LORRAINE CODE

AN ECOLOGY OF EPISTEMIC AUTHORITY

ABSTRACT

I offer an examination of trust relations in scientific inquiry as they seem to contrast with a lack of trust in an example of knowledge imposed from above by an unaccountable institutional power structure. On this basis I argue for a re-reading of John Hardwig's account of the place of trust in knowledge, and suggest that it translates less well than social epistemologists and others have assumed into a model for democratic epistemic practice.

For social epistemologists of the late twentieth and early twenty-first centuries, John Hardwig's work has been influential for having introduced a range of puzzles and conceptual issues that had been absent from Anglo-American analyses prior to the mid-1980s (especially Hardwig 1985, 1991). Drawing on the report of an experiment in physics where ninety-nine participants are listed as interdependent throughout the processes of inquiry for results they could not verify for themselves, Hardwig argues convincingly for the centrality of trust in making the experiment's conclusions possible. His analysis has been read by social epistemologists as exemplary for understanding the significance of trust in the production of knowledge as such. Here I will revisit Hardwig's work to propose that despite the place it has come to occupy in social epistemology, the analysis does not translate well enough into a model for the role of trust in knowledge that could inform a robust social-ecological epistemic position. Contrary to the larger claims that have been made for it, it lacks the explanatory power to contribute to evaluating epistemic practices more generally, or to show how trustworthiness and credibility, conceived more widely than within an enclosed community of scientists, can be well enough established to promote epistemically-ecologically viable social cognitive structures and practices. A structural limitation is built into the physics example itself, in consequence of its restriction to a presumptively homogeneous population of equally positioned and accredited knowers and to the

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exemplary status it accords to physics as a social-epistemological practice, whose implications are generalized to propose conclusions they do not support. Even though Hardwig's claims for the place of trust in scientific knowledge may be judged so commonplace as to merit no further analysis, a renewed emphasis on testimony and innovative developments in feminist and anti-racist epistemology, in epistemologies of ignorance, and in analyses of epistemic injustice[1] afford his work a renewed, and controversial, salience in thinking about the place of knowledge in our social cognitive ecology.

Hardwig's essays were innovative in affirming a constitutive role for interpersonal trust in scientific inquiry. They are credited with contributing to a shift toward understanding science and knowledge-seeking more generally as social practices, contrasted with the inexorable processes of formal, impersonal discovery and justification that had dominated Anglo-American epistemology as these are distilled in Karl Popper's famous dictum: "Knowledge in the objective sense is *knowledge without a knower*: it is *knowledge without a knowing subject*" (1972/1979, 109; italics original), with its brief for the definitive veracity of knowledge claims uncontaminated by human thought, hence closed to any prospect of naturalizing or socializing inquiry. Yet despite their part in initiating an epistemological breakthrough, Hardwig's analyses of trust exceed their explanatory limits when they are enlisted to address social knowledge in a cognitive ecology for which testimony has become a principal epistemic resource, and they do not open into new ways of engaging with issues of epistemic authority beyond the limits of professional scientific communities.

By contrast with the demographic homogeneity and epistemic univocality Hardwig's 1985 analysis assumes, consider an instructively different case that informs a fictional, yet nonetheless plausible, example of authority at work in institutional knowledge, depicted in Leo Tolstoy's *Anna Karenina*. It points toward an unacknowledged imperative for social epistemologists to examine how trust is implicated with authority, responsibility, and epistemic transparency. Tolstoy writes:

> The new commission for the investigation into all aspects of the life of the racial minorities was appointed and sent to the scene with extraordinary swiftness and energy, inspired by Alexei Alexandrovich. Three months later a report was presented. The life of the minorities was investigated in its political, administrative, economic, ethnographic, material and religious aspects. All questions were furnished with excellent answers, and answers not open to doubt, since they were not the product of human thought, which is always subject to error, but were the products of institutional activity. The answers were all the result of official data, the reports of governors and bishops, based on the reports of regional superiors and vicars, based for their part on the reports of local officials and parish priests; and therefore all these answers were indubitable. All the questions, for instance, about why there were crop failures, why the populations clung to their beliefs, and so on – questions that would not and could not be resolved for centuries without the convenience of the institutional machine – now received a clear and indubitable resolution. (1873–7/2000, 370)

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Here institutionally produced knowledge functions as the arbiter of truth and facticity, whose trickle-down effects in everyday lives play a constitutive-normative part in shaping the social order they analyze and inform. Hence their social-political effects call out more clearly for critical scrutiny than do the scientifically enclosed implications of Hardwig's example. The capacity of such "knowledge" to block questions about epistemic responsibility and epistemic injustice shows how issues of testimony and trust are complicated in so faceless a process, when the reports of testifiers-informants are removed from and insulated against both individual and collective accountability.

The Tolstoy excerpt is fictional, but it is no more fanciful than questions about fake barn facades as an opening into deliberations about who knows, and how. In its elaborated form, it points more clearly toward the complexity of larger textural issues in exposing the "place of knowledge in 'our' social cognitive ecology" than stylized examples, or indeed Hardwig's detailed but socially-ecologically contained example, can do. It shows how social knowledge can establish itself – if temporarily – in a social-political space where issues specific to the situation are germane in generating its intelligibility. Hence enlisting it also calls attention to some of the shortcomings of Hardwig's analysis.

For Alexandrovich, the commission's answers were not open to doubt because "*they were not the product of human thought, which is always subject to error*": an investigative trajectory that stretched from officials and priests to regional superiors and vicars to governors and bishops, with God and Mammon in collaboration at each stage, stands as the descriptive and normative source of certainty in knowledge, *ex officio*, so to speak. In the indubitability conferred upon their answers by the authority attendant upon the impersonal public facade of their offices, the investigators Tolstoy lists would not count as identifiable knowing subjects (*vide* Popper), and questions about their credibility, or the genesis or warrant of their knowledge, would be out of order. The context of discovery is closed to scrutiny by fiat; and no space is left open to analyse or interrogate the processes of justification. Yet the circumstances of the cognitive ecology where the inquiry is located – its genealogy, in Foucault's sense – confer a presumption of infallibility upon its pronouncements.

There is, indeed, an earlier Popper who maintained (in *The Open Society and Its Enemies*) that "objectivity is closely bound up with the *social aspect of scientific method*" (1949, cited in Shapin 1994, 24; italics original), and whom Helen Longino, among others, reads as having vacillated between endorsing this starkly a-social ideal of epistemology without a knowing subject, and promoting a social conception of criticism. In his vacillation, Longino discerns an ongoing epistemological challenge, "of reconciling the cognitive rationality of the sciences with the conditions of their practice": a challenge that is central to her larger project of assessing "the fate of knowledge" after the naturalistic turn (2002, 5–6).[2] The Tolstoy passage could open space for engaging with that challenge with respect to the "institutional activity" that produced the "official data". Read against the grain, the passage

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is about chains of public knowledge and authority constructed in and acquired from testimony; and it is implicitly about the conditions of their provenance and warrant. Hence I am reading it through a lens focussed on trust and authority in social epistemology, to reflect on the practices that must have produced this "clear and indubitable resolution", and to evaluate, consider, and reflect upon their social-ecological dimensions. Why should/could they be presumed to deliver indubitable knowledge? Whose interests, whose well-being, would be fostered by these findings? Diverging in the topography of the metaphysical assumptions that inform their thinking, yet converging in aspects of the epistemic imaginary that animates these pictures of knowledge and certainty, Popper and Tolstoy expose some social implications of these conceptions of rationality in ways that, with variations, preoccupy us still.

EPISTEMIC INTERDEPENDENCE

The epistemic trajectory implicit in the production of Tolstoy's *indubitabl*e answers is only distantly analogous to Hardwig's analyses, not because the Russian inquiry is distant in time and place or because it is fictitious, but because of differences between social scientific investigation and physical scientific experimental practices,[3] and because of Tolstoy's contention that "the answers were not the product of human thought." It is harder to determine how errors could be detected in the chains of inference that will have contributed to these "excellent answers" than in the circumscribed experiment Hardwig discusses because of the principled lack of transparency Tolstoy's description conveys. The imperviousness Tolstoy claims for the report and its makers explicitly excludes interventions designed to examine the trajectory of the results in order to evaluate their warrant: it removes the context of discovery from critical scrutiny. The declared indubitability of the inquiry's "resolution" speaks of a social cognitive ecology more closed, more anonymous, than Hardwig's community of scientists appears to be. This thought preserves a sense that the physics experiment is impeccable in its capacity to yield internally and externally valid conclusions based in relations of demonstrated expertise and incontestable trust.

In principle, the expertise and epistemic authority of the scientists/experts, listed by name in the experiment Hardwig cites, could be called upon to "double-check", revisit their findings, were a weak link in the explanatory chain to be suspected and confirmation required. Listing the names of the participants in the experiment offers a tacit warrant, an implicit if unspoken "trust me" assurance such as is concealed from view by the cloak of anonymity surrounding Tolstoy's commissioners.[4] Presumably, real human thinkers/knowers did produce Tolstoy's indubitable answers; but the assertion that these "were not the product of human thought" attests to the effects of a prevailing social-epistemic imaginary within which investigating and reporting could appropriately occur and epistemic authority be established *in absentia*, so to speak. Hence it is unclear how trust

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could rationally be placed, and less clear how the epistemic authority of the investigators could be confirmed or contested. Also germane is the fact that even though the participants in Hardwig's experiment are named and thence "personally" accountable, if only in principle, their scientific expertise and their expertise in performing the parts of the experiment and using the apparatus that for purposes of this experiment were *theirs* are taken for granted and these assumptions establish the central and single locus of their answerability. With a social scientific inquiry such as Tolstoy depicts, were the participants to be named and held singly accountable, or accountable as members of certain sub-groups in a hierarchal division of epistemic labour, a more diverse yet also politically freighted range of questions, capacities, inclusions, and exclusions would be exposed to scrutiny. Were such an intervention conceivable, it might initiate a shift toward the openness democratic social-political epistemic practices seek to enable. My point is not that physical science transcends politics, but that, for purposes of this discussion, it is less *visibly* political, hence the questions it generates appear to be quite different from those the Tolstoy inquiry poses.[5]

As I have noted, crucial for drawing out the implications of Hardwig's example is the extent to which the participants whose names are attached to the experiment are linked by trust in their credentials as *bona fide* scientists with the relevant qualifications, and in bringing their expertise appropriately to bear on the inquiry. Their epistemic responsibilities are circumscribed by the dictates of the experiment and the reach of their expertise. Because none of them can (in a strong sense) verify the results of each stage of the experiment for themselves, they are epistemically interdependent all the way down: any rupture in the line of trust, any short-cut or fudging of results, would jeopardize the entire sequence. Thus even despite their being named, the question persists: Who really knows? Whose knowledge is at issue here? For methodological individualists, it is an unsettling question, for the answer must be "anyone, and everyone".

To propose a contrast with the Tolstoy example, I have claimed more significance than many commentators do for Hardwig's contributors being named and thence, in principle, individually accountable for pieces of the larger whole and for the relations of trust they as named participants must honour. But my aim is not to ignore the urgency of Hardwig's asking: "Does anyone know that *p*? Is that *p* known?" The question, he says, is disturbing "because it reveals the extent to which even our rationality rests on trust, and because it threatens some of our most cherished values – individual autonomy and responsibility, equality and democracy" (1985, 348–9). Yet read through subsequent investigations in naturalized and social epistemology, this same question can be read as opening the way toward acknowledging the limitations of individualist thinking, initiating a re-articulation of those "cherished" values, and bringing epistemological projects down to earth.[6] Rather than representing a threat to democratic values, this revelation is commendable for its capacity to liberate epistemology from the unrealizable demands of an overblown autonomy ideal,[7] and from what Guy Axtell

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aptly calls a "platitudinous adherence to epistemic value monism" consequent upon a narrow focus on propositional knowledge (2008, 58).

In Longino's view, Hardwig's "puzzlement" could be dissipated by rejecting any suggestion that whereas the group as a whole knows, no individual within it does, and also by rejecting any simplistic claim "that it is possible to know vicariously". As a way around the apparent impasse, she proposes a distinction "between knowledge production and knowing". Although the group may have *produced* the knowledge, with "critical interactions at various stages and… critical discussion of the whole experiment" (2002, 152–3), once the experiment is completed and the paper published, the knowledge, she maintains, is *commonable*. Yet beyond reaffirming and extending a distinction between the contexts of discovery, justification, and circulation of knowledge, the suggestion offers little more than an unspecific appeal to commonability, with a reminder that people inside and outside the experimental process can rationally claim to know what an experiment makes publicly available if they do the work to familiarize themselves with "the whole result". Hence "knowers and producers of knowledge need not be identical". It is unclear, however, whether this proposal could translate across diverse social scientific examples or into social-epistemological issues where the quasi-Popperian limits of the physical sciences might hold differently, if they hold at all.

Hence, I am reading the Tolstoy example as exposing the epistemic underpinnings of an institutional authority that differs from the authority Hardwig's example must assume, in its (i.e. Tolstoy's) being explicitly presented as a "case" that is closed to the very possibility of anyone's asking "Does anyone know that *p*? Is that *p* known?" Yet re-reading Tolstoy's project description against the grain for its ironic meaning calls for a quasi-genealogical inquiry, after all, into how the process (the context of discovery) can have generated the results he constructs. The impact of naturalist-social inquiry for epistemology has been to create spaces for posing those very questions, as feminists and others have shown who eschew the epistemological individualism which, Hardwig notes, "dies hard" (1985, 339). They open into more pointed questions about whose knowledge "we" (epistemologists) are talking about, and about the place of testimony in the construction and circulation of knowledge, and in forging or reconstructing epistemic community. Comparing the two examples calls for examining how epistemic power and privilege differentials shape, and skew, patterns of commonality, interpretation, and trust. The apparent ease of appeals to mutual trust in the Hardwig example appears to contrast with an uncertainty about the wisdom of taking trust for granted in the Tolstoy example, where no space is left for the critical discursive interaction that, for Longino, is a defining feature of social knowledge.

Here I have been construing testimony widely, to cover a range from exchanges of knowledge-as-information – where A knows that *p*, tells B that *p* and, in consequence, B justifiedly claims also to know that *p* – through to the transmission and acquisition of knowledge from conversations, books, teaching, newspapers,

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lectures, and other forms of communication; thus also to the published record of Hardwig's experiment, and Tolstoy's report. My take on testimony falls roughly into the non-reductivist camp, in the minimal sense of holding that "one" (singular or plural) can come to know directly from testimony *when certain conditions to do with the reliability and responsibility of testifier(s) and hearer(s) are in place*. This is a significant caveat: such conditions routinely hold, I suggest, only for relatively simple "naive empiricist" exchanges where it is reasonable to assume that speaker and hearer are communicating on/from a level playing field and where, at the very least, commensurable material-social power and privilege distributions can be taken for granted to an extent that they cannot in Tolstoy's discussion. Appearances to the contrary notwithstanding, the extent to which they can be assumed in the Hardwig example needs also to be re-assessed. It is not a given that simple information-exchanging "telling-B-that-*p*" examples are paradigmatic of "things", "truths" people come to know from one another through testimony, so that more complex testimonial exchanges can appropriately understood as multiples of or derivative from these simpler knowledge-conveyings. Nor can would-be knowers assume, before the fact, that it is always easy to discern where trust can rationally be placed in such practices.

I have been working with a fairly benign conception of testimony and trust in relation to the Hardwig experiment and to everyday testimony in ordinary epistemic exchanges. Hardwig's participants, the account assumes, are connected by relations of testimonial trust, where, for argument's sake, I have left aside the vulnerability problem that looms large for detractors of testimony as a source of knowledge, and may inform Hardwig's worry about a loss of individual autonomy. As he notes, "the trustworthiness of members of epistemic communities is the ultimate foundation for much of our knowledge" (1991, 694).[8] Thus, it rests on a fragile ethical basis. While issues connected to the vulnerability of reliance on trust have indeed been pressed with respect to science, where competitiveness and personal reputation figure prominently, the Hardwig example is commonly cited as one where trust has prevailed, and where the relevant epistemic community is maintained by justified and relatively durable internal relations of trust. Thus, as with Longino, knowledge production, and hence the production of results in Hardwig's example, are represented as having accumulated "through a community's continued engagement in a set of practices guided by a given set of standards" (Longino 2002, 150), where "the epistemic dependence of scientists on one another… is a common, not an anomalous feature of scientific inquiry" (153). The observance of common standards stands as a safeguard against the fragility of trust and the vulnerability of the trusting. Yet, I shall suggest, even such observances are not wholly exempt from critical scrutiny.

For Tolstoy, trust must also be of the essence for so confident a claim about the inquiry's results to be advanced, although people reliant on those results are differently vulnerable, epistemically and in the minutiae of their lives, from those who would rely on the findings of the physics experiment. Differences in

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power, expertise, and authority such as are already evident in the contributors' rankings are as epistemologically significant as is unmarked membership in a linked chain of scientists, and not necessarily to the trust-enhancing benefit of the findings. While something like an *epistemic principle of charity* might inform an initial reading of a report such as Tolstoy details – a working assumption that the contributors' expressions of epistemic authority are "more or less reliable"[9] – it is unclear how such a presumption can be upheld in situations of declared social, cultural, and political invisibility and blatant inequality. In a well- or even a poorly-functioning democracy, it might be conceivable for people to assume the *internal* trustworthiness of an enclosed scientific community, even if its effects for the larger social order were to evince no comparable democratic structure or commitment. Yet the Hardwig example, despite the exemplary status it has claimed in analyses of *epistemic* interdependence, has less to offer to projects of developing a democratic epistemology than the Tolstoy example, even though I have been reading the latter as showing by negative example what might be required for it to model or pave the way for the proto-democratic social order Tolstoy's Levin hoped to inspire.

COMPLICATIONS

The initial contrast between the Hardwig and Tolstoy examples suggests a taken-for-granted epistemic transparency in the former, such as is presumed characteristic of inquiry in an open, democratic society; whereas the Tolstoy example appears to represent a complete, closed, and authoritarian contrast. Yet a closer reading of Hardwig suggests that the structural differences are not as great as they initially seemed to be with respect to the place of knowledge in a social cognitive ecology, and the contrast is instructive. Even though Hardwig has maintained an illusion of transparency among the scientists, and Tolstoy makes no such assumptions, the examples pose analogous challenges.

Despite Hardwig's experiment having been accorded an explanatory power that extends well beyond the confines of the experiment that prompted his analyses, it is less plausible to read the enclosed, apparently egalitarian epistemic-scientific community he draws upon as a model for social and epistemic interdependence more generally conceived, or for democratic epistemic arrangements, writ large, than its influence suggests. A contention of Sandra Harding's in *The Science Question in Feminism* – a founding text of feminist epistemology – is germane here. She claims: "a critical and self-reflective social science should be the model for all science, and … if there are any special requirements for adequate explanation in physics, they are just that – special." She continues: "Whereas the social sciences must consider physical constraints on the phenomena they examine, the objects, events, and process of concern to physical scientists are limited to those that can be isolated from social constraints" (1986, 44–5). While subsequent feminist and other post-colonial analyses of social constraints in the physical sciences have shown that these limits are not as hard-edged as they appeared to be in 1986,[10] Harding's

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claim nonetheless speaks to the truncated relevance of the Hardwig example for social epistemology, after all. I am re-reading her claims to support a suggestion that the Tolstoy example, *with* its instructive shortcomings, offers a hermeneutically and ecologically more promising route toward approaching these issues than does Hardwig's.

Reading the examples side-by-side, it appears that Hardwig's experiment is fully transparent, open, and immune to critical scrutiny, whereas the explicitly installed screen of anonymity behind which Tolstoy's "knowers" proceed invites wariness, caution. The explicit exclusion of the knowing subject(s) from the knowledge production Tolstoy describes calls for intervention, interrogation, in subsequent readings. The Hardwig example presents a picture of seamless and benign trusting relations where credibility is readily assumed and conferred, and in that too-easy assumption it invokes caution. Thinking about the place of such knowledge in "our" social cognitive ecology prompts questions about who "we" are, and about the extent to which Hardwig has taken on faith the intellectual character – the trustworthiness – of the testifiers on whose reported findings each stage of the experiment relies.[11] In so doing, he neglects to take into account such situational factors as bias and error, which cannot be ruled *hors de question* in evaluating the trustworthiness of testimony. Perhaps because of the demographic homogeneity of the community on whose results the experiment relies, Hardwig works from what amounts to a default assumption of the testifiers' trustworthiness in commending the process and the product he discusses. In this self-contained case the assumption may prove warranted, but in presenting it or in his concerns about individualism and democracy, he has not taken into account the extent to which, at least according to Kristina Rolin, "trust in scientific testimony involves trust in the community's ability to facilitate inclusive and responsive dialogue based on shared standards of argumentation" (2002, 106). For such conditions to hold, some sense of the inclusions and exclusions from which a scientific community is forged needs to have been achieved if the credibility of the contributors and the plausibility of the results are to count as more than default assumptions. Social epistemologists emphasize the centrality of such questions, which, admittedly, may not have been available to Hardwig at the time of writing, but are germane to the ongoing esteem for his epistemological contribution.

On the issue of epistemic exclusion, Nancy Daukas aptly observes: "In a culture plagued by racism, sexism, and other forms of oppression, most community members have internalized, to varying degrees, pernicious stereotypes regarding the cognitive and moral traits of socially constructed 'kinds' of people" (2006, 114). The unarticulated patterns and perceptual-epistemological habits that perpetuate such putative "knowing" are embedded in and enacted through an instituted social imaginary, which carries the often-implicit but effective systems of social meanings and interlocking explanations-expectations within which people, in specific times and geographical-cultural climates, enact their knowledge and subjectivities and craft their self-understandings and social understandings, both individual and

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collective. An imaginary is self-reinforcing, as self-fulfilling prophecies are: ongoing "successes" confirm and consolidate its sense of rightness. It is productive in locating and evaluating practices of inquiry, both scientific and "secular", and in generating images, metaphors, and operative idea(l)s that underwrite received patterns of epistemic legitimacy and credibility, and their opposites. Yet it is not contained by regulative assumptions within normal science, nor is it only about how knowable items in successive historical periods are spread out before "the" observant knower.[12] But when Daukas emphasizes the *epistemic* significance of how people are socialized within certain norms – including stereotypes of "human kinds" – that guide epistemic practices, she is affirming how stereotypes embedded within a received imaginary tend to "work their way into … background presuppositions" in which certain "kinds" of people will be deemed competent, epistemically and socially, across a range of circumstances, activities, and patterns of judgement (115–6). Such patterns seem to pertain without contest in the Tolstoy example: they are available to inform conclusions about why their crops failed, why they clung to their beliefs, and thence about how, socially and politically, they should be treated in light of the answers.

Such considerations, apparently, are missing from Hardwig's discussions of epistemic dependence and trust, and their absence has routinely been read as irrelevant to the social-political effects of his conclusions. On the surface, their absence from the Tolstoy example is less benign because the consequences of the inquiry for the social reforms that animated Tolstoy's thinking were potentially far-reaching for the very people whose lives were being investigated, and there is no good reason to believe that such stereotype-infused patterns in a highly stratified society would or could have been eliminated from the investigations or their consequences. While it might, behind a veil of ignorance, be judged sufficient for conferring trust on the knowledge produced in Hardwig's experiment to presume that the participating scientists were sufficiently qualified for that trust to be well placed, in the investigations Tolstoy outlines, more is required by way of understanding the epistemic situations and predispositions of the investigators before trust can be conferred. But juxtaposing the two examples suggests a different, less straightforward contrast. Indeed, the absence of such considerations from the Tolstoy example shows, writ large, one salient reason why this matters, in that it urges a re-reading of the Hardwig example to ask again about his 'named' scientists whose epistemic neutrality cannot so readily be assumed, after all.

Clearly it is impossible to legislate for bias-free, stereotype-free inquiry. Whereas a certain homogeneity of training is appropriately required of Hardwig's scientists, albeit with differential expertise according to the piece of the experiment and of the inquiry that is theirs, with Tolstoy's investigators, their named differences in rank and social "station" might seem to ensure sufficient diversity to allow for epistemic negotiation and debate. But even the Tolstoy example conveys a sense of homogeneity within the ranks that makes such an open process seem unlikely, recalling a crucial drawback for an epistemology without a knowing subject in

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its having no place for epistemic accountability. Accountability is excised by fiat from the Tolstoy example. Yet the difference between the two is not as stark as I initially propose, for accountability is more narrowly circumscribed in the Hardwig example than its significance in social epistemology appears to signal. If critical discursive interaction is to contribute to an inquiry intended to do epistemic justice to the subjects it studies, it has to convey a clear sense of respect for plurality in points of view, and of the possibility of dissent among the investigators; indeed, to allow for a plausible range of "human thought" and deliberation to have contributed to their production. (My point is not that diversity is an unqualified good, but neither is the consensus that results from undifferentiated sameness, which may well be merely a local effect of that very sameness.) Although declaring the answers were *not* the product of human thought might be a compressed way of affirming that differences had been negotiated and resolved to the point where taking individual, specific thoughts into account was no longer necessary, for specificity and diversity had been transcended, homogenized, the report of the report is not persuasive in this regard.

Tolstoy (as Alexandrovich) needed an investigative approach that would permit "the racial minorities" to be well enough known for appropriate governance and administrative measures to be initiated if he was to think and act well about the larger social-political changes that would follow from land reform, the emancipation of the serfs, and the inclusion of these "minorities" within a renewing social order. Such, presumably, was the mandate of his commission of inquiry. Yet there is no indication that the investigations were open to the extent of being able to take advantage of citizens' situated knowledge: of how things were for them, in the specificity of their circumstances. Given Tolstoy is writing of a society at a point of radical social change, and setting aside the issue of whether the "minorities" would have a voice, the question arises whether and to what extent the inquirers would have been open to hearing and taking seriously a range of compatible and incompatible possibilities for knowing well, or whether the results were determined before the fact, as if by fiat. In view of the answers' declared indubitability, there is not likely to have been space for criticism after the fact: the process is closed once the inquiry has been completed, and seems to have been closed before it began. But while the initial contrast I have drawn may cast Hardwig's example as benign, democratic in this regard, in fact his inquiry, too, is structurally closed. Even Hardwig's path-breaking model of cooperative inquiry does not offer the structural space to think well about social arrangements and just distributions of public epistemic authority.

I have looked at two illustrations of the concept of knowledge in our social cognitive ecology, taking some liberties with the extension of "our". Both examples are self-contained, epistemologically, and such containment is ecologically predictable with the Hardwig experiment and its subject matter. But universal, necessary and sufficient conditions are less readily discernible and less explanatorily effective than epistemologists and philosophers of science have

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thought them to be. The specific subject matter of a physics experiment or a social scientific inquiry assumes a significance in both "contexts" – of discovery and of justification – that it would not have claimed in orthodox philosophy of science/theory of knowledge. Despite being explicitly situated, the Tolstoy example, too, is contained in ways that raise questions about its social-political effects: its capacity to know the subjects it studies well enough to do well by them; in ways that are sufficiently sensitive to how they have to be known, if new systems of governance are to address and accommodate them as who they are and how they are situated.

Admittedly, it would be bizarre to expect Hardwig's experiment to manifest all of these possibilities, but the worries he evinces in consequence of acknowledging how much of our rationality rests on trust might on this analysis have to be recast as "paper doubts". For although there seems to be no room for dissent among the ninety-nine contributors to the experiment once the problem has been set and the tasks distributed, such loss of individual autonomy as there is seems on first reading to contribute to rather than to detract from knowing well. Although these examples are analogous in being collaborative, they are enacted differently with respect to the kinds of knowing involved and to the place/situation of the participants within them. They point to different understandings of the place of knowledge in relation to cognitive authority in a specific, relatively fixed domain, or across an incipient upheaval in the social order. More vitally, they caution against any temptation to encourage a collective imagination too narrowly focussed on dislocated issues of truth, fact, and certainty, to the neglect of engaging with the conditions and circumstances of their ecological and civic specificity.

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NOTES

1 See, for example, Longino (2002, 152–3), Rolin (2002), and M. Fricker (2007, 112 n.7). Hardwig's work is frequently cited in Lackey and Sosa (2006), a pivotal text in social epistemology.
2 According to Longino, this earlier Popper "is often considered to have originated social epistemology with his emphasis on the importance of criticism in the development of scientific knowledge" (5–6).
3 This thought may meet with resistance from adherents to a residual "unity of science" credo, but social epistemology contests that credo.
4 See Richard Moran's reference to the speaker's responsibility in testimonial exchanges both trivial and complex, as in where someone who tells me when a meeting will take place is giving me her *assurance* that it will happen. An informant participates as "a kind of guarantor, provid[ing] me with a characteristic reason to believe, different in kind from anything provided by evidence alone" (2006, 279).
5 Consider the epistemic significance of a relationship between disclosure and secrecy Mary Poovey discerns in novels depicting nineteenth-century financial developments

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in Britain, where the cognitive ecology of the market economy required "both that information be available to the public and that certain details and practices remain secret" (2002, 32).

6 Consider Mary Douglas: "our colonization of each other's minds is the price we pay for thought", quoted in Shapin (1994, 28).

7 See Code (2000). In like vein, Elizabeth Fricker asks "whether the supposed ideal figure of the autonomous knower, who refuses ever to trustingly accept another's testimony, a fortiori will never allow her own judgement to be corrected by another's, is really such an ideal after all" (2006, 239).

8 This article predates recent developments in virtue epistemology. Hardwig does mention (Code 1987), which he dismisses, together with (Welbourne 1986), as a " 'nonstandard' analysis" (1991, 694).

9 See Nancy Daukas's useful discussion of an epistemic principle of charity (2006, 110).

10 A small but representative sampling of texts includes (Kincaid, Dupré, and Wylie 2007), (Barad 2007), (Figueroa and Sandra Harding 2003), (Rouse 2002), and (Potter 2001).

11 I am indebted in some of these thoughts to Kristina Rolin (2002).

12 Here and elsewhere in this essay I am drawing on my discussion of social and epistemic imaginaries in (Code 2006), especially chapter 1.

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