What's Social about Social Epistemology?[1]

What do we mean when we talk about the social dimension of X? Both epistemology proper and philosophy of science have been loci of attention to the social dimensions of knowledge. In both one can find different understandings of what "social" means and different contrasts the word is used to signal. My contention in this paper is that most conceptions of sociality in this work are extremely thin. I will contrast the thinly social with more deeply social analyses of several phenomena addressed in the epistemology and philosophy of science literature, arguing that attention to scientific practice demands a deeper and more robust conception of the social than philosophers have yet to fully articulate.

"Social" has a variety of meanings in the majority of the mainstream social epistemology literature. The social dimension of X is often represented as the experiences of individuals with respect to X when among other individuals. In epistemology this has become the question: how do the individual cognitive agent's epistemological challenges and resources change when the agent's environment is expanded to include other individuals?[2] Other individuals are communicators of information as well as communicators of dissent. Thus, questions of the appropriate response to disagreement and to testimony have become salient issues in social epistemology. A second meaning of "social" has to do with groups of individuals. Here a major question, inherited from philosophy of social science, is whether groups can be said to possess properties or states that are not simply aggregations of the properties or states of their individual

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[2] This set of questions is nicely captured in the title to Alvin Goldman's book, *Knowledge in a Social World* (Oxford: Oxford University Press, 1999).

members. In epistemology, this translates into questions about the nature of group justification

and group belief, as well as questions about what can be said about individual cognitive agents,

given their membership in some group.[3] A third meaning of social is "shared." A belief or

attitude or category may be shared among the members of a group, in the sense that all members

hold that belief or attitude or are assigned to that category.[4] This is sometimes the sense intended

in analyses by social science scholars of science. In many cases, what they are interested in

understanding is *how* members of a group or community came to have the same belief about

some matter, as well as how members came not to have the same belief. Finally, there is yet a

fourth meaning of "social," also primarily found in science studies, including philosophy of

science. Here the concern is about the possible infiltration of scientific inquiry by non-cognitive

factors, as when commercial or political interests affect how data are collected and interpreted or

enter into theory or model choice.

There is, however, another meaning of "social" available to epistemology as well as to

other areas of philosophy. Individuals don't just respond to their environments or to their

groups, they interact with each other. By interaction, I mean the mutual affecting of two or more

agents in a way that alters each, consistent with their persistence as entities.[5] Interaction can be

verbal, as in conversation. It can be physical, as in a mutual embrace or a struggle. *Inter*action

is a stronger notion than that of *joint* action. We may both attend a concert together. This would

be a case of joint action. When we discuss the concert with each other afterwards, however, we

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[3] Margaret Gilbert, "Modeling Collective Belief," *Synthese,* 73, 1 (1987): 185-204; Jennifer Lackey, "What is Justified Group belief?" *Philosophical Review*, 125, 3 (2016): 341-396.
[4] See essays in Miranda Fricker and Michael Brady, eds., *The Epistemic Life of Groups* (Oxford: Oxford University Press, 2016).
[5] This is, admittedly, a crude characterization of interaction, a phenomenon that deserves much more analytic attention than it has hitherto received.

are interacting. In our exchange of ideas, we are each changed, at a minimum by learning what the other thought of the concert, but perhaps less minimally by modifying our opinion as a consequence of learning of the other's assessment. Interacting is also a different notion than that of sharing. We may share an identity, a nationality, a belief, without ever interacting. An interaction with respect to a shared belief or identity involves conversation or discussion about it. Whereas joint action involves doing things together and sharing involves holding things together, interaction involves exchange of some kind.

There are, thus, at least five meanings of "social" that can be intended when using the term: the sociality of individuals acting in a world populated by others; the sociality of two or more individuals acting together, that is the sociality of group or joint action, the sociality of sharing, holding something – a belief or a good—in common, the sociality of non-epistemic-value inflected beliefs, and the sociality of interaction.

Work in social epistemology that employs one or another of the first four of these senses tends to treat social questions as add-ons to the central questions of epistemology. In traditional epistemology the problems of individual knowers pose the central philosophical questions about knowledge. "S knows that p" is a focal analysand and S is assumed to be an individual. Accordingly, mainstream social epistemology pivots around two contrasts. One is between, on the one hand, the familiar challenges, such as perceptual illusions, facing individual cognitive agents who seek knowledge about their world and, on the other, dilemmas facing individual cognitive agents when they are in social situations. The other is between treating individuals as prototypical cognitive agents and treating groups as cognitive agents. In both of these areas of analysis, the problems of individual knowers are taken to pose the fundamental philosophical questions (how to respond to skeptical challenges, the reliability of perception or of memory, …)

and are the basis of answers to questions about knowledge. Social facts are merely contingent features of individuals. The task of social epistemology is to extend the principles of fundamental or basic epistemology to encompass individuals among other individuals or for groups of individuals.

In philosophy of science, questions of the social have had quite a different flavor. As just noted, one of the concerns in recent years has been how to think about the possible encroachment of social and pragmatic concerns on the more purely epistemic. So, one contrast is between views that treat scientific knowledge as socially inflected and those that attempt to give an account of justification practices in science that demonstrate its insulation from the social understood as biasing[6]. But there is another, related, contrast in philosophy of science between understanding the social as a family of departures, in some way, from ideal epistemic practices and understanding the social as constitutive of epistemic agents and practices. This constitutive understanding of the social has gained support in the philosophy of science in practice movement, which draws on analysis of contemporary research practices.[7] Here scientific knowledge is increasingly understood as the changing outcome of ongoing engagement with the world, in which new phenomena as well as new theories are generated in the course of research. In traditional epistemology and much traditional philosophy of science, by contrast, all is fixed save the doxastic condition of the cognitive agent.

Alexander Bird, making a similar observation about some systematic divergences in social epistemology, writes of Individualist Social Epistemology (ISE) as contrasted with Social

[6] The so-called science wars of the 1980s can be understood as engaged in this debate.
[7] See C. Kenneth Waters, "Presidential Address, 2016: An Epistemology of Scientific Practices," *Philosophy of Science,* 86, 5 (2019): 585-611, for an elaboration.

Social Epistemology (SSE).[8] Bird is concerned primarily with the possession of scientific

knowledge, the "we" in "we know that the earth rotates on its axis" and other such items of

general science knowledge.[9] Bird argued that scientific knowledge is a public object, not the

possession of any individual but of a (heterogeneous) society. The contrast I am instead

concerned with is between thinking of cognitive and epistemological norms as primarily about

the states of individuals and thinking of them as emergent from the interactions of multiple and

diverse cognitive agents. Sanford Goldberg's *To the Best of Our Knowledge* is another

outstanding exception to most mainstream social epistemology.[10] Goldberg's view that

epistemically proper belief depends in part on the norms of formation of that belief meeting the

legitimate epistemic expectations of the subject's community brings the social directly into

epistemic justification However, this still focuses on the individual's justifiedness and from the

perspective of this paper begs the question: how do the reasonable expectations become the

reasonable expectations of a community? And, given that norms of reliability can be articulated

independently of the subject's social context, how central are the social expectations to an

individual's justifiedness?

Thus, my concern is with the practices that are productive of knowledge, rather than with

the content and subject of knowledge. The conception of knowledge as the outcome of

individual practices facilitates thin accounts of the social as a contingent fact about some

individuals at some times, while a focus on scientific practice supports an understanding of the

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[8] Alexander Bird, "The Social Sense of 'Scientific Knowledge'," *Philosophical Perspectives, 24* (2010): 23-56. In pressing for a social subject of knowledge, Bird assumes social institutions that generate our shared knowledge. My approach supports a closer look at the functioning of such institutions.

[9] Of course, specifying the scope of "we" in such sentences is highly contentious.

[10] Sanford Goldberg, *To the Best of Our Knowledge* (Oxford: Oxford University Press, 2018)

social as a constitutive feature of knowledge. In this paper, I explore how the differences in approach between constitutive and contingent play out in the treatment of some topics common to both. In particular I will argue that attention to epistemologically significant practices in the sciences favors a strong conception of the social as interaction.

I   Science and Sociality

The 1960s Kuhn-Feyerabend challenge to conventional philosophy of science[11] and by extension its correlates in traditional epistemology inspired a firestorm of defenses on the part of most philosophers. Their challenge, however, opened the way to three other developments in science studies in the 1980s and 1990s – 1) an interest on the part of feminist (and other oppositional) scientists and philosophers in understanding the place of (non-cognitive, personal or social) values in science, 2) empirical studies by sociologists and social historians of scientific laboratories and work groups, focusing on the social and institutional organization of inquiry, and 3) philosophical interest in characterizing the practices (rather than the content) of scientific knowledge. Some philosophers were moved to think about ways to incorporate these new ideas in alternative approaches to inquiry, giving rise to conceptions of scientific knowledge that emphasize both pluralism and social interaction. James Griesemer and Leigh Star's conception of boundary objects and Peter Galison's concept of trading zones emphasize interactions across different research approaches.[12] Hasok Chang's pragmatist pluralism, Miriam Solomon's social

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[11] Paul K. Feyerabend, "Explanation, Reduction, and Empiricism," in Herbert Feigl and Grover Maxwell, eds., *Scientific Explanation, Space, and Time* (Minneapolis, MN: University of Minnesota Press, 1962) pp. 28-97; Thomas Kuhn, *The Structure of Scientific Revolutions* (Chicago, IL: University of Chicago Press, 1962).
[12] Leigh Star and James Griesemer, "Institutional Ecology, 'Translations' and Boundary Objects: Amateurs and Professionals in Berkeley's Museum of Vertebrate Zoology," *Social Studies of*

empiricism, Philip Kitcher's recent pragmatist-inspired account of science in democracy as well as Helen Longino's critical contextual empiricism are efforts to integrate the empirical work of social scientists, historians, and psychologists with the analytic and normative concerns of philosophers.[13] In what follows, given her explicit interest in epistemology, Longino's work will be taken as the exemplar of social approaches in philosophy of science, except where otherwise stated.

Longino's critical contextual empiricism (CCE) places interaction at the center of its analysis. CCE holds that scientific knowledge is content a) conforming to its intended object and b) accepted in a community as a consequence of critical interaction among community members in a context satisfying certain norms.[14] Longino proposes social, critical, interaction as a solution to the problem of objectivity posed by the underdetermination of theory by data. She also argues that CCE norms express ideals to which the sciences themselves at least give lip service and that are applicable (as ideals) to scientific practice. Unlike the proposals of Kuhn and Feyerabend this approach does invoke general norms, but these norms are norms of community structure and interaction, not directly norms of method or evaluation of content. Norms of method on the CCE view are developed within and by communities of inquiry. They may vary across communities and within communities over time. Borrowing a page from pragmatist philosophers, CCE understands norms of method as guided by and evaluable in reference to communities' cognitive goals, where these are understood as more specific than

Science ,19, 3: (1989): 387-420; Peter Galison, "Trading Zone: Coordinating Action and Belief," in Mario Biagioli, ed. *The Science Studies Reader* (New York: Routledge) pp. 137-160.

[13] Hasok Chang, *Is Water H$_2$O? Evidence, Realism, and Pluralism* (Dordrecht: Springer*,* 2012); Miriam Solomon, *Social Empiricism* (Cambridge, MA: MIT Press, 2001); Philip Kitcher, *Science in a Democratic Society*. (Amherst, MA: Prometheus Books, 2015); Helen Longino *The Fate of Knowledge*. (Princeton, NJ: Princeton University Press, 2002)

[14] Longino, *op. cit.* pp. 135-140.

mere truth. Communities' cognitive goals are for kinds of truth, over specific domains at particular granularities and about particular aspects of those domains at those granularities. Given that there are multiple forms of semantic success, Longino's CCE advocates what she calls conformation over truth as a goal, seeing the latter as a subcategory of the former.

Traditional epistemology is centered on the problems of individual cognitive agents: how to distinguish, among the beliefs of such agents, which, if any, count as knowledge, (or if short of knowledge, as justifiably held). Traditional philosophy of science was focused on the typical content of scientific knowledge, asking about its structure and the conditions of its achievement. The assumption of both, I think, was that epistemology was concerned with the characterization of individual epistemic agents, while philosophy of science just applied the ideas of epistemology to the scientific context. Because philosophy of science is responsive to the real challenges faced by scientific investigators, however, it has had to grapple with the problem of underdetermination. This problem is centered in the structure of scientific content and it has resisted efforts to solve it with the tools of formal methodology. CCE abandons efforts at formal solutions and simultaneously changes traditional conceptions of cognitive agency. It proposes to solve the problem by treating scientific knowledge as a status of scientific content at least partly conferred through social interactions among cognitive agents. CCE, therefore, reverses the traditional analytic priority. Scientific knowledge is the outcome of social interactions: communities are the focal epistemic agents and individual knowledge is derivative, dependent on membership and participation in communities satisfying CCE norms. These different orientations result in contrasting treatments of the same topics. In the next two sections I will show how first disagreement and then testimony elicit such contrasting treatment. I will

then propose that a thin conception of the social pervades much work on group knowledge. All these approaches miss significant aspects of knowledge creation in the sciences.

II Disagreement in Two Registers

From the point of view of traditional, individual cognitive agent centered epistemology, disagreement among peers poses a problem. Many, if not most, writers frame the problem in the first person or in the voice of an individual cognitive agent who believes themself to know, or at least to have a (well-) justified belief that p. The framing of the problem is from the point of view of S in "S knows that p." What do I or what does S do when, believing ourselves to be justified in our belief that p (or in having a high degree of credence in p), we encounter disagreement from one whom we take to be an epistemic peer? Why should disagreement be a problem? If an epistemic peer is one with the same data and equal epistemic competence (equal perceptual and reasoning powers), then the peer's disagreement seems to be evidence that my or the original agent's reasons do not support p (or the degree of credence invested in p). But I believed myself to have good, even decisive, reasons for p. Either my belief that p or my belief that my peer is really a peer must go. If my belief that p must go, I run the risk of undermining my self-trust. If my belief that my peer is really a peer must go, I run the risk of dogmatism. The task of the epistemologist is taken to be to solve this problem for the agent by recommending a course of action among some assumed alternatives. Should I or the agent hold fast to my/their original position or should I (or they) adjust my/their belief or credence in light of the peer's disagreement?

The answers philosophers consider vary from giving little or no weight to disagreement (also dubbed the "hold fast" option)[15] to giving equal weight to self and other (the "equal weight" option)[16] to reducing one's confidence in one's own view[17] to rationally unsolvable[18]. Their specific recommendations vary according to their preferred epistemological idioms (fallibilism, Bayesian updating, ideal rationality, how evidence and reasons are characterized, etc.) and depending on the kind of content about which disagreement is considered as well as whether belief is understood as an absolute or zero-sum doxastic state or as degree of credence. Even though they take different argumentative paths, in the end, the recommendations to the individual agent from some of the major players in the discussion are remarkably similar. They abandon a search for rules to govern object-level disagreement, by definition a symmetric situation, in favor of rules to identify and adjudicate meta-level asymmetries. These asymmetries, whether manifest in evidence about evidence, explanation of disagreement in terms of bias, beliefs about reliability, or other metalevel features, can then rationalize holding fast or deferring in particular instances.

In spite of their differences regarding which asymmetries are deemed relevant and in their views about what is prima facie rational and what is on reflection rational, these philosophers all have a common focus. For them disagreement constitutes a prima facie challenge to one's belief and rationality demands some response. Disagreement disrupts the smooth flow of the individual's cognitive life – a life that consists in recognizing and assessing

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[15] Thomas Kelly, "The Epistemic Significance of Disagreement" in John Hawthorne and Tamar Gendler, eds, *Oxford Studies in Epistemology* Vol. 1 (New York: Oxford University Press, 2005): pp. 167-196

[16] Adam Elga, "Reflection and Disagreement," *Noûs* 41 (2007): 478–502.

[17] David Christensen, "Epistemology of Disagreement: The Good News," *Philosophical* Review, 116, 2 (2007):187-217

[18] Sherrilyn Roush, "Second-Guessing: A Self-Help Manual," *Episteme*, 6, 3 (2009): 251-68.

evidence and adjusting one's beliefs as one's evidence accumulates. Disagreement from epistemic peers threatens to challenge the individual's confidence in their own reasoning and perceptual capacities. The philosophers tell us how disagreement should affect the individual's accumulation of information relevant to p in ways that preserve the legitimacy of the individual's self-trust. Indeed, the point of the discussion seems to be to identify forensic resources available to the individual for rational incorporation of the experience of disagreement back in the smooth cognitive flow. The difference among the philosophers lies in their recommended strategies of incorporation. articulated in each philosopher's preferred epistemological framework. Nevertheless, in spite of differences in framework, they seem to share an assumption about evidence – that it is monotonic: if e is evidence for p, it is not evidence for q where p if and only if not-q. If evidence is not monotonic in this way, it is hard to see how disagreement could constitute the philosophical problem it is taken to be in the disagreement literature.

Attention to scientific practice, however, gives us lots of reasons to reject the monotonic conception of evidence. And, from the point of view developed in social epistemologies for philosophy of science, disagreement is not only not a problem, but for approaches such as Critical Contextual Empiricism, it is a necessary resource. Epistemological analysis of the sciences must come to grips with the distance (semantic and descriptive) between the states of affairs that serve as evidence for hypotheses and theories and those hypotheses and theories themselves. This problem of underdetermination is not a function of the distance between instance and generalization, but of the distance between descriptions of phenomena of different categories: phenotypes and genotypes, change of position and forces, chemical reactions and molecules. Understood from a purely logical or semantic point of view, this seems an intractable problem. But the scientific community has resources that go beyond the logical or semantic

resources available to individual agents. First of all, the scientific community is a collection of diverse (though maybe not diverse enough) individuals who bring different metaphysical assumptions, different epistemic values, and different social values to their assessment of problems and data. These assumptions and values play a role in determining the evidential relevance of empirical data.   They are not (for the most part) subject to empirical assessment. But their plausibility and consequences can be assessed relative to the alternatives.[19] Secondly, whatever individual members of a community believe or do not believe, disagreement grounded in those different assumptions and values is the source of the criticism that helps reveal the assumptions and enables the community to evaluate the inferences made with their assistance. The epistemological endpoint need not be a coherently monotonic belief set but an ongoing dialogical engagement in which data, assumptions, and hypotheses are in constant revision. Individual researchers may have unificationist ambitions and certain contexts may require at least a temporary consensus, but these do not translate into universal epistemic criteria. The scientific community best protects itself from domination by arbitrary or subjectively grounded preferences by an ongoing, dynamic, engagement normatively guided by the dual aims of avoidance of error and achievement of specific cognitive goals.

Disagreement in mainstream epistemology is considered a problem. In the sciences, however, it has a function: to help reveal the assumptions, values, and standards that have legitimized acceptance of a set of measurements and observations as data and taking those data

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[19] One can ask how assumptions and values buttressing one theory or hypothesis have fared when employed in other contexts, one can ask how they are or are not consistent with other theories, one can evaluate their degree of conformation to epistemic values, one can assess the appropriateness of certain epistemic values to the specific context of inquiry.

as evidentially relevant to some hypothesis.[20] Data are evidentially significant when embedded in a context of assumptions and norms that assign them relevance to some hypothesis. Data do not stand alone. To understand reasoning in such a context as an epistemologist is to seek to identify the assumptions, values, norms, and standards within which a piece of reasoning has taken place, as well as the inferential norms supporting the reasoning. The relation $<e, h>$ is always embedded in some context. This is not, or not just, a point about fallibility. It is a point about the distance between our epistemic resources and our epistemic aims. Without the friction of disagreement to initiate the critical investigation that reveals this gap, reasoning continues along paths smoothed by unexamined shared assumptions.

The more strongly social epistemology of which CCE is one example originates in consideration of this kind of problem for the sciences. One need not subscribe to the relativism of the more extreme sociological approaches. The history of science (in Europe since the 15th century) has been a history of cognitive/doxastic changes. Guided by traditional epistemological concerns, the history of science looks like a series of unsuccessful efforts, always (mistakenly) understood at the time to be the beginning of some new and final theoretical foundations. Some change has been superficial and some profound, but a constant characteristic has been the generation of data that demand some degree of theory or model revision. Such data are made possible by the development of new instruments and new questions within a theory. Individual researchers may try to incorporate such data into the accepted consensus, while others may use

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[20] This is not to say that participants may not see things differently. Participants may seek to establish one hypothesis as true and see disagreement as an obstacle. The above remarks are made from the perspective of the epistemologist who should have no stake in the details or outcome of the disagreement.

them to disagree with it. The disruptiveness of new data is central to the modification of theoretical scope and to the development of new theories.

The advance of science simultaneously changes the scope of knowledge and the horizons of ignorance. This vision of the role of anomalies is consistent with that expressed by Larry Laudan in *Progress and Its Problems.* Laudan proposed that the content of science could be distinguished into partially independent ontological, methodological, and axiological domains.[21] Contra Kuhn, who had a more holistic conception of content, Laudan argued that scientific change could be prompted by alterations in any one of these. The inchoate incommensurability threatened by holism was avoided by the persistence of shared understandings in the domains of content not changing. Because of piecemeal connections among elements in the different domains a change in one eventually effects a change in the others and so science is propelled, not so much forward, as away from settled understandings, towards new ones. Whether one accepts Laudan's picture in detail, it does point to an important feature of scientific knowledge: its complexity. In particular, data are not univalent, but acquire different evidential relevance(s) in different contexts. The famous Michelson-Morley interferometer experiment manifested interference patterns that, in the context of pre-relativity physics, supported a claim about the contraction in direction of travel of physical objects traveling at very high speeds through the ether. In the context of relativity theory, the patterns are just what one would expect if light travels at constant velocity through etherless space.[22] The deliverances of microscopes are notoriously multivalent. Mitochondria are assigned one structure at one level of resolution and a

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[21] Larry Laudan, *Progress and Its Problems: Towards a Theory of Scientific Growth*. (London: Routledge and Kegan Paul, 1977)
[22] Gerald Holton. *Thematic Origins of Scientific Thought* (Cambridge, MA: Harvard University Press, 1973)

different one at a different level. The choice of level depends on what use is to be made of the imaged structure.[23] Measurement of lesions on tissue slides depends on criteria for resolving borderline cases and what hypotheses are supported by some set of observations depends on researchers' tradeoffs between minimizing false positives and minimizing false negatives in their statistical analysis.[24] The recent measurement of a record high temperature in Antarctica will not become an accepted observation, suitable to serve as evidence in climate science, until the initial report is reviewed and discussed by a panel of experts.[25]

So, the situation of disagreement as envisioned in individualist social epistemology, in which two or more individuals in possession of the same data, basic facts, and blessed with comparable reasoning and perceptual skills, disagree about conclusions based on those data, underrepresents the situation more likely to be found in the sciences. In the sciences, in addition to comparable intellectual skills and possession of the same data, disagreement will likely involve different background assumptions, whether substantive, methodological, or axiological, which confer different evidential import on the data. Christensen comes closest to appreciating this kind of complexity in his example of two medical researchers who, weighing accuracy of fit and simplicity differently, assess the import of the same data differently.[26] However, Christensen's treatment of the example ends up reinforcing the point already made about the restrictiveness of the traditional ISE approach. Christensen's approach requires the participants

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[23] Nicholas Rasmussen. "Mitochondrial Structure and the Practice of Cell Biology in the 1950s". *Journal of the History of Biology* 28 (1995): 381-429.
[24] Heather Douglas, "Inductive Risk and Values in Science" *Philosophy of Science* 67, 4 (2000): 559-579.
[25] World Meteorological Organization. "New Record for Antarctic Continent Reported." https://public.wmo.int/en/media/news/new-record-antarctic-continent-reported (14 February, 2020, accessed 24 March, 2020)
[26] Christensen, *op. cit.*

in disagreement first to seek an explanation for the disagreement, in, for example, one or another participant's bias towards accuracy versus towards simplicity. Where an explanation in terms of one or the other's bias is available, the biased individual should adjust her/his degree of belief towards that of the other. Since each may be susceptible to bias, in this symmetric situation, each should adjust towards the other. But this approach assumes that there is a context-free way to weigh accuracy versus simplicity (or other possible cognitive values) and to ignore the complex of beliefs, commitments, and cognitive goals that ground the preferences for accuracy on the one hand or simplicity on the other.

In the Strongly Social Social Epistemology, disagreement is one of the primary means by which the assumptions in light of which data are being assessed can be surfaced and exposed to scrutiny. Rather than taking routes one or two as described by Christensen, a third option is to inquire into what contextual factors favor one of the hypotheses in contention over the other. This occurs through challenge, but the outcome of challenge is not limited to thinking the other is *ipso facto* mistaken or that one should adjust towards the other, but the revelation of a complex of interrelated assumptions, values, inferential standards, and cognitive goals. These then become the object of discussion, but it takes work to identify which aspects in the complex contexts informing the reasoning of the disagreeing parties are responsible for the different evidential assessments. The basic understandings of data, evidence, the weight of different kinds of data, and different methods of generating those data are not given a priori, but are achieved through critical interaction among researchers, disagreement under a more benign name.

III Testimony

Taking the individual as the paradigmatic cognitive agent also has consequences for the understanding of testimony. On individual centered epistemology, testimony becomes a philosophical issue through contrast with an agent's own perception and reasoning. It suffers by comparison, for to accept the testimony of another with respect to some claim *p* is to be at one remove from the putatively direct source(s) of justification of belief that *p*, justification that must end in some individual's perception of the state of affairs picked out by *p*. Hume's injunction that we accept the testimony of others only in so far as we are assured by direct past experience that the other's witness accords with the facts is a standard starting point for many contemporary philosophical discussions.[27] Hume was particularly concerned with how to weigh another's testimony that *p* against one's (well-founded, or at least strongly held) belief that not-*p*. On this view, the reliability of testimony reduces to the reliability of direct experience – in this case, the experience of the one to whom testimony is offered of the past veracity or trustworthiness of the testifier. So, testimony does not constitute a totally distinct source of knowledge, governed by a distinct principle of testimony, in addition to sense perception and reasoning.[28]

Other philosophers, however, point out that most of what we would say we know, we believe on the basis of the testimony of others. What we believe about medication, about nutrition, about the safety of air travel, about the risks of automobile travel, about what is occurring in politics, etc., we believe on the basis of the testimony of doctors, scientists, news reporters, and so on. And our trust in these sources is not for the most part justified by our direct experience of their veracity. Testimony, it turns out, plays an outsize role in individual belief

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[27] C.A.J. Coady. *Testimony: A Philosophical Study* (Oxford: Clarendon Press, 1992) spurred the contemporary discussion of Hume's worries.
[28] See Elizabeth Fricker , "Against Gullibility," in B.K. Matilal and A. Chakrabarti, eds. *Knowing from Words* (Amsterdam: Kluwer Publishers, 1994) for detailed discussion.

formation. This has led some to think of testimony as a third kind of justification, or as a *sui generis* source of knowledge in addition to sense perception and reasoning.[29]

So, the primary questions about testimony in individual centered epistemology concern whether testimony is an independent source of justification, whether it is equal to or inferior to direct perception, and whether it can count as evidence for a claim or only a good reason to believe.[30]   As in the case of disagreement, the practice of science again offers a different perspective.

First of all, whatever one's philosophy of language or views about the origins of language, the importance of shared categories for representing phenomena in the sciences is undeniable. Observation and measurement are public practices, their results expressible in language that must travel. These shared categories are a large part of what students or apprentices learn as they are initiated into a particular scientific discipline: the concepts and the means of ascertaining their realization (or not) in the material under study. The advance of science is deeply dependent on testimony – the teaching of one's predecessors – so that each generation is not required to remake the same measurements, conduct the same experiments, perform the same calculations and inferences, develop the same classification systems anew. Were such direct personal engagement necessary, scientific knowledge would never get off the ground.

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[29] Michael Welbourne, "The Transmission of Knowledge," *Philosophical Quarterly,* 29, 114 (1979):1-9.
[30] Jennifer Lackey "The Nature of Testimony," (*Pacific Philosophical Quarterly, 87* (2006): 177-197) introduces a question orthogonal to the standard epistemological concerns about testimony. She urges a distinction between speaker testimony and hearer testimony (i.e., the communicative acts of a person towards others and the reception of communicative acts of a person by others). This allows her to treat as testimony expressive acts not intended as communicative or as communicative to a particular hearer.

The necessity to transmit shared concepts and categories across generations of researchers is not the only role of testimony in scientific practice. The division of cognitive labor also requires testimonial relations. In Individualist Social Epistemology, the division of cognitive labor is conceptualized in individualist terms. Imagine that there are several methods of addressing a problem and that it is not known in advance which will be successful. How can the community be assured that all methods will be tried? Ideally there will be a distribution of conservative tried and true approaches and radical, innovative, but untried and risky, approaches that maximizes the chances of the community as a whole achieving the best solution in the least time. Philosophers concerned with this problem consider how such a distribution can be identified and how it can be achieved. The community's system of rewards will facilitate what methodological choices are made by individuals with different motivations, and what system is likely to have the optimum distribution of efforts among the different methods.[31] Others consider what the individual practitioners need to know in order to make a reasonable choice.[32] The point to note is that problems are conceived in non-communicative terms: a matter of calculation on the part of individuals motivated both by curiosity/desire for knowledge and more mundane goals such as tenure, the next grant, fame, awards, etc. The questions of success are settled by which method gets to the finish line first. But what can be encompassed under the umbrella of division of cognitive labor concerns more than designing contexts to maximize the chances a community of individual decision-makers achieves a fruitful distribution of effort.

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[31] Philip Kitcher, "The Division of Cognitive Labor," *Journal of Philosophy,* 87, 1 (1990): 5-22, and Michael Strevens, "The Role of the Priority Rule in Science," *Journal of Philosophy*, 100, 2 (2003): 55-79.
[32] Ryan Muldoon, and Michael Weisberg, "Robustness and Idealization in Models of Cognitive Labor," *Synthese*, 183 (2011):161-174

i. Big Science and the distribution of expertise. John Hardwig was one of the first philosophers to worry about the epistemological consequences of the division of labor in science.[33] He was not concerned with the problem of dividing research resources among competing methodologies, but about the integration of disparate bodies of expertise in the performance of one research effort. Focusing on one particular experiment in high energy physics, he drew attention to the multiple subspecialities called on to generate a value for the measurement of a particular property (the photoproduction cross-section) of a charm particle. The article reporting the result lists 99 authors. Such a number of participants is necessitated by the variety of kinds of expertise required for successful performance of the experiment: detector design, materials science, statistics, theoretical physics, experimental particle physics, etc. The integration of these disparate bodies of information is represented by Hardwig as a complex set of conditionals. Not only do practitioners of any one subspeciality not know enough about the other subspecialities to independently validate their contributions, the Principal Investigator does not either. So, each participant is in position to discharge the antecedent (or perhaps one of the conjuncts in the antecedent) of one of the conditionals, but not others. Hardwig and others after him have used the case to debate two issues: whether the report of colleagues in the experiment counts as evidence or as a non-evidential reason to believe and who, if anyone, can be said to know.[34] But a practice-focused philosophical reflection concerns the relations among the researchers. Research team members are chosen on the basis of their past success (generally known through reputation), but no one in the team is in a position to independently and directly

[33] John Hardwig, "Epistemic Dependence," *Journal of Philosophy*, 82, 7 (1985): 335-349
[34] Frederick Schmitt, "On the Road to Epistemic Interdependence," *Social Epistemology,* 2, 4 (1988): 297-307; John Hardwig, "Evidence, Testimony, and the Problem of Individualism – A Response to Schmitt," *Social Epistemology,* 2, 4 (1988): 309-321.

assess that past performance. They are collaborators, not competitors, and depend on one another's testimony about their contributions to produce the final result.

ii. Trust and extending others' results. The Hardwig case (which characterizes much contemporary science) demonstrates reliance on testimony in integrating multiple disciplinary efforts to produce a single result. Torsten Wilholt focuses not on a group in which expertise is differently distributed, but on the ways in which individuals and groups build on the results of others.[35] For example, when I join S's result about the effects of C in substance Z or population O to my result about similar effects of C in substance X or population Q to support a more general claim about the causal powers of C, I am relying on S's testimony about their results. Every research report is an explicit act of testimony, but the content of that testimony is the result of decisions about reporting and recording format, of choice of effect measures, of decisions about how much diversity to seek in the data, and so on. It does not report what is called raw data. In assessing and extending results from a given research project or report to more general conclusions, one starts with data models that are the result of various data preparation processes. Wilholt expands the observations of Heather Douglas about the character of methodological choices in the design of research.[36] Douglas pointed out the variety of choice points where the decision is not determined by empirical considerations. Most research design involves choice of level of statistical significance, acceptable effect size, making tradeoffs between avoiding false positives versus avoiding false negatives. In addition, most research will present researchers with

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[35] Torsten Wilholt, "Epistemic Trust in Science," *British Journal of Philosophy of Science,* 64, 2 (2013): 1-21 and "Collaborative Research, Scientific Communities, and the Social Diffusion of Trustworthiness," in Michael S. Brady and Miranda Fricker, eds., *The Epistemic Life of Groups.* (Oxford: Oxford University Press. 2016) pp. 218-233.
[36] Douglas *op. cit.*

borderline cases that must be adjudicated as belonging or not belonging in the category of interest.

Douglas's point is that such choices, buried deep in the research design or process, in many cases have social consequences and that values are thereby hidden deep in the structure of research. Wilholt takes this one step further by arguing that when one researcher, A, takes the reported results of another to build on in some way, they must assume that those results are an outcome of the same tradeoffs A would make in designing A's research. Since the terms of the tradeoff are not part of the published research report, A must trust that the individual or group whose work A is using values the alternatives in the same way A does and has made the same methodological decisions A would. For Wilholt, this means that there is an ethical dimension deep inside the research process. Reliance on the work of another extends beyond the manifest content of the communicated result to trust that values partially generating that content are acceptable or at least shared. The scientific community is bound by relations of trust that cannot be discharged by ascertaining the general veracity of testifiers. The necessity of various techniques to transform raw observational and experimental input into evidence and the importance of shared values in selecting techniques for the performance of that transformation underscores the point made earlier: data do not speak for themselves. There is no bedrock of certain experience that researchers can turn to as grounding their reliance on others. That reliance is part of the process of determining just what the evidence is.

iii. *Criticism:* Reasoned disagreement depends on testimony. Disagreement in science is not merely the assertion of a statement contradicting the assertion of another. When I agree or disagree with a colleague or friend, I don't just say "yes" or "no", I give my reasons for my assent or dissent. Giving my reasons, which may involve citing my own laboratory results, the

results of others, the interpretations of others, is an act of testimony. The same holds for criticism. When a colleague criticizes some view of mine, I take them to be testifying to a set of facts, purported facts, or theoretical propositions that are negatively relevant to my view. Kuhn famously wrote about major disagreements in science –between large scale theoretical complexes like Aristotelian and Galilean/Newtonian physics. But scientific practice is full of criticism and disagreement – often engaged in a cooperative, but sometimes competitive, spirit – about what is visible on a particular slide or in a particular culture, about an experiment performed elsewhere, at a different time, about alternative explanations of measured phenomena about whose measurement we agree. These engagements take place among lab partners, across research groups, in the course of peer review, in the reception – positive and negative – of research reports in the scientific community.

In the course of this critical interaction observational and experimental results are validated, measurements are stabilized, and the inferential relations among these and explanatory hypotheses are established. The lab partners seeking to determine a gene sequence from a gel electrophoresis slide alike testify to one another as to what they perceive and they trust one another to report accurately what each sees. When reports coincide, an observation results and can be recorded and reported. When reports diverge, the researchers must work out what is responsible for the divergence. It may or may not be eliminated in the course of this process.[37] Given the challenge and response that is central to scientific reasoning, researchers constantly exchange information as part of reasoning. These exchanges precede the more formal communications that occur as part of the review processes that themselves precede the variation in uptake that will determine whether a research result achieves the status of scientific

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[37] Klaus Amann and Knorr-Cetina *op. cit.* offer an extended example.

knowledge. Survival in the course of (genuine) critical interaction at all these stages is the mark of scientific knowledge.[38]

In each of these kinds of case, criticism, extending or integrating results, testimony is part of a communicative exchange. It extends through time as part of an ongoing interaction. By contrast, as in the case of disagreement, mainstream or individualist social epistemology focuses on the static relationship among content, testifier, and testified to. For social social epistemology, the isolable testimonial act is only evaluable in the context of what comes before and after, that is, as an element in an interaction that unfolds in time.

Most of the philosophical testimony literature is concerned with the question: what kind of reason for *p* does S's testimony that *p* provide to R. Is the testimony evidence for *p* directly, is it evidence that S believes there is evidence for *p,* or is testimony as a ground of belief irreducible to other grounds?   The setting for this question presupposes a situation in which we already know what evidence and evidential relations are and what kind of warrant they provide and are inquiring about a communication from S to R (or, as in Lackey's example, in R's hearing an assertion by S). The one-way communication made problematic in much of the testimony literature just does not fit the scientific situation. In the former, a contrast is drawn between putatively more certain (one's own observation, one's own reasoning) sources of information and the questionable (testimony from others). In the scientific situation, by contrast, testimony

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[38] Bruno Latour *Science in Action* (Cambridge, Harvard University Press, 1987) pp. 1-59, describes the (often hostile) to and fro of endocrinological researchers competing to isolate growth hormone. True to the spirit of his investigation, Latour restricts himself to recounting the events in the laboratories and the claims and counterclaims. He proposes that one can just follow the action without engaging in epistemology or cognitive analysis. Latour is interested in what is required to give a causal explanation (or narrative) of how a group or individual settles on a belief/representation and he treats the cognitive as separate and apart from the causal. By contrast, the strongly social epistemological approach argues that the back and forth is an ineliminable part of the cognitive, not an alternative (and less theory-laden) description.

has already been involved in the performance of the cognitive tasks (observation, reasoning) that yield evidential relations. Whatever uncertainty attends testimonial relations diffuses throughout the sciences.

Whether agreeing or disagreeing, we are dependent on the testimony of others. On the strongly social view, then, testimony is as much a driver of scientific advance as is disagreement. Indeed, disagreement can be considered just a special case of testimony. Science is conducted in communicative networks. Testimony is not an optional cognitive resource that we can (or should) decide to do without. It is not a secondary mode of knowledge/belief acquisition that sits on top of and is validated by individual observation and reasoning. It is instead involved in the very performance of these cognitive tasks

So, both disagreement as envisioned in individualist social epistemology, in which two or more individuals in possession of the same data, basic facts, and equal reasoning and perceptual skills, disagree about conclusions based on those data, and testimony, conceived as communication from speaker to hearer against a background of shared linguistic understanding and shared concepts of evidence, underrepresent the situation more likely to be found in the sciences. In the sciences, in addition to comparable intellectual skills and possession of the same data, disagreement will likely involve different background assumptions, whether substantive, methodological, or axiological, which confer different evidential import on the data. This disagreement prompts scrutiny of those assumptions and impels the community to their abandonment, revision, or reaffirmation. The philosophical testimony literature considers the various situations in which S might affirm p to R as stand alone episodes. Different recommendations regarding the propriety or not of so testifying or of believing the testimony are considered. But these discussions are characterized by a shared presupposition: that we already

know what evidence and evidential relations are and what kind of warrant they provide and are now inquiring about a communication from S to R and whether such a communication satisfies the criteria.   In the strongly social epistemology, by contrast, testimony is involved in the performance of the very cognitive tasks (observation, reasoning) that yield knowledge/beliefs about evidential relations. The norms and categories presupposed in individualist social epistemology emerge from and in the course of scientific practices. Like disagreement, testimony is a constitutive practice of knowledge production.

When viewed from the perspective of scientific practice, then, disagreement and testimony are not epistemological outliers requiring special treatment, but integral to the processes of cognition. They do not stand in contrast to some more basic and certain means of acquiring knowledge or of validating beliefs as they are part of what transforms the subjective into the objective, from the assertability of "it seems to me that p" to the assertability of "p." This transition in status is a prerequisite for the absorption of any single result into the body of scientific knowledge.

IV Group Knowledge

One of the attractions of formal accounts of judgment aggregation is their ability to account for common belief as the outcome of collected individual beliefs, without positing an entity that floats free of the individual members. A number of social theorists and epistemologists argue that the aggregative approach is unable to encompass the full range of epistemological attributions to groups. They, therefore, focus on the group or community as the agent replacing the individual. In this strategy the group is now considered as the cognitive agent, albeit one with parts, and the challenge is to see how properties attributable to individual

agents can be attributed to groups. Lynn Hankinson Nelson argued in her feminist adaptation of

Quine, for example, that the community knows, but no individual member of the community

knows.[39] Later theorists have not been so extreme but have endeavored to spell out how a

community or group could be said to know or believe.

Margaret Gilbert, as part of her general program of analysis of the social, has been at the

forefront of this effort.[40] She seeks to identify the conditions that must be satisfied if a group can

be said to believe. She argues that group belief is not a matter of shared or common beliefs, not

an aggregative fact about a collection of individuals, but a matter of some group action that

constitutes a belief as the group's belief. One might think of a reading group settling on an

interpretation of a text (Gilbert's example) or a jury agreeing on a verdict. In both cases,

individual members may sign on to a group opinion from which they nevertheless individually

and personally dissent. In Gilbert's analysis the constituting action is a joint commitment that

imposes obligations on individuals insofar as they are acting as members of the group. If an

individual member holds an opinion to the contrary, this joint commitment requires, among other

things, that she make clear that she is not speaking for the group in expressing that opinion. The

reading group members cannot say, speaking as group members for the group, that the poem is

the worst form of doggerel when the group has come to a different consensus, although they,

speaking only for themselves, may say so.

Jennifer Lackey, who has done much to move social epistemology from its individualist

bias, is also concerned with group belief.[41] For example, she characterizes a group epistemic

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[39] Lynn Hankinson Nelson, *Who Knows: From Quine to Feminist Empiricism* (Philadelphia: Temple University Press, 1990). This is one horn of what Hardwig took to be the dilemma generated by the collaborations typical of big science projects.
[40] Margaret Gilbert, "Modeling Collective Belief," *Synthese,* 73,1 (1987): 185-204.
[41] Jennifer Lackey, "What is Justified Group belief?" *Philosophical Review*, 125, 3 (2016): 341-396.

agent by addressing the question what counts as a *justified* group belief. Like Gilbert, she rejects

both an inflationary account that treats groups as ontologically autonomous, free floating agents,

and a deflationary account that treats groups as mere collections of individual agents. She is

concerned with pre-constituted groups like juries or work groups (or reading groups). Her

question then is when such groups can be said to justifiedly believe that *p*. Her answer invokes

the coherence of the aggregated pairs of belief and reasons attributable to the individual

members. [42]

Both Gilbert and Lackey are concerned with the conditions under which paradigmatically

individual states, such as belief and justified belief, can be attributed to already constituted

corporate or collective entities. These entities are not just collections of individuals, but

collections of individuals bound into a unity by some constituting action for some shared

purpose. While rejecting summative or aggregative individualism, these philosophers also avoid

attributing a reality to the groups independent of their members. They are concerned, that is, to

avoid holism even while rejecting individualism. In both accounts the social has to do with the

states of the group or collective and the attribution to collectives of states that are not analyzable

as the simple sum of states of the individual members of the collective. It is states, not the

entities that are the subjects of those states, that are not reducible.[43] Something similar holds for

many of the essays in a recent collection, *The Epistemic Life of Groups*.[44] The intention of the

editors is to take the work by thinkers like Gilbert and Lackey into further doxastic or

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[42] Lackey also adds the condition that full disclosure of evidence relevant to *p* would not yield a total belief set that fails to make probable that *p*. This connects her analysis to work in individualist epistemology tying justification to absence of defeaters.
[43] For a useful discussion of the different forms of holism/individualism, see Christian List and Kai Spiekermann "Methodological Individualism and Holism in Political Science: A Reconciliation," *American Political Science Review,* 107, 4 2013: 629-643.
[44] Michael S. Brady and Miranda Fricker, eds., *op. cit.*

epistemological areas. With one or two exceptions, the authors of these essays address either the attributability of paradigmatically individual states (such as understanding, emotions, self-knowledge) to groups or the (more familiar problem of) impact of group conditions (such as social prejudice) on individuals.

Gilbert and Lackey both reject the aggregative or summative account of group knowledge because they think there are actions, relations, or obligations made invisible by the aggregative account. They are less concerned with analyzing the epistemological states that they attribute to groups than with the problem of how such states, already characterized in terms of individuals, might be ascribed to groups. That is, given an antecedent understanding of what it is for individuals to believe or to know, their concern is with how to reject a reductive individualism that treats group doxastic properties as the aggregation of properties of individual members without falling into holism.

In spite of their differences, then, the aggregationists, and the anti-aggregationists share one central assumption; that knowledge is primarily analyzed at the level of individuals. The business of social epistemology is to show how states already understood at the individual level can be ascribed to supra-individual entities, i.e. groups. The social is understood as commonality or as sharing. Theorists of disagreement and testimony differ in focusing on communication, but here, too, the assumption is that knowledge (and the rest of epistemology) is primarily analyzed at the level of individuals. The social world, other persons, constitute particular kinds of challenge to individual cognitive agents. The business of social epistemology is to incorporate these challenges to individual agents to those already a part of epistemological analysis (illusions, etc.). The result is analyses of static relations between an agent, some content, and the means of delivery of that content abstracted away from the situations in which disagreement and

testimony occur. The anti-aggregationists miss the interactions among members of the collectives; the theorists of disagreement and testimony miss the processes of knowledge-making that they are part of.

Attending to knowledge formation in science, by contrast, makes salient not states of entities, but of processes, especially interactive processes. The level of analysis is located between the levels of individual and fully formed group. In this mesolevel picture, individuals are connected via their interactions, and, in the epistemological approach favored here, become epistemic agents through their participation in deliberative interactions.   Group and community formation on this view is not the outcome of a single constitutive act or set of acts. Rather what constitutes groups is connectedness in a network of interactions. Such networks do not have specific structures but do have areas of greater and lesser density. Members may move into and out of these communities and have greater or lesser degrees of connectedness.[45] On this view, sociality is not just being in a world populated by others, or membership in a group; sociality is interaction. Sociality is not static, but dynamic, and social phenomena, too, must be understood as dynamic.[46] Epistemic norms, on this view, will target the interactions, rather than states of groups or of the individuals constituting groups.

One might object that this paper misunderstands the point of mainstream epistemology, whose task it is to provide analyses of epistemological concepts irrespective of the context in

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[45] Of course, organizations may form to impose some structure, as professional scientific societies arose in the 19th century, but the stringency of membership requirements has varied over time and has never fully encompassed what one might identify as the community. Not all scientists join the relevant society for one thing, and networks extend beyond those societies.
[46] In pressing for a social subject, Bird, *op. cit.* assumes social institutions that generate our shared knowledge (that the earth is round(ish), that gravity is pervasive, etc.) and criticizes an individualism that ignores such institutions. My point has been to draw out the contrast between a thin and a deep form of sociality in characterization of the social processes themselves.

which they may be deployed. But this objection, in turn, misperceives one central point of the argument, which is that the analyses of mainstream epistemology are not context-independent, but are themselves influenced by a strong form of individualism that takes cognitive agency to entail individual self-sufficiency.

V Conclusion

This paper has argued that contemporary discussions in social epistemology tend to assume that knowledge, evidence, justified belief, etc. are to be characterized in terms of the doxastic states of individuals. That individuals are located in social worlds and engage in epistemologically relevant interactions with other individuals is treated as a contingent fact about those individuals which on occasion generates challenges to conventional conceptions of knowledge. Social epistemology that takes this as a point of departure is not very social.

Can mainstream social epistemology become more social? This depends on its stance towards at least three questions prompted by the main points of this paper. First, epistemologically interesting disagreement is among the different, not among the same. Disagreement among the latter can generate interesting puzzles, but disagreement among the different is what drives science forward. From the point of view of some historically and practice informed philosophy of science that focuses on communities, disagreement is not disruptive but fruitful. In thinking further about this difference between individual social epistemology and more strongly social social epistemology, we might think about the tenability of assumptions about knowledge (monism, uniformity) that make disagreement seem problematic. What is the basis of such assumptions? Second, there is a different view of sociality to be had from strongly social social epistemology. Sociality is not just living in a

world that happens to be populated by others and not just being part of a group that shares certain properties or beliefs. Similarity and sharing are relatively thin modes of sociality. What assumptions about agency and personhood stand in the way of adopting a more robust interactive conception of sociality? Third, if scientific knowledge is the most reliable form of empirical knowledge, shouldn't our epistemological analyses start there? Cognitive agents in the sciences are not isolated, but are bound in complex networks of interaction. Cognitive agency is not just the exercise of individual capacities of reasoning and observation. It is participation in complex networks that involve both criticism and sharing information. What, then, should we take as focal exemplars in thinking generally about knowledge? Engaging these questions could only be philosophically fruitful.