The Unsettlement of Communities of Inquiry

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The Pragmatist account of the relationship between knower and known, and of the development of ideas, informs many of the accounts of theorizing in this book. A particularly strong pragmatist theme is the interruption of habit. For Richard Swedberg, James March, Karen Knorr Cetina, Karl Weick, and others, theorization occurs when routine streams of thought and intellectual work habits are interrupted or upset by “reality,” thus creating problems for conceptualization spurring ideational innovation (Peirce 1903; Peirce 1992b; Peirce 1992c; James 1950; James 1907; Dewey [1922] 1930). Applied to social scientific research, this emphasis on interruption suggests both a focus on how the bureaucratic humdrum of academic life can be subverted, and on designing various creative ways for theoretical speculation to develop out of the interaction between a scientist and her data.

In this chapter, we continue this theme of interruption, but we place theorizing explicitly in its social context and translate it as the idea of the unsettlement of a community of inquiry. We define a “community of inquiry” here as a scholarly community of variable size whose self-definition and occupation with certain problems make the networks of communication between the community’s members especially dense. We then consider how individual social scientists interact with such communities of inquiry, and how bridges that develop between a community of inquiry and aspects of that community’s
social environment can create un settlement in the community's communications, which in turn can spur the kinds of creative theorizing central to the project of this book. We thus analogize the un settlement of a scholarly community to the political, cultural, and social un settlements and restructurings that have long occupied scholars of collective behavior and social change. Much as "unsettled times" (Swidler 2001: 99–103) in societies are times of high rhetoric, ideology, and emotion, so too does the un settling of a community of inquiry prompt the kinds of abstract thought, conceptual reformulation, and emotional excitement that are the hallmarks of theorizing in social science.

Our development of these themes builds toward the following argument:

Theorizing, which via conceptual breakthrough advances social science, is (often, though not always) a product of collective conceptual un settlement that leads to renewed attempts at abstraction, redefinition of the core terms of an area of inquiry, and the creation of new problems and new perspectives. This happens via a process wherein communities of inquiry constituted by dense communication about research become unsettled via connections to various aspects of their environment. This un settlement creates a time of "high rhetoric," emotional energy, and conceptual reconfiguration, within which theoretical breakthroughs may be achieved. This process may lead to a reconfiguration of the community of scholars such that new boundaries are established, creating a new substantive field or area of inquiry.

Constructed as a hypothesis, we would propose that the relationship between un settlement and theorization follows an inverted-U function, with un settlement of a community of researcher increasing the level of theorization to a certain point, after which un settlement begins to cause the community itself to disperse, and that dispersion reduces theoretically innovative communication between researchers. Our work in this chapter, however, is dedicated to conceptual specification via the consideration of illustrative examples rather than hypothesis testing. After discussing the role of individuals in unsettling communities of inquiry, we propose a typology of sources of un settlement for these communities. These sources occupy a gray area between "internal" and "external" sources of intellectual change. The sources of un settlement are, two different kinds of anomalies in the object of study, often technological changes that affect the economy of theorizing, intercommunity idea migration, and something we call "bridging the zeitgeist."

This typology is given a specific twist, however. We emphasize the way in which these sources are different in the social sciences than they are in the natural sciences. We do this by arguing that sociology is both a science and one of the humanities. This brings us into dialogue with the literature on "what's wrong with sociology" (Cole 2001) and with the longstanding question of how to characterize the knowledge project of the social sciences and social theory (Bhaskar 1979; Bernstein 1978; Habermas 1971; Reed 2013), something we discuss before setting out the typology. Near the end of the chapter, our arguments about sociology as a human science and about bridges between a community of inquiry and its environment point us toward a third argument about the relationship between "sociological theory" and "social theory." This, in turn, leads to some practical suggestions regarding theorizing in the social sciences, with which we conclude.

**Theorizing in Social Context**

It is important to resist a view of theorizing as a relatively asocial process, done by a lone theorist in relation to her privately gathered evidence, in contrast to finished theory as that which is brought forth into public view when verification or falsification is required. This image of science is mistaken because it reduces creativity to an individual psychological process, on the one hand, and because it limits the social aspect of science to the singular process of intersubjective verification or falsification, on the other. Instead, we maintain that individual psychological processes of discovery or serendipity intersect in complex ways with communities of scientific inquiry. Thus, while we accept the classical point from the philosophy of science that the specific, idiosyncratic way in which a scientist comes to an idea (e.g., in a dream) should be separated, analytically at least, from an account of how that idea comes to be taken as true, we do not accept some of the confusions that have been attached to this point. Considered more broadly, both discovery and justification are deeply social, even if one can make a normative philosophical distinction between the two processes and how they should be idealized (Aufrechter 2010). Thus we attempt here to address how theorization is itself a social process, and to develop some hypotheses about theoretical growth and change.

We define theorizing as the process, within a community of inquiry, of developing abstract and generalizable languages for understanding and explaining social behavior. To specify how this definition relates to theory growth and change as it is typically understood in sociology, we can begin by noting the definitions of theory and the typology of theory growth developed by David Wagner and Joseph Berger (1985; see also Wagner and Berger 1986; Berger, Willer, and
Zelditch 2005). First, they divide theory into (a) meta-theoretical “orienting strategies”; (b) “unit theories” that model or propose to explain a variety of sociological phenomena, and, finally, (c) “theoretical research programs”—“set[s] of interrelated theories, together with research relevant to evaluating them” (Wagner and Berger 1985: 705). Then, given these divisions, they typologize theoretical change in the following way: elaboration (refinement for precision and explanatory power), proliferation (expansion of a theoretical application to another domain), and theory competition (for theory competition, see also Lakatos 1970: 115). These basic processes lead to some secondary processes as well: variation (specification of different ways a theory can be applied or used in building explanations), and integration (synthesis of different, and even competing theories).

This typology provides, in our view, a reasonable “internal” description of the process of theorizing, one that draws on Imre Lakatos’s concept of a research program, and that recognizes a link between “metatheory” and “unit theory.” And we believe many sociologists would be comfortable with the idea that theory develops through elaboration, proliferation, competition, variation, and integration. However, Wagner and Berger’s approach tends to underestimate (or perhaps deliberately exclude) the social context of theorizing, preferring instead to provide an extremely autonomous view of theory growth as the royal road to scientific rationality. Berger and Wagner do not connect theory development to broader changes in society, to shifts in the interests of intellectuals in general, or even to inputs from disciplines and subdisciplines at the boundaries of the community of inquiry that is doing the theorizing.

What, then, spurs theory growth, via elaboration, proliferation, competition, variation and/or integration? For the Wagner and Berger approach, the answer is quite clear: inconsistencies in explanations offered, and the inability of theories to explain certain social phenomena whose importance cannot be avoided from the perspective of a given research program. In other words, what spurs theorization is precisely what Thomas Kuhn called the accumulation of anomalies. But if we think about scholarly communities as communities, we will quickly come to see that anomaly accumulation is only one of many possible prompts to theorizing that can affect a community of inquiry.

In contrast to the Berger/Wagner approach, which defines theory development as a well but does not account for its social location, the sociology of knowledge provides several useful models for conceptualizing the impact of the social world on knowledge creation. Without undertaking a full review of the developments in this field, we can nonetheless look here for inspiration.

Pierre Bourdieu (1988) discussed the fiscal and demographic pressures on the French academy and how these were translated into struggles over symbolic capital. In the 1980s, Richard Whitley (2000) reconceptualized the sciences as work organizations, primarily competing over and redistributing the resource of “reputation.” And, in a perspective inspirational for the analogical theoretical strategies pursued here. Scott Frickel and Neil Gross reconceptualized intellectual change on the model of social movements, developing a theory of scientific/intellectual movements (SIMs). Taking up the Kuhnian project of accounting for scientific discontinuities, they describe for the conditions under which challenges to the scientific status quo are likely to emerge and to succeed (Frickel and Gross 2005: 204–05). They enumerate some of the social sources of intellectual change, such as generational shifts, lack of fit between the world views of certain high-status actors and those of the field at large, the different social backgrounds of those who enter the academy, and so on.

These perspectives from the sociology of knowledge gain insight from viewing scholarship as an institutionalized form of work, and more broadly, a social struggle. We agree that the social sciences and the humanities, like the natural sciences, participate in a modern university system and marshal various symbolic, organizational, and material resources in efforts to secure position within that system for lead investigators, their students, and their colleagues. It is furthermore clear that these scholarly endeavors operate in an environment that is somewhat autonomous from other areas of risk and reward in modern society, and is thus structured as its own field of struggle, as Bourdieu would emphasize.

However, in this chapter we address the gray area in between the internal development of anomalies identified by Wagner and Berger and the pressures, positionalities, and competition for resources (material and symbolic) that are the classic foci of the sociology of knowledge. When we look at this aspect of the knowledge process, new questions emerge about potential differences between natural and social science, questions whose answers might inflect our understanding of theorizing in sociology. In a way, the literature already recognizes this, albeit not as conceptually central. In Frickel and Gross’s account, and especially in Whitley’s characterization, some rather clear differences emerge between the intellectual dynamics of different disciplines. For Whitley, sociology, in particular, seems to have low interdependence between its workers, leading him to term it a “fragmented adhocrasy.” Why is this? And why is it, furthermore, that certain aspects of the Frickel and Gross model—such as the different world views or social backgrounds of new generations
of practitioners—seem to matter so much more in the social sciences and the humanities than they do in the natural sciences?

Sociology as a Human Science

For a long time, sociologists have debated the answers to these questions in terms derived from, or misappropriated from, Thomas Kuhn—arguing that the social sciences are not yet “mature” sciences, or, in contrast, that the social sciences are “multi-paradigmatic” sciences or constituted by multiple, competing “research programs” (Ritzer 1975a, 1975b; Berger and Zelditch 2002). In strict Kuhnian terms, a multi-paradigmatic science is impossible. Paradigm dominance—and thus the possibility of normal science—is constitutive of mature science qua science. In this way, discussions about Kuhnian paradigms in sociology, though iconic for “post-positivism” and quite common in humanistic parts of the discipline, are incomplete in the vocabulary they provide for understanding the production of social knowledge. In contrast, we seek here a shift in this language, and thus we discuss the social sciences from a different point of view.

In our view, many discussions about the social and/or intellectual structure of the social sciences tend, perhaps because of the continuing prestige and influence of the natural sciences, to undertheoritize a key issue, which is also essential for understanding the process of theorizing in the social sciences. They underestimate the degree to which the social sciences retain various bridges to the culture and politics of the surrounding society, and the movements, events, and emergent forces within that society, in a way that the natural sciences do not, or do to a lesser degree. In particular, communities of inquiry in the social sciences are subject to “external” influence not only in the forms familiar from the analysis of the political economy of big science, but also in more subtle, and more deeply discursive ways whereby the very problems, objects of investigation, and theoretical terminology of the social sciences can be transformed by shifts in the political orientations, cultural interests, and social backgrounds of those who involve themselves in social science research.

In a 1991 article, Zald argued that sociology as a discipline was a “quasi-science” and one of the “quasi-humanities,” and we develop that view here. He suggested that in its effort to achieve scientific status, sociology had neglected its opportunities to become a better humanistic discipline. Sociology could do this, Zald suggested, while maintaining a clear emphasis on “explicit comparison and concern for generalization” and a dedication to “evidential criteria for choosing among interpretations” (Zald 1991: 179). As part of the historical and empirical argument for considering sociology this way, Zald noted how, in sociology, “the press for reformulation may occur because of moral and political currents in the larger society; because events in the larger society and the moral and political evaluation of them lead one to reflect on the adequacy of current formulations” (Zald 1991: 178), as well as because “anomalies” build up. In our view, this has important consequences for how we view theorization in sociology, in particular, and in the human sciences more generally.

In Zald’s original formulation, the core concerns of problem-orientations in sociology follow a pattern that in some ways approximates the humanities more than the natural sciences. For, in sociology, the core concerns are, like in the humanities, “civilizationally rooted.” Zald’s example of this is the rise and fall in sociology of the study of formal organizations, which shows elements of both Kuhnian “normal science” (wherein “research findings accumulate and the research terrain is exhausted” (177)), and of how civilizational concern impacts the relevance and research energy devoted to a subfield (the paradigm faded in part because practitioners “lost their connection to the larger issues which had generated the original question”—namely, moral concerns about the overgrowth of the administrative state and increasing power of managers vis-à-vis stockholders).

To this we would add the point that there is a way in which the objects of research change in social science in a way that would appear quite odd to natural scientists (or for that matter, to a philosopher of natural science). Stephen Cole (2001) makes two arguments relevant here. First, standard textbooks in the natural sciences and the social sciences are quite different. In the natural sciences most of the texts describe key developments that are the accumulated consensus of generations of research; the frontiers are barely mentioned. In the social sciences, and possibly especially sociology, all or most of the text is devoted to current or recent topics of research; it is mostly at the frontier. Second, Cole argues that the objects of study in sociology are in a constant process of change. For example, if one is interested in the status of women in the professions, that status will have changed over some describable time period. Thus the ontology of social life is itself a historical object of analysis (Reed 2011, Hacking 2004).

Furthermore, the interests of faculty and students change as societal processes lead to rising and declining issues of public concern. One of us recently had the experience of asking an entering cohort of graduate students what their interests were. Hardly any of their specific interests would have been in the curriculum fifty years ago. Students were interested in public policy and the
knowledge. In this chapter, we specify that this notion of progress is autonomous from fact accumulation by suggesting that there are many different possible prompts to theorizing in the social sciences, and such theorizing can lead to progress, even though—indeed because—it upsets the working conceptual order of a community of inquiry.

If we consider sociology in this way—as a science and one of the humanities, and as capable of progress in quite different ways—it becomes clear that the prompts for intellectual transformation that Kuhn identifies as "anomalies" are, in the human sciences, much more varied, and much less strictly internal to the "puzzles" set by a paradigm than they are in the core physical sciences that were the true objects of debate for the classical philosophy of science, its modern adherents, and the Popper-Kuhn-Lakatos debates.

Our proposal, then, is that the ways in which a community of inquiry in the social sciences is influenced by its social environment will be significantly more varied qualitatively, and significantly greater quantitatively, than is common in the core natural or physical sciences. Some of the ways this is so have been well covered in social theory. For example, Anthony Giddens discusses the "double hermeneutic" that obtains between the social scientists' conceptual architecture and the concepts and working theories of those she studies (Giddens 1987). This is evident in Weick's work on firefighters, wherein Weick takes both his own and the firefighter's theories of organizations and reliability seriously and constructs a hermeneutic dialogue between them. This is one route whereby "outside concerns" might enter social science—and it is one much mediated upon by ethnographers. However, there might be other routes as well. What is needed is a model of the development of theory in communities of inquiry that accepts both variation in the degree to which "civilizational concerns" or "social meanings" affect the ongoing framing and conduct of research, and the different pathways whereby the influence of such concerns and meaning occurs. In what follows, we set out the preliminaries of a research program on this issue.

Change in Communities of Inquiry:
Individuals and Bridges

Consider as the basic unit of analysis the community of inquiry, loosely corresponding to subfields of a discipline in the contemporary academy, and, in sociology, to the kinds of research programs that make up Berger and Zelditch's
volume New Directions in Contemporary Sociological Theory (2002). These communities of inquiry are characterized by links between mentors and students, co-attendance at small, focused conferences, sustained e-mail communication, frequent co-authorship, and dense co-citation networks. They also share certain abstract theoretical terms that constrain research designs, create the interpretive schemas by which new problems are understood, and ultimately make up the language game of useful, central concepts that are essential to the sociological explanations built within the program. These abstract theoretical terms have varying cognitive ties to those of other research programs.

If this is the basic unit of analysis, then the question is: What causes theoretical growth, breakthrough, and ultimately the conceptual transformation of the abstract communication terms that help tie together a community of inquiry? Such an analysis, we believe, would have to be carried out at two levels simultaneously—one that examines the intersection of individual biographies with the dynamics of communities of inquiry, and one that examines how a community of inquiry develops relations with various aspects of its social environment. We examine each of these in turn.

Individuals

We see two essential ways in which individuals, or very small groups of individuals working together closely, contribute directly to the dynamics of a community of inquiry. First, it is clear that individuals, with their own idiosyncratic biographies, intellectual interests, and educations, can serve as a source of variation in the inputs that are brought into a community of inquiry working on a defined set of problems.

New students replace old ones, retirements shift the emphasis in problem choice, and the creativity, status-strategies, and charisma of individuals can matter a great deal, particularly if the community of inquiry is small, or if individual members of the community of inquiry are especially well positioned in relation to the institutional structures of the academy. Furthermore, individuals’ own idiosyncratic interests could lead to shifts in scholarly attention because individuals’ day-to-day lives may be less “walled off” from their scientific endeavors than we think. Sociologists of social movements may be in social movements; ethnographies can be particularly thorough if they draw on longstanding practical knowledge of a given milieu; and so on. In this way, discourse in the social sciences is subjected to a wider and more intense bricolage process than is likely to be found in the natural sciences.

One version of this “variation based in individuals” is directly connected to the theme of this volume, and is in fact evident in its very construction. For, while it is the case that the community of experts in a given subfield of social science may be largely concerned with transmitting to a future generation a narrow, technically demanding set of methodological practices, the historic connections of the social sciences to social theory and to general intellectual threads in the larger society imply that the palette of discourse for a given individual social scientist can reach far outside what is normative (or even known) within a specific community of inquiry. In James G. March’s case this is evident in his ability to reach into the classic political theory of Rousseau when discussing problems in the modern theory of representation and its tendency to measure the common good by aggregating the public opinions of individuals (March and Olsen 1984), and in his use of the contemporary social theory of Susan Sontag to address theorizing. Similarly, Karl Weick draws on the writings of the painter and installation artist Robert Irwin (1977) to elaborate his own process of theorizing. Perhaps this phenomenon could be used to measure individual variation: the breaching of boundaries around a subdiscipline or a topic might be indicated by the extent to which scholars who are part of a community of inquiry reference intellectual sources that extend beyond the citations that signal membership in the community, and particularly references that do not signal disciplinary membership either (see Shwed and Bearman 2010 for an example of measuring heterodoxy in citation).

But, second, it is also clear that the impact of individuals or small groups on a community of inquiry derives from the human capacity to synthesize disparate ideas and be creative. The capacity for intelligent individuals to reconstruct a community’s discourse, problems, and solutions to problems cannot be underestimated. A great deal of interpretive social theory focuses on producing careful internal accounts of this aspect of individual dynamism—reconstructing the influences on, and synthesis of, classics of social theory so as to spur a shift of attention in the field, for example. Thus, for example, Marx’s synthesis of political economy, socialism, and dialectics is shown to have been more Aristotelean than previously thought (Engelskirchen 2007), and Talcott Parsons is shown to have been engaged in a project of understanding the United States in contrast to the rise of authoritarianism in Europe in the 1930s (Gerhardt 2011). Individuals, then, may directly affect the direction of communities of inquiry via their idiosyncratic biographies or their synthetic abilities. But they may also serve as conduits that increase or strengthen
links between the community of inquiry and its environment. This brings us to the second source of unsettlement: the links between the community of inquiry and its larger environment.

**Links to the Social Environment**

The environment of a community of inquiry includes other communities of inquiry, the academic field at large, and the society at large. Links to this environment, or “inputs” to a community of inquiry from this environment, come in many forms, and they include both the pieces of social reality that are the focal point of study and other kinds of inputs from “society.” Because of this, it is essential to any model of the dynamics of a community of inquiry that we come to some basic theoretical understanding and categorization of these inputs and links. We offer the following typology, which we explicate below: anomalies that emerge within a paradigm or research program working on a certain defined set of social phenomena (of which there are two kinds): radical technological change; inter-community idea migration; and bridging the zeitgeist.

**Strict Kuhnian anomalies.** In the Kuhnian model, unsolved puzzles for a community of inquiry become overwhelming, leading to a sense that accepted theories, assumptions, and methods are inadequate, and thus to a search for possible reconceptualizations. This seems to be clearly what Berger and Wagner have in mind when they discuss research programs and theory development. For example, they discuss how the conflict-spiral theory in social psychology responded to anomalies that could not be ignored. Initially, the theory posited, and supported with a great deal of research, that the following mechanism obtained between interdependent actors: use of threats so as to project strength by A leads to a loss of face for B, so B responds with a threat of B’s own, leading to a “spiral of conflict” (Deutsch and Krause 1960) However, the theory could not account for situations in which threats between interdependent actors lead to the mutual coordination (such as when a threat of punitive action leads to concessions and the avoidance of conflict). Thus Shomer, Davis, and Kelley (1966) posited a difference between threat and actual use of punishment devices, and thus restated the basic theory while expanding its empirical purview. Sometimes, a la Lakatos, this kind of development in relationship to anomalies happens via competition between rather than refinement of various theories (example from Wagner and Berger 1985: 710–12).

**Object-change anomalies.** However, in social science, historical shifts in the nature of social relations can also produce anomalies, which then prompt theoretical revision. This appears to be the sort of anomaly that Michael Burawoy has in mind when he discusses Marxism as a research program. The nature of capitalism, and in particular its mechanisms of exploitation and consent generation, changes, and these changes are, furthermore, spatially and temporally uneven. As a result, Marxist researchers are always revising their theory of capitalism—and this is a mark of the way in which Marxism is a progressive research program rather than a degenerative one, as Lakatos himself believed (Burawoy 1990).

Another example of this type of object-change anomaly is the way in which research on social movements had to shift its overall theoretical architecture in response to the emergence of objects that were clearly social movements, and clearly very important ones, but which seemed to stretch the explanatory capacities of previous theories. In the 1940s and 1950s social movement research was dominated by a set of ideas centered on the core insight that social movement participation was a result of a flight from the anomic isolation of individuals in modern society. Although Erich Fromm (1941) and Eric Hoffer (1951) had very different backgrounds and life experiences, their (now classic) books Escape from Freedom and The True Believer reflected a similar understanding of the development of fascist and communist politics in the first part of the twentieth century. Neither book drew on extensive interviews or other empirical data, but both presented a diagnosis of their times. William Kornhauser’s The Politics of Mass Society (1959) converted this diagnosis into a theory of the organization of political participation that was much more subtle and persuasive. But it, too, centered on one kind of political participation—namely, the entry into politics by low-status individuals without elite and institutional guidance. However, the 1960s and 1970s saw the emergence of a different set of movements with participants from different social backgrounds, and the emergence of scholars who identified with the goals of the movements. This change in the object of study (and in the hermeneutic relationship between author and object of study) contributed to the rejection of the mass society theories. (See Buechler 2004, 2011, for a review and discussion of these issues.)

**Radical technological changes.** The imposition of massive technological change on a field can elevate the usual mundane cost-benefit calculations about scientific problem solving and methodological innovation to a qualitative change in outlook, leading to extensive theoretical justification of a new approach. The clearest example in sociology would be the way in which the lower time and money costs
of analyzing large sets of network or discursive data has allowed for a quantitative solution to empirical issues that would have not been possible prior to the new software analysis programs. So, Fiss and Hirsch (2005) demonstrate how quantitative discourse analysis contributes to the careful analysis of the transformation of the language in which globalization is discussed. Or, Shwed and Bearman (2010) employ network analysis to examine the development of scientific consensus through the growth of common references in substantive fields. In cultural sociology, an important debate is emerging on the use of "big data" in the subfield that has long embraced interpretive and ethnographic approaches (Bail, 2014). In the work of these scholars, the conceptual research design itself, and the theoretical justifications that accompany it, would have been senseless before high-speed computation became readily available.

It is important to understand what is really going on here. For a community of inquiry, technological innovation is unsettling not because it is a threat to theorization or will somehow change the goal of sociological explanation, but rather because it changes the contours and confines of theorization, suggesting new routes for thought. In other words, technological change shifts the "economy" of abduction, identified by Peirce and articulated by Swedberg: "Once you have gotten some new ideas through abduction, you have to make a judgment of economy since work on any one hypothesis entails a serious investment" (Swedberg 2012: 10). Technological changes affect the feasibility of testing certain hypotheses, and thus shift the economic structure (of effort, money, time, etc.) that influences (and rewards) theorizing itself.

Inter-community idea migration. As many different commentators have noted, the metatheoretical issues that underwrite the social sciences do not seem to disappear (Alexander 1981; Wagner and Berger 1985; Seidman and Wagner 1992). Rather, they seem to be inherent to the problem of studying human subjects scientifically. The self-referential aspects of consciousness, the question of the basic motivations of thinking human beings, the difference between moral and self-interested action, the emergence of collective action problems, and the tragedy of the commons—these are just some examples of problems in the social sciences that refuse to either disappear or obtain strictly empirical resolution. Rather, they exist at a presuppositional level. This intellectual situation has the effect of making the cross-migration of ideational accounts and root theoretical metaphors highly likely (e.g., Kahn and Zald 1990).

So, for example, sociologists' interest in collective goods was raised following the development of the concepts of "the tragedy of the commons" originally developed by Garrett Hardin (1968), a zoologist and microbiologist who focused on human ecology, and "the logic of collective action" developed by the economist Mancur Olson (1965). Another example in sociology is the contentious way "the rational actor" has migrated between different communities of inquiry in economics and sociology, and the different interpretations and criticisms of the concept that have been developed and put to use for the solution of research problems in different subfields (see Hechter and Kanazawa 1997; Goldthorpe 2007; Adams 2010 for commentary). We characterize this intercommunity idea migration as intellectual links enabled by various individual, institutional, and social, and institutional means.

Bridging the Zeitgeist. Political events, cultural movements, "civilizational" concerns, and so on routinely enter social science via a variety of pathways and mechanisms, both individual and institutional. These "human concerns," which circulate in and out of the social sciences routinely, can, under certain circumstances, exert tremendous influence on a given subfield or community of inquiry.

There are many examples. Social movements such as the feminist, gay rights, and environmental movements have contributed to the development of research, courses, and training programs. Widely noticed human-made disasters contributed to the development of research on them (Pettow 1984; Vaughan 1997), and also indirectly to the study of high-reliability organizations. Jeffrey Alexander's (1995) study of late twentieth-century social theory suggests a similar bridge between the world views that informed progressive politics, the intellectual currents of social theory, and specific sociological research programs such as modernization theory and world-systems theory.

Finally, consider again the study of social movements and revolutions. Forty years ago, students of movements and revolutions paid little attention to how society repressed them. When scholars did pay attention to repression, they assumed that most subjects tried to avoid the costs of pain, deprivation, and even loss of life, and thus that repression worked. But research in Latin America showed the risks that mothers would take in challenging regimes that had kidnapped or slain their sons and husbands (Lovenam 1998), and in particular, work after 2001 on terrorism has begun to adjust theoretical accounts to recognize the limits of generalized conceptions of costs and repression, and to develop more nuanced approaches to these issues (see also McAdam 1986 for an early example).
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Changing objects of inquiry, intercommunity idea migration, and bridging the zeitgeist can all be hypothesized to be more frequent occurrences in the social sciences than in the natural sciences, though confirmation of this requires further study, and—of course—the sociology of science has studied in detail the impact of social forces on science, such as the role of activists in AIDS research (Epstein 1996). Here, though, we want to point out that, if the sources of intellectual unsettlement are understood in this expanded way, the Kuhnian and Lakatosian ways of thinking about scientific change must also be revised.

First, the dichotomy between revolutionary science and normal science should be revised into a *continuum of conceptual unsettlement*. Instead of imagining periods of normal science followed by periods of crisis, consider instead that communities of inquiry can approximate, to differing degrees and because of different conjunctures of causes, the “normal” and “revolutionary” ends of this continuum.

Second, we hypothesize a link between unsettlement and theorizing in social science, based on an analogy to the unsettlement of societies or polities. Kuhn’s original argument in *Structure of Scientific Revolutions* was also based on an analogy to a specific kind of social unsettlement, namely revolution. Kuhn argued that, much as certain members of a political community might be fed up with a set of institutional structures and therefore advocate for a radically new set, so too might scientists act this way about a paradigm, and in particular its core theoretical components. This allowed him to suggest that, much as revolutions are times in which “political recourse fails” because the basic political rules of the game are in dispute, so too paradigm disputes are times during which *logical* recourse fails, leaving scientists to convince each other via exemplary practice. This is perhaps the most fundamental way in which Kuhn was “post positivist.”

We here pursue a similar analogical strategy, but with a different outcome. Research in cultural sociology has suggested that “unsettled times” are those in which high rhetoric, ideology, and fundamental values are contested. Thus, by theoretical analogy, we argue that *when influences unsettle a community of inquiry, theorization increases, because theorizing is for scientific communities the equivalent of what “high rhetoric,” disputations over values, and ideology are for polities*. Thus, as with ideology during unsettled times, so with theory when communities of inquiry become unsettled; its creation increases and intensifies, emotional attachments to (or against) certain theoretical arguments take on outsized importance, and new or long-neglected perspectives are quickly developed. Conceptual generalization is pursued, much energy is devoted to getting new research projects that use these concepts off the ground, and the possible applications of the new set of concepts becomes viewed as almost infinite. This moment of energy and unsettlement is what Clifford Geertz, drawing on the work of Suzanne Langer, describes as the way in which a new idea is taken up as “the conceptual center-point around which a comprehensive system of analysis can be built” (Geertz 1973).

For Kuhn, normal science was a space where, within a given paradigm, scientists could solve puzzles, and adequately verify or falsify each other’s solutions to these puzzles. Revolutionary science was, in his initial formulation, more rhetorical and by implication less rational—thus leading his interpreters to develop his ideas into a critique of the rationality of science, which he subsequently disavowed (Fuller 2001). Our argument is different: although communities of inquiry may need to be relatively “settled” to accumulate empirical knowledge in a straightforward way, *progress* is not the same as accumulation (Zald 1995), and thus unsettlement of communities of inquiry can lead to progress via theoretical breakthrough, and thus to (a version of) scientific rationality. Furthermore, the sources of unsettlement are multiple, and their relationship to the “internal” problems of a community of inquiry can be subtle. Indeed, it will be our argument that there is an important way in which the social sciences retain, via a generalized theoretical discourse, a well of potential unsettlement that can be returned to time and again, in the progressive pursuit of better social science.

The Sociological Theory/Social Theory Bridge

We see the argument of this chapter as a first step in a research program on the organization and intellectual structure of sociology as a human science. Here, however, we would like to articulate one clear implication of this model, as it has been discussed so far, for theorizing in the social sciences: a different understanding of the relationship between social and sociological theory.

In his chapter, Steven Turner—after noting how much theoretical exposition of the classics “confuses, not to say enranges, conventional social scientists”—distinguishes between “mundane theorizing,” “system building,” and “high theory.” In the latter, the theorist takes on the most essential arguments...
across generations and civilizational moments, constituting precisely the sort of conversation that would be incomprehensible from the perspective of a well-organized research program, and thus conducting high-concept bricolage. All of these forms of theorizing, Turner argues, but especially the last one, are likely bad for one’s career. The implication of this (besides depression for theorists) is that “social theory” tends to take place outside or on the periphery of the institutional structures of modern social science. Only outside the bureaucratic-professional machine, in other words, can the true bricolage of theorizing at the highest level happen. Simultaneously, Turner also admits that “ideological passions are not only the subject, but at the very heart of social theory.”

In nonetheless advocating for this sort of theorizing as essential, Steven Turner inverts the argument of his longtime opponent and sometime co-author Jonathan Turner, who draws a bright line between social theory as a kind of social philosophy, and sociological theory as the general concepts that make up sociology as an explanatory science, and who resolutely affirms the latter (J. Turner 1981, 1985). Those familiar with argumentation in theory journals in (American) sociology since the mid-1980s will immediately recognize this debate (for iconic examples, see Turner 1985; Lenski 1988; R. Collins 1989; Gleyzin 1982; Seidman 1983; Allan and Turner 2000; Lemert 2000). We should also note that Steven Turner’s ideas about the dominance of sociological theory over social theory apply to the United States more than to Europe—an issue that could be the subject of a separate essay that uses the typology of sources of unsettlement here as a starting point. But how should we think about this longstanding divide between sociological and social theory as it relates to the dynamics of communities of inquiry in the social sciences? Consider the following possibility.

Rather than treating social theory and sociological theory as antagonists, view social and sociological theory, together, as forming a bridge or link between cultural and political “issues,” societal concerns and movements, and generational shifts, on the one hand, and the progressive and accumulative development of research programs in the social sciences, on the other. Social and sociological theory, in other words, together form a two-way street between “civilizational concerns” and specialized empirical research in social science. They do this by creating a discursive, and to some degree institutional, space where social concerns can be articulated in abstract language, and wherein empirical social science can be made to “speak to” the concerns of the day.

First, social theory, as manifestly interdisciplinary, becomes a facilitator, precisely by its broad nature and multiple meanings, of inter-community idea migration. Social theory is an extra-disciplinary device whereby concepts from other disciplines can be translated into useful theoretical constructions for sociological research and vice versa. Second, social theory, with its more literary, appreciative, and normative dimensions, encodes social and political concerns and develops concepts to respond to them in an environment less burdened by the strict analytical and denotative/definitional constraints of explanatory sociological theories and models. In doing so, it may be more or less ideological, because being shorn of ideology so as to directly drive objective empirical research is not, in fact, the primary conceptual goal or utility of social theory. Rather, precisely insofar as it is not sociological theory, social theory thematizes at a generalized and abstract level of discourse matters of broadly social or “public” concern. These issues can then be translated from social into sociological theory. The reverse pathway is also possible—from empirical research, to newly revised sociological theories, to shifts in social theory with all their accompanying ideological implications.

Here are just a few examples of this bridge:

— In many of their texts, Goffman and his followers among interactionist sociologists remain relatively coy about their attitudes toward the interactional mechanisms they identify, and certainly tend not to make political pronouncements on what they signify for Western Civilization. But philosopher Alasdair Maclntyre (1984: 115–17 and elsewhere) does exactly this, suggesting that Goffman’s sociology, and especially the conception of the strategic actor dedicated mainly to self-enhancement rather than to accomplishing the social good, fits well with the social theory of Weber and Nietzsche in diagnosing the ills of modern society.

— The complex and contested relationship between strictly analytic or “scientific” Marxism (Little 1986) and its more normative or “critical” elaborations reveals an extended history of concept translation, normative interpretation of new conceptual developments in social science, and attempts to find scientific support for the possibility of certain normative or utopian goals.

— “Postcolonial” interpretations of texts, and reconstructions of historical narratives, started as an intellectual movement within the critical analysis of literature and history—two core disciplines of the humanities. It then
became "postcolonial theory," a highly evaluative, philosophical enterprise that took on directly foundational assumptions about "modernity" that informed many different social science disciplines (for an overview, see Gandhi 1998). It then moved into comparative historical sociology, and from there into a "postcolonial sociology," which, in its role as a sociological theory, proposes a series of abstract propositions about the nature of empire and its relationship to capitalism (Go 2008), yet simultaneously resists jettisoning its normative project, retaining a link to social theory more broadly understood (Go 2013).

Conclusion: Implications for the Cultivation of Theorizing in the Social Sciences

Given our argument, how should we cultivate the capacity for theorizing in social science? If the link posited between unsettlement and theorizing holds, then links between groups of scholars that are particularly designed to increase conceptual unsettlement are called for. The institutional structure of social science already has some venues wherein abstract ideas can be developed in a social context without the strictures of the verification of hypotheses being strictly imposed. The "informal" side of academic life appears, to some degree, to fulfill this purpose: graduate workshops, writing groups, professional conference presentations, mini-conferences, and colloquium series are all places of discussion and elaboration.

Perhaps, however, more specific and directed efforts are required. A journal could be developed that is devoted to papers that build, rather than test, theory, and thus applies different criteria in the evaluation of what makes a good paper. A series of publications in such a journal might focus on potentially scientifically useful links between social and sociological theory. Such a journal, in other words, could create a space designed to enhance the social theory–sociological theory circuit. This would have salutary effects, in our view, on empirical sociological research. For, even if the end product of sociology is viewed as knowledge qua empirical explanations that rely on analytic and middle-range sociological theory, the quality, scope, and power of these explanations will suffer in the long term if "social theory" cannot function as a constant source of unsettlement for communities of inquiry in sociology. Certainly, even without creating a journal, the relationship between sociological theory and social theory could be re-examined and developed, perhaps in a conference on the topic or in a series of critical exchanges at other conferences. The link between issues of public concern and theorizing is harder, of course, to control. But it may be also be that courses could be offered in "social theory and contemporary problems," "theorizing the financial crisis," and so on.

Ultimately, these suggestions rest on the core premise of our argument vis-à-vis social scientific knowledge and theorizing in the social sciences. That argument emphasizes (1) descriptively, the frequency and variety of intellectual links that connect a community of inquiry in social science to "the outside world," and the way these links tend to "unsettle" a community of researchers, and (2) normatively, the utility of these links for theorizing in social science, insofar as such unsettlements spur theorizing and potentially theoretical breakthroughs. In suggesting this normative judgment, we rely on the idea that progress in social science does not always come in the form of accumulation of findings, but also in such breakthroughs.

C. S. Peirce himself struggled with the way in which discoveries, intellectual breakthroughs, and new theoretical architectures were both individual and social projects. His fundamental category of the "community of inquiry" leaves open a great deal of space for argumentation about how precisely that community is, or should be, structured in the pursuit of truth—though his essay on belief makes clear that openness to falsification via evidence undergirds creative and competitive communication in a successful science. At the end of his life, Peirce began to reshape his understanding of the relationship of truth to action, including in it an individualist ethics. Perhaps, as his biographer Joseph Brent (1998: 340–44) argues, this reconceptualization was related to Peirce's own exclusion from the academic community of his time. Thus we are drawn to conclude that alienation used for creative purposes—a process that is emphasized, in different ways, by Richard Swedberg and Stephen Turner in this book—is an important part of theorizing. Simultaneously, however, we should recognize how deeply our theoretical communiqués, including those we engage in with ourselves, bear the imprint of habits derived from those in our scholarly realm who produce in us the fundamentally social experiences of solidarity and competition. These social relationships are subject to reconstruction, much as the individual mind is. Our argument here is that there are aspects of these relationships in social science that have been repeatedly disavowed. If, instead, they were accentuated by reflection, progress in social science—particularly progress characterized by theoretical innovation—could become more profound.