SOCIAL EPISTEMOLOGY
Martin Kusch

Introduction

“Social epistemology” (SE) can be understood broadly or narrowly. On the broad understanding, the expression covers all systematic reflection on the social dimension or nature of cognitive achievements such as knowledge, true belief, justified belief, understanding, or wisdom. The sociology of knowledge, the social history of science, or the philosophy of the social sciences are among the key parts of SE thus construed. Many contributors to Pragmatism, Marxism, Critical Theory or Hermeneutics also qualify. On the narrow understanding, SE dates from the 1980s, is primarily a philosophical enterprise, and has its roots in Anglo-American epistemology, in feminist theory, as well as in the philosophy of science. The perspective of this chapter lies between the narrow and the broad renderings.

Knowledge in a Social World

Goldman’s Knowledge in a Social World (KSW) (1999) has been crucial in giving structure and inspiration to the contemporary field of philosophical SE. The book is divided into three parts. Part One lays “foundations.” Goldman points out that SE can take different forms, depending on whether individuals or groups are taken as knowers. Adopting the former perspective, KSW investigates how individuals gain knowledge in and through their interactions with one another. Knowledge is taken in the “weak” sense of (merely) “true belief.” Goldman’s focus is on the evaluation of the truth-conduciveness (“veritistic quality”) of social practices. In order to develop “veritism” systematically, Goldman develops a semi-formal measure of “veritistic value.” One way to spell out this value involves defining the V-value of a true belief as 1.0; the V-value of a rejection of a true proposition as .0; and the V-value of withholding judgment regarding a true proposition as .5. An epistemic practice \( p_1 \) has a higher V-value than another epistemic practice \( p_2 \) if using \( p_1 \) leads to bigger increases in V-value than using \( p_2 \).

Having introduced his framework, Goldman turns to the veritistic analysis of “generic social practices” in Part Two. Such generic social practices are testimony, argumentation and communication. KSW’s main idea concerning second-hand knowledge is that our normal practice for assessing testimony can be improved by relying on Bayes Theorem. KSW goes on to distinguish fourteen standards for good argumentation. Goldman puts much emphasis on what he calls the “truth-in-evidence principle”: “A larger body of evidence is generally a better indicator of the truth-value of a hypothesis than a smaller, contained body of evidence, as long as all the evidence propositions are true and what
they indicate is correctly interpreted” (1999: 145). Part Two concludes with a wide-ranging discussion of “the technology and economics of communication.” Goldman seeks to refute the idea that “more total V-value will be achieved if speech is regulated only by free market mechanisms rather than by other forms of regulation” (1999: 194). Goldman shows that economic theory does not support this view and that non-market regulation is needed to prevent false but persuasive speech.

Part Three turns from “generic social practices” to the “special domains” of science, law, democracy, and education. Concerning science Goldman covers inter alia “sources of scientific success,” “the distribution of scientific research” (centralized agencies might be needed to make sure that different problem-solving methods are pursued), “scientific publication” (editors should aim to publish papers that produce the greatest increase in knowledge), and “recognizing authority” (novices can recognize authorities via observational verification and argumentation). KSW criticizes the veritistic credentials of the common-law tradition. Veritistic epistemology cannot accept that certain types of evidence (e.g. statistical data) are excluded simply because juries are unable to comprehend them. KSW also makes provocative claims concerning democracy. Goldman urges voters to acquire such knowledge as would enable them to decide which of the candidates would, if elected, produce the best outcome. Finally, regarding education, Goldman demands, among other things, that schools enable students to identify truths, that postmodern relativism be kept away from children, and that the curriculum must respect established scientific expertise.

Social Epistemology and the Classical Tradition

Goldman’s KSW is an intriguing intervention into several contemporary philosophical and political debates, a useful summary of much earlier work in SE, a clear map of the terrain (as Goldman sees it), and a stimulating outline of possible future directions. Many of these suggestions have been taken up in subsequent work both by Goldman himself and by many others. Since these developments have already been reviewed extensively elsewhere (e.g. Goldman 2002b, 2006, 2009b), I shall concentrate on the question of how SE should relate to epistemology as a whole. I shall use Goldman’s stance on this issue as a foil for my own view.

Goldman wants to convince the traditionalist—that is, the “classical epistemologist”—that SE deserves to be counted as “real epistemology” (2009b, 2010). To that end, Goldman distinguishes between three forms of SE: “revisionist,” “preservationist,” and “expansionist.” The three forms differ in how they relate to the basic assumptions of classical epistemology; to wit, that epistemic agents are exclusively individuals; that epistemology clarifies key concepts of epistemic evaluation (such as knowledge or justification); that these concepts are of universal validity; and that concepts of epistemic achievement are linked to an objective and mind-independent truth.

As far as revisionist SE is concerned, Goldman suggests, the traditionalist is right: it does not belong within real epistemology. Revisionists give up most or all of the assumptions of classical epistemology and aim for a “successor” project. Metaphorically speaking, revisionists tear down the building of classical epistemology and build themselves an altogether new intellectual home. “Postmodernism, deconstructionism, social constructionism, relativism, and social studies of science, including the ‘strong programme’ in sociology of science” belong here (Goldman 2010). Goldman pays closer attention only to the last-mentioned approach. Its two leading figures, Barry Barnes and David Bloor
deny that there are “context-free or super-cultural norms of rationality” (Barnes and Bloor 1982: 27) and they redefine knowledge as “institutionalized belief”; knowledge is a “collective representation of the world [that is] . . . held by the group as a convention” (Bloor 1991: 169). Bloor’s “Strong Programme” in the sociology of scientific knowledge is defined by an insistence on a number of “symmetries”: natural and mathematical sciences as well as the humanities and social sciences can be analyzed sociologically, and true as well as false beliefs are open to sociological explanation. Indeed, Bloor writes that “the same types of causes would explain, say, true and false beliefs” (Bloor 1991: 7).

Goldman claims that Strong Programmers intend to “debunk the epistemic authority of science” by showing that political interests and power—rather than good evidence—determine theory choice. Goldman objects that this position undermines itself. The empirical-historical case studies produced by Strong Programmers seek to present empirical evidence for the claim that empirical evidence is always outweighed by political interests (Goldman 2006).

Preservationist SE is a conservative extension of classical epistemology—and thus it counts as real epistemology. The preservationist adds a new storey to the epistemological house but sees no need for laying new foundations. Preservationist SE follows the tradition in restricting its interest to understanding and evaluating “doxastic decision making” by single epistemic agents: that is, to understanding and evaluating how a single epistemic agent adopts or retains attitudes of belief, disbelief, or suspension of belief towards a proposition based on perceptions, memories, or prior beliefs. Preservationist social epistemology goes beyond classical epistemology, however, in reminding us that we often gain vital information or evidence from other human agents. The most important phenomena thereby brought into the purview of epistemology are testimony and cognitive disagreement.

Expansionist SE proposes bolder forays into new territory—but it, too, is real epistemology. The expansionist builds a new wing to the old edifice of classical epistemology and this endeavor calls for the laying of some new foundations. A relatively modest expansion concerns the epistemic norms of assertion and argumentation. Three bolder steps are the assessment of scientific experts in terms of their track records (cf. Goldman 2001/2002), sensible ways of diversifying scientific research efforts (Kitcher 1993), and the introduction of collective believers and knowers (Pettit 2003; Schmitt 1994, 2006).

Sociology of Scientific Knowledge

I agree with Goldman that SE is real epistemology. But I disagree with him over two crucial points: I find his portrayal of the sociology of scientific knowledge (SSK) inadequate, and I regard his attitude towards classical epistemology as far too deferential. Both issues are significant since they influence which ideas and research programs we count as relevant to SE, or as part of it.

Correcting Goldman’s misunderstanding of SSK is important for two reasons. First, SSK must have a central role within, or opposite, social epistemology: after all, SSK is dedicated to the empirical investigation of social dimensions of knowledge. Second, philosophical forms of SE tend to use SSK as a foil. Goldman is a case in point. But for this to be an adequate maneuver, philosophers must get SSK right.

Goldman assumes that SSK is in the business of motivating skepticism about science. Or, as another influential philosophical critic puts it: SSK seeks to establish that
science “is just another social institution clamoring for power” (Brown 2001: 143). It is hard to square this interpretation with any of the existing historical SSK case studies. These studies aim to identify the differences between scientific and other institutions, and they are not concentrating on issues of social power. Consider, for example, the single most influential idea in SSK, Harry Collins’ “Experimenters’ Regress” (Collins 1985). Collins shows that in cutting-edge experimental science researchers face at least two kinds of uncertainty: they do not know which kinds of phenomena exist, and they do not know whether their detection instruments work. The one form of ignorance feeds into the other: their lack of knowledge about what exists prevents the experimenters from properly calibrating their instruments; and their lack of knowledge about how best to calibrate their instruments prevents the experimenters from having much confidence in their ability to detect. Collins goes on to document, on the basis of numerous interviews with scientists, that the latter seek to break the resulting regress on the basis of a wide variety of different considerations. Such considerations prominently include information about the social standing and the track record of different experimenters and their laboratories.

Philosophers who treat Collins as a skeptical “debunker” regarding scientific knowledge attribute roughly the following argument to him (cf. Godin and Gingras 2002):

[1] If scientists (at the cutting edge of research) assess each others’ contributions in light of the social criteria Collins mentions, then cutting-edge science does not produce knowledge.

[2] Scientists (at the cutting edge of research) do assess each others’ contributions in light of the social criteria Collins identifies.


Collins’ critics usually take issue with [2]. They contend that the experimental data plus relevant theories are always sufficient to break the Experimenters’ Regress. Call this the “direct” response to the skeptical argument [1–3]. Collins rejects the direct response. But this does not mean that he accepts the skeptical argument (Collins 2002). Using terms familiar from philosophical treatments of skepticism, we might say that Collins favors a “diagnostic” response to skepticism about science. The diagnostic response rejects [1]; it denies that the possibility of scientific knowledge in cutting-edge science depends upon the absence of social criteria for the assessment of knowledge-claims.

The point generalizes to other case studies and theoretical writings in SSK. Their goal is not to undermine science; their goal is to convince the reader of the social nature of all central scientific phenomena, from observation to experiment, theory choice to calibration. Or consider how Bloor tackles Saul Kripke’s (Wittgenstein’s) famous meaning-skeptical argument (Kripke 1982; Bloor 1997). Again the response is diagnostic: Bloor holds that the meaning-skeptical argument is compelling only as long as we have a single and isolated rule-follower in mind. Once we replace the individual with a group, meaning skepticism is defeated.

But what about SSK’s commitment to a “nihilistic” relativism (Goldman 2009a), and Bloor’s alleged view that “our epistemic reasons never make any contribution whatsoever to the causal explanation of our beliefs, so that the correct explanation is always exclusively in terms of our social interests” (Boghossian 2006: 112)? The answers are straightforward. The relativism of Barnes and Bloor is methodological not substantive. Their view “is not that all beliefs are equally true or equally false, but that regardless of
truth and falsity the fact of their credibility . . . calls for empirical investigation” (Barnes and Bloor 1982: 23). Bohr’s charge is refuted by Bloor’s statement according to which SSK is committed to the thesis that “our perceptual and thinking faculties are two different things and that our perceptions influence our thinking more than our thinking our perception” (Bloor 1991: 33). Bloor also rejects the suggestion that “knowledge is purely social”: “The strong programme does say that the social component is always present and always constitutive of knowledge. It does not say that it is the only component, or that it is the component that must necessarily be located as the trigger of any and every change: it can be a background condition” (1991: 166).

**Diagnostic Social Epistemology**

Goldman’s attitude towards classical epistemology is deferential. He deems its central assumptions fully adequate to its traditional task of understanding the socially isolated knower. And Goldman is eager to show that his favorite examples of SE remain true to many if not most of these assumptions. The two modes of deference result in too narrow a conception of SE: Goldman has no space for what one might call “diagnostic” programs of SE. Diagnostic epistemology tries to analyze, explain and criticize the foundations of classical epistemology. Or, to return once more to the housing metaphor: diagnostic epistemology studies the structure of the building, its central pillars and cornerstones, and the rock (or swamp) on which it rests. Diagnostic social epistemology does all this in social or political terms.

This can take a variety of forms. An *analytic* version aims to establish that classical epistemology is incoherent, and that—properly understood and reworked—it points towards SE. The problem with classical epistemology might be located in how it interprets or selects from our pre-theoretical intuitions. Or the problem might be in how the various core assumptions of classical epistemology fit together.

A *genealogical* SE offers an imaginary social history of the development of our current epistemic concepts. In constructing such “just-so” history, the genealogist treats classical epistemology both as a resource and as a target of criticism. Genealogy is speculative history. This is because it concerns itself with pre-historical events of which no records survive. The starting point of the narrative is a hypothetical pre-historical “state-of-nature.” Genealogy explains why “proto-epistemic” concepts were first introduced and how they related to human needs and interactions. And the social dimension does not end here: every step of the route from epistemic proto-concepts to our concepts is at least in part social. Conceptual change is social change.

Eventually the genealogical narrative will reach a stage from which historical records do survive. At this point genealogy turns into *historical epistemology*. This is the project of writing the social history of our current epistemic intuitions, practices, and theories. This social history can, but need not, discredit classical epistemology.

*Political* epistemology aims to unveil the political costs of classical epistemology and some of our current epistemic practices. This might take the form of convincing us that classical epistemology has the ideological function of excluding certain kinds of people (e.g. women, ethnic minorities) from qualifying as knowers or true believers.

Finally, *naturalistic* SE challenges the individualism of classical epistemology in the light of results in the natural or social sciences. This can come in two flavors. On the one hand, the naturalistic critic points out that some of our best scientific theories routinely attribute knowledge to *groups* of animals or humans. On the other hand, the
naturalist insists that we can only understand the possibility of contemporary scientific knowledge if we dismiss the individualistic intuitions upon which classical epistemology is built.

**Analytic Social Epistemology**

I shall now add some flesh to the skeleton of diagnostic positions. It is hard to think of a better representative of analytic social epistemology than Donald Davidson’s “Three Varieties of Knowledge” (1991/2001). Davidson argues that knowledge of my own mind, knowledge of the (physical) world, and knowledge of other minds “form a tripod: if any leg were lost, no part would stand” (1991/2001: 220). For present purposes the most important upshot of this argument is the idea that a social isolate is unable to know anything. The central strand of Davidson’s reasoning goes as follows. The starting point is the claim that, at least in the case of our simplest perceptual beliefs, their content is determined by their causes. But how can we pick out the relevant causes, given that every belief has an infinite number of causes—all the way out to the Big Bang, all the way in to unobservable brain processes? Davidson maintains that the problem is intractable as long as we consider a single perceiver in isolation. Only a second person, interacting with the first, is able to determine the right cause. The relevant, content-fixing cause is the “last common” cause in the chains of causes that end in the minds or brains of two people facing the same object or event in the external physical world. Davidson recognizes that his thought here is close to Wittgenstein’s idea according to which our concepts of objective truth and knowledge depend on interpersonal communication. The point is worth emphasizing since Wittgenstein has been a central resource or inspiration for other analytic social epistemologists as well. For instance, Wittgenstein is clearly a key influence for authors favoring a “dialectical” approach to justification. According to this approach, “being justified” is social status granted by others to the believer (see Annis 1978; Rorty 1979; Williams 2001; Kusch 2002; cf. Goldberg 2007).

**Genealogical Social Epistemology**

The modern classic of genealogical SE is Edward Craig (1990). Craig offers a hypothetical social account of the prehistory of our concept of knowledge. His narrative is constrained by two ideas. The first idea is that of an epistemic “state-of-nature,” that is, of an imaginary, early, social community composed of language-using human beings who are co-operative though not kin, and whose conceptual and reflective powers are somewhat weaker than our own. The genealogical narrative must make intelligible why these creatures found it useful or valuable to introduce an ancestor of our concept of knowledge. The second constraint is social-developmental: the genealogical narrative must explain—invoking social change as the central cause—why the ancestor concept was eventually replaced by our concept. For ease of reference, I shall refer to the ancestor concept and word (and their cognates) as *protoknowledge*, “protoknowledge,” etc.

Craig’s genealogy can be summed up as follows. In the state of nature, individuals depend upon one another for information. “Inquirers” need information they are currently unable to directly obtain themselves; “informants” offer such information. Inquirers must be able to separate good from bad informants. And it is natural to assume that meeting this need will involve concepts. Assume that *protoknowers* is the central
conceptual tool for dealing with this problem. Which conceptual components should *protoknow*er contain? What should we hypothesize our imaginary ancestors to want this concept for? Craig’s answers are that our ancestors want this concept as a tag for good informants and that the concept *protoknow*er (whether *p*) comprises the following elements: (i) being as likely to be right about *p* as the inquirer’s current needs require; (ii) being honest; (iii) being able to make the inquirer believe that *p*; (iv) being accessible to the inquirer here and now; (v) being understandable to the inquirer; and (vi) being detectable as a good informant concerning *p* by the inquirer. Craig is adamant that (i) to (vi) are not necessary and sufficient conditions. Finally, *protoknowledge* differs from *knowledge* in that: (a) only the former is closely tied to testimony; (b) *protoknowledge* is not a fully public concept insofar as it is indexed to the capacities and needs of specific inquirers; (c) *protoknowledge* can be ascribed only to others but not to oneself; and (d) protoknowledge is not undermined by accident or luck.

This brings us to the hypothetical social-historical narrative that takes us from *protoknowledge* to *knowledge*. Craig speaks of this development as a process of “objectivization” of *protoknowledge*. Key steps in this objectivization are the following. First, *protoknowledge* comes to be used in self-ascription. In response to the question “who knows whether *p*?” group members start to investigate themselves. Second, inquirers begin to recommend informants to others. This can be done in a helpful manner only if the perspectival or indexical character of *protoknowledge* is weakened. Recommending an informant to ever more inquirers makes protoknowledge increasingly harder to get. Third, inquirers begin to use “being recommended” as a property that indicates a good informant. This move dilutes the original detectability requirement. Fourth, in the context of group action inquirers cease to care whether the needed information is accessible to them as individuals; they are satisfied if it is accessible to someone in the group. As a result they will speak of protoknowledge even outside the context of testimony. And thus we reach our concept of *knowledge*.

Craig’s important book has inspired a number of other authors: Bernard Williams (2002) provides a genealogy of the epistemic virtues of truthfulness and accuracy; Miranda Fricker (2007) a genealogy of virtues of epistemic justice; and Martin Kusch (2009) a genealogy of epistemic value. Melissa Lane (1999) seeks to close the gap between epistemic and political states of nature.

**Historical Epistemology**

The development of our epistemic concepts and practices did not end in prehistory. Historical epistemology takes up the story from the point where written sources are available. Most exercises in historical epistemology are histories of the sciences. Spoiled for choice, I shall concentrate on one aspect of Steven Shapin’s social history of testimony (1994). This allows me to illustrate the contribution of SSK-inspired history of science to SE.

Bernard Williams (2002) suggests that epistemic concepts and virtues need to be studied in the context of other values and ideas. For instance, Williams proposes that there are important conceptual links between knowledge and freedom (2002: 146). Shapin’s study might be read as a social-historical contribution to understanding this link. Shapin’s topic is the role of testimony and gentility in the shaping of the investigative culture of English natural philosophy during the lifetime of Robert Boyle. Natural philosophers of seventeenth-century England accepted—notwithstanding the Royal
Society’s motto “Nullius in verba” (on no man’s word)—that testimony was needed to make natural knowledge, and that finding good testifiers was a difficult practical problem. Seventeenth-century literature on the topic suggested any number of maxims, such as “(i) assent to testimony which is plausible; (ii) assent to testimony which is multiple” or “(iii) assent to testimony which is consistent” (1994: 212). Alas, no sooner was a maxim proposed than critics found it to be of only limited help. For instance, maxim (i) could potentially and incorrectly exclude true reports that conflicted with dominant false beliefs; maxim (ii) was of limited value in cases where the multiple reports derived from one another; or maxim (iii) did not account for the experience that “too great a display of internal consistency” might be a signal “that a polished performance had been prepared” (1994: 233). Only one maxim was never challenged: “assent to testimony from sources of acknowledged integrity and disinterestedness” (1994: 212). On first sight, this might not sound like a helpful maxim; how were practitioners of the new science supposed to know who the disinterested reporters were?

This is where gentility and freedom became important. Shapin’s major historical thesis is that “English experimental philosophy . . . emerged partly through the purposeful relocation of the conventions, codes, and values of gentlemanly conversation into the domain of natural philosophy” (1994: xvii). And one of the central beliefs about the gentleman was that he was a natural truth-teller. The gentleman was thought to be a natural truth-teller primarily on the grounds of his possessing a special “disinterestedness” (1994: 83). This disinterestedness was taken to derive in good part from the gentleman’s economic circumstances. As Henry Peacham put it in his influential treatise The Complete Gentleman at the time: “whosoever labour for their livelihood and gain have no share at all in nobility or gentry” (Shapin 1994: 50). In other words, to be a gentleman was to be financially independent and secure. And a life that was independent and secure in this sense was equated with a free life. The overarching thought linking freedom, gentry, and testimony together was thus as follows: “Gentlemen were truth-tellers because nothing could work upon them that would induce them to be otherwise” (1994: 84).

The conventions of gentlemanly experimental philosophy did not allow for anyone to openly express disbelief in a report coming from a gentleman. The situation was very different for all those who did not make the gentry grade: women, servants, “the poor and the mean in general,” merchants, Catholics, Continental gentry, Italians and politicians. In the cases of all of these groups, their “unreliable truthfulness . . . was pervasively referred to their constrained circumstances” (1994: 86). This obviously did not mean that no one but a gentleman was ever believed: given that much of, say, Boyle’s experimental work was carried out by “domestics” such widespread distrust would have destroyed the whole enterprise of natural philosophy. One has to see the link between freedom and truthfulness as a resource: citing the constrained circumstances of domestics-technicians was a way in which a gentleman could explain experimental failures. And whatever information the domestics produced, it became knowledge, and thus a property of the gentlemanly community of natural philosophers, only once it was vouched for by Boyle (or another gentleman) (1994: Ch. 8).

Other SSK-inspired contributions to historical SE include Shapin and Simon Schaffer’s (1985) investigation into the social origins of the development of “the experimental form of life”; Donald McKenzie’s (2001) social history of attempts to mechanize proof; or Martin Kusch’s (1995, 1999) philosophical-historicalaccounts of self-knowledge and anti-naturalism. Lorraine Daston and Peter Galison (2007) owe little
to SSK but illustrate elegantly that epistemic virtues have a social history. Many of Ian Hacking’s (e.g., 1992/2002) historical-philosophical investigations also belong here.

Political Social Epistemology

Feminist epistemology is arguably the most important contemporary instance of political social epistemology. I shall here mention only one prominent view in this regard: Sandra Harding’s “standpoint theory.”

Standpoint epistemology has its roots in the Marxist tradition, especially in the thought of Georg Lukács. Lukács believed that one’s position in the process of production constrains what one is able to learn about society. Some social positions, or “standpoints,” are more limiting than others. The capitalist understands important aspects of the economic process but he is unable to recognize the workers’ essential humanity. The proletarian has no such blinkers. His standpoint is therefore superior to that of the capitalist, both epistemically and politically.

A number of feminists have adapted Lukács’ idea for the relationship between men and women. In Harding’s (1991) influential treatment, a standpoint is an “objective location” in society. Women’s lives constitute one such location. Only few standpoints are able to meet the standard of “strong objectivity”: to be strongly objective a standpoint must not only be highly truth-conducive but also be known to be so to its occupants. The latter knowledge must be based on systematic research into the social history of the standpoint. For instance, Harding insists that natural science can be strongly objective only if its core assumptions have been investigated and found acceptable by social-scientific research. She even goes so far as to write that “the natural sciences are illuminatingly conceptualized as part of the social sciences” (1991: 14). Strong objectivity contrasts with “weak objectivity” or “objectivism.” The latter position reduces objectivity to mere value-neutrality. As far as knowledge of the social world and some parts of the natural world are concerned, women’s standpoint is epistemically superior to men’s. This is because women are “strangers” to the social order; women are “outsiders within”; women have a strong interest in understanding oppressive social circumstances; women experiment with social reality; women are closer to basic material conditions; and women are forced to mediate the dualism of nature and culture.

Once the role of standpoints is understood, Harding suggests, it is easy to see what is wrong about classical epistemology. Classical epistemology assumes that knowers are “interchangeable,” and that social position and one’s embodiment are irrelevant to one’s epistemic position. Classical epistemology is therefore unable to support an epistemic case for affirmative action. Standpoint theory is a diagnosis and a criticism of this failing.

Naturalistic Social Epistemology

John Hardwig (1985, 1991) raises the possibility that the existence of our most cherished knowledge in the natural sciences remains obscure as long as we stick to the idea—central to classical epistemology—of individual knowers. Hardwig’s example is a highly collaborative experiment in high-energy physics of the 1980s. This research resulted in an article with 99 co-authors, many of which will “not even know how a given number in the article was arrived at” (1985: 347). Needless to say, producing the data for such a joint paper presupposes that scientists exchange information, and that
they take each others’ reports concerning measurements as evidence for these measurements. Empirically, it could not be otherwise. It is clear that such experiments could not be done by one person. None of the participating physicists could replace his knowledge based on testimony with knowledge based on perception—to do so would require too many lifetimes.

Hardwig brings out the importance of “epistemic dependence” to knowledge by asking who should be said to know the results reported in the co-authored physics paper. There seem to be three alternatives. Hardwig does not label them; I suggest “strict individualism,” “relaxed individualism,” and “communitarianism.” Strict individualism insists that knowledge is the possession of the individual, and that knowledge presupposes evidence based on one’s own “onboard resources.” A philosopher adopting this option would have to deny that anyone knows the results of the physicists’ paper. Relaxed individualism allows that individuals know “vicariously,” that is, “without possessing the evidence for the truth of what [they] know, and perhaps without even fully understanding what [they] know.” Communitarianism sees the community as the primary knower. Thus it is the community of physicists, perhaps the 99 co-authors, that constitutes the epistemic subject of the knowledge reported in the paper. Communitarianism allows us to retain the idea that a knower must be in “direct” possession of the evidence but it breaks with the assumption that such a knower must be, or can be, an individual. Hardwig regards both relaxed individualism and communitarianism as viable options, but he favors the latter: “The latter conclusion may be the more epistemologically palatable; for it enables us to save the old and important idea that knowing a proposition required understanding the proposition and possessing the relevant evidence for its truth” (1985: 349).

**Conclusion**

While no one has done more for establishing SE as a vibrant field in epistemology than Goldman, in some respects his vision of the field is too limiting. He shows too little interest in social-historical and sociological studies of (scientific) knowledge and subsumes diagnostic approaches under revisionism, where this is couched as postmodernism and deconstructionism. It is true that some diagnostic theories end up rejecting the pillars of classical epistemology. But that should not disqualify them from being “real epistemology.” If SE is to grow not just in width but also in depth, then diagnostic contributions will have to become more central in the future.

**References**


---

**Further Reading**

Two journals are dedicated to all areas of SE: *Social Epistemology* and *Episteme*.

METAEPISTEMOLOGICAL ISSUES

Brandom, R. (1994) *Making it Explicit: Reasoning, Representing, and Discursive Commitment*, Cambridge, MA: Harvard University Press. (Although Brandom is not normally classified as a social epistemologist, there are numerous themes here that are relevant for the foundations of SE.)

Coady, C. A. (1992) *Testimony: A Philosophical Study*, Oxford: Clarendon Press. (In addition to being the modern study of testimony, Coady’s book also contains many themes that are relevant for SE more generally.)


Fuller, S. (1988) *Social Epistemology*, Bloomington and Indianapolis: Indiana University Press. (Fuller’s version of SE is a form of science policy with the goal of making science more democratic and accountable to the public.)

Gilbert, M. (1989) *Social Facts*, London: Routledge. (Gilbert’s defense of collective intentionality is a key resource for authors defending the idea of collective knowers.)

Hutchins, E. (1995) *Cognition in the Wild*, Cambridge, MA: MIT Press. (This is one of the most important empirical studies defending the idea that cognition is embodied and distributed over many individuals.)


Tuomela, R. (1995) *The Philosophy of Sociality: The Shared Point of View*, Oxford: Oxford University Press. (One of the key texts in social ontology, this book also contains discussions of group knowledge and group justification.)