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Why Social Epistemology Is *Real* Epistemology

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1. INTRODUCTION

What is social epistemology? Or what might it be? According to one perspective, social epistemology is a branch of traditional epistemology that studies epistemic properties of individuals that arise from their relations to others, as well as epistemic properties of groups or social systems. A simple example (of the first sort) is the transmission of knowledge or justification from one person to another. Studying such interpersonal epistemic relations is a legitimate part of epistemology. A very different perspective would associate ‘social epistemology’ with movements in postmodernism, social studies of science, or cultural studies that aim to replace traditional epistemology with radically different questions, premises, or procedures. Although these enquiries examine the social contexts of belief and thought, they generally seek to debunk or reconfigure conventional epistemic concepts rather than illuminate the nature and conditions of epistemic success or failure. Under the first construal social epistemology is a bona fide part of the mainstream, and hence ‘real’ epistemology. Under the second construal, it is not part of epistemology at all. If protagonists of the latter-day movements marketed their products under the guise of epistemology, they would be imposters. Their products are not real epistemology.

‘Is social epistemology *real* epistemology?’ is a question posed by William Alston (2005). He raises it *en passant*, while noting that epistemology’s boundaries are controversial, drawn differently by different thinkers. To illustrate his point, he suggests that much of the material in my book on social epistemology, *Knowledge in a Social World* (Goldman 1999), ‘would be rejected by many contemporary epistemologists as “not real epistemology”’ (Alston 2005: 5). The epistemologists in question, says Alston, would relegate much of this so-called social epistemology to sociology, social psychology, or other social sciences, or perhaps to the philosophical foundations thereof.



What about this claim that Alston attributes to imagined readers of *Knowledge in a Social World*? Would these readers be correct in saying that the material in question does not qualify as ‘real’ epistemology? Setting aside the specifics of *Knowledge in a Social World*, which theoretical enterprises that fly—or might fly—under the flag of ‘social epistemology’ deserve the label ‘epistemology’ and which ones do not? Wherein does social epistemology differ or diverge from traditional epistemology, and what differences and divergences would carry social epistemology beyond the pale?

2. THREE CONCEPTIONS OF SOCIAL EPISTEMOLOGY

I shall divide conceptions of social epistemology (SE) into three types: (1) *revisionism*, (2) *preservationism*, and (3) *expansionism*. These conceptions will be framed in terms of their relationships to traditional or mainstream epistemology. I shall agree that revisionist SE should not be classified as ‘real’ epistemology. By contrast, both preservationist and expansionist forms of SE deserve to be considered bona fide branches of epistemology. The difference between the two is that preservationist SE clings more closely to traditional paradigms of mainstream epistemology whereas expansionist SE ventures a bit further into unfamiliar terrain.¹

As a prolegomenon to an assessment of SE’s prospects, then, let us consider central features of epistemology ‘as we know it’ (in the analytic tradition). Here is a catalogue of such features. (A) The epistemic agents of traditional epistemology are exclusively individuals. (B) Epistemology focuses on the study of epistemic evaluation or normativity, represented by such evaluative concepts as justifiedness, rationality, and knowledge. Traditional epistemology asks such questions as how individuals can acquire knowledge and maintain justified or rational credal states. (C) Traditional epistemology assumes that the normative standards of rationality and justifiedness are not merely conventional or relativistic, but have some sort of objective validity. (D) The central notions of epistemic attainment—knowledge and justification, for example—either entail truth or are closely linked to truth. A known proposition must be true; and justified beliefs, according to many mainstream views, are in some sense likely to be true. (E) Truth is assumed to be an objective, largely mind-independent, affair. (F) Traditional epistemology takes its central business to be the critical examination of doxastic ‘decision-making’ (DDM): adopting, retaining, or revising one’s beliefs and other doxastic attitudes. Let us call the foregoing assumptions the *core* assumptions

¹ It is really preferable to view preservationist and expansionist conceptions as representing *degrees* of distance or departure from traditional epistemology. There are no sharp dividing lines here. But often it is useful to talk in terms of binary or discrete categories, even when the underlying reality is messier than such categorization suggests.

of mainstream epistemology. The three conceptions of social epistemology can then be elucidated and distinguished by their different postures vis-à-vis these core assumptions.

3. REVISIONISM

The writers subsumed under the heading of revisionism rarely employ the label ‘social epistemology.’² Nonetheless, since they typically study science and other forms of ‘knowledge’-seeking, and they certainly emphasize the social character of these pursuits, mainstream philosophers might view their approaches as aspiring forms of epistemology. Under revisionism I include postmodernism, deconstructionism, social constructionism, and various social studies of science, including the ‘strong programme’ in sociology of science. These movements share a staunch rejection of many if not all of the core tenets of traditional epistemology. These rejections, moreover, are inspired by the social character of the phenomena they study.

A few philosophers such as Richard Rorty fall into the same category, sometimes waxing emphatic about the bankruptcy of traditional epistemology. Rorty (1979) declared the ‘death’ of epistemology and proposed a vague replacement for it in the form of a ‘conversation of mankind’. His mantra was to ‘keep the conversation going rather than to find objective truth’ (1979: 377).

One strand of social constructionism holds that truths or facts are not in or of the world; they are not ‘out there’ to be discovered but are mere social fabrications or constructions (Latour and Woolgar 1986). As Steven Shapin puts it, ‘truth is a social institution’ (1994: 6). While revisionists often use the language of ‘knowledge,’ they don’t understand it to be a truth-entailing, or factive, state. In their lexicon knowledge is simply whatever is believed, or perhaps ‘institutionalized’ belief. On the topic of rationality, we find proponents of the strong programme rejecting objectivist rationality, declaring that ‘there are no context-free or super-cultural norms of rationality’ (Barnes and Bloor 1982: 27). On the question of epistemic agents, at least a few revisionists deny that individuals are epistemic agents at all. Only groups or communities qualify as knowers, according to Lynn Hankinson-Nelson (1993):

In suggesting that it is communities that construct and acquire knowledge, I do not mean . . . that what comes to be recognized or ‘certified’ as knowledge is the result of collaboration between . . . individuals who, *as individuals*, know in some logically or empirically ‘prior’ sense . . . [T]he collaborators, the consensus achievers, and, in more general terms, the agents who generate knowledge are communities and subcommunities, not individuals. (Hankinson-Nelson 1993: 124)

² An exception is Steve Fuller, who used ‘social epistemology’ both as the title of his first book (Fuller 1988) and as the title of a journal he founded. Another revisionist, Martin Kusch (2002), uses a somewhat similar phrase for his position: ‘communitarian epistemology’.

Thus, revisionists dispute many of the main tenets of traditional epistemology. They might be called ‘social epistemologists’ by courtesy, but there is a wide gulf between their questions and those that traditionally go by the name ‘epistemology’. Researchers in these traditions are often absorbed with case studies in history and/or sociology of science, with only scant attention to epistemic implications as mainstream epistemologists would conceive of them. Several writers in the sociology of science advance political-military accounts of science, aiming to displace the traditional notion that science is a province of reason or rationality. Shapin and Schaffer remark that in scientific disputes, ‘[h]e who has the most, and the most powerful, allies wins’ (1985: 342). Latour (1987: 31) offers a detailed analysis of how scientific controversies in science are conducted by enlisting the support of forceful ‘allies’ such as prominent journals, Nobel prize winners, or granting agencies. What does this show about the *epistemic* properties of science? Latour’s political-military account is intended to debunk or undercut the alleged role of evidence and truth in science.³ This makes it natural for mainstream epistemologists to deny that such approaches constitute genuine epistemology, because little or no serious epistemic business gets conducted, as mainstreamers see it.⁴

Our attempt to draw a firm boundary between revisionism and mainstream epistemology may be a bit quick. Recent developments in epistemology and philosophy of language may argue for greater nuance. Is it true, for example, that mainstream epistemology firmly and definitively rejects relativism? Recently the scene has shifted a bit. There is a ‘New Age’ kind of relativism—as Crispin Wright (2007, 2009) calls it—that has gained popularity within analytic philosophy. Perhaps New Age relativism has much in common with what revisionists like Barnes and Bloor have claimed. If so, there may be less distance between revisionism and mainstream epistemology than suggested above.

However, we should not exaggerate the degree to which the popularity of New Age relativism closes the gap between revisionism and mainstream analytic epistemology. What Wright calls ‘New Age’ relativism is a view championed by John MacFarlane (2005), Max Kolbel (2003), and Peter Lasersohn (2005), among others, that says the following: for a number of regions of discourse

³ Whether it succeeds is another question. I have argued that the activities described by Latour can be reinterpreted in respectably epistemic terms (Goldman 1999: 227–30).

⁴ I am not prepared to endorse this denial in my own voice. For example, can’t one be a legitimate epistemologist and still endorse some type of epistemic relativism about justification or rationality? And what about a philosopher who examines arguments for and against relativism without taking a firm position of her own? Isn’t she doing real epistemology—despite the fact that one of the ‘core’ assumptions of traditional epistemology is the falsity of epistemic relativism? My purpose here, however, is only to explain or reconstruct the probable reasoning of those epistemologists who, according to Alston, would refuse to accept social epistemology as ‘real’ epistemology. I am not necessarily joining them in excommunicating (even) rejectionist social epistemology. Of course, rejectionist social epistemology isn’t the kind I personally favour. But it’s a more delicate question to decide whether it deserves to be considered epistemology at all.

the very same token claim can take different truth values when considered in different *contexts of assessment*. This doctrine is a version of contextualism, except in holding that an utterance's truth value depends on an index that varies with variable characteristics of hypothetical *assessments* (rather than, say, contexts of *use*). In discussing predicates of personal taste, Lasersohn suggests (at first pass) that in analysing a sentence like 'Roller coasters are fun', we let the *character* (in Kaplan's sense) of the predicate 'fun' yield different *contents* in different contexts, depending on the speaker (or some other referent). It might be claimed that the predicate 'fun' has a hidden argument place, filled by some sort of implicit indexical pronoun. Carrying this over to epistemology, New Age relativism would be the thesis that the truth of statements about epistemic justification is assessment-relative, with systems of standards providing the relevant assessment-contextual parameter (Wright 2009: 384). This kind of relativism is attracting non-negligible support among analytic philosophers.

However, as Paul Boghossian points out (2008: 414), New Age relativism is primarily a *semantical* thesis, and as formulated above it does not express everything—or the most important thing—that revisionist relativism means to assert. Revisionist relativism would agree on a central presupposition of New Age relativism, namely, that multiple epistemic systems are possible and justification claims can be assessed relative to any of these different systems. Revisionist relativism, however, also endorses a further thesis, which Boghossian sometimes calls 'epistemic nihilism'. Epistemic nihilism says that 'there are no facts by virtue of which one of these systems is more correct than any of the others' (Boghossian 2006: 73, 2008: 413).⁵ This, I submit, is the core thesis of relativism that revisionists advance and mainstream epistemologists dispute. So, even conceding the respectability of New Age relativism, there remains a significant divide between revisionist and mainstream views in the matter of relativism (in addition to other core differences).

In short, with respect to a body of work in the social sciences and the humanities that is sometimes dubbed 'social epistemology', epistemologists are well within their rights to deny it the status of *real* epistemology. If one is in the demarcation business, there are reasonable boundaries to be drawn.

4. PRESERVATIONISM

Nonetheless, many questions that currently preoccupy mainstream epistemologists or that interested historical epistemologists have an indisputable social

⁵ Another formulation Boghossian offers of the key thesis of classical relativism is a thesis he calls 'Equal Validity': 'There are many radically different [mutually incompatible] yet "equally valid" ways of knowing the world, with science being just one of them' (Boghossian 2006: 2). Again, a claim of equal validity goes beyond any semantical thesis of New Age relativism.

dimension.⁶ Suitable treatments of these questions would preserve the core assumptions of traditional epistemology. So if SE is equated with the sector of epistemology that scrutinizes *these* questions, SE is well within the realm of legitimate, genuine epistemology. I shall illustrate preservationism by reference to three types of activity studied by traditional epistemology, each of which has a social sector. Each social sector of these parts of epistemology qualifies as 'real' epistemology. The three types of activity, in their social variants, are (1) doxastic decision-making with social evidence, (2) gathering social evidence, and (3) speech and communication with an informational purport (assertion, debate, argumentation, etc.).

4.1. Doxastic Decision-Making with Social Evidence

As we have said, the central business of traditional epistemology is the epistemic evaluation of doxastic decision-making (DDM). In general there is nothing social about DDM. Belief-forming and belief-revising processes are not themselves inherently social, and evidence used as a basis for forming and revising beliefs need not involve subject matter (i.e. content) concerning other people either. But one sector of DDM is squarely based on the use of social evidence. Here the contents of the evidential beliefs do concern other people. First, the evidential beliefs may have contents concerning what other people have said or written. Second, the evidential beliefs may have contents concerning other people's opinions (or other psychological states), which the doxastic agent may acquire at second or third hand. In either type of case the doxastic agent uses social evidence.⁷

Two lively portions of current epistemology, *testimony* and *peer disagreement*, focus on the use of social evidence in these ways. Testimony poses the problem: 'When is a doxastic agent justified in accepting another person's testimony?' This is arguably the fundamental topic of social epistemology. It occupied such historical philosophers as Hume (2000) and Reid (1983), and occupies a prominent place in the contemporary literature on epistemic justification

⁶ In addition, there are paradigmatic questions of epistemology to which social *answers* have been offered—even if the questions themselves do not invoke social properties or relations. As long as these answers are defended in a characteristically epistemological fashion, this should also be considered traditional—albeit social—epistemology.

⁷ The term 'evidence' is used here in two different but related senses. First, it can refer to an object or state of affairs in the external world. This is the sense in which we speak of rings in a tree trunk being evidence (for the age of the tree) and of fossils like Lucy and Ida being evidence (about the evolution of Homo Sapiens). In addition to this external sense, there is a second sense of 'evidence' in which it refers to belief, justified belief, or knowledge *about* evidence in the external sense. Thus, to possess, gather, or acquire evidence is to have or acquire beliefs, justified beliefs, or knowledge about external evidence. In this chapter, 'social evidence' is sometimes used in the external sense, referring, for example, to statements uttered or written by other people, or their opinions (states of mind). Other occurrences of 'social evidence' refer to subjective states of a doxastic agent concerning external social evidence. I trust that the intended use will be clear from the context.

(Coady 1992; Fricker 1995; Lackey and Sosa 2006). Peer disagreement raises the question of whether rationality requires one to revise one's belief (or degree of belief) if one finds oneself in disagreement with someone else who shares roughly the same evidence and has comparable cognitive abilities.⁸ Each topic concerns epistemic justification or reasonability, and thereby falls within mainstream epistemology. Moreover, each features social evidence and therefore qualifies as a problem of social epistemology (SE). Hence, *some* social epistemological problems belong to the mainstream agenda. Some parts of SE unambiguously qualify as 'real' epistemology—real because they preserve or exemplify the tradition.

Notice that although different epistemologists take different stances in the dispute over peer disagreement, they all appeal to DDM principles to defend their positions. Those who advocate 'splitting the difference' with one's peer (Christensen 2007; Elga 2007) tend to appeal to Bayesian principles, whereas non-splitters tend to appeal to other epistemic principles (Kelly forthcoming; Sosa this volume; Lackey this volume; Feldman and Warfield 2010). There is no mystery, then, about how social and traditional epistemology can comfortably intersect. Roughly similar SE work in a preservationist mold antecedes the current debate over peer disagreement. This includes Lehrer and Wagner's (1981) model of rational consensus, in which agents assign levels of trust to self and others and update their opinions by taking account of others' opinions, weighted by the degrees of trust they are accorded. This model also treats agents as consumers of social evidence.

4.2. Gathering Social Evidence

Another important epistemic activity in addition to DDM is deciding whether and how to gather *new* evidence. Such evidence gathering is variously called 'enquiry' or 'investigation.' In science it includes the design and implementation of tests, measurements, and experiments. Recent epistemology, narrowly construed, does not display extensive discussion of the theory of evidence gathering, investigation, or experiment. But there is plenty of discussion in the philosophy-of-science branch of epistemology. In a memorable phase of philosophy of science Karl Popper (1962) defended a falsificationist approach to scientific experimentation over a verificationist approach. Another sample admonition in the theory of testing, endorsed by assorted philosophers of science, is the desideratum of seeking a *variety* of evidence. This maxim flows from the thesis, shared by many confirmation theorists, that more varied evidence lends greater confirmation to a hypothesis than less varied evidence. A more recent tradition in the theory of evidence is embedded in the Bayesian approach, especially Bayesian decision

⁸ Two volumes dedicated to these questions are Lackey and Sosa (2006) and Feldman and Warfield (2010). Most of the articles in the present volume also address these questions.

theory. I. J. Good (1967) explained the value of gathering new evidence in terms of increased (subjectively) expected utility, assuming that the cost of getting such evidence is negligible (cf. Skyrms 1990).

Here is another toy principle based on Bayesian principles, but where the assumptions about probability are not exclusively subjectivist and the expected benefits are purely epistemic. If it is known that one possible test that could provide evidence on a given question has a more extreme likelihood ratio than an alternative test, the more probative and hence choice-worthy test is the former, more discriminating, test. At least it is preferable if the agent uses conditionalization to update his probability assignments to the target hypothesis. This principle can be rationalized in terms of a ‘truth acquisitional’ goal if certain assumptions are made about the measurement of truth possession and the accuracy of the agent’s subjective likelihoods. A greater (objectively) expected increase in truth possession is associated with a more discriminating test than with a less discriminating test.⁹

To illustrate the last-mentioned result, suppose you want to gather more evidence about whether it will rain tomorrow. Two possible tests are available to you: listening to a weather report and flipping a coin. Concerning the weather-report test, suppose you antecedently estimate the likelihood of hearing a rain prediction on the weather report given that it is going to rain as .90, and you estimate the likelihood of hearing a rain prediction given that it is *not* going to rain as .15. Then the likelihood ratio of hearing a rain prediction using the weather report test is $6:1 = 6$. Turning to the coin-flipping test, assume that your subjective estimate of the likelihood of heads given that it is going to rain is .50, and you make the same estimate of the likelihood of heads given that it isn’t going to rain. Then the likelihood ratio of the heads outcome for the coin toss is 1. Obviously, the likelihood ratio for the weather report test is more extreme than for the coin-flipping test. Hence, according to the result reported above, if all of your likelihood estimates are *accurate* and you apply (Bayesian) conditionalization, there is a larger expected increase in truth-possession from the weather-report test than from the coin-tossing test. (The requirement of accuracy, of course, presupposes both subjective and objective likelihoods.) This comparison of the two tests in terms of their expected increase in truth-possession holds no matter what your *prior* subjective degree of belief may be vis-à-vis rain (assuming it is neither 0 nor 1). The commonsensical upshot is that not all tests are equal in terms of their prospects for enhancing degree of truth possession.

⁹ See Goldman and Shaked (1991). A proof of the relevant theorem appears in the reprinted version of the paper. The proof presupposes a linear measure of truth possession, which many would question because it isn’t a ‘proper scoring rule’. Nonetheless, other approaches to epistemic utility might offer related rationales for preferring one test to others (Maher 1990; Fallis 2007). Any such rationale would suffice for present (merely illustrative) purposes, so the argument does not depend essentially on the assumptions used in the Goldman–Shaked analysis.

Despite these examples, there are epistemologists who deny that evidence gathering is a proper subject for epistemological assessment. Richard Feldman (2000), in particular, resists this idea. He seems to hold that the only fundamental epistemic goal is to have reasonable beliefs, and this does not require evidence gathering.

If the fundamental epistemic goal is just to have reasonable beliefs, then nothing about evidence gathering techniques or the like follows as a means to that goal. This goal just has implications concerning what attitudes we ought to take given the evidence we have. By seeking out new evidence concerning some important proposition and then believing what that evidence supports, I don't do a better job of achieving the goal of believing reasonably. I achieve that goal at any moment by believing what is then supported by my evidence. It's surely true that there are times when one would be best off finding new evidence. But this always turns on what opinions one has, what one cares about, and other non-epistemic factors. As I see it, these are prudential or moral matters, not strictly epistemic matters. (Feldman 2000: 188–9)

What Feldman means by 'reasonable belief' seems to be reasonable *degree of belief*, where this can range from a strongly confident belief to complete agnosticism. Either type of state is as good as the other, if both are reasonable. This implies that avoiding or getting rid of evidence does not hurt one's evidential state. Getting rid of evidence implies that one must choose a different degree of belief, but one can still be as reasonable as before, and this is all that counts, according to Feldman.

The evidentialist principles stated here imply that one can get oneself into a highly rational state by ridding oneself of as much evidence as one can and then suspending judgment about virtually everything that comes to mind By believing very little, he'll then be highly rational according to evidentialist standards But that seems to be exactly the right conclusion. Once the person has lost his evidence, he has no reasons to believe much, and he'd be unreasonable if he did believe things that would have been well-justified for him had he been in more normal circumstances. (2000: 189–90)

To me, however, this indicates that Feldman's account of our fundamental epistemic goals is inadequate. It is easiest to show this by considering a case where one's *practical* interest requires something more than just reasonable degree of belief (understood as being satisfiable by reasonable agnosticism). The practical goal is best attained by an epistemic state that consists of either *true* (categorical) *belief* or a high degree-of-belief (HDOB) in a truth. Since *true* belief or HDOB is unlikely to be attained without having good reason or justification for it, the appropriate epistemically valuable state to obtain is a reasonable *belief* or *HDOB*, not simply reasonable agnosticism.

To illustrate, suppose that on my return from a shopping trip, I discover that my credit card is missing, and I evidently left it at one of two stores where I used the card while shopping. Where did I leave it? At the moment, I am agnostic about where I left it; that is, I am agnostic with respect to each of the propositions

'The card is at store X' and 'The card is at store Y'. Suppose that the two stores are at opposite ends of town, so I will avoid wasting my time if I make a trip to only one of them, not both. This can reliably be achieved if I form a true belief (or true HDOB) about where the card is. It cannot be reliably achieved if I remain agnostic about where the card is. (If I have to flip a coin to decide which store to return to first, there will be only a 50–50 chance of choosing the right one; so chances are even that I will have to return to both. On the other hand, if I get a true belief or true HDOB, I won't need to flip a coin; I can go to the right store straightaway and achieve the practical goal: retrieval of the card.) Of course, the way to obtain a true belief (or HDOB) of the requisite sort is to obtain a reasonable belief of this kind, assuming that reasonable beliefs are generally true. To form a reasonable belief about where I left the credit card, what I clearly need to do is gather some pertinent evidence. For example, I should telephone the two stores and inquire about my lost credit card. As this case shows, practical interests commonly dictate the desirability of having epistemic states that are not merely reasonable degrees-of-belief (DOBs) but reasonable *high* DOBs (HDOBs). And getting such epistemic states commonly requires gathering evidence. Not just gathering any old evidence, but gathering *enough* and *suitable* evidence. It is appropriate, therefore, for epistemology to counsel epistemic agents in the execution of this ubiquitous task. (Bad methods of evidence gathering, such as confirmation bias, should be avoided.)

What of Feldman's point that whether and how to get new evidence always turns on prudential or moral considerations, hence are not 'strictly' epistemic questions? He writes:

There are cases in which one can spend one's time gathering evidence about propositions concerning inconsequential and trivial propositions or about more weighty matters. Evidentialism provides no guidance about what to do. As I see it, this is not a weakness of evidentialism, since such choices are not to be made on epistemic grounds. (2000: 189)

Two comments are pertinent here. Undoubtedly, which topics deserve evidence-gathering attention or energy is often a purely practical matter, not an epistemic one. Nonetheless, *given* that topic T deserves attention or energy, questions about which kinds of evidence are relevant (and maximally helpful) and how to gather such evidence come into play. These choices are squarely epistemic, at least largely so. Second, even if a residue of the practical infuses these decisions (e.g. how much money and time to devote to evidence gathering), recent epistemology has abundantly shown that epistemic matters in general cannot be divorced from the practical. In the analysis of both knowledge and justification, proponents of the 'pragmatic encroachment' thesis have persuasively argued that standards of epistemic assessment are raised and lowered as a function of practical interests (DeRose 1995; Stanley 2005; Fantl and McGrath 2002). Finding that similar pragmatic factors play a role in evidence gathering should not remove this type of activity from the epistemic sphere.

We have not said anything yet about *social* evidence gathering specifically. This is a special province of SE under my proposal. It is not essential to SE's viability that there be wholly distinct principles of evidence gathering (or doxastic decision-making) for the social domain. There may be only general principles of evidence gathering that apply equally to social and non-social domains. This would not diminish the interest of their application to the social-evidence domain.

However, there may well be distinctive aspects of evidence gathering in the social domain. One is the choice among experts (or putative experts) from whom to solicit opinions. Sometimes one would like to know what the world's leading experts think on a given subject. Sometimes one might be content with getting an opinion from a reasonably competent expert, for example, one's personal doctor or dentist. In both cases, however, the question arises of who meets the specified condition. This is often difficult to determine, especially if the field is a relatively arcane one and the evidence-seeker is a layperson who is largely ignorant of the field (Goldman 2001). Since such a person has no (confident) opinions of his own in the area, he cannot determine various candidates' expertise by comparing their answers to his own in order to measure their accuracy. Should he seek meta-experts to tell him who are the experts? How shall he identify the meta-experts? This is a sample problem specific to SE, which can be viewed as a problem of social evidence gathering.

Special problems in the area of social evidence gathering may also be connected with issues that arise in the peer disagreement literature. In judging whether or to what extent another person is a peer (that is, has the same intellectual competences as oneself), is it legitimate to rely on one's own first-order beliefs to determine the other's competence—and hence trustworthiness as a source to employ for gaining *additional* evidence? If one uses same-mindedness as a criterion of trustworthiness, doesn't one run the risk of consulting those who share the same biases, false theories, or bad company that plague one's own first-order belief set? Does this mean that one should set aside one's first-order beliefs in deciding who are reliable sources of further evidence and turn instead to higher-order evidence about belief-forming methods and sources? Some epistemologists argue that higher-order evidence brings special problems of its own into the picture (Kelly forthcoming; Christensen forthcoming).

4.3. The Social Epistemology of Speech and Communication

Belief formation and evidence gathering are the most commonly discussed epistemic activities but they don't exhaust the field. Another type of social-epistemic activity is making statements and engaging in argumentative exchange. Such speech activities are social, of course, and the analysis of their epistemic properties is naturally assigned to SE.

A prominent topic in this corner of current epistemology is the question, 'What is the norm of assertion?' Specifically, what epistemic (or psychological) condition

must one satisfy to be entitled to assert something (to others)? Knowledge is one candidate answer,¹⁰ and others include true belief, justified belief, adequate evidence, and so forth. What kind of norm is sought here? It is assumed that it is an epistemic norm, a norm that ‘comes’ from epistemology rather than ethics or conversational practice, for example. But there is no incompatibility in saying that the source of the norm straddles two categories. A Gricean (1989) perspective suggests that some norms of speech arise from a cooperative enterprise for the social sharing of information (see also Craig 1990; Williams 2002). This cooperative enterprise is, effectively, a social-epistemic enterprise, so one can view the norms as having both a linguistic and an epistemological origin.

Turning from the simple act of assertion, consider more complex speech activities that invite epistemological attention, especially argumentative exchange. Argumentation and dialectic have long been part of the epistemological mix, though often more at the periphery than the centre. Sometimes this topic has been treated under the heading of logic rather than epistemology. Our interest, of course, is its suitability under the latter heading.

A successful dialectical approach to justification would position a social activity at a centre of epistemological analysis. One gets glimmers or whispers of such an approach in a variety of epistemological writings. But such an approach has not been thoroughly developed, and I myself think that the prospects are gloomy. Nonetheless, because it *would* be very important for SE if such an approach succeeded, it is worth assessing the prospects.

A purely dialectical approach to justification might say that a belief of S is justified if and only if S can satisfactorily respond to critics or challengers, to their requests or demands for reasons or evidence. Approaches to justification of this ilk appear in the work of David Annis (1978), Keith Lehrer (1990), and Michael Williams (2001).¹¹ It also has Wittgensteinian (1969) resonances. None of these writers, however, specifically labels his approach a ‘dialectical’ theory. Annis and Williams favour the term ‘contextualism’. Alston (1989: 236), while not endorsing a dialectical analysis of justification, argues that our justification concept developed because of the practice of challenging one another’s belief credentials and responding to such challenges.

How might a dialectical analysis of justifiedness go? To begin with, we should clarify the point that the analysandum, *being justified*, is a property of doxastic states or doxastic agents. Call this property *personal justification*. According to the envisaged approach, it is to be analysed in terms of a possessor’s disposition to interact verbally with others in some specified fashion. The requisite verbal

¹⁰ See Unger (1975), Williamson (1996, 2000), DeRose (2002), and Hawthorne (2004).

¹¹ Why doesn’t Robert Brandom’s (2000) discursive or communicational theory appear on this list? Because Brandom’s targets of analysis are *semantical* concepts, not *epistemic* ones. He is not, fundamentally, trying to illuminate epistemic justifiedness.

interaction may be called *interpersonal justifying*, something one does *to* or *with* interlocutors. A sample dialectical analysis might run as follows:

(IPJ) S is personally justified in believing P if and only if S can or would succeed in *interpersonally justifying* P to (actual or potential) challengers.

IPJ isn't a bald persuasion theory. S's being justified in believing P, according to IPJ, does not require that S could persuade (erstwhile) challengers of P to believe it. A persuasion theory would be both too strong and too weak. Too strong because challengers might be too stubborn or thick-headed to be persuaded even when offered robust evidence. Such intransigence should not keep S from being justified. Too weak because challengers might accede to a speaker's claim that P through excessive gullibility or deference. Such responses would not confer genuine justification on S's belief.

IPJ does not confront these liabilities, but it still faces hurdles. What does it mean to justify a proposition *to* another person? Is interpersonal justification to be defined in terms of personal justification, for example, as 'bringing it about (by verbal means) that the hearer is (personally) justified'? That would introduce a blatant circularity into the approach. One might try to offer a non-circular definition of interpersonal justification, but exactly how this would go is unclear. The definition would have to say things about deductive or inductive relationships between the asserted proposition (the 'conclusion') and the reasons the speaker might offer in its support. It would also have to say things about the status of those reasons or premises *for* the hearers or interlocutors. What is the requisite status? This is where things get murky. Moreover, there are problems of informational asymmetries between speaker and hearers. Suppose the speaker possesses (undefeated) counterevidence to his conclusion but doesn't disclose this evidence to the hearers. The hearers may be justified in believing P by what the speaker says, while the speaker himself is (personally) *unjustified*, in virtue of the defeaters he possesses.

These reflections make me dubious of the viability of the dialectical approach to being justified. It isn't *this* easy to establish social factors at the centre of the epistemic realm. However, there are neighbouring topics in epistemology to which speech and argumentation are germane. The philosophical study of dialectic has a long and substantial history, often subsumed under the heading of logic, or an autonomous field called 'informal logic'. I propose instead that the normative theory of dialogue, dialectic, debate, or argumentation be viewed as part of social epistemology.¹² Most contributors to this literature do not offer

¹² The literature on dialectical, or dialogical, approaches to logic and argumentation is substantial. It ranges from treatments oriented toward formal logic to treatments attuned to informal logic, natural conversation, and norms of argumentation. Prominent specimens of this work include Hamblin (1970), Barth and Krabbe (1982), Lorenzen (2001), Toulmin (1958), and Woods et al. (2000).

explicitly epistemological perspectives, but it is the most fruitful one, I submit (Goldman 1994, 1999: ch. 5, 2003).

Here is a simple illustration of how a social epistemological approach to argumentation helps makes sense of the norms of interpersonal reasoning. A principal task in this field is to explain which forms of argumentation are in some sense legitimate and which are not. Argumentative legitimacy is not equivalent to validity (or inductive strength). Indeed, a prominent fact that needs explaining is that *enthymemes*, which are invalid arguments as they stand, are commonly used in everyday speech; yet nobody finds them objectionable. What sense can be made of this? It does not suffice to observe that enthymemes can be turned into valid arguments by supplying additional premises. That's true, of course, but not every enthymeme would be acceptable in real discourse. Acceptable ones must bear other suitable properties. Which properties? A straightforward account and explanation is an epistemic one (Goldman 2003: 60). Suppose that the usual aim of argumentative discourse is to justify a proposition to one's intended audience, that is, change the epistemic state of the hearers so that they are (newly) justified in believing the conclusion. Assume further that in assessing the plausibility of a proffered conclusion, a hearer will not rely exclusively on explicitly stated premises. She will marshal all relevant evidence at her disposal, at least evidence that readily comes to mind. Finally, suppose that on a given occasion evidence E is relevant to the speaker's conclusion, and E would be perfectly appropriate to include as a premise in his argument. As the speaker realizes, however, E is already widely known in the discursive community. Then the speaker is entitled to assume that his hearers already know E and would think of it in contemplating his proffered conclusion. Why, then, bother to mention E? Conversational efficiency dictates otherwise, and epistemic considerations concur. A hearer will use E to help draw the same conclusion whether the speaker mentions it or not, and his *justifiedness* vis-à-vis the conclusion will be the same. Thus, presentation of the enthymematic argument, with E left unexpressed, is entirely in order. But this is so because of the audience's epistemic circumstances and their interrelationship with the speaker's epistemic end. Thus, an otherwise puzzling feature of discourse is nicely illuminated when viewed from a social-epistemic perspective.

In summary, many forms of SE are unquestionably instances of traditional epistemology. It would be arbitrary and misguided of epistemologists to contend that *no part* or instance of SE is real epistemology.¹³ However, if SE were restricted to the two topics identified earlier—testimony and peer disagreement—or even if we add to SE's agenda the other tasks sketched in this section, many *other* projects described in *Knowledge in a Social World* would remain beyond the pale.

¹³ Alston doesn't suggest that any epistemologists hold precisely this position. He says that many would regard 'much' of the material in *Knowledge in a Social World*—not all of it—as not real epistemology.

They would still not qualify as ‘real’ epistemology. Thus, restricting SE to the problems and projects that have clear precedents in historical and mainstream epistemology might still be unnecessarily limiting. In the rest of the chapter I argue for extending it beyond these confines.

5. EXPANSIONIST SOCIAL EPISTEMOLOGY

I argue for expanding SE’s scope by adding (at least) two or three further topics to its agenda. These are not explicitly addressed in traditional epistemology but are sufficiently continuous with traditional epistemology that they deserve inclusion under the heading of ‘real’ epistemology. The topics are, first, the epistemic properties of group (or collective) doxastic agents and, second, the influence of social ‘systems’ and their policies on epistemic outcomes.

5.1. Collective Doxastic Agents

Numerous philosophers in recent years have endorsed the idea of recognizing group or collective entities as subjects of intentional attitudes (Gilbert 1989; Bratman 1993; Tuomela 1990; Schmitt 1994a; Pettit 2003; List and Pettit forthcoming). Many of these philosophers are primarily interested either in the ontology of group agents or in aspects of practical reasoning and group commitment. Intentions have tended to be their favoured intentional attitude. But there are equally good reasons to include attitudes of affirmation and denial of factual propositions, that is, judging propositions to be true or false, as properties of collective subjects. These are attitudes with a mind-to-world direction of fit rather than a world-to-mind direction of fit. In everyday life we often speak of collective entities like committees, juries, courts, scientific panels, and governments as making judgements, or taking cognitive stances, of one sort or another. Once these kinds of intentional attitudes for collective agents are conceded, shouldn’t we reflect on the epistemic properties of these attitudes? If an individual’s judgements over a set of many propositions can be assessed for their (epistemic) rationality, isn’t it appropriate to make similar assessments of a collective agent’s judgements? And if epistemologists take an interest in cases that seriously challenge what is rational for the individual, shouldn’t epistemologists—social epistemologists—take an interest in cases that seriously challenge what is rational for a collective agent? As we shall see, challenges to rationality (and related normative desiderata) of a collective agent’s judgements are, if anything, even more extensive and sobering than challenges to an individual agent’s judgements.¹⁴

¹⁴ Someone might argue that what makes irrationality interesting for traditional epistemology is the concern with doxastic decision-making processes of individual agents. But collective agents

Worries about collective judgemental rationality were first identified by Kornhauser and Sager (1993) in the context of courts of law. They called this challenge ‘the doctrinal paradox’. Suppose that a three-member court has to render a judgement in a particular breach-of-contract case, and legal doctrine specifies that a defendant is liable for breach of contract if and only if he was legally obliged not to do a certain action but nonetheless did it. Then the court must render judgments on each of several propositions concerning the particular case:

- * The defendant was legally obliged not to do a certain action (first premise)
- * The defendant did the action (second premise).
- * The defendant is liable for breach of contract (conclusion).

How are judgements of the court—a collective body—to be fixed by judgements of its individual members? In other words, how are individual members’ judgements to be *aggregated* into judgements of a single, collective entity (the court)? A natural mode of aggregation is majority vote. Suppose, then, that the three judges make individual judgements on the three propositions as shown in Table 1.1 below. Using the rule of majority vote, these individual judgements are then aggregated into collective judgements by the court as shown in the bottom row of the table. Here the collective agent is inconsistent. It affirms that the defendant was obliged not to act, affirms that he acted, but denies that the defendant is guilty. Given the legal doctrine, this set of judgements is inconsistent. However, each individual judge has a perfectly consistent set of judgements. Thus, there is inconsistency at the group level despite consistency at

Table 1.1. Inconsistent Collective Judgement by Majority Vote

	<i>Obliged not to act</i>	<i>Acted</i>	<i>Breached the contract</i>
<i>Judge 1</i>	True	True	True
<i>Judge 2</i>	True	False	False
<i>Judge 3</i>	False	True	False
<i>Court</i>	True	True	False

don’t really engage in doxastic decision-making, one might claim. So why should a branch of epistemology take any interest in their irrationality? I dispute the assumption that collective agents do not engage in doxastic decision-making (DDM). Although collective agents are (presumably) not distinct centres of consciousness, this does not mean that they don’t engage in DDM. ‘Doxastic decision-making’ refers to whatever processes transpire to bring about, in a subject, a new or revised doxastic state. Which process this is in the case of collective agents is up for grabs. It might just consist in a set of distributed processes in their constituent members, together with an aggregation process that works automatically, without any ‘extra’ individual activity. An assumption that needs to be avoided is that group DDM must involve things like consciousness, attention, or purposefulness *in* the group entity.

the individual level. Assuming, as is standard, that judgemental inconsistency is irrational, irrationality emerges at the group level.

The fly in the ointment, apparently, is the majority vote aggregation procedure. Surely there is another aggregation procedure—probably many of them—that won't create this problem! Unfortunately, this is not the case. There is no judgement aggregation procedure that averts the problem. List and Pettit (2002) first showed this with an impossibility theorem, and several related impossibility theorems have been proved as well (see Dietrich and List 2007, in particular). To illustrate, consider the following reasonable-looking requirements for a judgement aggregation procedure, which would take sets of individual judgements as inputs and generate a set of group judgements as outputs.

Universal Domain. The aggregation function takes as input any possible profile of attitudes towards the propositions on the agenda, assuming that individual attitudes are consistent and complete. [Completeness here means that for every proposition-negation pair on the agenda, the agent has a positive attitude on either the proposition or its negation.]

Collective Rationality. The aggregation function produces as output consistent and complete group attitudes towards propositions on the agenda.

Anonymity. All individuals' attitudes are given equal weight in determining the group attitudes. Formally, the aggregation function is invariant under permutations of any given profile of individual attitudes.

Systematicity. The group attitudes on each proposition depends only on individual attitudes towards it, not on their attitudes towards other propositions, and the same pattern of dependence holds for all propositions.

The surprising impossibility theorem is that no aggregation function satisfies all of these conditions (List and Pettit 2002, List and Pettit forthcoming).

As with other paradoxes in which initially plausible propositions or constraints are found to be jointly unsatisfiable, theorizing proceeds by trying to decide which propositions or constraints to abandon (see List and Pettit forthcoming). The most familiar example in recent epistemology is the sceptical paradox featuring deductive closure for knowledge (Nozick 1981; DeRose 1995). Should the closure principle be rejected, or should another proposition be abandoned instead? In the present case we have a paradox of social epistemic rationality: Can there be systematically rational group agents whose judgements are suitably tied to those of their members? If not, what does this imply about epistemic rationality at the collective level? This question is sufficiently novel relative to epistemological tradition that it cannot be folded into preservationist SE. On the other hand, it is sufficiently continuous with traditional epistemology that it deserves a home somewhere in epistemology. A comfortable home can be found in what I call the expansionist district of SE.

5.2. Epistemic Evaluation of Social Systems

Large chunks of analytic philosophy, epistemology included, focus on everyday thought and talk, often teasing out norms that implicitly govern them. Epistemology should not be restricted, however, to this activity. A broader view of epistemology is present in the epistemology of science, which definitely does not assume that lay practices are the exclusive or final benchmark of anything normative. Philosophers of science are more sympathetic to the *meliorative* project of improving epistemic practices, especially in the scientific arena.

In addition to science, there are other arenas where meliorative epistemological projects make eminently good sense. Many sectors of social life feature practices and institutions ostensibly dedicated to epistemic ends, but where one is entitled to wonder whether prevailing practices and institutions are optimal. Subjecting such practices and institutions to epistemic evaluation is therefore in order. Are they the best practices or institutions (of their type) that can be devised? What alternative practices would work better in epistemic terms? This kind of social epistemological project was advocated in *Knowledge in a Social World*—and might be just the sort of project that Alston’s imagined critics would dismiss as ‘not real’ epistemology. Although I would grant that such projects are infrequently encountered in historical or mainstream epistemology, they would nonetheless be *continuous* with certain traditional epistemological projects. They would apply epistemic criteria of evaluation to admittedly non-standard kinds of objects: not individual agents or even collective doxastic agents, but social systems or policies that have a significant causal impact (for good or ill) on society’s epistemic outcomes.

5.2.1. Legal Adjudication Systems

Legal adjudication systems are an example of the kind of social system I have in mind. While not being collective doxastic agents in their own right, such systems are typically guided by a truth-seeking mission. In the case of the American system, the Supreme Court stated this clearly in *Tehan v. U.S.* (1966), ‘The basic purpose of a trial is the determination of the truth.’ How does the common-law trial system, used in Britain, the United States, and most former Commonwealth countries, go about trying to determine the truth? Which features of its design—at least according to its champions—make it conducive to this end? And how well does the design actually work in promoting a high proportion of accurate verdicts?¹⁵

¹⁵ In *Knowledge in a Social World* I restricted the relevant epistemic outcomes to ‘veritistic’ outcomes, i.e., true belief, false belief, and intermediate degrees of belief associated with the truth. In general what I mean by ‘epistemic outcomes’ in this paper is a wider family of epistemically valued and disvalued states, including justified and unjustified belief, and so forth. In the legal context, however, I am again interested in evaluations in terms of ‘veritistic’ outcomes.

The crucial feature of the common-law system is its adversarial character. Under this system a trial is essentially a structured competition between opposing attorneys (or teams of attorneys) who represent the contending parties. Lawyers are responsible for all of the main initiatives and conduct of the case. They file briefs and motions, gather and prepare the evidence, and at trial their battle of interrogation and argumentation dominates the proceeding. The judge is a referee who enforces conformity with legal requirements by restricting introduction of evidence and constraining counsel's argumentation. The trier of fact, often a jury, remains essentially passive.

Proponents of the adversarial system claim that it's as good an institution as can be devised for achieving the epistemic goal of accuracy of judgements (verdicts). The best way for a court to discover the facts, according to the theory, is for each side to strive as hard as it can to present to the court all evidence that supports its claims. The neutral trier of fact then decides on the basis of the evidence presented. Attorneys are expected to be partisan representatives of their parties, but such partisanship is not inimical to the goal of truth. Dueling partisanship cancels out, so long as the trier of fact is neutral. Juror selection practices are designed to ensure juror neutrality.

The adversarial process partly utilizes the technique of dialectic or argumentation. This allows each side to try to persuade the trier of fact of its favoured conclusion(s) by adducing evidence for its thesis, rebutting the evidence of the opposition, and trying to defeat the defeaters offered by the opposition to its own evidence. What better way could there be for truth to emerge than to produce as much (relevant and reliable) evidence as possible and let the contending parties thrash out what it implies?¹⁶ It is vital, of course, that all of the evidence be provided to the ultimate doxastic decision-maker. In a social fact-finding network, the network is likely to output an accurate verdict only if the network's designated decision-maker—in this case, the jury—receives and weighs all of that evidence. This is precisely how the system is designed to work. Jurors must sit through the entire proceeding and hear all of the evidence. They are not allowed to attend parts of the trial at their whim, choosing to attend only

¹⁶ Section 4.2 may have suggested that doxastic decision-makers must always take the initiative in gathering evidence. But as we see in the legal trial example, social systems can play a crucial role in producing or assembling social evidence relevant to the doxastic task at hand. In legal trials, the court issues subpoenas to relevant witnesses and requires them to appear in court and testify, which involves answering questions posed by attorneys. Witnesses' testimony, of course, is evidence for use by the jury. The jury itself, which is the doxastic agent, does not have to collect such evidence—indeed, they are barred from collecting evidence external to the trial situation. Many other social systems serve a roughly similar evidence-assembling function. Traditional enterprises like entire educational systems and public libraries are intended to do exactly this sort of thing. In the modern era the Internet and its assorted applications serve this function. A salient illustration is the online encyclopedia, Wikipedia. This may be viewed as a project in mass collaboration that uses certain methods to produce a large collection of social evidence (writings produced by multiple authors). How reliable and trustworthy is this evidence? This is another suitable topic for expansionist social epistemology (see Fallis and Sanger 2009).



when their favourite side (e.g. the prosecution) presents its case. They cannot cherry-pick the evidence, something that commonly happens in unstructured evidence-gathering contexts. In informal life people choose which newspapers to read, which talk shows to listen to, which friends to seek opinions from, and so forth, with no external enforcement of balance. The trial process aims to redress this defect in informal evidence gathering, and there are good epistemic grounds for this.

Thus, several features of the common-law system with its adversarial component promise epistemically good consequences. But meliorist social epistemologists should not stop here. They should be alert to other features of the system that may detract from its positive epistemic prospects. Here are two problematic features.

The common-law jury is a lay jury. This contrasts with the composition of the fact-finder under the principal alternative tradition in Western legal systems: the so-called 'inquisitorial' or 'civil law' system found in much of continental Europe. The jury of the civil-law system traditionally consists of professional judges, who jointly occupy the three roles of investigator, judge, and jury. The difference between lay jurors and professional jurors is substantial. In complex cases—for example, product liability cases—there is typically a battle between scientific expert witnesses for the two sides, who give conflicting testimony on highly technical and arcane matters. Untrained and inexperienced jurors find it extremely difficult to assess the relative merits of the two sides of the argument. They are likely to perceive the battle as a deadlock; so they might 'toss a coin' to make a decision. If they do find one side more persuasive, will this be a result of appreciating genuine confirmational relations between evidence and hypotheses? Or will it merely result from rhetorical skill, polish, or impressive-sounding credentials? To generalize the problem, good epistemic upshots are not assured simply by giving doxastic decision-makers lots of evidence, even good-quality evidence. If they do not understand and interpret the import of the evidence correctly, the quality of the doxastic choices may still be poor. For this reason many countries do not use lay juries in complex tort cases.

A second possible weakness of the common-law system can be discerned from a closer look at its adversarial component. Just how good is this competitive procedure from an epistemic point of view? Are there defects hidden in the woodwork? Here is one defect. It is all well and good for both parties to have partisan counsel that serve their respective interests, but does this level the playing field so that truth is likely to emerge? This is the theory behind the adversarial system and what wins it many adherents; but there is a hole in the rationale. Legal representation is not of uniform quality. Some lawyers command higher prices than others, presumably because their skilled efforts produce a higher probability of winning a case than would the efforts of other lawyers. If so, disparities in the quality of legal representation can undermine the system's



reliability. This worry is commonly raised in connection with the poor quality of legal defence provided by public defenders. Whether through inexperience or excessive caseloads, it is generally thought that defendants represented by public defenders incur higher chances of conviction. This probably indicates that the truth may not be adequately served where there is a large disparity in legal resources utilized by the opponents.¹⁷ Under the inquisitorial system, by contrast, where lawyers play a much less significant role in a trial, legal outcomes will be less sensitive to the quality of legal representation.

I don't mean to render an overall judgement about the comparative reliability of the adversarial and inquisitorial systems of legal adjudication. Obviously, these are highly complex matters. The simple point being made is that there is work to be done in assessing the reliability properties of different legal adjudication systems. Now reliability assessments are a species of epistemological assessments.¹⁸ A lot of epistemology's work is concerned with assessing psychological and formal 'objects' in terms of their truth-conduciveness. It's an epistemological question whether the process or faculty of 'intuition' is reliable. It's an epistemological question whether introspection is a genuine process of the mind, and if so whether it is reliable (and over what range of questions about the mind it is reliable). It's an epistemological question whether this or that inductive method, or statistical method, is reliable. To be sure, the project of evaluating social institutions and practices has not, generally speaking, been on the agenda of epistemology. That is why such projects should be assigned to an *expansionist* wing of social epistemology. It is clear, however, that it is naturally assigned to an expansionist wing of social *epistemology*, since the proposed mode of assessment is distinctively epistemological.

Can the quality of a legal adjudication system be assessed in exclusively epistemic terms? Many theorists would dispute this. Aren't considerations like fairness and impartiality crucial to the acceptability of a legal adjudication system? Yet surely fairness and impartiality are moral rather than epistemic factors. The critics would undoubtedly be right in saying that the criteria of a good legal adjudication system are not wholly epistemic. This doesn't show, however, that epistemic criteria are not highly significant, perhaps even paramount. Moreover, when examined closely, it is difficult to pinpoint the content of fairness or impartiality in dispute resolution without ultimately turning to epistemic desiderata (Goldman 1999: 280–1).¹⁹

¹⁷ For further elaboration of this point, see Goldman (1999: 298–300).

¹⁸ For defence of the thesis that many epistemological assessments in mainstream individual epistemology are fundamentally based on reliability considerations (or other truth-linked considerations), see Goldman (1979, 1986).

¹⁹ Let me briefly elaborate this point. Can impartiality be understood as not giving one party *any* advantage over the other in resolving the dispute? This is not quite right. If one party to the dispute has a dozen witnesses on her side of the story of what occurred whereas the opposing party has only two, the system confers an 'advantage' on the former by allowing all witnesses to testify. Does this

5.2.2. *An Epistemic Rationale for Freedom of Speech*

Expansionist SE of the ‘systems’ variety need not be restricted to the evaluation of entire social systems or institutions such as the common-law system of legal adjudication. It can focus equally on selected policy issues found in a social system. Consider, for example, a government’s policy on freedom of speech. Just as many theorists have championed the adversarial system of legal dispute resolution by appeal to epistemic consequences, so it has been argued by both historical and contemporary writers that the diversity of ideas and debates that freedom of speech promotes provides the best way to test and determine their truth. Hence, freedom of speech is the best social arrangement for generating good ‘veritistic’ (true belief) consequences. One famous statement (roughly) to this effect was made in John Stuart Mill’s *On Liberty*:

[T]he peculiar evil of silencing the expression of an opinion is, that it is robbing the human race; posterity as well as the existing generation; those who dissent from the opinion, still more than those who hold it. If the opinion is right, they are deprived of the opportunity of exchanging error for truth; if wrong, they lose, what is almost as great a benefit, the clearer perception and livelier impression of truth, produced by its collision with error. (1962: 142–3)

Similarly, Justice Oliver Wendell Holmes (in a dissenting opinion for the Supreme Court) wrote:

[W]hen men have realized that time has upset many fighting faiths, they may come to believe even more than they believe the very foundations of their own conduct that the ultimate good desired is better reached by free trade in ideas—that the best test of truth is the power of the thought to get itself accepted in the competition of the market. (*Abrams v. United States* 1919)

In other words, the best way for members of society to find and believe the truth is to have a free market for ideas in which speech goes unregulated (by the state). Both Mill and Holmes defend a certain type of state policy toward speech that is clearly couched in terms of epistemic consequences. It is therefore appropriate for social epistemologists to ask whether this epistemic-consequentialist claim is true—whether the deregulation of speech is the best way to maximize the acquisition of true belief (and error avoidance) in society. Although this could hardly be considered a well-recognized or well-trodden problem within

mean that the system is unfair or partial? Certainly not. Does impartiality consist in giving both parties to a dispute an equal chance of winning? This would suggest that flipping a coin would be a good decision procedure; but that would be absurd. A fair or impartial system, then, is better understood as a system that confers no advantage to a party *except* in virtue of her merit-determining properties, i.e. properties in virtue of which she deserves to win. If being on the side of the *truth* (as concerns the material facts of the case) is a principal merit-determining property, then producing decisions in accordance with the truth is what makes a system fair and impartial. According with the truth is, of course, a primary epistemic virtue (Goldman 1999: 290–1).

traditional epistemology, it is a perfectly good problem in expansionist social epistemology.²⁰

5.2.3. Epistemic Approaches to Democracy

As a final example of expansionist SE, consider the epistemic approach to democracy, a subject of active interest in contemporary democratic theory. A driving force behind an important strand of work on the epistemic approach is the Condorcet Jury Theorem, discovered in the eighteenth century. Suppose that members of a group are faced with a binary choice (a ‘yes/no’ question or a ‘true/false’ question) for which there is a correct answer. Condorcet proved that if the group takes a majority vote between the two alternatives, and if the voters all satisfy two conditions, *competence* and *independence*, then the probability that the majority picks the correct alternative increases to one (certainty) as the group size tends to infinity. What is meant by competence and independence? A voter is competent if she is more likely than chance to vote for the correct alternative. Independence is the condition that each vote is cast independently of the others. The Condorcet result is striking in its highly positive conclusion. It implies that large groups are virtually infallible even if their members are only slightly better than chance at selecting the better alternative. If majority judgements are indeed most probably correct in large societies, majoritarian democracy seems to receive strong support from an epistemic perspective.²¹ In other words, democracy can be defended as a good political system not on the basis of some abstract principle of equal rights but because it does particularly well at making correct political decisions (e.g. decisions that best promote the society’s interests).

Here is a way of spelling out the epistemic approach to democracy, one that makes the Condorcet Jury Theorem highly relevant. Jules Coleman and John Ferejohn (1986) propose that democracy might be taken to have epistemic value in virtue of the fact (if it is a fact) that the outcome of a vote provides *evidence* for the proposition that the winning policy or alternative accords with (what Rousseau called) the ‘general will’ (or some other criterion of political rightness). If the conditions specified by the Condorcet Jury Theorem are met, it is plausible that the outcome of a vote does indeed provide evidence that the approved policy accords with the general will.

The usual knock against the Condorcet Jury Theorem is that it reveals that voting per se is not such a significant aspect of democratic procedure. The

²⁰ This is how I have classified it in two earlier treatments of the topic, Goldman and Cox (1996) and Goldman (1999: ch. 7). In both cases it was argued that the strong form of the free-market-for-speech thesis, in which it is defended as a consequence of economic theory, is false. However, the specific position I take on the subject is not at issue here. The only issue here is whether this qualifies as a legitimate question of *epistemology*.

²¹ Franz Dietrich (2007) argues, however, that there is no notion of probability such that the conditions of competence and independence are likely to be satisfied simultaneously.

crucial question is which binary choices are placed before the voters. This is characterized as an issue of *agenda setting*. How do political bodies decide which, of the infinitely many possible questions or choices to place before voters, are actually put on the ballot? Such decisions are generally thought to be a matter of debate and deliberation, in some forum or other, among some political actors or other. The serious work of a political body, then, resides in the conduct of public debate or deliberation: who should be involved in the deliberations and what rules or practices should govern the deliberative process (if the system is to be genuinely democratic). If public deliberation (of an approved kind) is a good epistemic process, an emphasis on deliberation might provide a satisfactory epistemic approach to democracy.

Many democratic theorists focus their attention on public deliberation, including Dewey (1927), Habermas (1996), Rawls (1993, 1997), Cohen (1989, 1997), Gutmann and Thompson (1996), Anderson (2006), Estlund (2008), Misak (2000), and Talisse (2005). Not all of these treatments of public deliberation invite the label ‘epistemic.’ In particular, those who focus on consensus as the aim or mark of good deliberation would not (thereby) earn this label, in my opinion. However, it might be possible to carve out a characterization of *reliable* (i.e. truth-conducive) public deliberation, realizations of which would fulfill the reliability desideratum emphasized by some proponents of the epistemic rationale for democracy (e.g. those inspired by the Condorcet Jury Theorem). I tentatively call this a *reliabilist approach* to democracy, and defenders of this kind of rationale or justification for democracy *reliability democrats*.

I shall briefly sketch the idea I have in mind, although there is neither time nor space to develop it fully. We can imagine groups of people who satisfy certain constraints on responsive democratic deliberation but do not thereby achieve the sought-after ‘epistemic goods,’ that is, reliability or increased reliability. For example, consider a group of ill-informed—indeed misinformed—citizens who set out to argue with one another on political matters and arrive at some sort of (rough) consensus. Everyone is allowed to speak, they listen carefully to one another, and even make epistemically justified doxastic responses to what they hear. Unfortunately, their misguided initial belief states are so bad—either because of a dearth of evidence, or an abundance of misleading evidence—that the deliberative process does not boost their average reliability with respect to the questions discussed. If this is possible (and I think it is), it shows that rational public deliberation is not necessarily sufficient to yield reliable doxastic outputs, or even to increase reliability from a pre-deliberative state. However, it might be possible to add additional constraints on the standards of public deliberation so that if a group satisfied those constraints in addition to the first ones, public deliberation would tend to increase the group’s reliability. Such a set of requirements for democratic deliberation would constitute a reliabilist standard of deliberation.

If a theorist managed to formulate such a set of requirements and showed that satisfying these requirements has reliabilistically beneficial properties for groups, this would be an *epistemological* contribution to democratic theory. It would show, by means of epistemology—indeed, social epistemology—that certain features of a system promote an important epistemic desideratum, that is, reliability, or increased reliability. The Condorcet Jury Theorem makes exactly this kind of contribution, although it does not address public deliberation or debate. Writers who emphasize the way that genuine democracy makes use of ‘situated knowledge’ to improve the community’s overall knowledgeability make roughly this kind of social-epistemological contribution to democratic theory. In other words, they may be reliability democrats in the sense specified above. Elizabeth Anderson (2006) provides a particularly good example of this.²²

Is this kind of contribution to democratic theory a genuine (‘real’) piece of epistemology? It may be helpful here to distinguish between epistemological *theory* and *applied* epistemology, in parallel with the familiar distinction between ethical theory and applied ethics. Showing how some features of public deliberation would contribute to a group’s reliability, thereby providing a basis for a reliabilist rationale for democracy, would be, at a minimum, a piece of *applied* epistemology.²³ The question of how to taxonomize such work is probably not pressing. But it is important to recognize the general category of applied social epistemology, and to be prepared to recognize work in that category as specimens of *real* epistemology.

6. REVISITING THE CRITICS: ‘IT AIN’T REAL EPISTEMOLOGY’

One would expect Alston’s imagined critics to challenge the foregoing examples of expansionist SE. The refrain, again, would be, ‘This isn’t the real McCoy; it isn’t real epistemology’. However, it is not enough to say that these problems are not traditional parts of epistemology; this much has already been conceded. The question is whether these problems are sufficiently *continuous* with traditional epistemology. What would be the critics’ specific bones of contention? Which of the examples adduced above would they dispute, and why?

An obvious and expected line of criticism would concern the choice of ‘targets’. There are only two suitable targets in real epistemology, critics might claim: (1)

²² Another example is Michael Fuerstein (2009), who emphasizes the role of scientific method in democratically appropriate deliberation.

²³ However, we shouldn’t preclude a classification of ‘theoretical’ for possible work of this kind. The Condorcet Jury Theorem is plausibly classified as a theoretical contribution to social epistemology. Another example—directly germane to the topic of group deliberation—is a formal result by Christian List (forthcoming), which doesn’t, however, bear on issues of reliability.

beliefs and other psychological states, and (2) persons who are subjects of these states. Collective or group entities, the critics would continue, are not genuine subjects of psychological states, including doxastic states. So collective agents are not kosher targets for epistemological analysis or assessment. Finally, common-law systems of legal adjudication, free speech policies, and democratic systems are not psychological states or doxastic agents. So none of them is susceptible to being analysed or evaluated in epistemological terms.

What warrants the critics in restricting epistemological analysis or assessment to the two indicated types of targets? One obvious omission is (belief-forming) methods, a classical target of epistemic appraisal. True, collective entities and social systems are not methods. But social systems and institutions are something like methods insofar as methods are commonly evaluated by their (epistemic) upshots or outputs, which is exactly what is proposed for the epistemic evaluation of social systems. It is not claimed, of course, that traditional or mainstream epistemology has a longstanding preoccupation with evaluating social systems. But here we are discussing expansionist SE, which is presumed to mark a non-trivial departure from traditional epistemology. What is required for expansionist SE is that its *modus operandi* be significantly continuous with traditional epistemology. I argue that a reasonable continuity holds between methods and systems, each of which can be evaluated in terms of similar epistemic outputs.

Turn now to collective agents. This is not the occasion to launch a detailed metaphysical defence of collective doxastic agents. But if critics dispute the existence of such entities on ontological grounds, they will be picking a fight with a large community of philosophers. Moreover, this community of philosophers makes a strong case for such entities as the subjects of intentional attitudes. If a good case has been made for plural bearers of *certain* intentional attitudes, such as intentions, what are the principled grounds for denying the existence of plural bearers of doxastic attitudes?

What further objections might the critics have up their sleeves? Recall Alston's speculation that the imagined critics would 'relegate much of this so-called social epistemology to sociology, social psychology, or other social sciences, or perhaps to the philosophical foundations thereof'. Perhaps this would be their next tack. The sample problems I list under the heading of expansionist SE really belong to different disciplines entirely, not to philosophy.

Which disciplines do they have in mind? The problem about the adversarial system of legal adjudication would be assigned to law. The thesis about the epistemic upshots of freedom of speech might be assigned to economics, since proponents of the thesis often claim that it is derivable from economic theory. The epistemic approach to democracy would belong to political theory. The rationality of collective doxastic agents might be assigned to some mix of political theory, legal theory, and economics (especially social choice theory),

since the interesting paradoxes in that field take their inspiration from work in those disciplines. But aren't these disciplines entirely separate from philosophy? Wouldn't the critics therefore be right to say that at least these portions of SE belong outside of philosophy and hence outside of epistemology?

No. I accept the portion of the foregoing claim that says that the specified subjects belong *partly* to the specified, extra-philosophical disciplines. But this does not entail that they are 'entirely separate' from philosophy; I argue that they belong to philosophy *as well*. Several pairs (or triples) of these disciplines *overlap* or *intersect* at these junctures. All of these fields have theoretical or formal wings that intersect with applied portions of theoretical and formal parts of philosophy. It is no coincidence that the principal developers of the theory of group agency, Philip Pettit and Christian List, double as philosophers and political theorists. There are many other sectors of philosophy in which contemporary philosophers plow much the same turf as researchers in nominally different subjects. Logicians are found in both mathematics and philosophy departments. Decision theorists and game theorists are found in psychology, economics, and philosophy. Theoretical physics is debated in joint seminars involving physicists and philosophers. Psychologists, neuroscientists, and philosophers may all give papers at a single conference, and critique one another's claims. Similarly for philosophers and linguists. A recent book on the criminal law was written by an (erstwhile) philosopher of science and reviewed in a legal journal by a lawyer-philosopher team.²⁴ In many of these interdisciplinary interfaces, certain individuals specialize on the theoretical side of the field while others specialize on the empirical or policy side. Everyone recognizes, however, that satisfactory treatments of the issues require insights and angles of multiple kinds.

This laundry list of interdisciplinary research efforts is intended to highlight the fact that it's misguided to try to establish bright boundary lines between disciplines. Such bright lines would obscure the fact that a great deal of important research occurs at intersections of nominally separate disciplines. It is arbitrary to insist that such intersections be assigned to exactly one discipline. The next question is: which branch of philosophy do the topics we have surveyed belong to? By far the most plausible candidate, in each case, is epistemology. In most of the cases, a class of social objects is selected as targets of assessment, and in each case these objects are assessed (or guidelines are laid down for their assessment) in terms of characteristically epistemic criteria, such criteria as conduciveness to true belief or rationality of belief. In one case, the prospects are examined for justifying a certain type of political system (democracy) in explicitly epistemic terms. What other branch of philosophy do these investigations belong to if

²⁴ I refer to Larry Laudan's *Truth, Error, and Criminal Law* (2006), reviewed by Raphael Goldman and Alvin Goldman (2009) in the journal *Legal Theory*.



not epistemology? Their credentials for being *real* epistemology are the use of characteristically epistemic criteria of evaluation or normative appraisal in all of the examples.²⁵

²⁵ Thanks to Blake Roeber, whose comments on the penultimate draft led to several helpful clarifications. Earlier drafts of this material were presented at the Stirling conference on social epistemology, a University of Calgary graduate student conference, and the World Congress of Philosophy in Seoul, Korea (2008). I thank the audiences in all these venues for stimulating discussion and criticism.

