions on causality, we link up with the long-standing conversation inside ethnographic research circles about the roles of comprehensive or middle-range theory in ethnography (Becker 1960; Burawoy 1998; Glaeser 2006; Glaser 1978; Glaser and Strauss 1967; Ragin and Becker 1992; Vaughan 2009). We uphold the notion from this second methodological conversation that producing causal knowledge depends on using theory to define the ethnographic case to begin with (Burawoy 1998; Ragin and Becker 1992). We join and borrow from this conversation selectively, since others already have reviewed signal statements in the conversation, and in particular, the well-tread opposition between ''grounded theory'' and ''extended case method'' (Burawoy et al. 1991; Lichterman 2002; Tavory and Timmermans 2009).

Our engagement with ethnographers' discussions of theory leads us to contribute to a third, very recent conversation that looks closely at how ethnographers develop causal claims (Blee 2013; Katz 2001, 2002; Tavory and Timmermans 2013; Timmermans and Tavory 2012). This growing genre of work has developed both ''analytic induction'' and ''analytic ethnography'' in relationship to the idea of a causal explanation. This work, like our own, seeks to connect ethnographic interpretation to causal explanation in a new way. However, as will become clear, we view the role of theory somewhat differently. In a broader sense, however, what follows should be viewed as one in a diverse chorus of voices that reject a strictly rhetorical or ''postmodern'' understanding of what makes ethnographies compelling to other sociologists. For example, Katz suggests that clear, coherent, evidentially sustainable explanatory claims are an essential mark of quality in an informal culture of evaluation that surrounds ethnographic work. As he puts it,

''whatever the contribution of writing style, there are . . . good sociological reasons for the rhetorical effect'' of a compelling ethnography (2002:71). Our argument about theory use in ethnography in this article is pursued in this spirit.

## Preliminary Considerations: What Is "Theory"?

Discussions of ethnography in sociology often refer to ''theory'' in a way that departs from an understanding of theory as an interlinked set of general propositions about the world or a set of covering laws or proposed associations that can be falsified or verified. Instead, ''theory'' refers to the abstract terms of discussion that orient the investigation of cases or arguments about social processes and conditions that link different cases and different investigative concerns together. Theory, in this view, is one of the means available to sociologists for building truth claims about the world at varying levels of generality, whether or not these claims end up succeeding. Thus, ''theory'' participates in the positing of explanations of a well-defined phenomenon, the redefinition of a sociological research problem, and can even work to reorient the worldviews or ''sociological imaginations'' of researchers.[2] In this way, theory in ethnographic usage is related to—though not precisely the same as—what Goertz and Mahoney (2012:127-49) identify as the extended dispute over the definition of concepts that often accompanies qualitative comparative research. For many ethnographers, then, theory is a set of terms for interacting with one's empirical findings and with other researchers and readers; diverse guides to ethnographic method share this dialogical understanding of theory (Burawoy et al. 1991; Glaser 1978; Strauss 1987; Strauss and Corbin 1990).

   Thus, theory is the language game that binds together what C. S. Peirce would have called sociological *communities of inquiry*. For, while knowledge can have more than one audience or ''public'' to which it is directed, central to the understanding of ethnography developed here is that the *potential* analytic rationality of scholarship depends upon the collective process of disputation, argumentation, and competition that obtains in the community of scholars who occupy themselves with studying a given social phenomena. Such a community is, furthermore, informed by larger circles of discourses academics employ in the social sciences. Communities of inquiry are by no means consensual and are certainly not necessarily sources of solidarity and fellow feeling. They do not necessarily agree on what constitutes the best casing, nor on how to define the limits of relevant research literatures for a site at hand. They agree, rather, on a

critical-reflexive stance toward claims about evidence from an empirical world that may offer resistance to those claims and a critical-reflexive stance toward the *relevance criteria* (discussed subsequently) that bound conceptually what sorts of causes and counterfactuals will be of interest to the sociological investigator.[3]

This view of a community of inquiry differs from an alternative formulation current in ethnographers' conversations about theory. The alternative uses Lakatos' understanding of research programs along with Kuhn's understanding of anomalies to place the investigator within a ''paradigm'' (Burawoy 1990, 1998). This alternative pictures a scholarly community whose participants maintain a commitment to a paradigm's solid core of claims and augment and refine these claims via the development of subsidiary concepts. Within this kind of community, ethnographers encase observations in terms of the paradigm's categories and then mine the case for anomalies that can induce further theoretical work to improve the paradigm. So in contrast to the view of theory that we will develop, the study pursued on this alternative model develops in relation to the same theory all the way through. In our view of theory, social science communities of inquiry do not necessarily speak in paradigms, but this does not mean, ipso facto, that they don't speak theory.[4]

## Theory in Ethnography: The New Debates

Recently, the arguments of Vaughan (2009), Timmermans and Tavory (2012), Small (2009), Tavory and Timmermans (2013), and Blee (2013) have come to the fore as ethnography has engaged the discipline in a new round of epistemic debate. Vaughan sees inquiry as beginning with a ''theoretical orientation'' and a specific set of concepts, which are revised or innovated upon in the course of the study. Similarly, Timmermans and Tavory (2012) argue that the Peircean concept of ''abduction''—used by C. S. Peirce to philosophize about the creative leaps toward an explanation that he thought essential to scientific reasoning—describes the important way theory construction works, in ethnography, via the iterative dialogue between a conceptual story that the researcher posits as a tentative, guiding hypothesis (theory) and observations from the field (data) that may refine or else challenge that story.

We share these scholars' notion that ethnographic work begins with a theoretical orientation. However, our focus and our picture of theory's roles differ. Timmermans and Tavory (2012) present abduction as a sequence of mental processes and research strategies that produce discovery and

create the possibility for new theory. They identify three moments of that discovery process—"revisiting," "defamiliarization," and "alternative casing." In contrast, we wish to emphasize theory's roles in constructing the claims that move an ethnographic project toward a contrastive explanation, specifying relevance criteria for comparisons and counterfactuals and articulating the explanation in terms of social mechanisms that can link the project with other, not necessarily ethnographic lines of inquiry.

To build a contrastive explanation, as pictured subsequently, we start with a process similar to the "analytic induction" presented by Katz (2001, 2002) and advocated by Timmermans and Tavory as well. In terms of causal logic, analytic induction moves an ethnographic project "from how to why" when the ethnographer develops tentative causal claims and then seeks out potential counterfactuals that will test and substantiate the claims or else invite revision. In terms of site-selecting strategy, the ethnographer follows analytic induction by way of ethnographers' well-established "constant comparative method": Observations at an initial site or setting lead to tentative field hypotheses, which the ethnographer uses to "theoretically sample" a next set of field observations, narrowing the focus in one site or selecting other sites; those next observations in turn inform the researcher on existing hypotheses and suggest new hypotheses along with a new "theoretical sample" of next observations (Glaser and Strauss 1967; Strauss and Corbin 1990). Katz (2001, 2002) identifies seven *types of data* that can enhance the search for counterfactuals in analytic induction. We affirm and use analytic inductive logic along with the iterative site selection that embodies the logic.

However, we focus on the multiple and constitutive roles of *theory* in going from how to why. We show how theoretical choices, made at least partially independently of field observations, train the search for counterfactuals. Different theoretical choices can lead to different if plausible causal arguments whose value cannot be adjudicated on the basis of empirical findings alone, as we illustrate subsequently. This means that both field data and negotiations of theoretical difference within the community of inquiry become inextricably part of causal claims making. We show that communication with communities of inquiry is built into the process of developing causal claims from ethnographic data.[5]

We demonstrate further that for at least some research projects, developing causal claims depends on *meta-communication*—active reflection on foundational assumptions behind the concepts we choose or reflection on entire lines of scholarly inquiry or debate. Meta-communication with communities of inquiry becomes particularly important when researchers try to join ethnographic findings to inquiries grounded in other research methods

with different conventions. This notion of meta-communication links up recent work on Peirce's concept of the community of inquiry with his extensive writings on the role of doubt in scientific inquiry. ''Meta-communication'' enhances the potentially self-corrective nature of science, through the communication of doubt and criticism at several different ''levels'' (Hildebrand 1996; Laudan 1981; Peirce 1992a:114, 1998:77-79).[6]

If theory does play this sort of role, and if we recognize its relative autonomy in helping determine the sorts of causal questions that get asked and answered, then we are compelled to develop a different position from what has been set out in recent debates. For Tavory and Timmermans (2013), the community of inquiry consists mainly of bearers of alternative explanations from various literatures (2013:695) and devices of professional review (2013:697). We propose that in addition to these, a community of inquiry encompasses lines of inquiry and debate that organize alternative explanations and inform casings, the assumptions or commonsense notions that undergird casings and alternative explanations, and historically variable relevance criteria about which causes are particularly important to sociologists for a particular subject matter. Further, the community relevant to the ethnographer is not always a self-named circle of participating scholars.[7] To situate a study, ethnographers like other researchers sometimes reconstruct from somewhat disparate works a community of inquiry focused on a debate or line of inquiry, as in the case of our main illustrative study, or reconstruct ambiguities and lacunae in extant discussions in existing communities of inquiry, as in the case of two other studies we illustrate subsequently. Whether the community of inquiry is a reconstruction or an already self-acknowledging school of thought, it structures and limits an ethnographic study's imagination for causal relations. Thus we focus closely on how theory can heighten reflexivity in a community of inquiry: Theory can facilitate casings that depart from an academic field's common sense, and theory can offer relevance criteria that shape a contrast space.

### An Illustrative Study

We illustrate our approach with several studies, but the most extensive example for our argument will be Lichterman's *Elusive Togetherness*, a study of religious volunteer groups responding to welfare policy reform. The study participated in several communities of inquiry. These provided the theoretical languages used to sort the empirical material into an explanation and hosted the arenas of debate that could be entered with the

ethnographic findings at hand. One such community was the set of scholars concerned with "Tocquevillian" questions about civic associations and citizens' propensity to create civic ties as they work voluntarily to address public problems together. Research often conceived those ties with the concept of "social capital." Lichterman's study compared eight church-sponsored volunteer service coalitions and groups[8] that worked on a variety of projects in the pseudonymous metropolitan area of Lakeburg. The variation between projects in the Lakeburg study served to establish a tentative causal account and to cast doubt on alternative explanations made plausible by preexisting literature.

One of these volunteer coalitions, named Park Cluster, produced some small, community development projects involving collaborative relations with leaders of the people to be served by the projects. We will call these "two-way bridging" relationships to signal that these ties involved some amount of input on the meaning and purpose of the projects from the served as well as servers. The other projects in the study, whether sponsored by Park Cluster or other volunteer coalitions, created or tried to create goods defined by professional service organizers, or volunteers themselves, with no significant input from people representing the served. We will call these "one-way bridging" relationships to signal that these ties were directed from "servers" to "served." The two-way ties are remarkable when contrasted with the one-way relations between servers and served, or volunteer recruiters and volunteer labor, that are typical of American community service efforts (Allahyari 2000; Liebow 1993; Wuthnow 1998), and according to a critical literature (for instance, McKnight 1995), ultimately disempowering rather than citizenship-enhancing for the served.

Why did several Park Cluster projects create two-way bridging relationships while other projects sponsored by the Cluster, and by other coalitions in the same locale, did not? Beginning with a clue from Tocqueville—what Vaughan (2009) might call a theoretical starting point—but reconstructing and refining it extensively, *Elusive Togetherness* argued that the particular patterns of coordinating interaction inside the volunteer coalitions affected the kind of bridging ties outward that a coalition could sustain, one way or two way. How should we understand this knowledge claim, not only in its discovery and construction phase but also as a truth claim to have causally explained something in a way interesting to a wide variety of sociologists? Here, connecting empirical sociological research with the philosophy of science becomes extremely useful for clarifying muddy waters. We will explain below that this study and other ethnographies construct a version of "contrastive explanation" that has been the subject of significant

work in analytic philosophy and derives from an approach to scientific explanations termed "erotetic" (Garfinkel 1981; Risjord 2000; Van Fraassen 1980:97-157).[9]

## First Theoretical Moment: Casing

The ethnographer in the field produces a series of observational notes on social actions. As he considers these actions together, the question arises, what is this a case of? This question, which can reiterate itself through the process of fieldwork and writing up findings, is the first decision point linking theory and ethnographic evidence that we will discuss. We treat it briefly since it is the most well covered in the current literature, discussing it just enough to show how, in addition to what has been argued about casing, this process sets some of the parameters for later theoretical work *without predetermining the explanation*. Casing also highlights the role of meta-communicative dialogue with a community of inquiry.

As Ragin argues, classifying, or "casing," is an essential theoretical operation in social science. In the terms advanced here, the community of inquiry cultivates potential answers to the ethnographer's initial question of which observations will be most relevant in the field of observation and how the ethnographer should classify them. The study is located in a, or a set of, scholarly conversations, none of which necessarily are more naturally related than others to the empirical data "on its face."

So, for example, the Lakeburg religious volunteers study classified the volunteer coalitions as cases of "civic action." This encasement borrows the conceptual notion, cultivated in scholarly conversations (Putnam 1993, 2000; Skocpol 2000; Tocqueville 1969[1835]) that voluntary, self-initiating, problem-solving—or "civic"—action is fundamental to democratic citizenship. Loosely inspired by Alexis de Tocqueville's writings, those conversations have entertained a distinction between "bonding" and "bridging" social ties (Putnam 2000; Putnam and Goss 2002). "Bridging ties," the topic of the Lakeburg study, refer to ties that members of a civic group or project create with socially distant others beyond the group. There are very different potential forms of bridging ties—between individuals, between groups, between individuals and groups, between voluntary and governmental organizations, for instance (Warren, Thompson, and Saegert 2001; Wuthnow 2004:89), and of course different axes of social distance. The variety of bridging ties was a theme of the study we reconstruct here.

From among the many observations an ethnographer might make of religious volunteers, their encasement in terms of civic action and bridging

ties focused the researcher's observations. This encasement would not direct the ethnographer toward observations that would inform us about the volunteers as Christian worshippers, for instance, *unless* observations of worship could further inform the ethnographer about the volunteers' bridging ties in associations. An encasement focused on "civic action" selects a different if overlapping slice of social reality from that which would be carved out were the coalitions encased as instances of "religious activity."[10]

Many (though not all[11]) ethnographers further maintain that if the community of inquiry and its theoretical languages are one source of an ethnographer's orientation, then so are the extant meanings present among the researched (Tavory and Timmermans 2009). Ethnographic observation potentially offers a sharp view of the everyday meanings actors themselves share—even when some of those meanings originate or circulate far beyond the place or time of the ethnographic site. Those meanings will condition the work of casing because *those meanings are part of the action being encased*, and later, explained (see Glaeser 2006; Reed 2011). The relationship of the ethnographer to her scholarly audiences, on one hand, and her research subjects, on the other, has provoked much debate in sociology and related disciplines and is of course a central concern in the sociology of sociological knowledge. For our purposes, this juncture matters because it is one point where theory enables empirical claims making.

On one hand, making the encasement responsive to actors' own meanings yielded striking results: These were church-sponsored volunteer coalitions, but the meaning of "a good group" or "a good volunteer" turned out to be much more salient in everyday action than the meaning of Biblical dictates. Encasing these coalitions as instances of civic action, rather than "religiously driven action," made sense from the standpoint of actors' own meanings.

On the other hand, the study engaged in theoretical claims—meta-communication with the community of inquiry—to make those meanings *readily noticeable* to a community accustomed to finding different meanings. In order to case the coalitions as instances of simply "civic action," and not intrinsically religion-related action, the study named and challenged explicitly the conventional scholarly understandings of how "religion" works in public to begin with. It confronted the reality of conventional understandings in the community itself: While individual studies produced different evidence on whether or not religion strengthened or strained civic togetherness, they usually assumed that in a religiously identified group, religion was necessarily consequential for civic engagement. Meta-communicative engagement (pp. 31–34) put these studies together, noted several scholarly conventions

underlying them, and reconstructed a debate on religion and civic engage-ment, thereby claiming conceptual space for a different, more counterintui-tive encasement. This encasement allowed the possibility that religion might not matter so much or might matter differently than scholars have assumed. The goal of this theoretical work was not to advance a theoretical paradigm by making the paradigm account more effectively for an empirical anomaly. It was, instead, to justify a nonconventional way of encasing observations which later could be relevant to various theories and various potential explanations that were not intrinsic to the encasement. Absent a critical-reflexive stance, the power of conceptual common sense in a com-munity of inquiry can inhibit claims making even when empirical observa-tions would support the claims[12]: Other, well-received studies have ''heard'' actors in religiously identified sites as being driven by particular theologies or religious teachings even when evidence at hand suggests otherwise.[13]

As writings on the iterative relationship between theory and data in the ethnographic research process (e.g., Timmermans and Tavory 2012; Vaughan 2014) emphasize, interaction between inputs to casing derived from theory, and those derived from the field itself requires that re-casing is a con-stant possibility. Studies often require re-encasings as the constant compara-tive method generates novel findings which challenge either the overarching definition of ''what this is a case of'' or the relevance of particular theoretical categories used to order the data now at hand. For example, if observation had revealed that religious volunteers did indeed quote Biblical passages in arguments over how to serve a locale, then the encasement of ''civic action'' could reasonably have changed to ''religiously driven civic action,'' closer to that of other studies reviewed at the start of the book. Re-casing does not obviate the other two theoretical moments of an ethnographic study, or destroy the study itself, but rather implies that the other two theoretical junctures, if the study already has negotiated them, may now need revisiting in light of a new casing (Walton 1992).

## Second Theoretical Moment: Developing a Causal Question

When a study is encased, it creates a platform for viewing selected varia-tions within the study as highly significant. The second moment of the-oretical decision making builds up that platform more by identifying such variation and organizing it for a causal account. This happens when we develop a clear and well-defined causal question. In this section, we

draw on a philosophy of contrastive explanation to discuss how such causal questions about a specific case or cases can become *well defined.* This philosophical work provides a language for discussing how the investigator (e.g., ethnographer but also other sociologists with other methods) can choose from the many axes of variation within a case in a way that advances the search for an explanation that can be presented to the community of inquiry.

Contrastive explanation is a promising route because it provides a formal language for developing explanatory questions, and their causal answers, at varying levels of generality. Although some qualitative work in sociology does reach for highly general conclusions about necessary and sufficient causes for certain broadly defined outcomes (Goertz and Mahoney 2012:194-95), a great deal of ethnographically oriented research works at several levels of generality at once, using a variety of different theoretical constructs (thus following the model of ''interpretivist'' use of theory, see Reed 2011:89-121). The contrastive understanding of explanation, as developed in analytic philosophy, allows for this, because the formal logic that is the basis of contrastive explanation can be applied across a broad range of explanatory projects, from causal claims entirely focused on a singular event to explanations that use law-like generalizations such as Coulomb's law in physics (Woodward 1984).

We start, following Lewis (1973, 1986) with the notion that that which happens in the world (an ''event,'' ''happening,'' or ''outcome'') can be placed into a web of relationships with other events, and some of these relationships are causal ones. If one takes the web of causal relationships that lead up to an event, and even if one looks only into the (arbitrarily defined) ''recent'' past, one has a massive set of ''influences'' that predate an event. Then, the work of explanation consists, to a great degree, of (1) differentiating those aspects of a causal history that are of interest as opposed those which we take for granted or which should simply remain invisible for the purposes of a given explanation (Garfinkel 1981:172; Lewis 1986:217-21) and (2) developing ways of grouping certain causal relationships together based on some of their similarities and showing how they reappear at different points in space and time (''generalization,'' see Garfinkel 1981:34). Some of the causal relationships between events in the world can be grasped particularly well by the ethnographic method which also allows the researcher to examine empirically the potential role of meaning in directing action down one path rather than another. Thus, in our understanding, case-centered causal explanations (what Vaughan, following grounded theory, calls ''substantive theory'') can be informed by

theoretical work of a more general nature and can in turn inform such theoretical work themselves. It is this theoretical ''informing'' that we try to specify for ethnography, not as a process of discovery but rather as the development of justifiable explanations.

## Contrastive Explanation: Toward a Well-defined Why Question

The core insight of the contrastive philosophy of explanation is simple: In answering questions about why something happened, one is in fact answering questions based on a contrast between an *aspect* of something that happened and something else—either a counterfactual or a comparison that exists in the world and has also had a certain ''aspect'' highlighted. (Technically, such a comparison can provide a plausible counterfactual for the original event—this is what it means to highlight an aspect of a comparison case in a useful way.) For example, if a short circuit explains a fire in a church, what ''explains'' really means is that the short circuit explains the specific *difference* between ''fire in the church'' and ''no fire in the church.'' In contrast, the presence of potassium in the walls does not explain the difference between "fire" and "no fire," but rather why the fire burned purple rather than yellow (Woodward 1984).[14]

This general approach, then, addresses one of the most obvious problems facing the ethnographer. Participant observation throws the researcher into a bewildering web of interconnections in the field, and he must draw from this world a true causal story of relevance to a community of inquiry. The researcher must effectively and meaningfully narrow a potentially *unlimited* set of answers to the question ''why did A happen?''[15]

The contrastive approach gives us a way of determining what is salient, given certain purposes.[16] The approach to explanation known as ''erotetic'' (Garfinkel 1981; Khalifa 2004, 2006; Risjord 2000; Temple 1988; Van Fraassen 1980) makes dialogue about ''why?'' central to the explanatory enterprise and is specifically designed to make more rigorous and transparent the process, whereby the possible answers to why questions are given limits. The primary point here is that why questions presume *rather than* clauses. So, when a researcher asks ''Why A?'' the question really is ''Why A rather than {B, C, or D}?'' The researcher also specifies what other presuppositions a why question contains, because those presuppositions will enable us to judge better and worse answers to the question.

To elaborate further in terms of the erotetic tradition, for a why question to be *well defined,* we must know its topic, its contrast space, and its

relevance criteria. Let us use an example from Garfinkel's *Forms of Explanation* to make these terms clear. Take the question ''Why did Richard Nixon get the republican presidential nomination in 1968?'' This question has as its topic ''Nixon got the republican presidential nomination in 1968.'' It might have as its *contrast space* the following set of individuals getting the republican nomination: {Romney, Rockefeller, Goldwater}. This implies the question is really ''Why did Nixon rather than Romney, Rockefeller, or Goldwater get the republican nomination for president in 1968?'' It might have as part of its set of *relevance criteria* both the restriction that the answer we search for—which will give us information that differentiates the topic from the rest of the contrast space—focuses upon the ''social and political dynamics within the party and the electorate since 1960,'' as well as knowledge of some presuppositions that delimit potential subquestions or related questions, and thus avoid infinite regress. In this case, for example, we are presupposing that we already know the answer to the question ''Why must only one person get the Republican nomination?'' and thus that such information is not part of a relevant answer to this specific why question.

It is extremely important to note that questions with the same topic (the phenomenon to be explained, e.g., ''Nixon got the Republican nomination in 1968'') can have different contrast spaces. And, *different contrast spaces lead to different, though equally true, explanatory answers*. ''Why did Adam eat the apple, rather than giving it back to Eve?'' admits ''he was hungry'' as a good answer. ''Why did Adam eat the apple, rather than a pear?'' does not.[17] This contrastive dimension of why questions already limits the answers that can be given to a why question. Specifying carefully ''rather than'' clauses could go a long way, we think, to differentiating between disputes that are really about different explanations of the same causal difference, and pseudodisputes that actually are between explanations with the same topic, but different, implicit, contrast spaces.[18] Why questions are also importantly limited by *relevance criteria* that further specify appropriate answers. These require further elaboration.

## Relevance Criteria Help Shape the Causal Question

To explain to your neighbor why some weeds grow in your garden, you might give an account of certain plants prone to grow at that time of year. But to an expert gardener who routinely sprays herbicides to kill weeds, your answer to the question ''why do you have so many weeds growing?'' might instead involve an extended explanation of why you chose not to use

weed killer this year, based on your objections to herbicides. Thus, why questions and their answers are also conditioned by whom their answers are for—much as encasing is conditioned by a given community of inquiry. This is the issue of relevance criteria. What makes the answer to a why question "relevant"?

The most elementary relevance criterion is that an answer to a why question must differentiate the topic from the rest of the contrast space. But beyond that, further constraints often come into play. Note, for example, that causality itself is a relevance criterion that we tend to apply when we want to call the answer to a why question an "explanation." The ratio of wine/beer consumption probably differentiates nicely Prussia, France, and the United Kingdom in 1789, but we are unlikely to accept it as (part of) a good *explanation* of why there was a revolution in France and not in the other countries (unless it can be shown that wine prices were essential to the economic collapse of the regime . . . ).

Beyond differentiating contrast space from topic and locating plausible causality, we propose that it is the use of theory from the community of inquiry that conditions the "interests" of the ethnographer via relevance criteria. This is also evident in the Lakeburg study: For example, the construction of a contrast space over the course of the study depended on at least two theoretical understandings external to the evolving contrast space of the study, yet not intrinsically mandated by the Tocquevillian theory that opened the study.

One such understanding was that intercongregational *coalitions* were important apart from single religious congregations. This distinction between coalitions and single congregations is not a "natural" given or immediately available in the data; it is theoretically informed: After all, both church committees and coalition committees are formally constituted groups of decision-making individuals responsible to larger constituencies, who associate at least nominally with a religious body. Very recent studies of religiously based civic participation had investigated church committees alongside interchurch coalitions without sharply distinguishing evidence from the two kinds of ethnographic site (Warren 2001; Wood 2002). However, well-supported sociological theories had emphasized the specific importance of broad coalition dynamics, separate from the dynamics of member organizations, for mobilizing collective action. Thus, the Lakeburg study crafted a why question, topic, and main contrast cases only from interchurch, coalition efforts, and gave them the great bulk of observational attention, with only very limited attention to church social advocacy committees for further comparison. The study proceeded on the knowledge that

interchurch coalitions had been relatively little studied up close and yet were heavily implicated in U.S. community service efforts (pp. 32-33). The study also took at the start that these coalitions were good examples of Tocqueville's ideal of civic participation (pp. 11-12) that moves citizens beyond their closest circles of associates. Yet church social advocacy committees could have played that conceptual role too. It turned out a contrast space constructed on the basis of church social advocacy committees rather than intercongregational coalitions would have yielded a different causal argument (pp. 230-35).[19]

A second theoretical relevance criterion reflected distinct understandings of methodologies in the relevant community of inquiry. This too affected what sort of causal account could emerge. Some though not all[20] ethnographic communities of inquiry within sociology highlight the value of distinguishing everyday settings, and the study made setting specificity another relevance criterion shaping the Lakeburg study's contrast space. Doing so made potential differences between church committees and coalition committees perceptible; each was a different setting even if both shared a lot of the same participants or similar kinds of participants.

The *potential causal* role of actors' meanings was a relevance criterion too. Sociologists widely agree that ethnographic research has distinctive strengths, as well as limits, and one widely shared relevance criterion stipulates that ethnography should be able to attend to links between meanings and actions.[21] Ethnography's combination of participation and close-up observation has been used to explore many kinds of links between meaning and action, from the explicit discursive meanings that actors openly talk about and apply to their actions when asked about them, to more subtle, implicit meanings that are instantiated in shared patterns of talking and acting. This currently is a live issue in cultural sociology (see, e.g., Jerolmack and Khan 2014; Maynard 2014; Pugh 2013; Vaisey 2014). While the potentially causal role of meanings in action was a relevance criterion, it is important to clarify that a relevance criterion does not dictate that this link *must* be part of the final causal account.

From a broader point of view, contrastive explanation may capture well many kinds of sociological explanations beyond ethnographic ones and especially explanations developed in comparative–historical sociology. We discuss this issue in our conclusion. Here, we engage the guts of the explanation that emerged in *Elusive Togetherness*, showing how it followed closely the logic of contrastive explanation and how the essential "rather than" clause of this explanation was informed by theory. We thus delve into the study in depth.

## Contrastive Explanation in the Lakeburg Study: A Why Question and Contrast Space

The study of church-based volunteer coalitions in Lakeburg focused initially on the "Humane Response Alliance" (HRA). This was a series of volunteer projects initiated by a nonprofit service and education network, made up of roughly 50 mostly Christian congregations, named Urban Religious Coalition (URC). The URC's director envisioned all of the projects of the HRC as efforts to create what we can call new bridging ties, new "civic connections" as the director puts it (p. 62), just as drastic social welfare policy reforms were taking effect in 1996. The study reported the ethnographer's immediate goal as understanding, then, what kinds of bridging relations these projects would build. The Lakeburg study defined a "bridging tie" as a routinized relationship that a group has to an individual or group perceived as outside the group; evidence that counted as a routinized relationship included ongoing transfer or sharing of material resources or culturally sanctioned expertise (Lichterman 2005:44-45). Observation conducted over four years revealed a pattern of outcomes that we summarize here in terms of the topic and a growing contrast space that made that pattern evident; the next subsection goes further into theoretical moves that propelled the study toward grasping that pattern.

Two distinct projects in the Park Cluster coalition created ties that we call "two-way bridging" ties[22] between middle-class churchgoers and a low-income, minority neighborhood's residents. Recall that a two-way bridging tie is one in which both parties to the tie or their representatives have some influence over the definition or use of the resource or expertise that anchors the tie. Two-way ties need not be completely horizontal, but each side has some noticeable influence. The two-way bridging ties constituted the *topic* for explanation. One good example of a two-way bridging tie discovered over the course of the study was the project called an "eviction prevention fund." Park Cluster initiated the fund with the hope of increasing residential stability in the Park neighborhood by offering loans to tenants in danger of eviction for nonpayment of rent. The idea for the fund was communicated to the Cluster by a social worker who said that Park residents complained to her that neighbors moved too often for a stable sense of community to develop in Park. The fund was shared by the Cluster, which wanted to offer service at least partly on residents' terms, and people representing the neighborhood—including two African American pastors. The pastors argued that potential recipients' dignity was best served by inviting them to "give something back to the community." The social

worker advocated in contrast for administering rent money as aid with
no obligations. Cluster representatives agreed with the pastors' view of
neighborhood recipients, to the social worker's chagrin.

In our terms, four projects constituted the initial *contrast space* in rela-
tion to the two projects with two-way bridging ties. Two of these were
smaller volunteer programs—a short-term summer day camp and weekly
free meal project sponsored by Park Cluster. Thus, the Park Cluster orga-
nization itself straddled the topic and contrast spaces, giving more leverage
on a major field hypothesis that will be described subsequently. Two other
occupants of the contrast space were big projects under the HRA umbrella
but outside Park Cluster: an evening entertainment program for "at-risk"
teenagers, named Fun Evenings, and a proposed network of church-based
food pantries. Three of the four contrasting projects produced some type
of one-way tie, either between server and served, between networker and
volunteer, or both. For example, the meals project made dinner to the ser-
ver's specifications, not to the tastes of the served, who complained repeat-
edly that they felt demeaned by the project. The fourth occupant of the
initial contrast space, the food pantry project, did not end up creating any
new ties at all. The project was supposed to encourage different congrega-
tions to collaborate while serving low-income neighborhoods (Lichterman
2005:71). As a planning-intensive effort rather than one focused on routine,
scheduled events and tasks staffed by casual volunteers, it offered a good,
secondary contrast with the other three projects within the contrast space.
But churches clung to their turfs and did not agree to collaborate with other
churches. After a time-consuming grant proposal and consultations, the
project folded.

Eventually, the analysis added two further comparisons from outside
the HRA: The first, Justice Task Force, focused on educating Lakeburg
churchgoers about welfare reform, thus explicitly taking on the "politi-
cal" issues that other HRA groups tended to avoid. This comparison
could be used to investigate the counter-hypothesis that volunteer human
service projects are especially unlikely to cultivate two-way ties because
they eschew a political imagination that could dignify recipients as
oppressed fellow citizens, instead of clients (Eliasoph 1998).
The second, Adopt-a-Family, paired ex-welfare receiving families with
volunteer groups from different churches, for mentoring and informal
assistance. It was part of an explicitly evangelical Protestant coalition,
Project Hope, outside the URC altogether and theologically conservative
rather than liberal as most URC churches were. This second comparison
would investigate the counter-hypothesis that evangelical Protantism,

with its focus on deep relationships (Smith 1998), could induce these groups to create some kind of two-way bridging ties. As the study reported, both projects created one-way bridging ties, and Adopt-a-Family soon folded altogether.

Our argument illustrates that expanding the contrast space through analytic induction *also relies upon theoretical work*, external to the purely empirical part of the study, that functions to inform the development of a contrast space.[23] The notion that ''being explicitly political'' would matter for building two-way ties, for example, was not intrinsic to pursuing the why question or enlarging the contrast space around it. The idea became ''relevant'' not from inside the analytic inductive process, but from the external context of the study: That context was a growing community of inquiry focused on civic and political participation in the United States, which repeatedly has implied that egalitarian, public relationships across unequal social statuses develop mainly in a socially critical, at least somewhat politicized milieu (e.g., Addams [1902] 2002; Eliasoph 1998, 2011; Fraser 1992; Habermas [1984] 1987). Choosing an evangelical Protestant comparison case for the argument similarly was an adoption of the community of inquiry's relevance criteria more than an immediate answer to an empirical puzzle inside one of the cases. At the time of the Lakeburg study, scholars of religiously based civic engagement (Hunter 1994; Smith 1998 see also Wuthnow 1988) were concerned by the differences between theologically conservative and theologically liberal strands of America's culturally dominant religious tradition—Protestantism (pp. 34-35, 141-42). The putative ''relationalism'' of conservative evangelicals might reasonably produce bridging ties different from those of theological liberals in the study. In short, our account of contrastive explanation builds on analytic induction, complementing and extending it by highlighting the role of theory in the process.

A compendium of the Lakeburg cases and their function in contrastive explanation appears in Table 1. Taken together, cases in this chart represent a well-defined why question, with a topic (the two-way bridging ties developed by two Park Cluster projects), a contrast space (a series of projects that developed one-way ties or folded), and implicitly, a series of relevance criteria offered by communities of inquiry as discussed previously. Theoretical relevance criteria were central to defining the why question and the incumbents of the contrast space and differentiating the two. They induced examination of coalition settings rather than church committees. Given the why question, the next step is, of course, to identify a plausible answer and cast doubt on alternative hypotheses.

**Table 1.** Lakeburg Cases and Their Function for the Argument of *Elusive Togetherness*.

| Case | Group sponsorship | Function for the argument |
|---|---|---|
| Parish nurse program | Park Cluster/Urban Religious Coalition | Topic: Two-way bridging ties |
| Eviction prevention fund | Park Cluster/Urban Religious Coalition | Topic: Two-way bridging ties |
| Summer camp | Park Cluster/Urban Religious Coalition | Initial contrast space: One-way bridging ties |
| Free meal project | Park Cluster/Urban Religious Coalition | Initial contrast space: One-way bridging ties |
| Fun evenings | Humane Response Alliance/Urban Religious Coalition | Initial contrast space: One-way bridging ties |
| Food pantry network (proposed) | Humane Response Alliance/Urban Religious Coalition | Initial contrast space: Failure of the project altogether |
| Justice task force | Humane Response Alliance/Urban Religious Coalition | Extended contrast space-investigate alternative hypothesis: One-way bridging ties |
| Adopt-a-family project | Project Hope | Extended contrast space- investigate alternative hypothesis: One-way bridging ties, ultimate failure |

## Answering the Well-defined Why Question: Developing and Refining a Causal Account

In principle, what happened with the two-way projects in Park Cluster has an infinite number of counterfactuals and an infinite number of real causes in its history. Encasing all the coalitions and projects as instances of civic action limited but did not strictly determine candidates for a good causal account. Developing the contrast space, assisted by theory as we have described, enabled the research project to produce an answer to a well-defined why question. In this process, a rough hypothesis derived from Tocqueville became much more specified. Eventually, continued observation of Park Cluster led to the hypothesis that the *same* group might host two different models of relationship. Observations supported the claim. Field notes based on the assumption of one model for one group needed recoding and formerly puzzling observations took on new significance (p. 173), such as the observation that attending Cluster meetings could sometimes be like watching two movies at the same time (p. 178). Constant comparison of

observations revealed that each model incorporated not only different relations but a different set of meanings for "volunteer" work.

One of the two models in Park Cluster made Cluster members into "helpers," adjuncts who took direction from either a social worker or a staff person from the office of the URC that first initiated Park Cluster. Within these relationships, Cluster members assumed "volunteering" meant benevolent service to needy others, planned by experts who know what is best for the served. This model of relationship and this definition of volunteering produced one-way bridging ties in projects that constituted the contrast space. Listening to conversations at monthly meetings, the researcher saw how volunteering on this model led to after-school tutoring and free meal servings, for instance.

On the other model, Cluster members were planners and collaborators rather than adjunct taskers. Within these relationships, "volunteering" itself meant being a "partner" who had obligations to other partners, more than a helper obligated to a manager. Volunteering on the model of "partnership" induced different kinds of conversation. While as "helpers," Cluster members spent meeting time reporting on tasks accomplished, as "partners," they sustained much more open-ended pondering, critical and self-critical conversation. The study reported that when Cluster members related to each other on this partnership model, not the helper model, they would talk self-critically about how white and middle-class church people like themselves should relate to residents and leaders of a low income, African American, and Hmong neighborhood. This model was dominant when members worked on the two projects that constituted the "topic" cases of two-way bridging ties.

Other volunteer groups and coalitions outside Park Cluster in the Lakeburg study's contrast space operated on a small variety of models but not on the partner model. These sustained one-way bridging ties or folded altogether. In short, something about the partner model made the difference between the topic and the contrast space. There was no natural relation between the model and the type of service offered by a project. Members easily could have acted as "helpers" to the parish nurse,[24] for instance, instead of trying to "partner" with her and act on behalf of neighborhood concerns. Observing that there was a contrast between topic and contrast space even *inside* the Park Cluster coalition helped the research refine the guiding hypotheses again by highlighting differences in everyday interaction that went with each model.

Eventually, the study developed terminology to build these observations into an explanation of variation: Different models of group interaction

allowed different amounts of *social reflexivity*. The study defined social reflexivity as the kind of conversation in which actors talk openly and critically about how they relate to other groups, institutions, or population in the larger social world (Lichterman 2005:50). This term captured and colligated many details that distinguished Park Cluster projects acting on the partnership model from those that acted on other models. When Park Cluster members acted on the partnership model, they sustained a lot of social reflexivity, as when they routinely asked the nurse to document which services the different ethnic populations in the neighborhood wanted. They would talk about what their role as white and middle-class outsiders in the Park neighborhood should be, and consider when they were overstepping it. When acting on the other models, in contrast, both Park Cluster and the other groups in the study would actually *cut off* conversations about how to work with different populations or how to decide "how do people like us fit in here" (for instance, pp. 127-29; 154-56), particularly at points where a broader, nuanced conversation about external relations could conceivably have opened the way for two-way ties. Social reflexivity distinguished the topic and contrast space.

## Why Questions, Ethnography, and "Meaning"

The different meanings of volunteer activity in the Lakeburg study (e.g., "helper" vs. "partner") certainly were not the only meanings that existed for people in the volunteer coalitions under study. For example, Cluster volunteers identified with different Protestant denominations. Given a very different topic and contrast space, developed in relation to a different encasement, it is possible that these denominational differences could have mattered for a different sort of causal argument. But for the why question constructed previously, the different meanings of "being a volunteer" turned out to be inextricably part of the forms of group relationship that made the causal difference for two-way bridging ties.

Many ethnographers take "meaning" as inextricably part of any analysis. However, for the type of contrastive explanation we propose to follow here, we must emphasize that the causal history of the Lakeburg groups might have turned the analyst's attention to noncultural as well as cultural or meaning-centered causes in attempting to answer the well-defined why question that was set out. Perhaps a difference in material resources, or opportunities or constraints placed by governmental agencies, made different Lakeburg groups and projects more or less able to cultivate two-way ties. As described previously, initial case selection helped narrow the

possibility that these would be determining factors. Then the research examined these alternative accounts (pp. 74-82, 163-65, 209-10) with the cases at hand, finding that while relations with county welfare agents varied among the projects in the study, the groups' bridging ties were almost all one-way when accomplished at all. The resources available to the HRC's food pantry project, from local congregations and governmental advice, were not less than those available to Park Cluster from its members' smaller number of home congregations, yet the food pantry project foundered. A focus on models and meanings of relationship did not necessarily imply that resources and institutional opportunities would play only little part in the Lakeburg groups' development. These other factors, however, did not clearly differentiate topic from contrast space. Thus, the study could speak to a broader community of inquiry inspired by Tocqueville, some of which was concerned with noncultural factors in civic action and some of which was concerned with meanings. For ethnographic research that wants to engage topically related research that uses other social research methods, the emphasis on well-defined why questions and their causal answers provides an important communicative bridge, since it renders clearly the stakes of argument about what ''really'' explains the topic of study.

## The Emergence of Ethnographic Explanations via Answers to Well-defined Why Questions

At this point, we can take a step back from our description of the Lakeburg study and highlight two distinctive features of the knowledge claims produced by our method of explanation, illustrating each with a different study.

1. The explanation derives its intellectual torque from the highly specific difference for which it can account.

The ethnographic explanation is shaped by a closely specified topic and contrast space, informed in various ways by theoretically driven relevance criteria, dialogue and meta-dialogue with communities of inquiry. This process leads to the identification of a precise difference that needs to be explained. The ethnographer uses intimate knowledge of the setting to answer the why question that articulates this precise difference.

Thus, the explanation need not be a *totalizing account of its case,* either in terms of all factors relevant to its ''outcome'' or in terms of all the meanings in the ''cultural system'' that may manifest in the case. Rather, it addresses

some causal factors, possibly including meanings, which differentiate the specified topic from its built-up contrast space. One implication of this is that ethnographers can enter wide-ranging, methodologically varied dialogue about a general phenomenon, such as "civic action" or "urban poverty," by *specifying and narrowing* their causal claims about the case that they have studied in depth.[25]

The Lakeburg study was able to develop causal precision because it acquired in-depth comprehension of different settings—the varied, sometimes short-lived projects of the HRC, for instance. The contrast space expanded, as the varied projects under study became *precise counterfactuals* for the two Park Cluster projects that produced two-way bridging ties. This is a point where contrastive explanation meets Katz's insistence that ethnographers use "luminous data" to build explanations. As the specifics of the contrast space evolved in dialogue with relevance criteria, certain pieces of data *became* luminous, because they specified "the other way things could have gone" with Park Cluster projects that produced two-way bridging ties.

This is one of the reasons that ethnographers may develop particularly strong causal claims about their own cases. In the literature on comparative–historical sociology and comparative politics, researchers have struggled to articulate an understanding of what makes a counterfactual "good." For example, Goertz and Mahoney (2012) attempt to apply the "minimal rewrite rule" for counterfactual scenarios to actual branching points in history. This is how they propose to articulate the intuition that, for example, "Al Gore wins the 2000 U.S. Presidential election" is a better counterfactual than "Mother Teresa wins the 2000 U.S. Presidential election." But the minimal rewrite rule does not seem to be an adequate formalization; some counterfactuals, though "not minimal" might nonetheless be "plausible." Ethnography's relatively many observations of precisely situated action over the course of a study make it potentially a vital route to good counterfactuals. We suggest that this kind of explanatory precision, produced by identifying what particular aspects of one line of causality are different from those of another, is one of the reasons ethnographies are often written in monographs. In this sense, they share a great deal with explanations in the historical profession. Over the course of their texts, in other words, ethnographers try to understand the difference between "plausible" and "possible" (Hawthorne 1991) and then explain the difference, for a given sequence of action, between the plausible and the actual. Ethnographers do this by building comparisons from within the setting.

David Smilde's (2007) *Reason to Believe*, on evangelical conversion in Venezuela, is an excellent example of how in-depth familiarity with the field, combined with theory, sensitizes the pursuit of counterfactuals. Smilde's ethnographically centered, multimethod study immersed him in the environs of evangelical churches in poor neighborhoods of Caracas, where he could come to know evangelical men, their nonevangelical friends, and former co-believers. Previous researchers equivocated on whether it really was possible for converts to "decide to believe" in order to enjoy this-worldly rather than properly spiritual benefits; they had implied that deciding to believe had to involve other reasons than simply wanting to become more materially secure, happy in marriage, or free of alcoholism (pp. 5-7). After meta-communication with the community of inquiry about its ambiguous treatment of this issue, Smilde built a conceptual platform that could support the question of whether or not people do in fact make a reasoned *decision* to believe, for this-worldly ends. The platform was a theory of "imaginative rationality" which grants that we need not assume any a priori, fundamental difference between this-worldly benefits and eternal salvation as practical reasons for converting. Thus, the study's central why question was *not* the standard "why do some Latin Americans convert to evangelical Protestantism?" but "why do some *decide to believe* while others do not?"

The study compared converts with nonconvert men whose social and life circumstances were similar to those of converts, as well as men who decided to believe but then left the evangelical life. Among evangelicals, it compared men who used evangelical terms to explain why they were able to give up drugs or infidelity with men who used evangelical terms to explain why they had so far been unable to do so. Among men who had failed to make good on the worldly benefits that had made them decide to believe, the study found cases in which a failure of worldly benefits led men to drop their evangelical commitments and cases in which failure did not end evangelical commitment.

Identifying and investigating these contrast and subcontrast cases would not have been possible without opportunities to become familiar with evangelicals and ex-believers and their social worlds over an extended period (pp. 41-43). Interview questions designed to explore sensitively how the men used evangelical reasoning to explain personal failures or successes depended on knowledge gained from considerable ethnographic work first (p. 135). But further, as with the Lakeburg study, the procession of contrast cases in this study highlights the role of distinctive relevance criteria that stem from theoretical interests and not directly from empirical findings

themselves. Smilde *made* cultural meaning a relevance criterion for a causal account that also highlighted the factor that other scholars of conversion had highlighted, the role of social ties; for Smilde's study, a relevant explanation would need to combine meaning and network structure. Successive contrasts enabled Smilde to argue that different this-worldly reasons had differing religious meaning, and some were more likely than others to lead to a decision to believe (pp. 119-26). We think of these contrasts as making the why question especially precise, producing plausible counterfactuals vis-à-vis the topic of the investigation.

    2.  Ethnographies combine this sort of case-based counterfactual reasoning with a "process-oriented" approach to causes in social life.

In the Lakeburg study, the difference between two-way ties and one-way ties or projects that folded altogether was first traced to differences between "partner" and "server" understandings of volunteering. Then, elaborating on this reflection via field notes, these two understandings were identified as producing differing levels of social reflexivity, which in turn made two-way bridging ties possible. Thus, this explanation emerged as *processual* in the way defined by Andreas Glaeser. Glaeser (2006:31) has argued that ethnography can and should picture "the social in terms of a dense thicket of processes analyzable in terms of interconnected, often projectively articulated action-reaction effect flows."

Blee's (2012) *Democracy in the Making* offers a marvelous example of a process-oriented approach. As Blee put it, "instead of groups, I compare sequences of action and interpretation as defined from the perspective of activist groups themselves" (p. 14). Blee cased her main subject of study as "emerging activist groups" and collected a massive body of data that could illuminate relatively "established" as well as very newly emerging activist groups in Pittsburgh (p. 10) by way of group documents and interviews with members as well as systematic ethnographic observations.

The study answers the question of why some tiny, incipient activist groups *became* the relatively well-established social movement organizations that other scholars then study, while many took a variety of paths to relative obscurity, or dissipation (p. 6). The answer was that "sequences of action and interpretation" opened or closed options for future action (see also Blee 2013) in different ways, somewhat parallel to the Lakeburg study's reasoning that either strong or short-circuited processes of social reflexivity led to different kinds of bridging ties. Unfolding action itself was the scholarly interest in emerging activism. The study cleared conceptual space for that

interest by pointing out (pp. 6-8) that the community of inquiry around social movements was much more accustomed to macrostructural causal factors and focused little on small-scale sequences of emergence.

Note, furthermore that this emphasis on process complements the development of a *precise difference to explain.* The study did not attempt a totalizing explanation of emerging activist groups, one that reasonably would include the large social and political structures other researchers have focused on. Rather Blee's account closely specified a topic—emerging group pathways, and constructed a contrast space trained by distinctive relevance criteria, including the notion of turning point (pp. 38-39). Adapted from Emirbayer and Mische (1998), the notion served as a relevance criterion for what otherwise might have become an indefinitely expanding contrast space of relatively path-dependent trajectories (pp. 35-37) with myriad varying features. The criterion of searching for turning points turned Blee's attention to particular events or collective decisions within trajectories that could switch collective action onto a different track. An animal liberation group in Blee's study, for example, decided to try forcing local restaurants to stop serving foie gras, produced from the bulging livers of force-fed geese. Retracing the trajectory of the group revealed that this campaign was not just like others mounted against fur or meat-eating, in part because members defined it as a signal of allying with a national animal rights movement. The focus on turning points shaped the contrast space, providing a precise basis for comparing how different ''action-reaction effect flows,'' as Glaeser puts it, created different collective self-understandings that would shape activists' subsequent action, closing off former options for acting collectively.

## Third Theoretical Moment: Generalization From Ethnographic Explanations, or, Meaningful Mechanisms

The third theoretical moment of interpretive explanation in ethnography condenses the investigator's knowledge of variation within the study into a theoretical account of that variation. That theoretical account can ''travel'' to other cases, including nonethnographically based ones, other sociological explanations, and debates about other encased social phenomena. Thus, in this third theoretical incision point, we address the ever-present tension between particularity and generality in sociological ethnography. To do so, we borrow from the literature on social mechanisms, which can enable the explanations that emerge from ethnography to be quasi-generalized.[26]

## Mechanisms: Two Approaches

The proliferation of talk and writing about social mechanisms risks judgments of some incoherence. One thing is clear however: Mechanisms are considered to be "causal structures," in the sense of configurations of entities, elements, or processes that are patterned in an identifiable way (they are "structured"), and which bring about effects, net the effects of other mechanisms (they are "causal").[27]

We propose to avoid the confusion about and multiple definitions of social mechanism by focusing on a single distinction: Does mechanistic reasoning in sociology adhere to the criterion of modularity? Answers in the affirmative hew to one, highly analytic definition of "mechanisms." Answers in the negative move toward an understanding of mechanisms commensurate with a "process-oriented" approach to ethnography, and perhaps, sociology as a whole.

"Modularity" is the criterion, introduced by philosopher James Woodward (2002, 2003), that to model causal mechanisms, one must be able to break down a mechanism into component "modules"—relationships between variables that continue to obtain under intervention and which can be independently "tweaked" or intervened upon, without upsetting the interrelationships between modules within a mechanistic system. Invariance of the relationship under intervention defines the module as causal.[28] It remains to be determined precisely how far this understanding of mechanisms can be carried into the social sciences, whose discussions about mechanisms range wildly over different definitions. Woodward himself expresses skepticism about psychology (2002:377; for more positive views of the potential of this approach, see Knight and Winship 2013; Morgan and Winship 2007:219-42). But surely, this criterion, when applied to sociology, favors an austere, analytic approach to mechanisms that is confident it can "dissect the social" (Hedstrom 2005) and cut the world at its joints in generating causal models.[29] Recently, Diane Vaughan has connected "analytic ethnography" to "analytical sociology" in part by suggesting that ethnographers, too, identify and explain with mechanisms in this manner. Vaughn attempts to mediate somewhat Hedstrom's extremely parsimonious and individual-based account of mechanisms by suggesting that, for analytic ethnography, "the assumption is that individual choice is rational, but the social context shapes what is perceived as rational at a given moment" (701-702). Both Vaughan and Hedstrom agree, however, that such explanations are founded in a singular theory of action. Grounding explanation via mechanisms in this way certainly has its advantages: A theory of action, particularly of the beliefs, desires, and opportunities kind, can be constructed that accommodates

modularity criteria, in so far as, within the actor, one belief or desire could be altered without altering the others, and thus the outcome of the larger causal system predicted.

However, these models of action are problematic for many ethnographers who would see their austerity as disabling interpretive validity. This pushes ethnography toward an alternative understanding of mechanisms, which we extend here. The second approach to mechanisms retains the notion of a mechanism as a causal process that can be traced via evidence and thus explains *why* a certain aspect of social life went the way that it did. However, it drops the modularity criterion in favor of a more holistic understanding of a social world that emerges at the conjuncture of several intertwined and overlapping "mechanisms" or processes. Rather than model these parsimoniously, the second approach to mechanisms sacrifices some amount of parsimony in the name of empirical nuance.

This, second view of mechanisms is evident in Neil Gross' pragmatist approach.[30] First, Gross places action on a spectrum from habit to creativity, emphasizing actors' interlinked, habitual responses to problems as driving everyday action, while allowing that creative responses may occasionally rework collective habit. Second, Gross argues for a semiotic approach to meaning as an accompaniment to a pragmatist theory of mechanisms; this distances his view from the reduction of meaning to belief or "propositions about the world" (Hedstrom 2005; cf. Gross 2009:369). Thus, Gross moves to a position wherein meaning is not conceptualized as "possessed" by individuals with certain beliefs but rather is prone to emerge as parts of various settings, situations, and interactions. Tavory and Timmermans (2013) pick up where Gross left off but take a turn toward C. S. Peirce's semiotic theory of meaning, and away from habit, creativity, and problem solving. They argue that chains of signs provide the ground for understanding "mechanisms."

At this point, we are rather far from the original goal of mechanistic thinking, particularly since chains of signs may be of varying importance to a given topic of explanation. To forge a way forward for a nonmodular approach to mechanisms, we can build on this discussion, and especially upon Small (2009:25) and upon the notion of process tracing in qualitative research cultures (Goertz and Mahoney 2012; Mahoney 2012), to define mechanisms in the following way. A *mechanism* is a social process that is reliably traceable in multiple locations (or across case studies) and thus can be pictured in abstract language and exhibits some regularity in its tendency to push toward a certain kind of outcome. This, we propose, is the core of what process-oriented sociologists mean when they use the term "mechanism" without reference to modularity.

Meaningful mechanisms, then, are subset of social mechanisms defined in this way. Meaningful mechanisms explicitly involve signification and interpretation as part of the causal pushing and pulling on the world that they produce. For, while all social mechanisms are embedded in chains of signification, only via a well-defined why question and the ensuing construction of an answer can we discern if and how signification "made the difference." This is, in our view, the most sustainable definition of mechanisms for ethnographic work, one that we hope speaks to nonethnographic research as well.

## Group Style and Social Reflexivity as a Meaningful Mechanism

Previous work had shown that organizations, whether formal or informal, coordinate themselves by way of "group style." In this literature, group style referred to a set of relatively stable, shared definitions of what constitutes good membership in a group setting. The researcher can identify group style in a setting by looking for three interrelated dimensions: how participants define boundaries between "we" and "not-we" on a larger, shared map of reference points; how participants define group bonds or the kinds of obligations they have to each other while in the group setting; and how participants share speech norms that valorize some kinds of conversation and discourage or silence others (Eliasoph and Lichterman 2003). Different sets of map, bonds, and speech norms constitute different group styles. The same group of people may change style. Rather than say a group exists and then chooses a style, group style is what constitutes a collectivity as a particular kind of group. Hence, the same "group" of people might enact more than one style together and that is what happened in Park Cluster. Group style is fuzzily perceived both by actors and the ethnographers who study them, and conversational awkwardness, unintended slights, and disagreements over "what we're really doing here" are good clues (Eliasoph and Lichterman 2003; Lichterman 2012). The Lakeburg study proposed that a crucial part of the meaningful difference between a "partner" understanding of volunteering and a "helper" understanding was a difference in group style, at least roughly akin to other differences in group style in previous literature.

Thus, the argument emerged that a distinctive group style made Park Cluster sometimes capable of the social reflexivity that led to two-way bridging ties. Volunteering in the partner style, rather than as helpers, involved imputing different bonds of obligation to members, invoking a different imaginary map of the wider social context, and valuing different kinds of speech. As volunteer "helpers," Cluster members mapped themselves mainly in relation to the neighborhood social worker they wanted to assist, with Park

neighborhood residents as a relatively distant "needy" population off to the side. Members depended on the social worker, obligated each other to carry out tasks efficiently, and valued reporting on tasks rather than exploratory conversation. In contrast, as "partners," Cluster participants put the Park neighborhood itself at the center of their map and obligated each other to take responsibility for the Cluster rather than relying on the social worker. After enduring a series of strains and embarrassments, Cluster members instituted a more formal group format, symbolizing that they had mutual responsibilities as partners (Lichterman 2005:180-83). And, the "partner" style routinely opened opportunities for social reflexivity, while the "volunteer helper" style actively shut social reflexivity down (pp. 193-95) in favor of more task-oriented "doing" and less complicated talk.

This mutual implication of a specific "partner" style and the opportunities for social reflexivity that style created is a meaningful mechanism of the sort Gross theorizes. Blee (2013:657) implies that something closely akin to group style crystallizes relatively soon in the life of grassroots citizen groups, opening or closing later possibilities for action. The implicit problem group style addresses is "how shall we coordinate ourselves to work together?" It is an element of culture that is conceived to be relatively "deep" (Sewell 1992), one that actors themselves perceive fuzzily (Cicourel 1993) and take for granted most of the time, though not so settled as to be incontestable over time. By specifying this interlocking between a particular group style and social reflexivity, as a mechanism, furthermore, we can sort the evidence for the existence of the mechanism[31] from evidence of the mechanism's consequences—in this case new bridging ties. To summarize the meaningful mechanism at work: When a particular group style induced social reflexivity, then Park Cluster eventually could share a growing number of two-way bridging ties with the Park neighborhood. Those two-way ties in turn reinforced a particular group style along with socially reflexive conversation formats that could conceive and embrace more two-way ties. The mechanism led to a cyclical relationship, then, and the cycle of partnership group style/social reflexivity and two-way bridging ties produced plans for several new community development projects (p. 214).

## The Role of Relevance Criteria in a Multimethod Community of Inquiry

Ethnography has long attempted to contribute to "middle-range" generalizations that are often taken to be the lifeblood of sociological theory (Becker 1960; Becker et al. 1961; Hedstrom and Udehn 2009; Merton

1968). Here, we are suggesting that developing abstract accounts of ''meaningful mechanisms'' is a particularly useful route to take to this goal, because such mechanisms may be useful to other case studies that colligate chains of meaningful action. This logic of middle-range generalizability seems well-suited to the way in which ethnographic studies of social phenomena such as urban poverty are entering into dialogue with studies of the same phenomena by sociologists trained in other methodologies (Small 2009).

This logic of generalizeability, like that of expanding the contrast space, depended on theoretical relevance criteria which, working together, gave a distinctive platform to the Lakeburg study. Those criteria were hardly automatic and resulted from dialogue with selected communities of inquiry. Those communities offered at least two quite different notions of mechanism, group style and social capital, which potentially could grasp what made the difference between the topic and the contrast space. The first chapter of *Elusive Togetherness* made clear that ''social capital'' was the standard concept Tocqueville-inspired researchers used for explaining data of interest to students of civic engagement. The Lakeburg study did not just depart from the standard concept unannounced but engaged in meta-communicative dialogue with the entire debate on social capital—detractors as well as proponents of the concept (pp. 26-30)—before leaving the concept aside. The Lakeburg study *could* have set out to expand the social capital concept to grasp meanings more closely—as proponent Robert Putnam has implied it might do (Putnam and Goss 2002). That is what immediately preceding, prominent studies of religious advocacy groups had done (Warren 2001; Wood 2002). Instead of taking this seemingly more conventional route, the study appealed to ''listening up close'' (pp. 30, 39-40) with conceptual metaphors that grasped ''conversation'' more precisely than the capital metaphor could. The study used a central relevance criterion of ethnographic research—analytic attention to everyday conversation—as a reason to pass up the social capital concept as a potential mechanism.

As Khalifa (2004, especially p. 49) points out, different methodologies induce different relevance criteria. We add that methodology may induce but does not necessarily guarantee the embrace of distinctive relevance criteria; the accomplished ethnographers cited just before produced their studies affirming if in effect expanding the social capital concept. To summarize, the decision about a mechanism of explanation depended on active, selective theoretical choices, external to the immediate findings of the study. These choices shaped what would count as a finding or an outcome.

The significance of theoretical relevance criteria may be magnified in multimethod communities of inquiry. The social capital concept referred to above is grounded in quantitative studies of civic participation (Putnam 1993, 2000); more ties or networks equal more capital. The Lakeburg study not only addressed this research, as many other researchers do, but the author also noted (p. 3) that his own interest in the subject had been piqued by Putnam's earlier work. If a methodology comes with distinct options— not necessarily mandates—for relevance criteria, as Khalifa implies, then efforts to generalize causal explanations across methodological approaches have a special need for careful meta-communicative dialogue that clarifies which relevance criteria are shaping which studies and why. The alternative is cross talk, unnecessarily frustrated expectations or else missed opportunities to identify different mechanisms of social life.

A different project might have chosen the social capital concept and might have made other discoveries. One potential discovery, implicit in the data but not claimed in the study narrative, would have involved relational proximity. Park Cluster was the only organization in the study that had direct, face-to-face relations over time with the Park neighborhood's leaders and residents. Social capital researchers may have invoked Robert Putnam's social capital concept here, claiming that the trust and reciprocity central to that concept were cultivated differently in Park Cluster than elsewhere, and hence its ability to cultivate two-way bridging ties. Research in this vein may have led to some elaboration of different relational forms that constitute different kinds of social capital. Such an argument would not make the group style mechanism less valuable. *Each* account of a mechanism may have illuminated different causal chains of action, inviting different comparisons to different other studies in sociological literature. Thus, there need not be a single, universally "best" mechanism for a collection of cases. Relevance criteria help limit a potentially endless search for mechanisms, much as they limit the search for counterfactuals. They structure but certainly do not strictly determine the way data will contribute to communities of inquiry.

Ours, then, is pluralist approach to mechanisms that disciplines the pluralism with a careful regard for relevance criteria. Each of our three different theoretical incision points may summon diverse bodies of theory, ones that may not belong conventionally to the same research program or theoretical constellation, just as "group style" was not the conventional mechanism for Tocquevillian studies. Here our stance complements the recent argument of Timmermans and Tavory (2012) on how diverse conceptual sources, rather than one overarching paradigm, can propel the discovery

process. A scholarly decision about casing no doubt conditions the possible why questions but does not entirely predict them and neither necessarily predicts decisions about a meaningful mechanism. The epistemic leverage of mechanisms, in particular, derives precisely from potential explanatory relevance across different substantive areas of study. An abstract account of a mechanism such as social capital or group style may be ''imported'' from another subfield or substantive area of study, if it matches and colligates well the social processes the ethnographer has reconstructed from field notes. This potentially brings diverse subfields with their specific theoretical languages into dialogue with each other about empirical foci.

## Conclusion: Theory in Ethnography and Causality in Sociology

Ethnography is known for engaging the particular in human social life. Specific settings, local rituals and traditions, and idiosyncratic members' meanings are all, if not the singular provenance of, at least well-known territory for the sociological ethnographer. Ethnography has been associated with ''humanist'' understandings of sociological science, which view ethnography as an open-ended method that can create multiple interpretations in trying to understand what other people are up to. We share this understanding of the ethnographer as an interpreter of people, suspended between scholarly colleagues and the people under study—but we propose that the consequences for ethnography's contribution to causal accounts have been misunderstood. So we have examined how ethnographers can use theory to build causal explanations and contribute to middle-range generalizations in sociology. Doing so, however, involves *embracing* ethnography's close-up access to the relationship between meaning and action, while pursuing a contrastive explanation that can answer a well-defined why question. Here, we distill some main implications for ethnographic observation and writing from the three theoretical incision points we have discussed. Then, we propose that in developing contrastive explanations, ethnographic practice connects up with and perhaps informs a more general understanding of sociological explanation.

### Contrastive Explanation in Ethnography: Synthesis

We have argued that theory articulates ethnographic investigation all the way through a study, from the initial focus or refocusing provided by casing, to decisions about the topic and contrast space for an explanation, to the choice

of concepts we use to discern causal mechanisms. Our exposition highlights these points:

1.  Casing emerges through ongoing, sometimes critical dialogue with communities of inquiry.

Casing means articulating a set of observations in relation to preexisting concepts, debates, and lines of inquiry—"theory" in short. Casing may involve meta-communicative dialogue that scrutinizes assumptions underlying those concepts, debates, and lines of inquiry. Different studies have greater or lesser need for extensive meta-communication, depending on the initial research question and the inquiring communities at hand. The evangelical conversion and emergent activism studies we referred to before, as well as the Lakeburg study, all show how casing with relatively extensive meta-communicative scrutiny can clear conceptual room for new questions, and ultimately new causal accounts, that are difficult to conceive within the constraints of received debates or frameworks. By casing or re-casing, the ethnographer may actively reconstruct a debate or line of inquiry from previous studies rather than treating studies as fixed elements of preconstituted literatures. For this theoretical moment, then, the essential task of the ethnographer is to construct an argument that the study has a case of X, where "X" is drawn together from theoretical terminologies. In so doing, the ethnographer may elaborate on or reconstruct in a minor or major way the language games of one or more communities of inquiry.

2.  Ethnographers build contrastive explanations on theoretical platforms.

After articulating a series of observations with the theoretical terminology of communities of inquiry via casing and re-casing, decisions about *relevance criteria* also evolve to specify the empirical and conceptual reach of the study. Ethnographic explanation begins with a why question about a topic that is specified in terms of a distinct if expanding contrast space of counterfactual cases. Relevance criteria inform and limit the otherwise indefinitely open process of expanding the contrast space. Those criteria may originate from debates, lines of inquiry, or broader substantive concerns external to the immediate process of analytic induction itself. Extended immersion in field sites expands opportunities to discover potential counterfactual cases, and those counterfactuals become luminous for a study in part because the study's theoretical platform highlights their value. For this theoretical moment, then, the essential task of the ethnographer is to develop a well-defined why question; such a question relies heavily on

theoretical terms for the precise elaboration of its contrast space and relevance criteria.

>   3. The search for social mechanisms happens in the context of a process-oriented approach that produces partial, not totalizing, explanations.

Each of the three studies featured here specified their topics as processes or lines of action the ethnographer could access or reconstruct from data at hand. These studies by no means ruled out the contribution of macrostructural determinants to the topic but intentionally limited their main focus to patterns in everyday action. They condensed those patterns as causal structures (mechanisms) that regularly produce distinct outcomes and could be traced reliably across a variety of cases. The studies demonstrated the conceptual value of *meaningful mechanisms*, such as Blee's (2013) ''discursive rules,'' a subset of social mechanisms that researchers treat as involving meaning integrally in the causal chain of action. Thus, ethnographic explanations may invoke mechanisms from inside or outside the communities of inquiry with which the research began, and from ethnographic and nonethnographic sociology, if those mechanisms conceptually condense patterns of action that helped account for the difference between the contrast space and the topic. For this theoretical moment, then, the essential task for the ethnographer is to join a more general theoretical conversation about causal processes in social life, via an abstract rendering of those aspects of the case study that helped answer the well-defined why question.

## Ethnography, Counterfactuals, and Sociological Explanation

John Goldthorpe's classic statement ''Causation, Statistics, and Sociology,'' contrasts the ''generative process'' approach to causality with both the ''robust dependence'' and ''consequential manipulation approaches.'' The first is the basis of mechanism-centered sociological explanation and the preferred route for Goldthorpe. Consequential manipulations, on the other hand, are based on the logic of experimental design and are the understanding of causality most closely allied, in Goldthrope's view, with counterfactual dependence. However, more recently, an understanding of a connection between mechanistic causality and counterfactual dependence has emerged in those areas of quantitative social science that take up the treatment-and-control approach (Imai et al. 2011; Knight and Winship 2013), buoyed by a similar conclusion in the philosophy of science (Woodward 2002).

In quantitative work, this interdependence between counterfactuals and mechanisms is clarified via the modularity criterion. In ethnographic work, a similar interdependence obtains, but for very different reasons, since modularity has been dispensed with, given the process approach to mechanisms outlined previously. In ethnography, the intellectual operation whereby a contrast space is built (outlining a good causal question) and answered (accounting for variation within the particular context of the study) is followed very closely by efforts to conceptualize in mechanistic terms the process that produced a difference between topic and contrast space. In ethnographic explanation, then, comparisons that emerge from what is, informally speaking, a particular context (religious community service groups in Lakeburg), highlight *each other* in a way that points to particular *aspects* of a causal chain as worthy of attention. In this case, something about Park Cluster illuminated something about the HRA. In seeking out contrasts to Park Cluster, *Elusive Togetherness* created a kind of internal conceptual system that could identify the specific difference-making aspects of the Cluster projects that produced two-way bridging ties. The ethnography as a whole discerned the productive mechanism of group style and social reflexivity, which accounted for the difference between projects that produced two-way ties and projects that produce one-way ties or no ties at all. But this difference had a very specific, concrete meaning, given by the way in which the other cases in the study provided an understanding of "how things could have gone" in the Park Cluster projects that ended up producing two-way ties.

Thus, within the conceptual space that emerges from observing a particular arena of action, comparisons illuminate each other by identifying particular aspects of the study as causes worth focusing on in detail. The answers to the well-defined why question emerge via the illumination of difference-making aspects of a given case. This illumination—which, as we have shown in this article, relies deeply on theory—points ethnographers toward precise counterfactuals vis-à-vis that which is being explained. These counterfactuals then support the mechanistic argument that is built on them in the third moment of theory use.

The account of ethnographic explanation we propose here does not draw on the experimental logic that underpins counterfactual approaches to causality currently favored by some quantitative sociologists (Morgan and Winship 2007). However, it does share an overall intuition with them, albeit one given a much more case-focused, interpretive cast. This intuition is that explanation is an intellectual operation of accounting, via the causal history of an event, outcome, or phenomenon, for the *difference* between what

happened and what "would have happened," where the "would have" is instantiated by an effective real-world counterexample. In counterfactual explanation via treatment and control, the real-world counterexample is the control group; in ethnographic explanation, it is the fully elaborated contrast space. Furthermore, the view of ethnographic explanation presented here shares with recent developments in comparative–historical methodology an emphasis on using quasi-general theories of mechanisms to explain very carefully defined or "encased" events or outcomes (Gorski 2009; Mahoney 2004, 2012:570; Tilly 1995:1601-2). In a paper in progress, we attempt to develop the idea that, by building contrast spaces out of historical cases, comparative–historical sociology can also develop more precise, theoretically informed counterfactuals as part of its causal arguments.

The intensive ethnographic case study has long been a subject of sociological debate concerning the validity criteria for ethnographic research and the relationship between these criteria and those that apply to other methods in social science. One recent contribution to this conversation has argued that "ethnographers facing today's cross-methods discourse and critiques should pursue alternative epistemological assumptions better suited to their unique questions, rather than retreat toward models designed for statistical descriptive research" (Small 2009:28). It is important to note that this argument—which calls for "logical rather than statistical inference, for case rather than sample-based logic, for saturation rather than representation"—is made in the spirit of engagement with standard quantitative methods in sociology, because certain substantive areas of sociology involve scholars trained in different methodologies. Our argument comes from a similar spirit of engagement, regarding a different though related problem, namely the link between ethnography and theory in the pursuit of explanatory claims. Our understanding of that link, presented here in three parts, is possible because sociological thinking on causality and explanation has developed so far beyond typified images from the philosophy of science of the mid-twentieth century. This article is an attempt to extend those intellectual currents by bringing ethnography into the conversation.

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## Notes

1. As with many essays on methodological choices, our account here is both descriptive and prescriptive, but intended in a pluralist spirit. We do not propose that all ethnographers must follow our three-part use of theory, nor that all must produce causal explanations in order to have worthwhile studies.

2. Abend (2008) has suggested seven different uses of the word ''theory'' in contemporary sociology. The first, "theory1," is what we identified as a logically interconnected set of general propositions above; In contrast to this, we would argue that methodological discussions of ethnography tend to draw on what Abend identifies as ''theory3'' (a way of making sense of a given phenomenon) and ''theory5'' (''an overall perspective from which one sees and interprets the world,'' 179), and very occasionally ''theory4'' (''the study of and the students of the writings of authors such as Marx . . . ,'' 179). We argue that theory use in ethnography can result in partially generalizable claims to what Abend calls theory2 (''an explanation of a particular social phenomenon,'' 178).

3. These communities also participate in and work to enforce certain norms of publicity and argumentation as constitutive of what qualifies as inquiry. What this creates is certainly a field of struggle, constituted by certain core values about the pursuit of truth (Weber 1946), but in what follows, we view these struggles as Peirce did. We see them as oriented toward, and sometimes successful at, bringing knowledge into a better ''conforming'' relationship to that which it seeks to represent (Longino 2002:117-23).

   In the ''Fixation of Belief,'' Peirce (1992a) details four conceptual methods whereby doubt can become belief—the method of tenacity, the method of authority, the a priori method, and the method of science. The latter is considered to be that which enables the production of truth about the world, because it is public, *in principle* accessible to all who are willing to learn it, and which ultimately puts our theories into contact with (1) the resistances that the world offers up and (2) the combative criticisms of our colleagues. As Talisse (2004:25) writes, for Peirce, the method of science creates a community of inquiry, while the other methods are likely to produce communities of doctrine.

John Dewey built upon Peirce's ideas but emphasized much more how the norms of democratic interaction are a cognitive imperative for scientific inquiry, and furthermore how several communities of inquiry need to intersect—particularly so that expert knowledge does not become insular. These considerations have been taken up, in different ways, by the philosophers of social science James Bohman (1993:186-231) and Helen Longino (1990).

4. Michael Burawoy's articulation of the extended case method maintains this vision of theory and its community. For extensive discussion of this approach, from varied viewpoints, see Timmermans and Tavory (2012), Tavory and Timmermans (2009), and Lichterman (2002, 2005). It is also worth noting that Burawoy's combination of Lakatos and Kuhn was itself a synthetic departure from the *opposition* between "paradigms" and "research programs" that was common in the philosophy of science at the time.

5. For a wildly different but relevant version of this argument that the use of theory somewhat independently of data is essential to understanding and contributing to scientific progress, see Leifer (1992).

6. An interesting parallel between classic quantitative sociology and our argument about ethnography emerges here. In both cases, meta-communication in the community of inquiry that articulates with theory, data generation, and the emergence of well-defined why questions becomes essential to scientific progress. In his account of how why questions relate to data-generating procedures in social science, philosopher Kareem Khalifa (2004: 44) discusses Otis Dudley Duncan's departure from the 1947 National Opinion Research Center (NORC) survey that asked respondents to rank the standing of occupations, and toward Duncan's own socioeconomic index, which ultimately led to Blau and Duncan (1967). Khalifa writes, "To establish the superiority of his index, Duncan argued that it supplemented other methodological conventions—specifically theorizing, replicating data, and producing new data—better than the NORC scores." He later reiterates, "As discussed earlier, the theoretical consideration that prompted Duncan to favor his index over the NORC score as a reliable data-generating procedure was that he could provide a plausible account of why occupational status was a function of median income and education...In contrast, NORC's data-generating procedure posited the avowed attitudes of individuals as the only cause of occupational status" (50).

7. This accords well with Peirce's intention; he in fact imagined science, and the understanding of reality on which it was based, in terms of an *indefinite* community of inquiry: "The real, then, is that which, sooner or later, information and reasoning would finally result in, and which is therefore independent of the vagaries of me and you. Thus the very origin of the conception of reality shows that

this conception essentially involves the notion of a COMMUNITY, without definite limits, and capable of an indefinite increase of knowledge" (Peirce 1992b:52).

8.  These projects included orchestrating a summer camp for kids in a low-income neighborhood, staging nighttime entertainment programs for "at-risk" teens, mentoring ex-welfare recipients in an "adopt-a-family" program, and running educational workshops on the politics of welfare reform, among others. The study investigated nine, but the ninth was a very short-term effort by two women to serve free meals; the study employed it for a very brief contrast with another case, and it was the only grouping whose core participants did not span different churches.

9.  A variety of other methodological statements already have discussed how ethnographic researchers make tentative discoveries. We do not fundamentally contest the broad outline of this discovery process, which has been codified as the "constant comparative method" (Emerson, Fretz, and Shaw 1995; Glaser and Strauss 1967; Strauss 1987; Strauss and Corbin 1990). Core to the process is comparing and coding successive observations detailed in ethnographic field notes (for instance, Lofland et al. 2006; Strauss and Corbin 1990). Ethnographic approaches with markedly divergent goals and relations to theory already have characterized and affirmed the constant comparative process in strikingly similar terms (Burawoy 1998:17-18; Strauss and Corbin 1990:59). Our goal here is to show ethnographers can use the constant comparative method in connection with contrastive explanation. In our example, the development of a causal account benefitted from negative cases to challenge and refine, not only support, emergent causal hypotheses, similar to the logic of analytic induction (Katz 2001, 2002; see also Becker 1953).

10. As Ragin (1992) notes, the term "case" itself is a slippery one in sociological conversation. Some studies define a case in terms of the conceptual significance of the empirical observations, while others designate cases as empirical observations or sets of observations themselves—the "n" of cases. Accepted sociological parlance may treat the study depicted here as analyzing either a single "case" of civic action with specified variations or nine "cases" of civic action. Social science commonly seeks out "comparable instances of the same general phenomenon" and the act of casing or encasing those instances according to their commonality is one of the hallmark moves distinguishing social science from other forms of claims making about social life (Ragin 1992:1-2). Given the absence of consensus on the use of "case," we interpret the spirit of these remarks as consonant with our usage: "Civic action" is the shared casing for eight comparable cases; several of those cases have a great deal in common while others were chosen for their contrast value, but the contrasts make sense *inside* the shared category of civic action.

11. A good example of the relatively rare, noninterpretive ethnographic study is Baumgartner's (1988) study of suburban moral order, which constructed its concept of ''moral minimalism'' from observation and suburban residents' reports on action, without claiming to discern what action tagged as moral minimalism might mean to actors in their own terms.

12. The study strongly implied, for instance, that the community of inquiry's common-sense blinders affected the researcher, slowing his recognition that Park Cluster members sometimes did speak in religious terms—in a particular way. The author asked ''what is religious here'' (pp. 216-17) and puzzled over the fact that Cluster members almost never used religious *reasoning* to justify their volunteer work. It was a hard-won realization to see that Cluster members in fact had been using religious terms, but as tags of social identity—not as reasons for action in the way scholarly common sense presumed that religion would become manifest.

13. For example, a study of a Catholic social service agency claimed that a religious vision of charity ''permeated'' the agency, yet evidence clearly suggests some volunteers at the agency changed or shut off religious identities they expressed outside the agency (Allahyari 2000:33, 135). Another study made a similar claim about religious motives uniformly pervading community organizing campaigns, while the author's sensitive ethnographic work clearly conveyed that racial and ethnic identities made religious sensibility multivalent rather than uniform, and sometimes trumped religious identity, frustrating efforts to build intergroup solidarity (Warren 2001).

14. One of the foremost philosophical works on explanation begins its extensive analysis of scientific explanation with a famous joke: a priest asked Willie Sutton ''Why do you rob banks?'' to which Sutton answered, ''Well, that's where the money is.'' The joke relies for its humor on the way the same why question (why do you rob banks?) can really be two (or more) different questions. Sutton was, given his orientation to the world and purposes within it, explaining why he robbed *banks* (as opposed to, say, libraries or convenience stores); the priest was, given his orientation and purposes, asking Sutton why he *robbed* banks (as opposed to using them legally; Garfinkel 1981:21).

15. Martin (2011:24-71, and especially 37-39) points to the tremendous problems with an ''unlimited'' or strictly objective approach to counterfactuals.

16. As Alan Garfinkel (1981:172) writes, ''explanations . . . are essentially pragmatic. The art of explanation *is* the art of throwing away almost all the data and forgetting almost all the conditions.''

17. This example is adapted from Van Fraassen (1980:127).

18. Mark Day (2004) makes precisely this point about explanations of the French Revolution.

19. The assumption about the distinctiveness of coalitions, made at the start of the study in 1996, turned out to be accurate beyond the study too. Systematic research later found that congregations and intercongregational coalitions have different cultural reputations and make different contributions to human service, political and civic relations (Wuthnow 2004).

20. Well-regarded, widely read ethnographic studies of religious advocacy organizations do not necessarily distinguish between settings; see for example Warren (2001); Allahyari (2000).

21. Manuals on participant observation methodology ordinarily teach the need to discover "lived" meanings or those meanings that inhere in the site under study (see, e.g., Emerson et al. 1995; Lofland et al. 2006, especially 132-36).

22. The study used the phrase "public good" rather than "two-way bridging tie." We use the latter term to avoid potential confusion over different meanings of "public good"; our term encapsulates the study's intent with the original term.

23. Thus, as Khalifa (2004) shows in detail, the relevance criteria and contrast space for an explanation develop together, with each influencing the final form that the other takes.

24. In fact that is what the nurse at first expected, but Cluster members insisted otherwise.

25. This presumes that the ethnographer and her counterparts in a community of inquiry share certain commitments. See Small's (2009:6-11) discussion of this issue.

26. In taking up this position, we are following a suggestion from Daniel Little (2012:1), the philosopher of social science, who has argued that "the social mechanisms approach to explanation has filled a very important gap in the theory of social explanation in the past twenty years, between the covering law model and merely particularistic accounts of specific events." It may indeed be that there are certain social processes, the understandings of which are not well served by the metaphor of "mechanism." Here, we focus on the utility of that metaphor as a starting point for this third theoretical moment, the possibilities for which could potentially be expanded upon in later work.

27. There are myriad definitions of social mechanisms; see especially Hedstrom and Swedberg (1998), Elster (1989), Stinchcombe (1991), and Hedstrom (2005) for different versions. It is perhaps particularly important to note that the degree to which regularity should be part of the definition of a mechanism is disputed. As Knight and Winship (2013) point out (see also Woodward 2002), if interpreted strictly, the idea that a mechanism is a process that *regularly* produces the same outcome from the same trigger pushes the mechanism approach to causality toward the same Humean problems that it was in part designed to avoid.

28. To take a classic example used by Woodward (2002) to make his point, consider the relationships between air pressure (A), barometer readings (B), and the probability of a storm (S). In the history of the philosophy of science, this has been a classic example for developing criteria for differentiating *truly causal* correlations (i.e., between changes in air pressure and the probability of a storm) from *noncausal* correlations. In Woodward's rendering of these relationships, the correlation between B and S is *not* causal, because that relationship will not hold if we intervene and manually move the barometer dial. On the other hand, an "intervention" on the air pressure A will change the barometer reading B and the probability of a storm S in a predictable way; the relationship between these variables is "invariant under intervention." So A → B and A → S are causal. Note that changing B *via a change in A* does not count as an intervention on B for the relationship between B and S, because it independently affects S through the causal relationship A → S. Note also that Woodward's understanding of "intervention" is designed to be nonanthropocentric; a human does not have to actually be able to change the air pressure for this model of causal mechanisms to work.

29. In addition to Hedstrom's statement, see also Goldthorpe 2007; Elster 1989; Schelling 1998; for a discussion of the relationship of methodological individualism to explanation via mechanisms, see Demeulenaere 2011:12-24).

30. Gross (2009:365) points out that the understanding of mechanisms that has emerged from rational action theory has been criticized by sociologists who find that tradition relies, often axiomatically, on a notion of "an individual armed with beliefs and desires who steps out of the flow of action to face and evaluate a choice between competing means." Sociologists note that this is in fact an exceptionally rare occurrence.

31. Evidence for the existence of a style, and social reflexivity, came from relatively early in the observations of the cases; evidence for outcomes came later, once the mechanism had crystallized (pp. 275-78). This complements Blee's (2013) recent argument about how activist groups already start to set limiting parameters on action relatively early in their existence.

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