



Sociology and philosophy in the United States since the sixties: Death and resurrection of a folk action obstacle

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Published online: 24 January 2020

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Abstract

This article uses participant objectivation in sociology and philosophy as two knowledge fields to provide a reflexive comparison of their synced field effect in historical circumstances. Drawing on the philosopher and historian of science Gaston Bachelard, I theorize fielded knowledge as a social relation that combines the prior presence of folk knowledge with a socioanalytic exchange between field and folk that includes positions of either defense, replacement or critique. A comparison of post-Wittgenstein Anglophone philosophy and post-sixties American sociology describes their mutual confrontation with folk psychology as an “epistemological obstacle” that generates a remarkable concern with action as a position-taking on the folk relation. A reflexive objectivation of folk knowledge is therefore necessary for a revised understanding of action that correlates with the distinction of sociology’s knowledge capital and how it fares as an explanatory resource in competitive circumstances. The article concludes by leveraging the synced field effect even further to make a recommendation that sociologists can increase the distinction of their knowledge capital by producing discourse that can recognize, legitimate and officialize experiences that otherwise remain obscure, nameless or impossible within the bounded universe of folk psychology.

Keywords Bourdieu · Knowledge · Field theory · Sociology · Philosophy

Folk psychology refers very generally to a historically-developed and culturally-rooted capacity to refer to “the nature, causes, and consequences of those intentional states—beliefs, desires, intentions, commitments ... [that] dominate the transactions of everyday life” (Bruner 1990: 32). It has a parallel with what has been labelled as spontaneous sociology or non-networked belief in approaches to scientific knowledge like field

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theory and actor-network theory, respectively, because it allows for a base-level social reasoning that maintains social relations on far larger scales than fields or scientifically-composed networks (Bourdieu 2001: 82; Latour 1987: 182–83). Given the broad phrasing of folk psychology, and its circulation in social space against fielded knowledge, it raises significant questions about the role and reception of that knowledge, its conditions of possibility, and particularly the folk relation to sociology as fielded knowledge that is entangled with the same things as folk practice, habituation and “points of view” that are *always already there*.

This article attempts to make sense of these many relations, their significance for sociological knowledge, and the cognitive aspects of fields by developing a novel case comparison: historicizing the post-sixties American sociological field and comparatively grasping its connection with another field, namely the post-Wittgenstein Anglophone philosophical field. The two fields appear in a strange and specific sync, I argue, because they both constitute an engagement with “the folk.” The folk are unfamiliar in sociology (though see Swedberg 2018) although in Anglophone philosophy, they are very familiar, and they are increasingly familiar in science (Keil 2010). As I use it here, “the folk” are what those outside of a knowledge field appear to be for those who are inside of a knowledge field. From the perspective of a field, the folk appear as a *way of knowing* relative to a differentiated or *fielded* way of knowing. The sync between post-Wittgenstein Anglophone philosophy and post-sixties American sociology arises from a historical trajectory in each field that seeks to bring fielded knowledge closer to *knowing as the folk know* rather than simply knowing as the *field* knows. The alignment becomes particularly strong in reference to what I label “folk action,” or knowing as the folk know *action*, its motivation, causes, and meanings, and action-explanations that involve familiar folk psychology. This is despite the fact that the same effort finds different key sources (Ludwig Wittgenstein for philosophy, “the sixties” for sociology), and the two fields do not historically coincide in the position-taking inspired by this field/folk relation (philosophy before sociology).

Sociology and philosophy are field-specific pursuits, and so in both cases the knowledge they produce is *fielded*. Folk psychology, meanwhile, is *non-fielded* knowledge, which means that it finds different modes of acquisition and conditions of application. Non-fielded folk knowledge is acquired through habituation and repetition; fielded knowledge through explicit pedagogy and construction. Both fielded and non-fielded knowledge can be turned into propositions and explanations, but this is much less explicit for non-fielded knowledge: generally, it only applies when things are not “as they should be” (Bruner 1990: 40). This article reveals instances in which those in a field confront folk knowledge and engage in position-taking. It also reveals instances in the relation between the American sociological field and Anglophone philosophy when sociological knowledge becomes *interfielded*, or when knowledge in one field is attributable to a transfer from another field (Camic 2013: 198).

Such an approach wagers on the fact that between folk and field a *cognitive* differentiation separates them, distinct from other epistemological (ex post facto) or “network” differences in their knowledge (see Latour 1999: 180). The historian and philosopher of science Gaston Bachelard’s (2002[1938]; 1984[1934]; 1968[1940]) concentration on the “psychologically formative” aspects of science, particularly what he calls the “epistemological obstacle,” serves as a guide to highlight the cognitive

preliminaries and effects of fielded knowledge. Epistemological obstacles demonstrate how field-specific knowledge emerges from a more or less hierarchical, deferential or egalitarian relation to “the folk,” or what I refer to below as socioanalytic exchange.¹ As Bachelard (2002[1938]: 24) argued, scientific knowledge cannot be understood without appreciating its presence in social conditions shaped by an *always already* present folk knowledge. Among other reasons, this matters because it sensitizes us to a scientist’s subjectivity and dispositions in knowledge-production—why topics appear *interesting*, what is *appealing* about a certain knowledge claim, why certain concepts seem *intuitive*, why the scientific consciousness *suffers*, why scientists play “the dangerous game of thought” by *not* connecting knowledge to needs—to which Bachelard’s philosophy of science pays unique attention, missing from nearly every other comparable tradition (Popper, Kuhn, Lakatos).² What Bachelard calls the “formation of the scientific mind” provides insight into the suspensions, breaks, removals of naïve adherence, and obstacles through which knowledge fields secure a differentiation, on *cognitive* grounds, relative to pre-existing knowledge.

If sociologists make knowledge claims using their own constructed objects, they do not appear to use folk psychology; in fact, they would likely shun it as a profanation: “methodological individualism.” Yet what worries some in the field (see Watts 2014; Abbott 2001[1992]) is that when they try to put their objects *into action*, sociologists almost invariably rely on a folk-psychological scheme. Not only that, they often construct field-specific objects in ways that make them folk-psychological by making otherwise familiar but inaccurate assumptions about cognition (Turner 2018; Martin 2015; Strand and Lizardo 2015). Contrary to an ahistorical commentary on “common sense” in sociological explanation (Watts 2014; Boudon 1988), however, for which folk knowledge in the field is often tantamount to carelessness, this article historicizes these tendencies to the *post-sixties* period in American sociology, a period that finds dramatic changes to the field and its prevailing relations to the folk. This history offers a long-view in which to see positions that sociologists now take as part of a trajectory that has, arguably, reached a kind of crossroads. Over the last couple of decades, sociologists have removed folk psychology as an undiscussed part of fielded knowledge, confronted it as an obstacle, and taken positions (see Hedstrom 2005; Reed 2011; Martin 2011; Watts 2014; Strand and Lizardo 2015; Turner 2018). This changes the possibility of drawing on folk psychology unproblematically or unnoticeably as a resource for translating sociological objects into action or, more generally, for constructing a field-specific object (like culture, structure or agency).

Reading this history through the prism of the post-Wittgensteinian philosophy of action shows what it means for sociological knowledge to have a *philosophical* content,

¹ Although Bachelard’s own examples come mainly from physics and chemistry, sociology is particularly sensitive to the tendentious issues surrounding epistemological obstacles. Durkheim (1982[1893]: 38–39) recognized that sociology did (and should) deviate from “prenotions” and commonsense familiarity. However, with less “accumulated knowledge capital” than physics or chemistry (or philosophy), sociology is more vulnerable to criticisms from heteronomous publics who are *also* involved in the “struggle over the legitimate view of the social world” with no other disciplinary parallel (Bourdieu 2001: 88). Among the extra-disciplinary criticisms that are particularly salient for sociology is the claim that its research does not comply with common sense or that it complies *too much* with common sense (see Boudon 1988).

² Bachelard’s efforts now seem, in many ways, more contemporary than those more famous counterparts because of the advance of cognition against epistemology in the study of knowledge (see Nichols 2004; Keil 2010; Gerken 2017).

and how this is different from any persistent tendency in the field of reading texts designated “philosophy,” transferring their capital into the field and making abstract commitments. The argument below is that, like all knowledge fields, philosophy is defined by the kind of field-specific capital obtainable by a knowledge claim that *counts* at a given time as “philosophical.” In the post-Wittgenstein period of the Anglophone philosophical field, the knowledge the counted as philosophical bears a subtle but striking resemblance to knowledge that counts as “sociological” in the post-sixties American sociological field. So much is this true that sociologists can and do draw from the post-Wittgenstein field for their own knowledge claims. Although the content they draw from may seem surprising to some, it becomes less surprising when we understand it in relation to the same epistemological obstacle that sparked the “philosophy of action” in the Anglophone field, which enjoyed a meteoric rise, but whose peak interest was short-lived (Bernstein 1971). The reflexive comparison pursued in this article allows for a participant objectivation of the current sociological field in order to recognize an objectified history and the significance of actual and possible position-takings. If Anglophone philosophy after Wittgenstein serves as any indication, the stakes involved in a “principled displacement” (Churchland 1981) of folk psychology are not insignificant.

In what follows, I first describe Bachelard’s understanding of epistemological obstacles and their relation to the cognitive dimensions of fields. I situate folk psychology in relation to sociology and argue that in addition to a critique of the stumbling blocks of rational action theory, a means/ends framework or normatively-oriented action (see Gross 2009; Whitford 2002; Joas 1996), a revised theory of action needs an objectivation of folk psychology. This helps demonstrate why action-explanations are significant for the distinction of sociology’s knowledge capital. The article then compares these two instances when the same epistemological obstacle presents itself for position-taking.³ In both cases, folk psychology loses, if ever so briefly, its silent self-evidence, is bracketed for the purposes of knowledge-production (“for the sake of argument”) without the threatening accusation of *making no sense*, and enters a universe of argument as a folk action obstacle that drives a pattern of position-taking.

The article concludes by arguing that post-sixties sociology confronts newly competitive circumstances from data science not dissimilar from post-Wittgensteinian philosophy confronting neuroscience. It advocates a different position, however: to ensure the distinction of their knowledge capital, sociologists should produce extraordinary discourse that can surpass the limits of folk psychology.

Bachelard’s obstacles

“Even when it first approaches scientific knowledge,” Bachelard argues, “the mind is never young. It is very old, in fact, as old as its prejudices” (2002[1938]: 24–25). For

³ The suggestion here is that confrontation with a folk action obstacle occurs in post-Wittgenstein Anglophone philosophy and post-sixties American sociology as two key instances, which is not to omit others. In post-behaviorist American psychology, for example, the work of Daniel Wegner (starting in the mid-1970s) demonstrates another confrontation with folk action, most notably leading to conclusions like “people experience conscious will when they interpret their own thought as the cause of their action” though it does not *precede* the action (Wegner 2003).

Bachelard’s “epistemological history,” scientific knowledge is the historical result of the development of “counter-concepts” constructed *against* prior or non-scientific “first knowledges.” Because “the development of scientific thought requires a break with common sense thought,” scientific knowledge can never be “‘first knowledge,’ it is always reflective knowledge ... the product of reflection on previously held beliefs and ways of thinking” (Tiles 1984: 58). This not only places a unique emphasis on cognition for the history and definition of science, it also introduces *epistemological obstacles* as mechanisms of change in knowledge fields. These are fields that produce a distinct form of knowledge capital and manage a boundary with other fields, other *non-knowledge* fields with different types of capital and, as I argue, between the field and the folk that is significant for the field’s knowledge capital and its content.

Defined broadly, an epistemological obstacle is anything (e.g. method, terminology, conviction) that affects the “cognitive situation” of a knowledge field by preventing it from making an epistemological break with pre-existing “first knowledge” (*idées reçues*) that is *not formed or acquired within a knowledge field*. Knowledge does not have to be formed in a field; but when it becomes “fielded” in this sense, this makes a cognitive difference. For Bachelard (2002[1938]: 246), what it means to rupture or *break (rupture épistémologique)* with epistemological obstacles becomes most evident when obstacles are understood as “psychologically formative” for a field, which involves more than “simply replacing” first knowledge with scientific knowledge. Scientific “structures of thought,” rather, “give reality to new forms of experience” within the field when they break with and establish a proper distinction from “primary experience” (Bachelard 1984[1934]: 171). Common sense, meanwhile, “translates needs into knowledge,” is pragmatically oriented, and privileges the maintenance of what seems manifest and self-evident (Bachelard 2002[1938]: 25). To break with epistemological obstacles means trading an indubitable “feeling of being in the right” that is given value by common sense for a commitment to uncertainty, reflexive self-scrutiny and the tough to win “satisfactions of reason” (Bachelard 2002[1938]: 244).

Bachelard’s attention to obstacles was shaped by the decade that he spent teaching physics and chemistry in a French secondary school (Chimisso 2001: chap. 2). It is not a surprise, then, that the types of obstacles that Bachelard mentions in detail draw a close connection with problems of pedagogy or teaching.⁴ Bachelard (2002[1938]: 19ff) highlights three general types of obstacles: (1) *primary experience*, or “the vivid, concrete, natural and easy ... profusion of images ... You only need to describe [a phenomenon] and marvel. And you think you understand it”; (2) *verbal obstacles*, or “false explanations obtained with the help of explanatory words ... that considers itself to be developing thought by analysing a concept”; and (3) *substantialism*, or “the monotonous explanation of properties by substance.”

Other kinds of obstacles include “*general knowledge*,” or “instances where a single image or indeed a single word constitutes the entire explanation” (81) and “*unitary and pragmatic knowledge*,” or the tendency to assume unity to phenomenon, the “inability to [conceive of] experience as self-contradictory and compartmentalized. What is true of something large must be true of something small” (94) and the tendency to associate knowledge with a “purely human usefulness” (99). These latter kinds of obstacles

⁴ “Epistemological obstacle” still has a live presence in the pedagogical literature, especially in mathematics (Schneider 2014).

concern the “valorization” of knowledge, or the “epistemological value” that grades and weighs appropriate knowledge (Tiles 1984: 154–55).

While Bachelard uses the term “obstacle” rather loosely, he is much more precise about the implications he draws from it, which are always explained in a historical vein by analyzing the genesis and consequence of an epistemological break. In a vivid description, he includes a passage from Goethe’s *Werther* in which people at a summer party hide in fear of a distant lightning storm. Bachelard then explains that.

it would seem impossible ... to include a narrative of this kind in a modern novel. Such an accumulation of silliness would seem unrealistic. Nowadays, the fear of thunder has been overcome. It is experienced, if at all, only in solitude. It cannot affect us in a social group since we have as a group entirely rationalised the theory of thunder; individual fits of irrationality are now but oddities that are concealed. We would laugh at Goethe’s hostess closing shutters and curtains in order to protect a dance (2002[1938]: 35–36).

Goethe’s “pre-scientific book” describes a world *prior* to an epistemological break and thus prior to a situation in which “natural phenomena have been rendered harmless by being explained” (35). But this also implies a certain relation between knowledge produced by a field whose existence is in some way contingent on an epistemological break with folk knowledge that circulates about the same topics, objects or phenomenon.

Returning to contexts like this is not simply of historical interest, then, but allows for an understanding of fielded knowledge that translates into reflexivity and can be generative of new knowledge in the field by making possible further epistemological breaks. Returning to historical instances of break with a certain attention to the granular detail about how the break was made, makes it possible to understand the process through which fielded knowledge has been “achieved.” For Bachelard, this provides a tool of self-defense, of sorts, for *not* being overcome by unconscious tendencies that arise from forgetting (e.g. “anamnesis”) the cognitive conditions and social relations that make fielded knowledge possible.⁵ As he writes:

When a problem is posed, experience has to be put into a rational form and unless this happens and there is *constant recourse to an explicit rational construction*, we will allow a kind of unconscious of the scientific mind to be constituted, which will then require slow and difficult psychoanalysis if it is to be exorcised. In Edouard Le Roy’s strikingly dense phrase ‘Everyday knowledge is unconsciousness of oneself’ ... This unconsciousness can however also affect scientific thought. In that case, criticism must be reinstated and *knowledge brought back into contact with the conditions that gave it birth*; we must *keep returning to that ‘nascent state’* which is the state of psychic vigour, *at the very moment when the answer has come from the*

⁵ Bachelard’s argument here has strong parallels with the genetic epistemology of the psychologist (and Bachelard’s contemporary) Jean Piaget (1970), for whom knowledge was also social, practical and grounded in action, and never individualistic or purely cognitive in a representational-symbolic sense.

problem. It is not sufficient to find a reason for a fact in order to be able to speak in any real sense of the term of the rationalisation of experience (Bachelard 2002[1938]: 49; emphasis mine).

Thus, epistemology must always include investigations into the construction or genesis of knowledge by returning to the “state of psychic vigor” when the knowledge is active, the moment when a *break* is made with an obstacle and “the answer has come from the problem,” which ties epistemology to questions of historical genesis.⁶ This provides insight into the “epistemological profile” of a topic, object or phenomenon as a reflection of its history of epistemological breaks and objectifications, rather than an either/or between competing epistemologies (realism, rationalism, post-positivism) decided *a priori* as abstract commitments. The epistemological profile of action reflects a limited history of epistemological break and so remains heavily weighted toward “naïve realism” (Bachelard 1968[1940]: 42–43).

Bachelard’s distinctive approach to science reflects the “scientific spirit” that was given broad attention in the early twentieth century French educational system, and which itself was underscored by Comte’s distinction between “pre-scientific” and “scientific” stages (Tiles 1984; Chimisso 2001). However, in no sense are Bachelard’s arguments linked to an inevitable stage development. The points he raises are meant to draw attention to a theory of scientific knowledge that takes *both* history and cognition seriously and takes as a principal concern the differences between folk knowledge and scientific knowledge. For our purposes, the most important implication of Bachelard’s merging of philosophy and history of science (a “psychoanalysis of science” in his idiosyncratic terms) is that it makes any relatively coherent knowledge field and the capital that it produces a reflection of the history of its epistemological breaks and the relations it maintains with the folk that determines a folk/field boundary in addition to other field/field boundaries.

What is folk psychology? or, the folk action obstacle

For a topic like folk psychology, Bachelard’s arguments seem particularly relevant, as becomes evident in a brief exegesis of the term itself. References to “folk psychology” appear to start with the German psychologist Wilhelm Wundt, famous for his development of experimental psychology, but who in a 1916 book entitled *Volkerpsychologie* summarized a different topic that had preoccupied him for nearly a half century: namely, the differences in cultural beliefs *about* the mind, their teleological development and the role that these played in social organization. For the psychologist Jerome Bruner (1990), experimental psychology only became predominant to the neglect of folk psychology until the “cognitive revolution” shifted away from computationalism and its strong objectivism. This revived a general concern with

⁶ Bachelard also recommends a pedagogy based on the creeping familiarity of both folk and fielded knowledge in the scientific unconscious. He argues for the need to balance what he calls a “teacherly soul” that transmits certainties and is “proud of its dogmatism” with a “soul desperate to abstract and reach the quintessential,” which involves “a suffering scientific consciousness ... constantly disturbed by the objections of reason, time and again casting doubt on the right to make a particular abstraction yet very sure that abstraction is a duty” (Bachelard 2002[1938]: 31).

cultural beliefs about the mind “[that] dominate the transactions of everyday life.”⁷ Because folk psychology is a “reflection of culture,” Bruner argues, “it partakes of a culture’s way of valuing as well as its way of knowing. In fact, it *must* do so, for the culture’s normatively oriented institutions—its laws, its educational institutions, its family structures—serve to enforce folk psychology (1990: 14).

For Bruner, then, folk psychology “is not just a set of self-assuaging illusions, but the culture’s ... working hypotheses” (1990: 32) *about* cognition as the central aspect of persons, “a culturally shaped ... view of themselves,” that most importantly facilitates “everyday transactions” and institutional scripts (137). This treats folk psychology as an empirical *topic* rather than an (assumed) resource. On a similar basis, Stephen Turner (2018) advocates for a “separation between intentions as mental objects and intentions as part of a cultural practice.” For him, this bracketing of the “standard belief-desire model of action explanation” that circulates in everyday life inaugurates a “*radical exercise* because the known facts have been described in terms of belief and intention” (2018: 112–113; emphasis added). If there is any substance to these efforts at rendering a basic practice like folk psychology, entangled deeply with everyday sense-making, into a cultural artifact, and therefore naturalistically *arbitrary*, then they should provoke a defensive counter-response; *and they do*, as evidenced, for example, by the philosopher Jerry Fodor’s breathless assertion that “if commonsense intentional psychology really were to collapse, that would be, beyond comparison, the greatest intellectual catastrophe in the history of our species” (Fodor 1987: xii).⁸

The argument below builds on these efforts to theorize folk psychology as a cultural artifact, arguing that a similar recognition enables both the post-Wittgenstein Anglophone philosophical field and the post-sixties American sociological field to *confront* folk psychology as an epistemological obstacle. This arises because of specific histories in both fields that lead toward their mutual concern with a folk/field boundary. The presence and formulation of folk psychology in these two fields can be glimpsed in some reflexive detail, drawing from three summaries of folk psychology: two drawn from the sociological field and one from the philosophical field.

⁷ Bruner’s arguments on folk psychology have been largely eclipsed by the rapid advance of developmental psychology and arguments about “theory of mind” as part of a psychological-developmental process that normally results in an “understanding of other minds” (Baron-Cohen, Tager-Flusberg and Lombardo 2013). According to this argument, something like folk psychology generally develops at about age 4 when children acquire a theory of mind and start to apply “belief” and other propositional attitude concepts to themselves and others. However, this naturalization of folk psychology rests on shaky grounds, namely its cognitive assumptions, because it hinges on an *evolutionary* genealogy and problematic inferences about prior function among humans’ evolutionary relatives as the generative source of folk psychology (*qua* theory of mind). Bruner’s argument, by comparison, becomes more convincing because it proposes a more modest *historical* genealogy: folk psychology is cultural, in other words, as a “learned narrative practice” (see Hutto 2008: chap 11).

⁸ As Fodor continues, “... if we’re wrong about the mind, then that’s the wrongest we’ve ever been about anything. The collapse of the supernatural, for example, didn’t compare; theism never came close to being as intimately involved in our thought and our practice—especially our practice—as belief/desire explanation is. Nothing except, perhaps, our commonsense physics—our intuitive commitment to a world of observer-independent, middle-sized objects—comes as near our cognitive core as intentional explanation does. We’ll be in deep, deep trouble if we have to give it up” (1987: xii). This kind of response is not unfamiliar to “symbolic revolutions” that involve the “objectivation of implicit schemata of thought and action [that] constitutes an attack against the very structures of consciousness” and provoke a defensive response rooted in the defense of “something like ... mental integrity” (Bourdieu 1996: 5–6).

‘Folk psychology’ denotes the pre-scientific, commonsense conceptual framework that all normally socialized humans deploy in order to comprehend, predict, explain and manipulate the behavior of humans and the higher animals. This framework includes concepts such as belief, desire, pain, pleasure, love, hate, joy, fear, suspicion, memory, recognition, anger, sympathy, intention, and so forth. It embodies our baseline understanding of the cognitive, affective, and purposive nature of people. Considered as a whole, it constitutes our conception of what a person is (Churchland 1998: 3).

The recipe goes like this: if you want to explain why people do what they do, all you have to do is link what people want to what they believe in order to derive some plausible story (e.g. one that preserves the ‘rationality’ of the actions that ‘follow’) for why persons do one thing and *not* the other in some sort of setting or situation... (Lizardo 2013: ix).

If we want to explain why Mr. Smith brought an umbrella today, we can point to a specific set of desires, beliefs and opportunities, such as (1) he believed it would rain today, (2) he desired not to get wet, (3) there was an umbrella for him to bring. Given this set of desires, beliefs and opportunities, we have made the action intelligible and thereby explained it (Hedstrom 2005: 39).

In all three of these statements, there are certain commonalities. If we were to summarize folk psychology, it consists of a meaning-making practice that (1) involves the explanation of action, (2) by ascribing certain mental states (beliefs, desires, hopes, fears, wants, intentions) to actors, (3) deducing from these mental states toward action as an intentional consequence, and (4) making that action seem rational/meaningful or predicting what it would be rational/meaningful for actors to do given their beliefs and desires.

So what, then, does it mean to turn *folk psychology* in common sense into a *folk action obstacle* in a knowledge field? Consider Duncan Watts’ (2014) recent argument, as a position-taking in the sociological field, about the problems of *not* maintaining a clear folk/field boundary when it comes to the inclusion of folk psychology. As Watts (2014: 315) argues, sociologists today utilize such “common sense concepts [like folk psychology]” in everyday life. They “appear valid on the basis of everyday personal experience [and] seem self-evident.” For Watts, this sort of “understandability” in sociological research is plagued by invocations of “actors’ intentions, habits, beliefs, opportunities, circumstances and so on [that] generalize poorly to novel scenarios” (315). Even if this is not made explicit in research or writing, much of the argument found in published sociological articles relies, according to Watts, on “rationalizable action” that interprets the findings “with reference to individual psychological states ... intentions, beliefs, circumstances, and opportunities of the actors involved” (316).

The problem is that sociologists do not leave this folk knowledge behind when they engage in the work that marks their participation in their own knowledge field. According to Watts, all of these “assumptions implicit in the folk theory get incorporated into sociological theories without even being explicitly articulated ... [Even] when the theories themselves become contested, the common sense assumptions on which they rest are left unchallenged” (ibid: 315). This would seem to constitute a textbook case of an epistemological obstacle, then, as folk psychology is incorporated into the field with little critical awareness, has an impact on the knowledge produced

and, ultimately, the kind of knowledge capital *the field* produces. But if we look historically at the kind of obstacle that draws Watts' attention, it becomes clear that, at least for sociologists, it has not always been an obstacle.

While the label “folk psychology” is a contemporary term of art, there is a classical precedent for something closely similar in sociology: the *Verstehende soziologie* of Max Weber (see Turner 1983). For Weber, certain “norms of our thought” are irreplaceable elements of sociological research because they ensure that the research is not meaningless and does not lack sufficient comprehension (e.g. *Evidenz*).

As far as the method of investigation is concerned—*how* it proceeds—the ‘guiding point of view’ ... determines the formation of the conceptual tools employed by the scholar; but, in applying those tools, he is obviously, here as everywhere else, bound to the *norms governing our thought* (Weber 2012[1904]: 121; emphasis added).

There is nothing specifically sociological about the “norms governing our thought” in Weber’s view; they are contingent not on the existence of a knowledge field but on what he calls the “language of [everyday] life” (2012[1904]: 135; orig. addition) that *commonly* persists in a given time and place.

Weber’s arguments were influential in framing *Verstehen* as a recognized pursuit in sociology that placed particular emphasis on a familiarizing “understanding” as an essential component of sociological explanation. His commitment to action and subjective meaning sets him apart from other classical thinkers, and this has been interpreted variously as a transposition (and complication) of categories from the jurisprudence that marked Weber’s legal training (Turner and Factor 1994: 13ff), as part of his critical encounter with modernist culture (Scaff 1989: 83ff), or his maintenance of Kantian moral autonomy in a rationalized world (Brubaker 1984: chap. 4). As argued below, this makes Weberian action theory distinct from the present rejuvenation of concern with action in the post-sixties American sociological field, which does not participate in these same efforts, share these concerns, or have these same extra-field interests or influences. This suggests that “action” remains a key site of the folk/field relation for sociology, a marker of cognitive demarcation, with or without a *theory* of action, just as ordinary language is for post-Wittgensteinian Anglophone philosophy.

If Weber’s claims often draw attention, he was not the only one to recognize folk psychology and make something like it foundational to sociological knowledge. A *Verstehen*-like argument takes a different form in Mead, Cooley, Thomas and Park, but achieves a similar purpose of making cognitive attributions that familiarize actors (Hinkle 1963).⁹ These versions of action theory (among others) preceded Talcott Parsons’ publication of *The Structure of Social Action* in 1937, which would effectively monopolize theories of action for the next several decades. Parsons

⁹ Cooley’s “sympathy” argument is arguably the most prominent of these: “a person can be real to us only in the degree to which we imagine *an inner life* which exists in us ... and which we refer to him” (1902: 60). This concern with action among those who would pass through or be key influences on the University of Chicago as an early site of American sociology had a connection with the kind of inductive theorizing cultivated by the university’s department of sociology (see Camic 1995: 1014–15).

himself recognized that “the action schema” as he referred to it is “deeply rooted in the common-sense experience of everyday life.” “Sophisticated thinkers” (Marshall, Durkheim, Pareto, Weber) had simply “selectively ordered [it] in a particular way so that a peculiar conceptual scheme arises [but] it retains certain common features throughout” (1949[1937]: 51). As Parsons suggests here, the recognition of a “common-sense” action schema (to use his terms) has often been characterized in the history of sociology by its defense and incorporation into the discipline in a way that lends sociological claims distinct and irreplaceable characteristics (i.e. “understanding”).

Importantly, however, *Verstehen* itself has not been without controversy in the history of field. Like Watts, there are precedents for recognizing a folk action obstacle, and it is significant that such recognitions take place once action carries less of an extra-field significance (e.g. as part of “parallel conceptual worlds” like modernist culture or jurisprudence) and becomes more of a field-specific concern that impacts sociological knowledge according to its *own* criteria. Theodore Abel, for example, summarized *Verstehen* as the “postulation of an intervening process ‘located’ inside the human organism, by means of which we recognize an observed—or assumed—connection as relevant or ‘meaningful’” (Abel 1948: 214). For Abel, all of this was contingent on a first postulation: “an inner-organic sequence that could explain the reason” why a particular action took place (216). But this was inevitably faulty, in his view, as it depended upon the “application of personal experience to observed behavior” that lacked a true “method of verification.” This made *Verstehen* different from social knowledge produced from within the “realm of scientific research” (216).

The “little stories” that Abel associated with *Verstehen* have a close parallel to what Andrew Abbott (2001[1992]) later found to be a similar connection between variable correlations and the need to “justify that entailed relation” with the “creation of a plausible narrative about particular cases” (55). For Abbott, often these narratives boil down to a very simple form that is not brought into question by the field: “a short, simple phrase establishing that a certain actor had certain interests or face certain conditions coupled with the assumption that actors will act on their interests ... together they produce a one-step narrative running from condition to action via interest” (59). These basic narratives have an explanatory appeal even for “standard positivist articles.”

Abel and Abbott are only two (relatively prominent) examples (see also Mills 1940), but as a counterpoint to Weber, Parsons and pre-Parsonian action theory, they show the different stakes involved in action as a sociological concern, and historical context matters for what these stakes are, as I argue below. Watts (2014) distinguishes between “scientific validity and understandability” as two different criteria for evaluating sociological research, and he associates validity with the ability of that research to make predictions. Abbott (2001[1992]) identifies a tension between the demands to “be narrative” or “be analytic” that does not cleanly match case-based research and larger-N studies but reflects the presence or absence of “little stories.” Finally, Abel (1948) distinguishes between the “application of knowledge validated by personal experience” and knowledge that can serve as a “means of verification,” which he associates with experimentation and testable propositions.

In each of these arguments, there appears a common tension between a version of “scientific knowledge” and some version of “folk knowledge.” The contrast is made more or less strongly and does not automatically lead to a dismissal of the former. It also matters, as I argue below, that Abel (1948) makes his claim in a *pre-sixties* sociological field while Abbott (2001[1992]) and Watts (2014) both observe a *post-sixties* field. All three cases show how knowledge capital in a field can be defined and elaborated through its connection to folk knowledge and how this has direct implications for a field/folk boundary and relation that, from Bachelard’s point of view, would concern everything from published research, to pedagogy in the classroom, to the required coursework in the undergraduate major curriculum. This makes folk psychology different from an explicit theory of action, which also makes the reflexive objectivation attempted here different from a theory critique (Gross 2009; Whitford 2002; Joas 1996). Attention is cast on a cognitive marker of the field and its boundaries with the folk, based on the objectified history of the field rather than abstract commitments, the question whether sociological explanations are translatable into familiar action-explanations, and whether action should itself be a construct that does not conform to folk psychology.

“Folk science” and socioanalytic exchange

Altogether this suggests, in alignment with Bachelard, that knowledge fields are marked by cognitive demarcations that involve, among other things, differences in the presence of “primary experience,” “verbal” familiarities, “substantialism” and the epistemological value of intuitive or counterintuitive argument. If epistemological obstacles involve a cognitive difference, and this concerns a folk relation to fielded knowledge (and how much field participants *seek* to be folk-compatible), then occasions of epistemological break are a relevant problem for the sociology of knowledge. If Bachelard is right, then a knowledge field is defined by its field-specific constructs and how these form through a relation to pre-existing folk knowledges, familiar to the scientist herself, that are *always already there*.

In this section, I theorize the socioanalytic exchange and draw certain relevant connections between the argument below and a growing cross-disciplinary discussion around the role of “folk science” in the development of disciplinary science.¹⁰ Socioanalytic exchange refers to the social relation maintained between a field and the folk that has the kind of direct bearing on the knowledge capital of a field that Bachelard suggests is characteristic of all epistemological obstacles. This adds substance to the point, shared by both field theory and actor-network theory, that social relations and interactions are the “building blocks” for the abstractions that constitute epistemologies, for instance, “realism,” “positivism” or “post-positivism” (Martin 2011: 92; Latour 1987: chap 5; Bourdieu 1969: 112). A socioanalytic exchange is *social* because the folk/field relation can be more or less deferential,

¹⁰ Prior discussion of “folk science” ranges from folk biology (Ingaki and Hatano 2002) to folk physics (Proffitt 1999), to folk chemistry (Au 1994) to folk cosmology (Siegal et al. 2004). Swedberg’s (2018) recent analysis of folk economics in Trump’s 2016 election demonstrates the significance of folk science for broader concerns across a range of sociological subfields.

hierarchical or egalitarian. The exchange is *analytic* because this affects the cognitive demarcation that exists between the field (and its constructs) and the folk (and folk knowledge).

The socioanalytic exchange that marks a field becomes most evident in the pattern of position-takings that appear in knowledge fields when they confront epistemological obstacles. These position-takings include *defending* folk knowledge, *replacing* folk knowledge, or *critiquing* folk knowledge. Abstractions like “natural science” and “human science” can be redefined more in terms of the pattern of social relations that form around field-specific constructs. Natural sciences consist of a stronger replacement orientation. Human sciences have a stronger defense orientation. These orientations are only made explicit in historical moments of epistemological break and their recapitulation in pedagogy. Following Bachelard, then, the difference between natural and human science does not ultimately vary according to the *object* studied. The more important difference concerns the *relation* between the field and the folk as established historically by the strength of the cognitive boundary between fielded knowledge and folk knowledge.¹¹

This helps theorize the nature and status of folk knowledge, folk theories and folk science more generally. The relation between fielded knowledge and the folk takes place within universes of argument that have some *distinction*. To confront an epistemological obstacle like folk psychology involves socioanalytic exchanges with the folk at the boundaries of a field that generates and changes a field-specific knowledge capital that circulates within it and is transferrable outside of it. This involves how recognizable that knowledge *should* be relative to a “norm of thought” *not* developed within the field. Contrary to Bachelard’s claims, to engage with an epistemological obstacle that will affect the autonomy of a knowledge field is not prompted simply by the “internalist” appearance of an explicit problem (see Camic and Gross 2004). Rather, and to underscore Bachelard’s own cognitive point, it simultaneously involves the difference between a knowledge field and folk knowledge as a *social* relation whose specific dimensions help constitute fielded knowledge.

Epistemological obstacles, and efforts to break with them or not, therefore reflect the larger situation of a field itself. Socioanalytic exchanges with the folk are prompted by field-level processes, the *nomos* that dictates how a field constructs its objects, the co-presence of those objects in different fields, in “other struggles” and among the folk,

¹¹ There is a close parallel between Bachelard and Alfred Schutz (1953) on these points, in particular Schutz’s claim that the “scientific constructs are designed to supersede the constructs of common-sense thought ... constructs of the second degree” (Schutz 1953: 3). The difference is that Schutz makes this relation between field and folk contingent on “subjective interpretation,” which in Schutz’s view (following Husserl) defines first-person experience *categorically* (3). For Bachelard, this would commit Schutz to a kind of Cartesian “inability to doubt,” by identifying something that could never be made an epistemological obstacle that will invariably serve as a *foundation* and will therefore do more to generate knowledge than anything else. The reverse track is to find nothing foundational and generate knowledge only *in retrospect* instead, that is, from a formative relation to “previous knowledge” that gives it a revised status (Bachelard 2002[1938]: 24). Neither Bachelard’s nor Schutz’s claims are merely epistemological statements, then: they are both “practical techniques” (*phénoménoteknique*). For Schutz, “subjective interpretation” could *never* be previous knowledge; the propositions made on this basis could not be made less “epistemologically valuable.” This likely reflects phenomenology itself as initially a defensive position-taking in a philosophical field against an empirical psychology.

Table 1 Patterns of response to folk-derived epistemological obstacles

| | Defense | Replacement | Critique |
|-------------------------|------------------|-------------------------|-----------------------|
| Knowledge Capital | Puzzle-Solving | Strong Validity | Objectivation |
| Epistemological Content | Folk-Compatible | Strong Meaning Variance | Weak Meaning Variance |
| Epistemological Break? | No | Yes | Yes |
| Socioanalytic Exchange | Folk-Deferential | Hierarchical | Egalitarian |

that all affect a field's autonomy and means of participation. In Bourdieu's terms, the "true subject" of position-taking in relation to epistemological obstacles is "nothing less than the specific manner in which" a field provides a distinct means for "[grasping] the world" (1993: 8). Not all research carries these kinds of stakes.

Table 1 provides a summary of the three types of response to an epistemological obstacle that involves the folk. Knowledge Capital refers to what appears from the socioanalytic exchange as knowledge with high epistemological value. Puzzling-solving refers to knowledge formed within a settled analytic framework where certain basic propositions are not disputed but can be applied, with greater effect, to more cases. Strong validity refers to knowledge that emphasizes the formation of conclusions for which the propositions do not have to be sound for common sense. They just have to be reached in a valid way. Objectivation refers to knowledge claims that pair folk meanings with field-specific constructs of them.

Epistemological Content refers to how the socioanalytic exchange constitutes knowledge of a specific sort, with meaning variance referring to the relative differences between the referents of similar terms within different frameworks. Strong meaning variance involves a replacement or omission of familiar terms like "belief," "desire," "action" or "person." Weak meaning variance retains familiar terms but contrasts them with unfamiliar referents (e.g. beliefs as practices; desires as bundles of relations; consciousness as learned). Folk-compatible means that the terms are used in ways that are familiar to the folk and do not presuppose a cognitive demarcation.

Epistemological Break and Socioanalytic Exchange are closely connected as they both attempt to capture relations with the folk that dictate knowledge in connection to an epistemological obstacle. A break refers to the presence of alternative knowledge claims that are field-specific and establish a contrast with folk knowledge. An exchange can defer to the folk, in which case the folk "norm of thought" is given field-specific application; the exchange can be hierarchical relative to field constructs by presenting folk constructs only to generate doubt about them; or it can be egalitarian where both folk and field constructs are equally non-arbitrary. Here folk knowledge is treated more as a *topic* than a resource.

Defense does not constitute an epistemological break with folk understandings but instead gives them a field-specific elaboration and application, which can take varied forms. This approach has minimal to no meaning variance with folk understandings. The epistemological content is highly folk-compatible because it seeks to *know as the folk know*. Deference to the folk is the socioanalytic exchange, and the burden is on fielded knowledge to "make sense" in this respect, not in a field-specific way but

relative to norms of thought that are not contingent on the existence of knowledge fields.

Replacement produces a knowledge capital that puts epistemological value on claims that are instead defined by the strongest type of meaning variance from folk understandings. This demands the acquisition of entirely new categories that may or may not be borrowed from a different field. A replacement argument does consist of an epistemological break, the socioanalytic exchange with the folk is not equal, and constructs from the field are not arbitrary while folk constructs *are* arbitrary.

Critique, finally, produces a knowledge capital that appears from an egalitarian exchange, where both folk knowledge and discipline-specific knowledge are equally non-arbitrary. Epistemological value is given to objectivation that generates a weaker type of meaning variance than replacement because the folk terms are retained, holding the folk and the scientific side by side as a way of generating fielded knowledge out of folk knowledge. This deploys “extraordinary discourse” as a device to disclose ordinary experience but not replace it.

Below, I use a comparison between post-Wittgenstein Anglophone philosophy and post-sixties American sociology to understand how an epistemological obstacle arises in a field, the position-takings that it generates and their consequences. The comparison between sociology and philosophy is relevant and novel for three reasons: (1) they share the same humanistic concern of generating knowledge *about* the folk while recognizing that the folk generate knowledge about themselves (see Turner 1983; Giddens 1979: 234); (2) there have been persistent “interfield transfers” of arguments about action from Anglophone philosophy to American sociology for some time (Camic 2013); and (3) while Bachelard (1984[1934]: 27ff) acknowledged the presence of epistemological obstacles in human science as much as natural science, rarely have they been subject to a detailed empirical case study or comparison.¹²

Because obstacles are understood cognitively, the difficulty of “breaking” with them presents a practical challenge. Not only that, an *interest* in breaking with them is unusual in fields that are far more human-scientific than natural-scientific, relationally defined. The accounts below attempt to explain both *how* and *why* the folk action obstacle emerges in each field and a break attempted. The comparison also attempts a participant objectivation of the sociological field, in a “psychologically concrete” sense (Bachelard 2002[1938]: 54), explaining why folk psychology holds a certain appeal, why breaking with it also holds a certain appeal, and how all of this runs contrary to an ahistorical assessment of “common sense” or an abstract commitment to an epistemology (“post-positivism”) by recognizing the cognitive difference that a field makes. This requires two histories to make sense of current position-taking in the American sociological field as an interfield phenomenon and an *accumulated history*.

¹² Two prominent exceptions would be Foucault (1973[1966]) and Althusser (2008[1965]) who both offer human-scientific cases that feature epistemological break (e.g. between *epistemes* and between the young and old Marx). Both are shortsighted in different ways, however, because they tend to link break or obstacle in human science with anti-humanism. This is unnecessary, in part, because any mid-level social ordering, like *fielded* knowledge, does not predetermine the kind of knowledge claims that will be made vis-à-vis an epistemological obstacle (which in Foucault’s case is often read as a structuralist determinism in which breaks *automatically* occur somehow), and also because (at least in Althusser’s case) there is not a necessary association between realism (or truth) and breaking with an obstacle, i.e. the claim that Marx broke with his youthful humanism because of “[his] discovery of real reality beyond ideology” (81). The first problem involves a *how* question, the second involves a *why* question about obstacles and breaks.

First epistemological obstacle: Anglophone philosophy¹³

From the analytic worldview to the philosophy of action

In 1971, the philosopher Richard Bernstein observed that any future historian glancing over the history of philosophy in the English-speaking world in the twentieth century would see that, starting rather suddenly in the 1950s, the “status and nature of action, and such related concepts as intention, purpose, teleology, motive and reasons [moved into] the very foreground of philosophic discussion. Examining the literature that was foundational to the development of analytic philosophy, he might be surprised to discover that [prior to this period] there is scarcely any direct concern with the nature of human action as a philosophic problem” (Bernstein 1971: 234).¹⁴

Much of that literature would show the influence of Bertrand Russell, G.E. Moore, Rudolf Carnap and other members of the Vienna circle. They all shared what Dummett (1996) calls the “analytic worldview” based on a shared background in a deviant trajectory within European philosophy, which starts if anywhere with Carnap’s attendance at Gottlob Frege’s mathematical logic seminar at Jena, his encounter with the work of Russell, and Frege’s recommendation that Ludwig Wittgenstein go to Cambridge to study with Russell. It was the logical analysis of delimited “concepts” identified through language that was Frege’s method of “extruding thoughts from the mind,” and it was distinct in this respect from the competing approach developed by Frege’s young rival Edmund Husserl and his phenomenology, which proved seminal to continental philosophy.

For Carnap, the clarifying power of logic helped dissolve purely philosophical problems. In the opening chapter of his *The Logical Structure of the World* he quotes Russell: “Wherever possible, logical constructions are to be substituted for inferred entities” (Carnap 2002[1928]: 5). Carnap’s “constructional system” would attempt a

¹³ Both Glock (2008: 73ff) and Dummett (1996: 1-2) dispute the “Anglophone” or “Anglo-American” geo-linguistic moniker, suggesting instead the more accurate label “Anglo-Austrian” or the “Anglo-Austrian analytic axis” respectively, while insisting on the limits of *any* accurate classification. Still, as Leiter (2011) suggests, the Anglophone or analytic label may make little intellectual or geographic sense and still make *sociological* sense as a persistently realized category: “since it allows graduate programs in philosophy to define spheres of permissible ignorance for their students.” This speaks to a field boundary in which exchange occurs *between* philosophy departments in Anglophone countries, but not between philosophers classified as “analytic” and “continental” in Anglophone countries. This is not to mention the further complication demonstrated by American pragmatism that long predates “Anglophone philosophy” in the United States. Glock takes the sociological meaning a step further, into schemes of perception, arguing that when a continental philosopher crosses the Anglophone field boundary (or vice versa) the reception is often predictable (an example being the Derrida/Searle exchange in the mid-1970s): “Responding to a continental article, the analytic commentator would engage in a flurry of ‘What do you mean by this?’, ‘What is the justification for that?’, and ‘How are we to understand the next thing?’ The continental respondent to an analytic piece, by contrast, would ignore the general gist, pick out some tiny detail, and engage in comments about etymological or historical aspects surrounding that detail” (Glock 2008: 257)

¹⁴ A Google N-gram of the “philosophy of action” searching English-language texts between 1930 and 2018 demonstrates a clear bell-shaped trend that closely matches Bernstein’s periodization of Anglophone philosophical interest in action. It has almost no presence prior to the mid-1950s. It reaches a peak in the late 1960s before declining across the 1970s and 1980s, though maintaining a persistent presence ever since.

“step-by-step derivation or ‘construction’ of all concepts from certain fundamental concepts” (5). This situated analytic philosophy as an expression of high modernism, alongside The Bauhaus in architecture, likewise seeking ultimate clarity in form and structure (Galison 1990). The early Wittgenstein was a main inspiration for this endeavor. Carnap and those in the Vienna Circle read aloud each sentence of Wittgenstein’s *Tractatus-Logico Philosophicus* (1921) to each other in 1926, *twice*.¹⁵ Russell would write “The Philosophy of Logical Atomism” (1918) as an attempt to define “logical atoms” based on what he had learned from Wittgenstein as the latter worked on the *Tractatus*. The paradox is that from this starting point to the philosophy of action, it was Wittgenstein who provided a seminal push in the *other* direction.

If the analytic worldview had persisted completely in Anglophone philosophy, then action could not have become a “philosophic problem” because philosophy itself had no clear (field-level) distinction in this worldview. Rather, it attempted to dissolve the boundaries that had hitherto *made* problems into “philosophic” problems. Wittgenstein had, after all, declared in the *Tractatus* that philosophic problems are “*not* problems at all [but] arise from our failure to understand the logic of our language.” This fit perfectly with the analytic worldview’s effort to eliminate philosophy’s distinction. But it did not count on the inconsistency of Wittgenstein himself.

The surprising appearance of the “late Wittgenstein” as a radical departure from the Vienna Circle’s antiphilosophy is less of a mystery once we account for his “intellectual self-concept” as a *genius* who retained a deep ambiguity about the intellectual integrity of philosophy itself. This was “the story” that Wittgenstein told himself and others about who he was as an intellectual (Gross 2008: 263). Ray Monk (1991) argues that the philosophical contributions of Wittgenstein cannot be understood independently of the fact that he considered himself a philosophical genius, and he would not have continued on as a philosopher if he had not been authentically *inspired* in this way. For Monk, this exhibits a lingering influence of the Vienna of Wittgenstein’s youth. Genius was a paramount theme in the cafe culture of the city, and it assumed a self-destructive, even apocalyptic tone (Monk 1991: 10–12; see also Janik and Toulmin 1973: 22).¹⁶ It does not seem a stretch to say that Wittgenstein’s “genius,” as a product of this *fin-de-siecle* habitus, combined with the Augustinian style of his thinking, which favored self-examination, would lead him to find an interest in common sense about something as fundamental as action as appealing to interrogate and dismantle, demonstrating and confirming itself in the profound subtlety required to even *think* beyond this.

Anglophone philosophy, despite taking on the analytic worldview would therefore still retain a distinct knowledge capital in part because of what Wittgenstein would

¹⁵ The Vienna Circle that helped launch the analytic tradition was a small invisible college that met regularly in Vienna from 1924 to 1936 and included physicists, mathematicians, and other “outsiders to philosophy” among its ranks. Otto Neurath, a sociologist, was a leading member. Many of its members would later come to the United States as emigres from Nazism.

¹⁶ A depiction of this intellectual self-concept, as Monk (1991: 19ff) and Janik and Toulmin (1973: 176–77) both suggest, is found in Otto Weininger’s *Sex and Character* (1903). Effectively a convoluted screed justifying Weininger’s misogyny and anti-Semitism, it became a public sensation in Vienna after the author’s dramatic suicide in Beethoven’s old house. The book recommended the fateful option—genius or suicide—and included the almost *programmatically* statement: “Genius is the highest morality and, therefore, it is everyone’s duty.”

subsequently do and how this interacted with other changes affecting the Anglophone field in both its English and American branches. In Bachelard's terms, the late Wittgenstein's pursuits generated a profound "psychic vitality" that could be *routinized*. Anglophone philosophy would be organized around a capital very different from the system-building that had prevailed in philosophical activity of the past: dense, laborious, distracting. It would revolve instead around short bursts and the *sudden insight*, focused on ordinary language terms that were *assumed* to be familiar: pure, seemingly effortless, disclosing genius (see Glock 2008: 164ff).

The English branch

As Wittgenstein argued in his *Philosophical Investigations* (Wittgenstein 1953; hereafter *PI*), "... the most that can be said is that we *construct* ideal languages." That is how he summarizes the early tendencies of analytic philosophy, including his own. But, as he continues, "the word 'ideal' is liable to mislead, for it sounds as if these languages were better, more perfect, than our *everyday language*; and as if it took the logician to show people at last what a correct sentence looked like" (§ 81; emphasis added). The Oxford philosopher P.F. Strawson gives a retrospective sense of the shift by arguing that "it is the most general, the most fundamental, *most ordinary* ideas which give rise to the major problems of philosophy ... Why would it be supposed that the only way to gain understanding of the words which express the philosophically puzzling concepts was to translate sentences in which they occurred into sentences in which they did not occur?" (Strawson 1960: 103–104; emphasis added).

What Wittgenstein and Strawson both demonstrate here is an exchange with the folk that would prompt a series of position-takings in the Anglophone field. This concerned the cognitive boundaries that would mark the philosophical field and make it distinct, despite having adopted the "antiphilosophy" of the analytic worldview (Galison 1990: 709). If any single phrase conveys this field-level change, and its tension-filled genesis of sudden insight, it is "philosophical psychology."

For those like Carnap, it would have been nonsensical to put "philosophical" in front of "psychology." That would only add linguistic obfuscation to what should be reduced to something more basic, like a disposition (see Carnap 2002[1928]: 245). Yet beginning in the 1950s a book series through Routledge and Kegan Paul appeared with exactly that title: *Studies in Philosophical Psychology*, and it included influential books like Peter Winch's *Idea of a Social Science and its Relation to Philosophy* (1958), R.S. Peters *The Concept of Motivation* (1958) and A.I. Meldon's *Free Action* (1961).

But what is "philosophical" about psychology? The appearance of this phrase corresponded in no small part to the greater autonomization and consolidation of the philosophical field in England. From the "Dons" organization of the prewar years, a world that was small, insular, informally incorporated, with a weak distinction from fields like mathematics, thus mirroring the Vienna Circle, the postwar (and post-Wittgenstein) field constituted a more settled and specialized university pursuit, where university training expanded, helping the incorporation of philosophy into more general courses of study like Oxford's "Philosophy, Politics and Economics" where it supplied students with a set of "meta-tools" useful for *any* sort of reasoning. Increasingly, the audience for philosophy consisted of other philosophers (Glock 2008: 77ff).

What philosophers discussed under the guise of philosophical psychology is what we would expect from the confrontation with an epistemological obstacle that has folk characteristics. Folk psychology and all of its entailments (intention, belief, desire) are so apparently indispensable to an accurate picture of human beings that they cannot be ignored. But should what “the folk” understand about themselves be taken seriously? This is a question that could only be *seriously* entertained once the reductive analysis of the earlier generation of analytic philosophy was dropped and philosophers would not only engage with “ordinary language” but could also seek their own distinction from it.¹⁷ The entry point for doing this proved to be a problem of *action*.

If the epistemological obstacle was first engaged with anywhere, Wittgenstein’s remark in § 611 of *Philosophical Investigations* serves well: “‘Willing too is merely an experience,’ one would like to say ... I cannot bring it about.” Scholarship on this passage generally points to Wittgenstein’s quoting of William James here, from the *Principles of Psychology* (rumored to be the only philosophy book on Wittgenstein’s shelf in his rooms at Cambridge in the early 1930s), and Wittgenstein’s taking issue with James’ “ideomotor theory” and the claim that we can choose between available thoughts of movements (Scott 1998). But I want to argue that his singling out James is largely because Wittgenstein finds James to articulate folk knowledge, which is Wittgenstein’s real aim. This is made clear when we account for later passages in the *PI* and Wittgenstein’s earlier musings on action.

The proper context for his claims is as an attempt to bring folk knowledge and ordinary language into the philosophical field. He would do this because, as with many things in Wittgenstein’s wildly influential work, the charisma of his claims convey a demonstrable genius (Monk 1991). The epistemological obstacle that Wittgenstein confronts is the notion that willing is something that ordinary people can bring about. However, since we cannot *do* an experience, Wittgenstein suggests that willing is *not* something we can bring about. He gives the idea free rein in § 620 of *PI*:

Doing itself seems not to have any volume of experience. It seems like an extensionless point, the point of a needle. This point seems to be the real agent. And the phenomenal happenings only to be consequences of this acting. ‘I do’ ... seems to have a definite sense, separate from all experience.

After this, Wittgenstein continues with his famous question from this series of entries in *PI*: “But let us not forget this ‘when I raise my arm,’ my arm goes up ... And the problem arises: what is left over if I subtract the fact that my arm goes up from the fact that I raise my arm?” (§ 621).

These passages have been subject to many conflicting interpretations. Regardless of the possible and various philosophical influences on Wittgenstein’s argument (which he does not make explicit), I emphasize the point that what he is engaging with here is the ordinary language phrase “I do ...” and whether this phrase has a sense separate from all experience. An ordinary folk understanding says no. But *that* is what Wittgenstein draws into question. He clarifies his main point with the following claim, taken from

¹⁷ It was in this context that the Oxford philosopher J.L. Austin (1962: 3–4) defined the “scholastic view” as a preoccupation with designating all possible meanings of an ordinary word rather than relying on its ordinary use.

the *Philosophical Grammar*, compiled from notes written during the seminal 1931–34 period:

Very well, but there's no doubt that you *also* have experiences when you voluntarily move your arm; because you *see* (and feel) it moving whether or not you take up the attitude of the observer. So just for once try to distinguish between all the experiences of acting plus the doing (which is not an experience) and all those experiences without the element of doing. Think over whether you still need this element, or whether it is beginning to appear redundant (Wittgenstein 1991[1931–34]: 97).

If Wittgenstein's remarks are cryptic, the debate he inspired is not. When he says "you" here the meaning should be explicit. What we glimpse is an exchange with "us", the folk, that signals the kind of knowledge capital that would come to circulate in the field through engagement with ordinary language as a philosophical problem (see also Ryle 1945).¹⁸ For the initial wave of concern with action in the field, the post-Wittgenstein period signals a cluster of arguments concerned largely with the nature of the language used by the folk to talk about their action and the question whether this was *separate* from their action. The universe of positions in the field that would form around this question constituted a way of engaging with the folk that concerned the boundary and relation between the philosophical field and folk understanding.

For those like Peters and Melden, actions can either *not* be explained by citing causes and only reasons explanations qualify, or a kind of strict mechanical causation is not applicable to human action, though other sorts of causation might be. As Bernstein (1971: 279) observes, these efforts sought out "conceptual, a priori or necessary truth" present in ordinary language. This provoked questions about what it *meant* for reasons or intentions to cause actions. Ultimately, it changed the analytic worldview, which sought to remove purely philosophical distinctions (and in some sense "end" philosophy), to include an interrogation of the folk from across a cognitive boundary now marking a field.

The American branch

In the United States, philosophy had a rich history that preceded the influence of the analytic worldview, but in many ways that prehistory prepared the American branch of the field to be absorbed by it, though not without tension (Kuklick 2001: 257ff). The philosophy of action in the United States was influenced by transatlantic connections, especially between Harvard and Oxford. But it was not framed around the problem of "ordinary language," but more around problems that characterized the philosophy of mind as the equivalent subfield to philosophical psychology. In

¹⁸ Gilbert Ryle at Oxford, a contemporary and sometime rival of Wittgenstein, proposed that relevant differences in philosophy are not really between different schools of thought (or "isms") but more importantly between "philosophers and non-philosophers" (Ryle 1945: 158), putting emphasis on a cognitive field boundary. Ryle famously proposed a different type of philosophical practice, one that directly engages with folk meanings (e.g. "ordinary language").

many ways the major influence of certain arguments, all published within a 5-year period around the early 1960s, cannot be understood independently of the sweeping changes to the United States university system in the postwar and Cold War period. This affected the practice of research, including philosophy. The field consolidated around growth and productivity geared toward validation by other philosophers: “the ‘Harvard Model’ became standard: even schools that served regional needs or catered to specialized groups of students downgraded service and teaching and hire and promoted faculty on the basis of credentials beginning with the doctoral degree and eventuating in productivity evidenced by writing” (Kuklick 2001: 261). Further changes to the American branch in the mid-1970s would affect the philosophical field differently.

When the American philosopher Wilfrid Sellars gave his influential lecture “The Myth of the Given” in 1956, which he would later published as “Empiricism and the Philosophy of Mind,” his main target was the philosopher C.I. Lewis’ argument about “the given” and how knowledge of the external world is ultimately justified by indubitable sense information. Sellars would argue, by contrast, that such a view could not account for the influence of mind as signaling our ability to have knowledge of things. The way in which Sellars develops this argument is significant for our purposes because it demonstrates a philosophical position-taking on something that looks like folk psychology.

For Sellars to make this argument about mind, he must explain where it comes from, and he takes a quasi-evolutionary point of view. His exchange with folk meanings first involves his elaboration (Sellars 1956) of a fable or myth that tells its origin story, what he calls “The Myth of Jones.” Sellars was influenced by Ernst Cassirer and so was less concerned with truth of the myth than with drawing attention to a folk standard. In the Myth, a person in the distant past (Jones) invents the “manifest image” that provides “the terms in which man appears to himself” as a being that has interior states like beliefs, desires, hopes, wants (etc). Sellars visits a time when inner, mental episodes did not exist. In Sellars’ Myth, we encounter beings (“Ryleans”) who think and talk like us but without any vocabulary for “inner states” like beliefs, desires, thoughts or sensations.¹⁹

For Sellars even if the “given” is a myth in this case, it does not mean that its rejection is in order. The “scientific image” by contrast cannot “replace the manifest image without rejecting its own foundations” (Sellars 1963: 21). Sellars insists instead on what he calls a “stereoscopic or synoptic view.”

...[The] conceptual framework of persons is the framework in which we think of one another as sharing the community intentions which provide the ambience of principles and standards (above all, those which make meaningful discourse and rationality itself possible) within which we live our own individual lives. A person can almost be defined as a being that has intentions. Thus the conceptual framework of persons is not something that needs to be

¹⁹ Rorty (1979: 70ff) later replicates Sellars’ Myth using the case of the mythical Antipodeans who understood themselves relative to their brain structure instead. This demonstrates what would happen if neurology and biochemistry had been the first disciplines in which “breakthroughs had been achieved” vis-a-vis the given rather than Jones’ proto-Cartesian philosophy.

reconciled with the scientific image, but rather something to be *joined* to it (Sellars 1963: 40).

The scientific image is the image of “man-in-the-world” produced by fields like physics, biology or chemistry. For Sellars, the philosopher is uniquely positioned to confront these “two conceptions, equally public, equally non-arbitrary, of man-in-the-world and he cannot shirk the attempt to see how they fall together in one stereoscopic view” (5). The stereoscopic view appears from an exchange with folk knowledge from which knowledge capital derives by holding *both* scientific and manifest images in view.

Sellars’ argument about the role of the philosopher is presented much differently from those in the English field. I argue that Sellars’ relation to the folk demonstrates a position-taking of *critique*. To understand the difference it makes, consider a second position-taking that also confronts folk psychology.

Starting in 1963 with his essay “Actions, Reasons and Causes,” Donald Davidson develops a different position-taking, one that *defends* the folk knowledge that explanations of action in terms of reasons (including intentions) are a form of causal explanation. For Davidson there is no other way to explain the “mysterious” *because* relation between a reason and an action other than in a causal way. Davidson deviates from a strong volitionist argument by focusing his defense on “mental events” that include the onset of mental states, the noticing of external situations and the forming of intentions (Davidson 1963: 694). All of these mental events create a “pro-attitude” that is presupposed by the action in question. Counterfactually, the action *would not* have occurred without this mental attitude *and* a primary reason, that actors themselves make explicit. As Davidson concludes, “some causes have no agents. Primary among them are those states and changes in state of persons which, because they are reasons as well as causes, make persons voluntary agents” (Davidson 1963: 700).

Davidson would ultimately reaffirm the folk-psychological insight that beliefs and desires are causes of action because of how the structure of reasoning is *normative*, or the standard according to which actions are necessarily understood (e.g. a “conceptual scheme”):

[An] explanatory scheme like “folk psychology” [identifies] the phenomena to be explained, and the phenomena that do the explaining, as directly answering to our own norms; reason-explanations make others intelligible to us only to the extent that we can *recognize something like our own reasoning powers at work*

...We have noticed the obvious fact that a belief and a desire explain an action only if the contents of the belief and desire entail that there is something desirable about the action, given the description under which the action is being explained. This entailment marks a *normative* element, a primitive aspect of rationality (Davidson 2004[1987]: 114–115; emphasis mine).

For Davidson this meant, that “psychology as philosophy” (Davidson 1974) is irreducible to empirical psychology because philosophers can *alone* reveal the workings of this conceptual scheme. Davidson’s position and this (anomalous)

bracketing maneuver has, in many ways, proven the most influential. But the same year he published his initial essay, a third and much different position-taking took shape in the American branch.

The philosopher Paul Feyerabend had Austrian roots, but he trained as a philosopher of science with Karl Popper in London. When he moved into the American field in the late 1950s, this debate over “common idioms” appeared to him from a position more immediately shaped by natural scientific replacement claims, which retranslated meant “mind-body dualism.” Feyerabend engaged the folk question, then, by arguing that

...such [common] idioms are adapted not to *facts* but to *beliefs*. If these beliefs are widely accepted; if they are intimately connected with the fears and hopes of the community in which they occur; if they are defended, and reinforced with the help of powerful institutions; if one’s whole life is somehow carried out in accordance with them ... then the language representing them will be regarded as most successful. At the same time, it is clear that the question of the truth of the beliefs has not been touched (Feyerabend 1963: 51–52).

What Feyerabend suggests here runs directly counter to the kind of bracketing maneuver of Davidson. He warns against arriving at anything normative or necessary through a philosophical analysis of ordinary language. Around the same time, Richard Rorty (1965) would make a similar warning against this way of proving the “irreducibility of entities,” especially mental entities. Feyerabend develops a *re-placement* position-taking that differs in its image of the philosopher: less as an autonomous conceptual analyst who probes ordinary language, and not one who holds a stereoscopic view of both manifest and scientific images together, but rather one who demonstrates points of convergence and possible replacement between folk meanings and scientific meanings, or at least appreciates the dubitability of folk meanings, the fact that they can change.

For Sellars and Davidson, their versions of the philosopher represent a kind of assertion of the autonomy of the philosophical field during a relatively flourishing moment.²⁰ They leveraged the “manifest image” and “psychology as philosophy” (respectively) as irreducible to natural scientific replacement. In the early 1960s, when Davidson and Sellars both published their groundbreaking

²⁰ When he published “Actions, Reasons and Causes” in 1963, Davidson was in his mid-forties, had been trained mainly in classics not philosophy and, with the exception of some co-authored articles on the formal aspects of decision theory, was largely unpublished to that point. His situation exemplified the growing autonomy of the philosophical field at this time, as an extension of the mass university expansion of the postwar and Cold War periods (Isaac 2013). Sellars himself also exemplifies the unique condition of the philosophical field. His ambition suggests the vacuum in the field left, at least in part, by the waning influence of logical positivists, including members of the Vienna Circle. While Sellars had name recognition in the field (his father being the prolific Roy Wood Sellars), he lacked a PhD in philosophy and was eclectic in his publishing early in his career. His main contributions came in the form of co-editing collected volumes (*Readings in Philosophical Analysis*, *Readings in Ethical Theory*). As he wrote about his own writing during the influential mid- to late-1950s period: “I soon discovered that spinning out, as I was, ideas in a vacuum, everything I wrote was idiosyncratic and had little direct connection with what others had said. Each spinning required a new web to support it” (Sellars 1975: 292).

claims, the American branch of the Anglophone field was just beginning an upward surge in degrees granted across BA, MA and PhD levels (Hoekema 1989: 850). The tenure system, meanwhile, became increasingly based on productivity evidenced through published writing, combined with more professors now needing to write.

It was not long after this period, however, that a downward turn cleared the way for a reconfiguration of the field. From relative obscurity to influence, the career of philosophers Paul and Patricia Churchland signal these further changes. Paul Churchland's "eliminative materialism" argument (Churchland 1981; 1979) was published at a time when the American branch of the field was in different state than it was during Davidson, Sellars or Feyerabend's time. Increasing numbers of PhD recipients, made possible by institutional growth of the earlier period, confronted a steady retraction in academic jobs (Hoekema 1989: 845ff). This was also a period shaped by professional hostilities between non-analytic "pluralists" and analytic philosophers who exercised hegemonic control over the professional apparatus in the United States (Gross 2008: 217ff).

Paul Churchland had studied under Sellars at Pittsburgh and spent a fruitful but largely anonymous decade and a half at the University of Manitoba with his wife Patricia (also a philosopher) "pursuing whatever they liked" (MacFarquhar 2007). Once he published the strongest argument from the replacement position sketched by Feyerabend (see Churchland 1979), this launched him and his wife into prominence. Claiming that "our commonsense conception of psychological phenomena constitutes a radically false theory," Paul Churchland argued that "both the principles and the ontology of that theory will eventually be displaced, rather than smoothly reduced, by completed neuroscience" (Churchland 1981: 67). This replacement strategy is rooted in a hierarchy of knowledge. For Churchland, folk psychology and scientific images are *not* equally non-arbitrary. Rather, they are trying to do the same thing, and one is *better* than the other. We do not need a manifest image. Using completed neuroscience, a "Martian could justly describe to us the familiar run of mental states, even though his own psychology were very different from ours" (69).

Contemporary position-taking: Extending, mixing, switching

Churchland's openly favoring a scientific replacement invited a new kind of capital in field, with the philosopher now an intellectual companion to the growing empirical discipline of cognitive neuroscience. Patricia Churchland's provocative *Neurophilosophy* (Churchland 1989) drew the connection even closer. This coincided with the Churchlands moving from Manitoba to the University of California-San Diego in 1984, the hub of the connectionist movement in cognitive science and its attention to sub-symbolic aspects of mind (see Smolensky 1989). Philosophers like Andy Clark and David Chalmers demonstrate more recent iterations of the philosopher from this position, and how capital from the Anglophone philosophical field can be transferred into cognitive neuroscience and vice versa (Thagard 2009). This helps generate interest in the *counterintuitive* finding rather than (to paraphrase the philosopher Thomas Nagel) dismissing an argument on those grounds alone (see Dennett 2017: 4–5).

In the current configuration of the field, this position-taking gives philosophers a connection to an extensive journal apparatus, funded research centers, degree programs and ties to financially powerful artificial intelligence research. In many respects, the subfield that still exists as the philosophy of action has not essentially moved past the positions originally staked by post-Wittgensteinians in the English branch and Davidson, but rather continues to engage them in a kind of repeat loop, with Davidson (and his “standard causal story”) continuing to be the principal figure who inspires agreement and disagreement. This now involves *mostly* exegesis, which more recent practitioners have expressed concern about (Velleman 1992; Hornsby 2008).²¹

The subfield also makes available crosses between positions and shifts between them. As a new entrant to the philosophical field, Daniel Dennett (1971) engaged in position-taking related to Davidson but more deflationist, calling it the “intentional stance” as an explanatory strategy (alongside the “design “and “physical” stance) that implied nothing about the “composition, constitution, consciousness ... of the entities falling under it” (100).²² He would later (Dennett 1988) adopt an eliminativism about phenomenal states (“qualia”) but bracket this from the intentional stance, thus straddling and mixing pre-established positions. Stephen Stich, meanwhile, would start as a staunch eliminativist (Stich 1983) but would later have “serious inklings that perhaps all was not well” with eliminativism. He would ultimately recant (Stich 1996) and switch to what he calls a “folk semantics” of reference that performs a kind of semantic ascent on both Davidson’s and Churchland’s positions: any understanding of mind (whether folk or physical) cannot be dismissed on the grounds of whether its terminology actually refers to anything. This coincides with Stich’s movement in the field from a relatively marginal position bridging philosophy and cognitive science (akin to creative peripherals like Churchland, Rorty and Feyerabend) with reflexive skepticism about philosophy’s singular merit, to a kind of quasi-aristocratic assertion of philosophy’s irreducibility at a moment when this is threatened. Dennett, meanwhile, becomes omnivorous in response to similar threats, reading broadly, remaking the philosopher in the form of intellectual cross-germinator, holding contrasts together at once and innovating throughout the span of a remarkably long career; as a virtuoso, he occupies a position that likely cannot be replicated. If Bachelard is right, such persistence, movement and mixing, without *new* positions, should be expected when the stakes of a field involve position-takings formed in relation to an epistemological obstacle, as they did during the 1953–1981 period in Anglophone philosophy.

²¹ I thank Berislav Marušić for this insight.

²² According to Dennett, something is an intentional system “only in relation to the strategies of someone who is trying to explain and predict behavior” by ascribing to the system “beliefs and desires (hopes, fears, intentions, hunches ...)” (Dennett 1971: 87). A different relation involves the “design stance” and the “physical stance.” All of these are “relatively uncluttered and unmetaphysical, [abstracted] from questions of the composition, constitution, consciousness, morality, or divinity of the entities falling under [them]” (100). The suggestion, then, is that the intentional stance *just so happens* to be an effective strategy for explaining humans, the design stance for explaining computers, and the physical stance for explaining the heart. Though in principle different stances can, and are, taken, Dennett maintains this position-taking, stated and restated in memorable phraseology: folk psychology is an “intuition pump” about intentional systems like humans (“to a remarkably good first approximation”); it need not refer to anything “sub-personally” and *still* be good at this (Dennett 2013: 73ff).

Second epistemological obstacle: American sociology²³

Philosophies and theories of action

The appearance of folk psychology as an epistemological obstacle in sociology is not independent of this history of its appearance in Anglophone philosophy. Davidson, in particular, is a key intellectual resource for position-taking in the American sociological field (see Hedstrom 2005; Reed 2012), as are Churchland, Peters, and Gilbert Ryle to a lesser extent (Martin 2011; Strand and Lizardo 2015; Strand and Lizardo 2017). Wittgenstein himself remains a timely interlocutor.²⁴ In fact, even if this philosophical capital is resisted (Watts 2014: 319) this *still* replicates a position-taking in the philosophical field (in this case, replacement) because both fields share the same problem. As I argue, the Anglophone philosophy of action seems to interfield resonate in American sociology not because it gives sociologists problems to solve, but because it appears *anticipatory* at the moment, providing just the right tools at just the right time, to “solve a practical problem” (McDonnell et al. 2017: 9) that appears from within the sociological field itself.²⁵

The following analysis provides a history in order to make sense of a categorization like “post-sixties American sociology” as *objectified history*. The goal is to enable a

²³ “American sociology” comes under less dispute than the overtly exclusionary “Anglophone philosophy” but it implies a problematic commitment to methodological nationalism (Krause 2018). I use it here to emphasize the morphological connection between American sociology and United States institutions, particularly the university system. The label itself begs important questions about scale and boundaries, because arguably the post-sixties American sociological field acquires the profile of a global field (see Hiller 1979), with a degree of “vertical autonomy” (Buchholz 2016) in relation to other sociological fields, which makes the capital obtained in American sociology transferrable to other sociological fields, though not vice versa, and which makes other sociological field define their sociology in relation to “American sociology” (Abend 2008). Arguably, this reflects a process set in motion during the post-war period, and if for no other reason than this morphological connection with the United States university system and its vast institutional support, completely unrivaled in its breadth and resources (particularly tuition-paying/debt-incurring undergraduates), combined with the surge in popularity of sociology on United States university campuses between 1966 and 1976, reflected in more PhDs granted from United States sociology departments, which themselves expanded and increased during this time (Turner and Turner 1986). The extent of this institutional presence (albeit very specific) has no parallel in other national contexts, which arguably gives the accumulated history that is “American sociology” an impact that scales beyond national borders but still makes those national borders essential to recognize. The label “American” is indefensible, excepting for its alignment with the major professional association’s own deception: *American Sociological Association*.

²⁴ At least since Rubinstein’s (1977) late-Wittgensteinian critique of social action, Winch’s own engagement with social science also prompted by Wittgenstein (Winch 1956), and the Edinburgh Science Studies Unit’s adoption of Wittgenstein as a major intellectual precursor (Bloor 1983), Wittgenstein has held a remarkable pride of place for various conversations in sociology, which must include Geertz’s (1973) many appropriations. In fact, from Bloor’s (1983: 183) point of view, the late Wittgenstein also sought to “end philosophy,” not by dissolving it into language but into “the sociology of knowledge.”

²⁵ Such connections between philosophy and sociology are, in a general sense, not unusual (see Durkheim 1984[1893]: 364); though arguably the *deference* given to philosophers by sociologists in the contemporary American field is unique in some comparative perspective. In pre- and post-1968 France, for instance, the direction of influence had more the characteristic of a pendulum: for a time, from social science to philosophy, then back again, explicitly in competition (Bourdieu and Passeron 1967). Jacques Derrida, to take one prominent example, made his reputation as a philosopher in France only by engaging with Levi-Strauss, who had successfully rehabilitated a Durkheimian social science against Sartre’s post-war existentialism. This reactive interfield relation generated the “postmodern” as a transatlantic importation, alongside Derrida himself, though not in philosophy (Lamont 1987).

reflexive comparison with post-Wittgenstein Anglophone philosophy in order to demonstrate what it means for knowledge fields to confront folk-derived epistemological obstacles and to clarify the stakes involved in the interfield sync between the two fields, particularly for sociology.²⁶ In the same manner that Anglophone philosophy carries its own disciplinary unconscious, so too does American sociology as a history of the discontinuities and re-evaluations of knowledge, the methods used and the questions asked, all entangled with the *differentia specifica* of field-relative objects.

For post-Wittgenstein Anglophone philosophy, “action” becomes an object as the result of Wittgenstein claiming that philosophers should *know as the folk know* by taking ordinary language as their point of focus for generating fielded knowledge. For Wittgenstein this confirmed genius, and it introduced a psychic vitality in the potential of a folk/field break that could be routinized in the professionalizing conditions of the post-war philosophical field across the Anglophone world. The story is different for post-sixties American sociology which shares a same epistemological obstacle but, *mutatis mutandis*, not *because* of an interfield transfer with Anglophone philosophy.

What is unique about the American sociological field over the last couple of decades is how research programs have appeared or been proposed in the same relatively short window of time, each consisting, to some degree, of position-taking on the role that folk psychology should or should not play in sociological research (Hedstrom 2005; Reed 2011; Martin 2011; Watts 2014; Strand and Lizardo 2015; Turner 2018). These are boundary-marking and knowledge capital-generating pursuits; all fit under the umbrella of “action” (see Gross 2009: 359). They are expected when an epistemological obstacle arises from folk knowledge—immediately familiar, “ostensibly concrete and real,” seemingly unquestionable—and becomes a point of contention in a field. An Anglophone philosophical content applies *ex post facto*, through transfer, to support some sociological position-takings; however, the sociological effort to know as the folk know in order to generate fielded knowledge *already* occurs because of a field-specific history, that involves sociology as identity and practice, an *epistemological* history, in other words, of “social conditions masked and forgotten” (Bourdieu 1993: 50).

Theory in sociology since the sixties

The turning point begins in the mid-1960s with the apex of the “science ideal” that had obtained a symbolic capital at a time when the field, by the most conventional of measures, itself had obtained arguably its highest degree of autonomy and political (if not public) influence.²⁷ The science ideal consisted not in a commitment to a particular philosophy of science (e.g. “positivism”), or it did not consist primarily of this (Steinmetz 2005). It consisted rather of social relations that allowed for the prevalence

²⁶ I refer to “sixties” instead of 1960s to convey the trope and not the decade, to capture event-like shifts that very loosely correspond to the decade (see Marwick 1998: chap 1), though not to imply a post-hoc declaration either pro or con: the “sixties” as a narrative starting point for *political* reasons (Townsend 2002; see also Agar 2008; Jameson 1984).

²⁷ This is suggested by volumes like *Sociology Today: Problems and Prospects* (1959; edited Merton, Bloom and Cottrell), which declared sociology’s status as “the generalized social science” (13), *Sociology: The Progress of a Decade* (1961; edited by Lipset and Smelser), and *The Uses of Sociology* (1967; edited by Lazarsfeld, Sewell and Wilensky), which was organized around the theme of the 1962 ASA conference and featured claims like the public relates to sociology as “clients” of the discipline (x).

of positivistic claims in the form of hierarchical exchanges with outside parties, of counterintuitive arguments and controversial recommendations, all based on a stronger meaning variance between “common objects” and “sociological objects” (see Barton and Lazarsfeld 1961). The enactment of these relations, and the reaction against it, reveals the content of the science ideal in the American sociological field in the mid-1960s. Its decline would have the effect of priming the post-sixties field toward a more folk-compatible orientation, reflected in a shift in the field’s knowledge capital, with different positions creating a different epistemological value for what counts as sociological knowledge.

James Coleman’s *Equality of Educational Opportunity* (1966) and Daniel Moynihan’s “The Negro Family: The Case for National Action” (1965) provide two primary cases for seeing how this enactment of hierarchy and the reaction against it triggered such a shift in the field. Both exemplify the political-bureaucratic influence of sociologists at the time, in conjunction with President Johnson’s Great Society reforms: with Moynihan’s report written while he was Assistant Secretary of Labor in Johnson’s administration, and Coleman’s report at the request of the then U.S. Department of Health, Education and Welfare, under the auspices of the 1964 Civil Rights Act. Both reports employed a method of partialling or statistical control and other tools of a sociological toolkit built over the preceding half-century (particularly at Columbia where Coleman received his PhD). Both reports would also claim a mediating or background effect of the family on black/white differences in employment and education outcomes. The Coleman report additionally found a mediating effect of “attitudinal variables,” translating this as students’ “self-concept: what they believe about themselves” (Coleman et al. 1966: 320–21).

It was the reception to the two reports that proved so impactful. Both were subject to wide condemnation (inside and outside the field) as examples of victim-blaming, at best, and thinly veiled racism by out-of-touch elite white experts, at worst, that targeted black students’ family life and their self-concept for educational outcomes and made similar claims about unemployment among adult black males (Kilgore 2016; Geary 2017).²⁸ The seeming neutrality of “family” or “self-concept” as variables conflicted with the political and moral valences attached to similar terminology outside the field, especially in the context of the Civil Rights Movement (see Hamilton 1968). The Moynihan and Coleman reports showcased how sociological knowledge produced in ways largely freed of non-field influences could easily *not* signal an archetypical Weberian value-neutrality, but moral judgment instead, or patronizing reprimand and willful ignorance. The trauma of this reception, a failed exchange with crucial outside

²⁸ The historiography on each report emphasizes how exemplary they were by the science ideal standards at the time and how, in the reception, *that* provoked controversy: “the public release of the Moynihan Report sparked a political and an intellectual crisis for ... liberal social scientific poverty research,” even though the report “was not distinctive for its findings, which simply ‘reflected what we saw as a consensus among social scientists writing in that generation,’ ... Even such attention grabbing-language as the ‘tangle of pathology’ was borrowed from existing scholarly reports” (O’Conner 2001: 204). “To many antiracist social scientists worried about the Moynihan Report, Coleman’s approach was troubling. While Coleman treated the broad context of housing and employment segregation shaping the nation’s increasingly segregated urban cores as an unfortunate but unavoidable part of the urban landscape, the most salient background factors shaping achievement gaps, the Coleman Report held, were those typically appealed to in deficiency paradigms: ‘poverty, community attitudes, and low educational level of parents.’ Even social scientists ... questioned the ways the Coleman Report leaned towards blaming the victim” (Gordon 2017).

parties, proved a cautionary tale for the sociological field that would attempt stronger meaning variance: it could not take a field prerogative and treat sociological objects as if they had *no* other presence beyond the field (Bourdieu 2001: 88).

It is in this sense that the reports, their making and their reception, prove indicative of consequential post-sixties changes to the folk/field boundary away from the conditions in the American sociological field that had enabled “reports” of this kind and allowed for these controversial effects of meaning variance.²⁹ On the one hand, this encouraged the safer distance and neutralizing effect of quantitative data “that treated race and class as demographic variables rather than as either cultural or political economic facts,” its own form of folk-deference (O’Conner 2001: 210). On the other hand, by dissolving the science ideal, the controversy made a latent distinction *overt* and explicitly recognizable. Robert Merton (1972) would take stock of the period that featured the Coleman and Moynihan events by anatomizing what seemed (to him) the appearance of “insider” knowledge as a “new credential,” resting in direct “*acquaintance with*” rather than detached “*knowledge about*.” “Outsider” knowledge seemed older by comparison, and Merton mentions limits to its “margin of autonomy [that transcends] extraneous group allegiances” (42). He would advocate for a sociology of *both* knowledges, a shift in social relations *cum* epistemology, giving place to two “distinctive and interactive roles in the process of truth-seeking ... the consolidation of group-influenced perspectives and the autonomous values of scholarship” (36).

These cases of high-profile backlash to sociological knowledge could have been part of the field’s death knell. Undergraduate degree-taking would plummet shortly thereafter, dropping by nearly half between 1976 and 1986, shrinking the discipline’s university presence, and cutting the sociological field off from its “morphological” lifeblood: undergraduate enrollment and majors, especially in non-top tier departments (Turner and Turner 1990: 146). The American sociological field would be resurrected from this “near death” and subsequent “doldrums” (Collins 1986) in large part because of women’s participation on all levels in large numbers, and its becoming a disciplinary venue for feminist theory and practice (see Turner 2014: 70ff).³⁰

The dissolution of the science ideal in the field and this almost fatal collapse provided the circumstances in which the American sociological field would become both a more *porous* and more *unsettled* field, where knowledge capital became more available for dispute and change (Steinmetz 2017). Outside constituencies became significant in a way that ran counter to trends that had allowed for the “orthodox

²⁹ Another contextual example is Gouldner’s (1968) challenge to Becker (1967) on Becker’s answer to the question “Whose side are we on?” that sociologists “take a side based on their personal and political commitments [and] use our theoretical and technical resources to avoid the distortions that might introduce into our work” (Becker 1967: 247). Gouldner would dispute this as “partisanship unable to transcend the immediacies of narrowly conceived political commitment ... another form of market research” that still fit the agenda of the liberal welfare state just in a different way: a “sociology of young men with friends in Washington.” Gouldner instead favored a commitment to “values, not to factions” which he understood as retaining a field-specific “objectivity” by still attempting to speak for a “greater unity” that contained all points of view and affected all of them. By not accounting for this, by going *too* deferential toward non-fielded points of view, in other words, Gouldner argued that “partisan sociology” tacitly made the “liberal assumption that the policies of this bureaucracy equitably embody the diverse interests of the larger public” and thus could declare social problems to be *social* but only help individuals in order to solve them (1968: 116).

³⁰ Smith famously characterized the significance of this as follows: “The women’s movement has given us a sense of our right to have women’s interests represented in sociology, rather than just receiving as authoritative the interests traditionally represented in a sociology put together by men” (1974: 7).

consensus” of the mid-1960s. The field’s deferential exchange with social movements contributed to the emergent theme of “self-consciousness ... as a reaction against overbearing systemization” (Agar 2008: 584–85).³¹ This drew sociologists away from the symbolic capital of a cloistered science ideal that made headway in the postwar period and its search for a behavioral science—personified by the “Capitoline triumvirate” of Lazarsfeld, Parsons and Merton. It sparked opposition to this old guard, encouraged ritualized profanation of the three, and changed the very pursuit of sociology as both identity and practice among a now “disobedient generation” (see Turner and Sica 2005).

Four arguments, in particular, proved significant for dissolving the science ideal and changing the prevailing folk/field relations in ways that would shape the *post-sixties field*: standpoint epistemology, ethnomethodology, grounded theory, and what Jeffrey Alexander (1988) calls the “new theoretical movement” (hereafter NTM). These four constitute position-takings on what changes to the sociological field during “the sixties” should mean. They have subsequently had a strong influence on the knowledge capital available in the American sociological field, as epistemological claims about what sociological knowledge should be and *simultaneously* as claims about what the social relations between folk and field should be in opposition to mid-1960s hierarchy.³² The questioning of this relation and its generative connection with post-sixties knowledge capital persists in several still-resonant bits of *nomos* that become prevalent during this period and have remained prevalent ever since: how autonomous a field that produces knowledge *should* be from the folk who also produce it, how much of the aspects of ourselves should or *can* be left behind when we produce knowledge under the auspices of the sociological field, how social positionality matters *within* the field, whether and how “publics” outside the field *should* shape the prerogatives of the field, and how knowledge, whether fielded or not, remains entangled with the “sense of reality” that is *always* of political concern.

Contrary to Watts (2014), then, there are *field-specific* reasons why sociologists would appeal to common sense as Watts describes it. If sociologists do invoke “little stories” (Abbott 2001[1992]) in putting their objects into action, this is not unrelated to the set of factors and varied efforts that change the epistemological value of a sociological knowledge claim *after* the sixties. But this field-level trajectory from Coleman and Moynihan forward still does not explain the contemporary confrontation with folk action. To understand that, we

³¹ For Agar (2008) the sixties threw experts into prominent public display alongside the appearance of social movements that sparked political contest on the basis of knowledge. The combination would give rise to “studies” disciplines on university campuses, including black studies, women’s and gender studies and science and technology studies (see also Rojas 2007). Jameson (1984: 186), meanwhile, describes the resistance to social-scientific attempts at a “single fundamental experience,” epitomized by behavioral science and structuralism, that erupted in the sixties.

³² Glaeser and Strauss (1967), for instance, draw attention to the sociological “consultation” to evoke an entirely different model of this relation: “Grounded formal theory, like substantive theory, earns the trust of laymen and sociologists alike. Both consultant and consultee must have this trust in order to work together ... Seldom is a general theorist (if you can find one) called in for consultation by other sociologists, laymen, organizations or governments. Most consultants are well known for their research and everyday experience in a particular area ... The transferability of formal theories to diverse substantive areas is seldom done in sociological consultation because most formal theories are ungrounded, and therefore not trusted by either sociologists or laymen when they face ‘real life circumstances’” (1967: 98–99).

need to understand how the post-sixties shift in the American sociological field meant a shift toward claims-making about cognition, as Merton (1972) noticed early on, as a way of *settling* the post-sixties condition.

It *had* to mean this, if Bruner (1990) is right, because any engagement with the folk, particularly in resistance to the heavy-handed constructs of a field, cannot happen *without* also engaging with folk psychology: “the folk,” after all, cannot appear without making an attribution of mind (see also Martin 2011: 18; Turner 2018: 211ff). In particular, this meant appeals to non-fielded points of view as “tests” or checks on sociological knowledge (see Smith 1974: 11). This involved (and still involves) appeals to “experience.”³³ For the cultural turn, representation and interpretation were indispensable to human-scientific explanation because this promised access to “agents’ experience” despite any distance in time, space or social position (see Biernacki 1999). Ethnomethodology arguably epitomized this effort by appealing directly to the experience of social order *as the folk know it*, which is to say as “an ongoing achievement of the organized activities of everyday life” (Garfinkel 1967: 34).³⁴

For Alexander (1988), the post-sixties dissolution of Parsons’ systems theory “created a new and surprising shift in the progress of general theory” and had the effect of “justifying the project of general theory itself” (77) despite also significantly diminishing the knowledge capital now obtainable by general theory, thus generating a *new theoretical movement*. It did this by concentrating on a single problem that directly *mirrored* the post-sixties changes in folk/field relations situating the sociological field: namely, the problem of multi-level relations; or, how does agency relate to structure, the micro to the macro, the folk to field-specific constructs? The unsettled field produced a kind of theoretical anomie, a cacophony of points of view or “theory groups” (Mullins 1973), none of which cut through the noise alone but which together tended to arrange themselves as participants in two opposing revolutions:

On one side, there emerged radical and provocative schools of microtheorizing, which emphasized the contingency of social order and the centrality of individual negotiation. On the other side, there developed vigorous schools of macrotheorizing, which emphasized the role of coercive structures in determining collective and individual action. These movements transformed general theoretical debate and permeated empirical practice at the middle range (Alexander 1988: 77).

³³ Consider, for instance, Glaeser and Strauss’ (1967: 98–99) appeal to “everyday” or “real life experience” as being inaccessible to formal theory, “ungrounded” because it lacks an experiential link. For Garfinkel (1967: 270), the “problem of rationality for the sociological theorist” is partly resolved by “deciding on the ground of the examination of experience *rather than* by an election of theory which of the behavioral designata go together.” For Smith (1974: 11), “the sociologists’ investigation of our directly experienced world as a problem is a mode of discovery or rediscovering the society from within.” Collins (1990: 23ff) later places unique emphasis on “the connection between experience and consciousness” as inextricable from distinct standpoints.

³⁴ Radical reflexivity, from ethnomethodology’s point of view, is a proposal about the symmetry of field-specific constructs themselves, that they are not different in their task from other organized activities that give the experience of social order (Garfinkel 1967: 31–34, 96–103).

Fundamental disagreements persisted between the two loosely coherent sides, but “there is one foundational principle about which they [agreed]. Neither micro or macro theory is satisfactory. *Action and structure must now be intertwined*” (Alexander 1988: 78; emphasis added). Heroes of the NTM (e.g. Bourdieu, Giddens, Habermas, Archer, Collins, Alexander, to name just a few) would set the terms of general theory in its seemingly ageless “contemporary theory” mode (nearly fifty years old now) through position-takings that dictated not only this problem but the universe of its possible solutions.

According to later observers, however, such efforts proved to be far less revolutionary than they might have seemed at first, reaching a *détente*, or a “resounding verdict of ‘both’” (Martin 2011: 4), and an exhaustion with attempts a resolving persistent oppositions and antinomies that once appeared so important, i.e. agency/structure and micro/macro (see also Lizardo 2010: 684–85; Krause 2012). If this is theory in sociology since the sixties, then it marks a significant anticlimax. It rightfully deflates general-theoretical concerns and decreases the epistemological value of “theory” as a specialization: non sequitur in a post-sixties landscape, or at most a series of abstract commitments from which to choose. If we (of later field generations) take these old positions, in our pursuit of contemporary theory, we will likely find ourselves tilting at windmills. So we tend to look elsewhere to find our theoretical pulse, in different abstract commitments. But if we bring the post-sixties tendency toward folk knowledge together with the attention given to action, rather than treating one as part of the social history of the field, the other as part of “social theory now,” then the otherwise placid, uninteresting surface left by the NTM becomes far more interesting, disruptive even, because it becomes an integral part of the post-sixties, its *nomos* and shifting poles, that *does* achieve a cross-domain appeal and “permeate research practice at the middle range” (Alexander 1988: 77).

In the same manner that the post-sixties trajectory of the field involves a drift toward the folk, nearly every significant, field-specific object inherited from the NTM (culture, structure, agency) implies something about cognition, but this is not *merely* a theoretical statement. The drift of theorizing these things has *tended* toward cognition, just as the field has shifted away from strong meaning variance toward the folk-compatible.³⁵ This seemingly requisite cognitive argument did not go unnoticed by the NTM (see Giddens 1979: 4–5), though it remained largely in the background, a “historical

³⁵ In addition to those mentioned below, consider the following prominent examples: Sewell (1992: 8) revises Giddens’ structuration effort in the NTM by replacing “rules” with “schemas.” However, it is not clear “where Sewell gets schema” (see Stoltz 2018). What is clear is that, by any definition, schema is a *thickly* cognitive term. Alexander (1992), meanwhile, problematizes the category of “agency” by arguing for a greater focus on *actors* and invites attention to the “internal environment of actors.” Alexander develops this point by educing a *thickly* cognitive process (e.g. “representation”): “agency is inherently connected to representational and symbolic capacity ... Because actors have agency, they can exercise their representational capacities, representing their internal environments through what is called externalization” (1992: 10). Finally, Hedstrom and Swedberg (1998), in making their case for theorizing “general social mechanisms,” concentrate on three—“the self-fulfilling prophecy (Robert Merton), network diffusion (James Coleman), and threshold-based behavior (Mark Granovetter)—which they argue all exemplify “the same basic *belief-formation mechanism*” (18) This means that while these arguments are each very different, they all concern “the way in which individuals’ beliefs are being formed” (20). Similar to Sewell and Alexander, this effort makes an attribution of mind in order to resolve an NTM problematic of agency/structure or micro/macro; “belief-formation” implies a series of cognitive commitments.

transcendental” presupposed by positions taken on *other* things and not itself a focal point.

The contemporary sociological field, by contrast, does not ignore cognition, though (with some exceptions) it does not talk very specifically about it. What it seems to talk a lot about are things like “scripts,” “codes,” “frames,” “logics,” “categories” and, of course, “meanings.” All of these are field-specific constructs or sociological “objects” that demonstrate the differentiation of the field, their meaning reflecting their objectified history in the sociological field, with a very definite field-specific resonance that the folk are not privy to; but if we translate them further into what they would be like *in action*, it seems like we would eventually reach the level where Abbott’s (2001[1992]) “little stories” and Watts’ (2014) “rationalizable action” would start to appear. In other words, we would reach the *non-fielded* part of this knowledge that would include a folk-psychological taxonomy of some sort. It is also at this level that the sync with the post-Wittgenstein Anglophone philosophical field would become apparent. Should we ask a question about the folk-psychological ingredients of the explanation, the likes of Wittgenstein, Davidson, Churchland, Feyerabend, and so on would have anticipated the question and given an answer, one way or another.³⁶ In fact, interest in the post-Wittgenstein philosophy of action would likely appear strange from a point of view not *already* in a field like post-sixties sociology, as these arguments can easily seem banal, obvious, and to have *no point* to those not otherwise primed by field-specific concerns.

The contemporary rejuvenation of “action” in sociology is therefore different from prior (Weberian, pre-Parsonian and Parsonian) iterations, as it becomes a proxy for folk-compatibility, involved more generally in a post-sixties field where knowledge carries a high premium if it claims to know as the folk know rather than maintaining a hierarchy to the folk in the way a physicist might by recognizing only fielded ways of knowing. For Watts (2014; 2011), this draws an all-too-ready invocation of “rationalizable action” into sociological research that makes it dangerously similar to folk knowledge. This is one demonstration of action as a site of contention alongside boundary-level changes to the folk/field relations that mark the sociological field. In a different sense, however, the interfield sync between post-sixties American sociology and post-Wittgenstein Anglophone philosophy provides an opportunity to reflexively identify this interfield relation, and possibly choose a different path.

Positions in the field

If folk psychology allows for a generalized epistemological content in the American sociological field, this not usually remarked upon, because it does not have to be. The

³⁶ It bears mentioning that this relation between the two fields finds a precedent a generation earlier through mediations attempted by prominent figures like Anthony Giddens and Clifford Geertz. Giddens (1979: 53), for instance, makes action philosophical in order to (re)make it as a post-sixties sociological object; Geertz (1973: 12) does something similar with cultural meaning. Significantly, their mediations prove to be “vanishing” rather than persistent because while the transfer can be known, its effect (as philosophical) *mostly* remains unknown. Giddens appropriates Davidson’s 1963 position-taking even while he critiques it. There is a definite authorization from Davidson to Giddens, allowing the latter to theorize a “knowledgeable actor.” Geertz (2001: xvi), meanwhile, remains consistent in his direct appropriations from the late Wittgenstein. The significance of the transfer he performs effectively vanishes as the philosophical lineage becomes secondary and its transferred effect (e.g. “webs of significance”) becomes primary.

response to a question like “are there actually scripts?” resembles the response to a question like “are there actually ideas?” among the folk: “interesting” though in a condescending kind of way; unimportant for the task at hand. Folk psychology has become a topic of more focused attention, however, among those in the field making “general-theoretical” claims, which among other things means they bring folk psychology out of the undiscussed, make it an obstacle and take a position. As they take positions, theorists also reveal the larger inter-field system in which sociology itself is situated, not only relative to a philosophical field, but in relation to different sorts of capital, an interface either made by sociologists, made by sociologists in conjunction with extra-field actors, or made by sociologists *attempting* to develop such a relationship. In all of these cases, the relations make “action” an explicit topic of interest. At the boundaries of the field, it becomes important for identifying sociological knowledge *as* a distinct form of capital.

However, the appearance a folk-derived epistemological obstacle creates specific ambiguities in the case of the American sociological field: if deferential position-taking in the Anglophone philosophical field involves the quasi-aristocratic assertion of an irreducible nomic language, because it makes sense *to the philosopher* who need not account for her own conditions of possibility, what does this transfer mean for position-takings in the sociological field that are similarly folk-deferential? Meanwhile, a figure like Sellars maintains a reflexive skepticism about his own legitimate handle on the “manifest image” by constantly checking it against a scientific image. Creative peripherals in the philosophical field who seemed drawn to a similar skepticism of philosophical certainties, and philosophical authorizations of *any* sort, retained a degree of radical marginality (Feyerabend) or outright ostracization (Rorty). What does a homologous *critique* position mean or imply in a different field, like sociology?

In the contemporary American sociological field, a first position involves a *defense* of folk psychology. Both cultural and analytical sociology offer examples, which in both cases lends itself to a kind of cult of technique, prizing a skillful application of a formulas and rules. Cultural sociology, for instance, is a research program dedicated to the “case-centered insight” that explains action by revealing the effect of “historically-specific meaning structures” (Reed 2012: 32). Empirical phenomena are puzzling within the dimensions of a basic commitment to things like meaning structures and “reasons are causes” as stated principles. Cultural sociology can therefore advocate for a pluralist use of theory and method because challenging these “definatory propositions” is not at stake (Reed 2012: 36f; Norton 2019: 10–11).

A parallel approach is found in analytical sociology and its emphasis on mechanistic explanation that references folk-psychological “cogs and wheels.” Social phenomena are rendered as the colligated outcome of belief-desire driven actions from this perspective (see Elster 1989). Analytic-focused explanations accept (belief/desire) reasons are causes as indisputable (Hedstrom 2005). Empirical phenomena are rendered both “puzzling” and familiar according to the same set of unambiguous criteria (Hedstrom and Bearman 2011: 15). As many acknowledge in analytic sociology, this follows what Coleman (1986: 1322) famously diagrammed (Coleman’s boat) as micro-macro relations arising from specific “individual orientations” (like religious belief) and how they collectively spawn macrosocial orders (capitalist economic system). For many analytic sociologists, the belief-desire-opportunity (DBO) model, and its capacity to define social mechanisms that give rise to macrosocial orders, serves as the theory of action,

and it is one for which Davidson's philosophical psychology can be drawn upon in support of its defensive position-taking, just as it is for cultural sociology.

However, in Watts' (2014: 342) assessment, this sort of framework irresistibly seduces sociologists because its "ability to tell causal stories is so powerful that it seems superfluous to subject them to tests of scientific validity." This hinders the development of "scientifically rigorous [but] empathetically unsatisfying explanations" in the discipline. He represents a *replacement* position in relation to the defensive and engages in a different socioanalytic exchange with the folk. There is precedent for this argument in network analysis, Watts' own specialty. There is a lineage, for example, in White's (1992: 64) argument that talk of persons and internal motivations is not relevant because "interactions, ties in socio-cultural context, are coming to supplant persons as building blocks-and a person may come to be seen as a knotted vortex among social networks." The difference, however, is Watts' emphasis on prediction as a new knowledge capital in the field, one that should tie "scientific validity [to] forcing sociologists to make predictions" (2014: 313).

Because folk psychology is an epistemological obstacle in sociology, this means that critiques of folk psychology produce meaning variance when they convey a discontinuity between beliefs and desires in a folk-psychological rendering versus a field-specific form. The question is whether that meaning variance should be weak or strong. From a *critique* position, beliefs and desires become objects (topics rather than resources) and are demonstrated to have a different referent than internal mental states. This means they are *not* contingent on a folk-psychological cognitive attribution, like mental representation, though this also means that any alternative will appear to lack clarity and can seem unfinished. This would include attributing beliefs or desires to relational dynamics instead, that apply in a field, and turn these manifest traits into distinct positional sensitivities (Martin 2013; Green 2013; Strand 2015).

A replacement argument, by contrast, argues for strong meaning variance because explanations become counterintuitive and lack "understandability" as a result of their strict departure from folk psychology. If action-explanation itself is problematic, this is because it relies on arbitrary folk tropes like belief, desire, intention, and other mental states that give "empathetic explanation" more epistemological value than "scientific validity" *qua* prediction. The problem, as Watts (2014: 315) argues, is that sociologists "utilize common sense concepts [like folk psychology]" in everyday life. Folk knowledge "appears valid on the basis of everyday personal experience [and] seems self-evident." The result is that sociologists fail to make an epistemological break because "assumptions implicit in the folk theory get incorporated into sociological theories without even being explicitly articulated ... even when the theories themselves become contested, the common sense assumptions on which they rest are left unchallenged" (Watts 2014: 315).

If true, then folk psychology's presence in sociology prevents a rupture with a "pre-existing organization of thought" (Bachelard 2002[1938]). Nevertheless, there are clear differences on the role that such a "pre-existing organization" should play, and these differences are entangled with larger sets of claims about sociology as a knowledge field. Reed (2012: 38; 2011) argues that "desires and beliefs" as motivating "internal mental states" are "absolutely necessary" for sociologists if they want to effectively "classify and analyze the subjective origins of social action." For Hedstrom, the defense is similar: "desires and beliefs" create a "motivational force" that sociologists must

identify in order to “understand and, therefore, explain ... action” (2005: 39). Martin (2015: 218), meanwhile, argues that there are “good empirical reasons to jettison” folk psychology (or “good old-fashioned action theory”) not least of which is that it allows sociologists to use “tautologies” to explain action, like “bringing into play more and more norms” (240–41).

Mapping the sociological field

If we were to map the field based on these four responses to the folk action obstacle, it becomes clear that they do in fact occupy different positions in the current moment of the American sociological field. This also gives some sense for different types of capital conversion (cultural, economic, bureaucratic, scientific) which all appear to be refracted within the field as a heightened concern with action (Fig. 1).

Watts (at the time) writes from a position at Microsoft and details his programmatic statement about prediction with examples drawn largely from proprietary research by tech giants. As Watts argues, “the combination of low-cost development, large number of users, and rapid feedback cycles allow for many variants of virtually anything to be tested and selected on the basis of performance” (2011: 189). As Watts continues, “at a higher level the Web as a whole can ... be viewed as a form of crowdsourcing ... In principle ... one might be able to aggregate all this [internet] activity to form a real-time picture of the world as viewed through the interests, concerns and intentions of the global population of Internet users” (193).

Watts’ arguments appear corroborated by ensuing developments in the brief time since he published these words. By this point, it seems a gross understatement to say that the internet has “created opportunities” for the commercial exploitation of social knowledge (Couldry and Mejias 2019). The business models of Amazon, Google, Facebook and Netflix trade access to personal data for the ability to perform classifications using algorithms that appear to surpass the sociological imagination in their capacity to designate who people are and *predict* what they want. Watts (2011: 196) calls this strategy “measure and react.” His emphasis on scientific validity *qua* prediction issues from a position in the field that closely aligns with brands of social knowledge that have this enormous potential for commercial exploitability in no small part because of their ability to surpass limited cognitive capacities using algorithmically-derived matches that can only be “understood” after the fact.³⁷

As Watts suggests, the advent of big data and propriety social knowledge is not benign or irrelevant for the sociological field, nor is its impact limited to new methods and new data sources. It poses fundamental questions about sociological knowledge itself, its potentially profitable and scientifically advantageous *replacement*, and challenges the field’s university-based *raison d’etre* with the emergence of an aspirant data science. The number of highly-paid specialists in social knowledge at tech giants and in the marketing departments of Fortune 500 companies dwarfs the number of

³⁷ Watts’ claim here also resembles Chris Anderson’s (the former editor in chief of *Wired*) argument in his provocatively titled essay “The End of Theory” (2008): “Who knows why people do what they do? The point is they do it, and we can track and measure it with unprecedented fidelity. With enough data, the numbers speak for themselves This is a world where massive amounts of data and applied mathematics replace every other tool that might be brought to bear. Out with every theory of human behavior, from linguistics to sociology. Forget taxonomy, ontology, and psychology.”

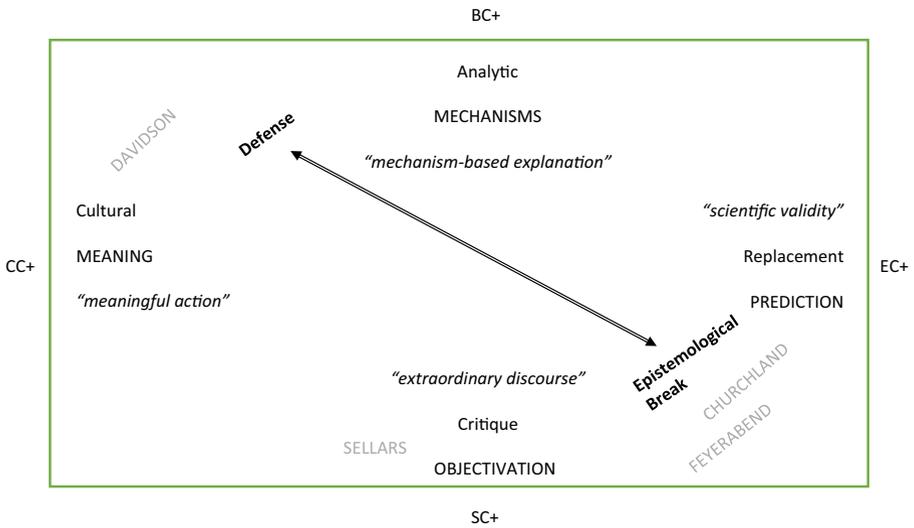


Fig. 1 Disciplinary field positions, American sociology

sociologists employed in university departments. This leads some to stop referring to the “knowledge or tech economy” to make sense of capital’s present moment and to choose a different label instead: the “social sciences economy” (Epstein 2017: 2). A social knowledge capital, based on *classification*, acquires unprecedented value and transfer at the present moment for the purposes of general capital accumulation, which may seem auspicious for the sociological field. Yet almost none of those who profit from “social science” use or even need a sociological knowledge base to do so.

For Watts, sociologists should respond to this with an almost *mortal* concern, consider very closely how their fielded knowledge differs from (and improves upon) folk knowledge, and strategize about how to position sociology in an environment being reshaped by the presence of large-scale administrative and private sector data, that requires unfamiliar computational procedures, and where predictive social knowledge acquires an increasingly high economic capital conversion rate. Entering this fray throws a post-sixties commitment to action as a proxy for folk-compatibility into question, and Watts takes Davidson himself to task, arguing that Davidson did not arrive at his “conclusion [that reasons are causes] from any more fundamental logic or empirical evidence so much as he asserted it to be self-evident” (2014: 319). A sociological knowledge claim that relies on this sort of self-evidence gives “understandability” a misleading epistemological value, because it simply confirms the limits of folk knowledge, now given authorization by a philosopher who simply confirms his own intuition, rather than confirming anything new. A knowledge field that rests on such an edifice is in trouble.

If this replacement argument reflects a position with (potentially) high economic capital, it is willing to sacrifice post-sixties commitments to folk-compatibility as being less valuable for sociological knowledge and reflecting uncertain (and dated) commitments. This should no longer be *expected* of sociologists, because it seems that, contrary to the sixties, social knowledge could now be good for something different. A defense argument is the reverse, as the expression of a high cultural capital position

that corresponds closely with the post-sixties development of cultural sociology in the field, featuring extensive historical case studies, broad reading, empathetic explanation, theory omnivorousness, and knowledge of popular and rarefied cultural forms. The attraction of folk psychology from this position is as a “universal account of action,” a passport with which to travel, to cultivate a seeming folk-deference to actors whose motivation is contingent on meaning, and to make the most valuable form of knowledge to know as the folk know, in this case, *as interpreters*. This implies nothing problematic about cognition that cannot be left to folk psychology, just as it tends to be by cultural-capital centric fields like history and literature. When necessary, a reference to the post-Wittgenstein field can help bolster points that could seem problematic: for example, Davidson’s “principle of charity” or Wittgenstein’s argument against “private language.” This enables the broad focus of cultural sociology and its translations of cultural capital into field-specific capital by providing the broadest range of possibility with which to identify structures of meaning, give an interpretation and *find* an interpretation, and demonstrate a virtuoso application of basic rules.

As Watts’ “non-empathetic explanation” mirrors a situation in which non-fielded social knowledge of a certain type achieves high economic capital when it can predict, Reed’s “meaningful action” assumes a kind of Diltheyean opposition that, at this moment, is less reflective of the classic *Geisteswissenschaften* and *Naturwissenschaften* division, but resembles a cultural versus economic capital division as increasingly relevant poles in the contemporary sociological field. This reveals the porousness of the field and how otherwise very different factors can *similarly* bring folk action to attention as an obstacle that prompts a position-taking. Hedstrom’s defense argument, meanwhile, and its attention to “mechanism-based explanation,” arises from a different field-specific appeal for folk psychology.³⁸

In one sense, analytical sociology shares a certain technical capability and sensibility with Carnap and Russell’s analytic worldview, seeking clarity against semantic confusion. In line with a post-sixties changes, it depends above all on analytic precision at the level of the actor and “explicitly recognizes that social action requires not only a verb, ‘to act,’ but also a noun as subject, ‘the actor’” thus appealing to the “most basic” theory of action, namely “purposive action” (Coleman 1986: 1320). Coleman himself situates this framework in the post-sixties American sociological field that gave rise to “a new kind of research ... *social policy research* [which] has come directly into the functioning of society—no longer standing outside it but instead modifying the articulation between corporate actors and persons” (1986: 1323; my emphasis). This presents a post-sixties version of policy-relevant sociology tempered now by an insistence on the “micro level” of action, which in turn makes it more deferential toward bureaucratic capital.³⁹ The outside-the-field correspondence of this position reflects this influence from non-commercial sources, including government and foundations, and the epistemological value placed on knowledge that is “accessible” to

³⁸ This should not neglect a still different defense argument that transfers Davidson’s position-taking on folk action into a defense of *personalism*, which translates into the sociological field, with its post-sixties objectified history, as an effort to avoid “de-agentified” social theory (Porpora 2015: 129ff).

³⁹ Coleman argues that “sociology, in its research and its development of theory, exists on the sufferance of society. If society’s support for this research and theory development is to continue, there must be some reciprocation. Some benefits must flow from the discipline to the rest of society” (1996: 349). To ensure this, he advocates a theory of action on its “simplest foundation [as] purposive action” (348).

those outside the field, enabling them to understand the work, seeking “deliverables” and “clear” conclusions with obvious policy translation.⁴⁰

Critique, meanwhile, is also formed through a socioanalytic exchange with the folk in which a sociologist cannot claim analytic privilege. But from this position this does not mean that the predominant epistemic stance in the post-sixties field does not *still* remain a social relation of “epistemic authority” (Martin 2011: 5).⁴¹ Action, then, becomes a venue in which to demonstrate the non-arbitrariness of folk psychology but *also* to show its limits. Position-taking from the critique position, then, does not look like “mechanism-based explanation,” “meaningful action,” or “non-empathetic explanation.” Rather, the equivalent from this position is “*extraordinary discourse*,” or fielded terminology that breaks the connection between folk psychology and familiar experience by finding the limits of the former and developing a new vocabulary to fit the latter, creating a “change of state” (Bourdieu 1977: 170). Folk psychology and its conveyance of mental events carries a different texture than extraordinary phrases like “bundles of relations,” “sense of necessity,” or “practical belief.” These words carry a kind of charisma bordering on notoriety because they are extraordinary, seeming unfamiliar but referring to what is *most* familiar as though it had been “lived namelessly” before. They *also* make a cognitive argument, but through a formative rather than conservative effort: not taking folk psychology at face-value but promoting meaning variance with it and demonstrating the validity of entirely new terms that capture experiences previously familiarized without hesitation by folk psychology (Turner 2018: 217).

For this task, cognitive science serves as a different source of exchangeable capital not obtainable before. As a laboratory, neuroscience provides a departure from the urgency that otherwise applies to attributions of mind made in everyday life and the pragmatics of sense-making (Latour 1987; Knorr-Cetina 1992; Bachelard 2002[1938]: 22). Cognitive science more generally consists of an interfield venue for making attributions of mind as itself a form of *fielded* knowledge rather than of social reasoning, with goals set by scientific practice and conditions of knowledge-production. Brought into a laboratory setting, folk psychology becomes a matter of rectified error as subject to “experimental proofs” that provide strong *prima facie* evidence that folk psychology is not about cognition but about something else (Hutto 2012). For “experimental proofs” to have their effect requires that we “deform [*sic*] our initial concepts, examine these concepts’ conditions of application and above all incorporate *a concept’s conditions of application into the very meaning of the concept*” (Bachelard

⁴⁰ This is not dissimilar from the dispositional “hybridity” that shapes knowledge in the “peculiar social universe” of think tanks, where the prevailing idiom remains highly folk-compatible, associates being “articulate” with doing “important, serious” work, and participates in only a narrow universe of argument and a far larger universe of what *must* remain undiscussed (see Medvetz 2012). Journalistic media appears similarly reliant on folk psychology and intentional action, and tends to “evoke the rhetoric of conspiracy” when it becomes revelatory in even a *minor* sense (see Boltanski 2014: 260–61).

⁴¹ There is a parallel between critique in this form, that finds “epistemic authority,” and the trajectory of post-Bourdieuian French sociology toward “postcritique” (see Boltanski 2008). Critique in the American sociological field would not disagree with what this means. It remains critical because it seeks to find limits: concepts *always* connected to conditions of application. Likewise, for Boltanski, the conditions of application for concepts (like actors) are “orders of worth,” for Latour (like actants), they are “trials of strength.” This does not negate critique. It simply asks sociologists find conditions of application for their concepts, lest they risk turning into the most “gullible sort of critique” (Latour 2004: 229–30) or run the risk of being received as not epistemologically different than “conspiracy theory” (Boltanski 2014: 238–9). Here the conditions of application are the “cognitive components” of action. I thank the reviewer for drawing my attention to this parallel.

2002[1938]: 243; emphasis original). In brief, cognition is *not* folk psychology’s “conditions of application.” Cognitive science produces a distinct vocabulary *relative* to folk psychology, then, because it makes attributions of mind that do not have to mediate between (presumed) familiars; rather, it needs a vocabulary that can mediate between a scientifically-established phenomenon and the familiar and constitutive domain of ordinary language, where experience is not “validated by neuroscience” (Varela et al. 1991: 73). This effort at *translation* generates an extraordinary discourse.⁴²

This has a twofold significance for the sociological field, as the critique position holds this scientific capital side by side with a post-sixties sociological *nomos* of folk-compatibility and turns folk-psychological tropes (beliefs, desires, intentions) into topics, seeking to objectify them. First, the maintenance of folk-psychological assumptions about mental representations as actively produced, prior to action, and semantically accessible becomes a preference: *one way* of objectifying cognition, one “module of inference” for making attributions of mind, and one that draws inferences “just about reasons” and diminishes most of mental existence as pure mystery and speculation (Mercier and Sperber 2017: 328).⁴³ Second, to become part of cognitive science changes the interfield transfer and the content it gives to sociological claims: still cognitive *but not philosophical*.⁴⁴ This invites new theorizing that does not privilege the neuroscientific as the “most fundamental level of explanation” but uses this now fielded knowledge to make a conceptual revision—“devaluing what seems self-evident or intuitively obvious” (Tiles 1984: 154)—in this case, scrutinizing arguments that carry spontaneous appeal by asking *why* they carry spontaneous appeal; casting doubt on particular claims because of what they imply about cognition, yet remaining certain that a claim involving cognition *must* be made.

Conclusion

In 1967, a few years after he published his paradigmatic “Actions, Reasons and Causes,” Donald Davidson published a lesser-known essay, but one of equally significant proportions. In “The Logical Form of Action Sentences,” Davidson argued that in

⁴² There is a convergence, for example, between social theory and cognitive science around a vocabulary of “practice” or “enactivism” (Engel et al. 2015).

⁴³ For Turner (2018:214), sociological constructs that rely on folk-psychological schemes should be prefaced by an “as if” qualification as a reminder that they are *one form* of construction with definite limits, but not the only form. To use his example: claims about “assumptions” affecting an outcome can only claim that it were “as if” assumptions affected the outcome. Assumptions only reference what someone might need to know in order to understand the outcome and are relative to a social relation of explanation (*ex post facto*). They are a construction, in other words, the conditions of application of which are not the action itself, or its cognitive components, but this social relation.

⁴⁴ This would also seem to distinguish a critique position from a pragmatist theory of action (McDonnell et al. 2017; Gross 2009; Whitford 2002; Joas 1996), which appeals to a philosophical precedent but of a very different sort. It matters, in this regard, that the history of American pragmatism draws from empirical arguments about mind, and so arguably has less vulnerability to a “philosophers’ blindness ... disguised as timeless, universal self-evidence” (Bourdieu 2000: 29). The terms of action theory still come from the reading of texts designated “philosophy” and a transfer of their capital, and includes concepts with high folk-compatibility arguably ripe for epistemological break (e.g. “problem-solving”). This would make a pragmatist revision of action theory different from attempts at interfield translation from cognitive science, despite many similarities (e.g. “strong practice theory”; Lizardo and Strand 2010).

the usual form of action sentences like “It was intentional of x that p ” the agent be subject to a kind of double presence: both in the intention and in the action itself. This made it possible to “describe the action in the light of certain attitudes and beliefs and particular persons” and to ensure that what transpires really is *action* (Davidson 1967: 95). In 1970, Paul Churchland, in his first published article (“The Logical Character of Action-Explanations”), critiqued Davidson’s position, arguing that “ordinary action-explanations” like this presuppose some “fairly sophisticated ... principles or ‘laws’” (1970: 215) that are much more substantive than Davidson appreciates.

For Churchland, the lawlike nature of ordinary action-explanations only really becomes clear in objections that can be legitimately raised against them and, under the influence of Sellars, he refers to a fantastic example from the distant past to demonstrate this, a past in which adding machines grow naturally in the wild and, with the push of a button, provide numerical calculations. A roving philosopher at the time comes up with an “action-explanation” for these machines that explains them in an equivalent manner as Davidson explains ordinary action: “reasonable in the light of” the states that these adding machines are in at the time they spew out their numbers, equivalent to states like attitudes and beliefs for human action. But for Churchland (1970: 236) this would ignore a critical “extra-nomic logical relation.” In the case of these wonderful adding machines, it would be numbers and the arithmetical relations between them, of which the philosopher in the distant past knows nothing; in the case of humans, it would be “*psychological states and episodes*” and the relations between these and the “full-blooded action” that humans do, which Davidson *could* know but relies on folk psychology instead. Churchland (1981) would go on to argue that because of what they imply about “psychological states and episodes”—implications about which they *must* make—ordinary action-explanations constitute a remarkably regressive research program, one that has generated very little knowledge in 25 centuries.

If this discussion seems terribly obscure, the wager of this article is that, at the very least, it *might* not be, should a member of the current American sociological field now be reading this. In fact, a critique not dissimilar from Churchland’s is found in Stephen Vaisey’s (2009) article that finds limits to drawing a clear link between motivation for action and “cultural meanings as propositional, articulated and logically complicated” (1681). The “power of this argument,” Vaisey claims, “is in its simplicity,” which not unfaithfully we could translate as its apparent *folk-compatibility*. But such a link, like Davidson’s ordinary action-explanations, makes a significant assumption: namely, that motivation for action has “to be a deliberative, logical affair” (1681). On the face of it, “this premise seems uncontroversial” because it is folk-compatible; but it quickly becomes controversial in a similar manner that Churchland finds controversy in ordinary action-explanations, when we understand that the assumptions this argument makes about “psychological states and episodes” are, at best, not to be relied upon, at worst, demonstrably false and possibly mystical. This could lead sociologists toward a kind of replacement argument, which Vaisey claims Ann Swidler’s (1986) “toolkit theory” of culture in action constitutes a (weak) version of. Such an argument would accept the presumption that motivation must be “a deliberative, logical affair” and, since it is *not*, argue that motivation for action is not something that sociologists should be interested in, though the folk *are* interested in it. Vaisey does not himself go in that direction, despite drawing out the folk-psychological assumptions and breaking with them. Unlike the Churchland analogue, he cites the significant decline in the

epistemological value of a sociological claim about action that *does not* seem to be about its motivation, a value that remains determined by its folk-compatibility. So Vaisey takes a different position instead, using extraordinary discourse to explain motivation *in lieu* of folk psychology: including things like habitus and practical consciousness, translating a “practical, nondiscursive side of cognition” from the fielded knowledge known as cognitive science (Vaisey 2009: 1707).

The premise of this article is that these alignments between Anglophone (analytic) philosophy and American sociology are strange but also explicable. They arise because what Davidson, Churchland, Vaisey and Swidler all confront in these instances is folk action, or action *as the folk know it*, from a point of view looking outside of their respective knowledge fields. They are all interested, at least at first, in knowing action as the folk know it. The objectified history of their respective fields makes this critical to the kind of knowledge capital they can obtain with their knowledge claims. But soon this presents them with a large question: should the folk-psychological way in which the folk seem to know action be trusted? And if it is not to be trusted, what kind of position can a participant in these knowledge fields take given the *nomos* of both fields at these historical junctures, post-Wittgenstein and post-sixties, formed (respectively) against the construction of ideal languages in opposition to “ordinary language” and against a science ideal apparently oblivious to the coexistence of its objects in spaces beyond the field?

The purpose of providing the histories of these fields in such detail is to capture the difficulty of the question and why, for philosophy, it *carried* big stakes and why, for sociology, it *currently* does.⁴⁵ If Watts (2014) is right, such debates revolve around the very distinction of sociology’s knowledge capital in what he appreciates to be newly competitive circumstances. They would be similar to the stakes presented to Anglophone philosophy after Wittgenstein. The post-sixties trajectory of the sociological field shows the dangers of strong meaning variance with the folk; but the question now is whether, like Churchland (1981) addressing Anglophone philosophy, a sociological knowledge capital tempered by this history can maintain its distinction?

Sociology *now* meets a powerful non-fielded social knowledge, of the proprietary kind, that dispenses with concepts so completely that, as Watts suggests, it creates its own space of social emergence with its *own* horizons unavailable to most of sociology as it is currently configured.⁴⁶ We could be skeptical of changes in store for sociology from data-centric social knowledge; that to Churchland facing neuroscience there is no real parallel to Watts facing data science, and here is where the interfield sync falls apart. Yet Watts takes a position sensing a radical shift that will change the post-sixties sociological field, averse to prediction, in a way that seems to mirror what Churchland (1981: 88-89) advocated in shifting from post-Wittgensteinian philosophy, averse to

⁴⁵ Consider, in this respect, the controversy surrounding a position like the one that Vaisey takes: the skepticism it seems to raise about trusted methodologies like interviews (Pugh 2013) or whether, through such explicit commitment to cognition, it advocates a pale form of naturalistic reductionism that alters the epistemological value expected of a (post-sixties) sociological claim so much so that it becomes strange and unsatisfying, far too much to ask despite any misgivings about folk psychology.

⁴⁶ “Older concepts of social explanation, based not on tracking sentiments and connections but on evaluating the wider context of people’s lives more broadly ... risk being cast into shadow by data colonialism’s new social knowledge. How will concepts such as poverty, which in the nineteenth century emerged from sifting many statistical analyses, survive in competition with proxy logic?” (Couldry and Mejias 2019: 148).

making “inroads into the aura of inconceivability that commonly surrounds the idea that we might reject folk psychology,” to *neurophilosophy*.

In attempting a participant objectivation, this article tries to understand these position-takings in objectified history and to see where a trajectory might lead. We can see where the trajectory lead with post-Wittgenstein Anglophone philosophy: there were “untapped possibilities” beyond folk psychology for that field. Meaning variance with folk-psychological schemes *did* carry a distinct psychic value that is still being transformed into significant knowledge capital. To capture this would not, however, mean that sociologists follow Watts in his pursuit, nor that they maintain what would now be a *de jure* defense position. For Churchland, a similar quandary meant a replacement claim. Post-Wittgenstein Anglophone philosophy itself remained committed to language that should be replaced; so, under the rubric of “scientific realism,” he trades folk psychology for a terminology of neuroscience in order to alter ordinary action-explanations. The problem is that Churchland is still trying to know the folk as he does this (claiming “perceptual plasticity” [Churchland 1979]), which means he runs afoul of a basic Bachelardian point: what separates him from the folk is a *field*, a cognitive differentiation, two different knowledges.⁴⁷ There can be no simple translation.

Here, I argue, sociologists can take a position that carries forward the “principled displacement of folk psychology ... as one of the most intriguing theoretical displacements we can currently imagine” without making Churchland’s mistake. To make its knowledge capital more distinct in competitive circumstances, I propose the following position-taking: the sociological field should keep (*pace* replacement) its commitment to the folk and seek to know as the folk know from across a fielded knowledge boundary, but *without* folk psychology (*pace* defense).⁴⁸ The caveat is that appeals to folk psychology are appeals to cognition (Bruner 1990; Hutto 2012). But since folk psychology is not cognition, the folk will not be lost by making *different* cognitive claims; they will simply appear through extraordinary discourse.⁴⁹ This should make folk mental vocabularies subject to rectified error, self-scrutinizing the use of this language and wary of its residue of inherited meaning. This would be vigilant against the tendency to conflate knowledge with familiarity, “charitable understanding” with conditions of application that *seem* boundless. This would find limits and invite extraordinary discourse as a legitimate claim on sociological knowledge. It would not bracket validity as far as concepts go, applying it only to emergent patterns, but it would demand new conditions of application: *beyond* the bounded universe of folk psychology. This would not replace folk vocabularies with neuroscientific terminology;

⁴⁷ Churchland would ultimately take the edge off this early “across the board elimination” of vocabulary that refers to familiar mental states and recommend a strategy that echoes Bachelard: “revisionary materialism” about this vocabulary.

⁴⁸ “I” being of rural working class, United States origin, white, male, first-generation college graduate, for whom my current occupation signals a trajectory of upward social mobility, who was a philosophy undergraduate in a post-Wittgenstein Anglophone-centric department, who traversed through a sociology department not marginal but not top-tier for graduate school, who was trained in cultural sociology in both established and non-established forms, and who, dispositionally, tends toward contrarians and contrarianism regardless of the objectified history (in people or things) encountered.

⁴⁹ For example, instead of knowing as the folk do by referencing (tacitly or explicitly) *beliefs about*, it is possible to know as the folk do *practically* through extraordinary discourse like “belief is habit” (Strand and Lizardo 2015). Belief-based experience can be like “grabbing and missing” (Strand and Lizardo 2017).

it would seek to name hitherto nameless experiences that remain obscure to familiar terminology. It would objectify them and make them public, “capable of being spoken about publicly,” to legitimate, recognize and officialize them. It would be critical of institutional supports (legal, moral, political, economic) for which folk psychology consists of “authorized language” (see Martin 2011: 344ff).⁵⁰

To appreciate what such a position-taking *could* mean, we can take a single example, previewed by the past effort of philosophers to engage with this line of questioning, but ultimately failing. As Bernstein writes, in drawing from the analytic worldview to interrogate ordinary language, a defensive position in the Anglophone philosophy of action claims a “logical right [as] sufficient to draw metaphysical conclusions about what man really is” (1971: 260). But this ultimately cannot omit out of hand the fact that, “no matter how basic, useful, and practical” folk action schemas might be, they are not impervious to “change and alteration ... influenced by further empirical research” (291). As Bernstein concludes, however, “any ‘new’ theory or conceptual scheme that is offered as a serious candidate for replacement must be able to give a satisfactory answer to questions like, ‘Well what was I talking about when I said that I did x for the following reasons?’” (297).

For Bernstein, these questions appear in the Anglophone philosophy of action, and they fueled its meteoric and improbable rise. Any field with a commitment to folk psychology, that makes it into a folk action obstacle, and provokes this field-level confrontation will likely fall into a similar dialectic spiral.⁵¹ In philosophy, these questions have never been resolved, arguably because the critique position was quickly overshadowed by a strong either/or between replacement and defense. The potential is that these questions could be resolved in a sociological field that brackets folk psychology and develops extraordinary discourse, that recognizes attributions of mind as fielded knowledge, but seeks, as a cognitive science, to translate these into explanations that show where folk action applies and where it does not.

Compliance with ethical standards

Conflict of interest The author has received no grants or any payment whatsoever. The author declares he has no conflict of interest, but a real taste for trouble.

⁵⁰ The clearest precedent for undermining “the commonsense confidence in one’s ability to know one’s mind” is psychoanalysis and its objectivation of mind as the unconscious operating according to a primary process (of some kind). As Mercier and Sperber put it, “Freud’s challenge to the idea that we know our reasons has been, if anything, expanded” (2017: 114). Yet the wall between psychoanalysis and cognitive science remains large, perhaps unbreachable. Between the psychoanalytic unconscious and the cognitive unconscious lies a difference between “the laboratory” and “the couch” as the controlled condition that produces knowledge about one or the other: an epistemological difference that is a *fundamental* social difference (see Knorr-Cetina 1992; Krause and Guggenheim 2013). If critique is to maintain an egalitarian relation to the folk, unlike psychoanalysis, the challenge is to *not* make “the truth of X [lie] in the making of the person into someone who holds X” (Martin 2011: 84–85).

⁵¹ Legal fields, for example, confront a similar line of questioning which, while on the margins to this point, could at least in principle prove destabilizing should a folk action obstacle also become prominent for position-taking in that field: “The law will be fundamentally challenged if neuroscience or any other science can conclusively demonstrate that the law’s psychology is wrong and that we are not the type of creatures for whom mental states are causally effective” (Morse 2015: 262).

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