Two Kinds of Social Epistemology
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Abstract

Steve Fuller's programme of Social Epistemology was initiated some 25 years ago with the launching of a journal and the publication of a monograph with those very words as their title. Since then, the programme has evolved in a constant critical dialogue with other players in the fields of epistemology and science studies. Fuller's main confrontation has been with analytic epistemology which, in its classical form, adopts a contrary position on most key issues. However, analytic epistemologists have gradually moved in the direction of Fuller's views and even adopted the term "social epistemology" for their emerging position. Still, substantial disagreement remains between the two identically named programmes with regard to the proper philosophical approach to knowledge as a social phenomenon; in this article, I try to pinpoint the locus of this disagreement. However, Fuller has also been engaged in minor skirmishes with his Science Studies fellows; I also examine these clashes. Finally, I express my wishes concerning the future direction of social epistemology.

Introduction

The first issue of the journal *Social Epistemology* appeared 25 years ago, as the launching pad for Steve Fuller's identically named research programme in science studies. At about the same time, Fuller used this label once more for a monograph in which the key ideas of the programme were laid out in great detail (Fuller 1988/2002). Since then, the programme has evolved in a critical dialogue with other players in the fields of epistemology and science studies. Fuller's main confrontation has been with analytic epistemology which, in its classical form, adopts a contrary position on most key issues. However, a number of prominent epistemologists, with Alvin Goldman as the leading figure, have broken away from the analytic mainstream and gradually developed views that place them in the general vicinity of Fuller's position; they even adopt the term "social epistemology" for their emerging position. That name also forms part the subtitle of the journal *Episteme. A Journal of Individual and Social Epistemology*, which is the main publishing outlet for the group. Still, substantial disagreement remains between the two identically named programmes concerning the proper philosophical approach to knowledge as a social phenomenon. The main aim of the present article is to pinpoint the locus of this disagreement.

Such a comparative project might proceed along two routes. One would have broad scope and would present the key tenets of the two positions, and their mutual differences, by examining a selection of canonical contributions from the two journals (and related publications). Such an undertaking would no doubt be worthwhile, but cannot be pursued successfully within the narrow compass of a single article. Moreover, many delicate and ultimately unanswerable problems lurk within the little

word "canonical", since there is hardly an orthodox doctrine on either side.[1] Therefore, I have chosen another, simpler approach, which will focus narrowly upon Fuller's own contribution to the programme, counterposing it to that of his opposite within analytic social epistemology, Alvin Goldman. Their work will be subjected to examination in what follows. It should be kept in mind, however, that this study is meant to throw light on two more general approaches, of which Fuller and Goldman respectively have been the key representatives. Thus, when I discuss "Fuller's programme" and "Goldman's programme" — also known as "analytic social epistemology" — in the following, I shall be understood to refer not just to the work authored by these two representative figures, but more generally (and rather vaguely) to the work for which the two similarly named journals serve as the chief outlets.

Steve Fuller's Social Epistemology

Steve Fuller's programme of Social Epistemology offers a cornucopia of original and challenging ideas about the nature of human cognition. In his long and ever-growing list of publications on the subject, Fuller in turn highlights different elements of the total cluster; moreover, there is clearly a development in Fuller's thought from his earliest to his most recent works. A plausible reconstruction of the basic argument, however – one suitable for the purposes of the present paper – might be as follows: We start with the insight that human cognition is always *implemented* in various material structures, in the very broadest sense. Among these implementations are the human brain and body, but also books, computers, material tools and instruments that "black box" knowledge and, most significantly, various societal organizations and power structures. Once this point is granted, everything else in the programme follows readily: Such embodiment offers various affordances and hindrances to cognition, the effect of which we cannot divine *a priori*. Hence, we must leave the philosophical armchair and examine the matter empirically; we have to go *naturalistic*. When we do this, we soon discover that the main implementations are of a social and communal nature, not individual possessions: thus, epistemology must henceforth primarily be *social*. Add to this a final point, that social epistemology subscribes to a *normative* agenda with regard to cognition, aimed at reforming our societal knowledge-producing practices and institutions, and you have the basic framework of Fuller's programme.

Given this fact, it is no surprise that Fuller's main confrontation has been with analytic epistemology which, in its classical form, tends to deny all of the above, except the normative stance. This controversy has not been mollified by the fact that

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[1] Fuller's programme of social epistemology does not lend itself easily to canonical formulation. In the preface to the foundational text, *Social Epistemology*, Fuller advises the reader not to treat it as "the usual monolithic monograph, but rather as a parcel of provocations, a sourcebook of ideas, and directions for further work" (op. cit., xxx). Most of his later books have the same somewhat kaleidoscopic and *ad hoc* character. Goldman's programme comes closer to a canonical format, thanks to the availability of a couple of anthologies in which the programme is set out in contributions from prominent figures. One of these anthologies is entitled *Social Epistemology: Essential Readings* (Goldman & Whitcomb 2011), where the word "essential" in the subtitle would seem to have roughly the import of "canonical". Characteristically, no contributions from Fuller or other representatives of Science and Technology Studies (STS) have been included. In any case, my division of the field into two competing factions is pragmatically justified by the fact that both sides seem to operate with some such bifurcation.

some of his opponents have gradually taken steps in the same direction and indeed adopted the name of "social epistemology" for their emerging position (and in the subtitle of their chief journal). There remains a substantial disagreement over which side provides the better understanding of the nature of knowledge as a societal phenomenon – this disagreement spiced up, perhaps, by a hint of a priority dispute over the name.

As it happens, Fuller has been waging a war on two fronts, as he has also been engaged in minor skirmishes with his comrades in Science and Technology Studies (STS). [2] This engagement has largely been concerned with different understandings of STS's own role in the world of societal knowledge production. We shall look at both these engagements in turn, but with most attention given to the confrontation with analytic epistemology. I shall proceed as follows: First, I provide a sketch of classical analytic epistemology and of its recent social turn, as a foil for an account of Fuller's rival conception and of the differences between them. Next, I analyse the (rather infrequent) exchanges between representatives of the two positions, arguing that the themes adopted serve as proxies for the real underlying disagreements. Then I examine Fuller's occasional confrontations with fellow STSers, in particular Bruno Latour. Finally, I express a sympathetic onlooker's wishes concerning where social epistemology should go in the future, with special emphasis upon the prospects of a collaboration between its two feuding schools.

Classical analytic epistemology

We start with a look at analytic epistemology, Fuller's main antagonist. I will first characterize classical analytic epistemology and then sketch out the development within this position that to some degree (but not fully) has moved it towards Fuller's position. The remaining distance between the two positions defines the area of current controversy. I shall argue that, unfortunately, the key issues involved are not directly addressed but have been replaced by proxies. My brief account of classical analytic epistemology will necessarily involve numerous simplifications; but these simplifications are consonant with Fuller's way of mapping the territory, hence, I hope, are legitimate in the present context.

In line with their methodology of conceptual analysis, traditional analytic philosophers of science and of epistemology (here I shall refer to them collectively under the latter label) have delivered purely formal characterizations of knowledge. Typically, they consist of specifications of the abstract nature of empirical evidence, and of the inferential steps that take us from such evidence to general knowledge about the empirical world, both in an everyday context and in science.

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[2] I use the term "Science and Technology Studies" in a very broad sense, to encompass the entire trend towards empirical studies of science that started to emerge in the early 70s, in explicit opposition to traditional *a priori* and rationalistic philosophy of science. At the outset, these efforts, under the name of "Sociology of Scientific Knowledge", SSK) would primarily concern themselves with the sociology of scientific theory formation; an influential early version was the Strong Programme of the Edinburgh School (Bloor 1976/1991). Later, focus would shift to laboratory practice, and technology would be included among the objects of investigation (Latour & Woolgar 1979/1986, Latour 1987, Pickering 1984, Pickering 1995). Fuller counts as a representative of Science and Technology Studies in the broad sense.

The format of such specifications is familiar from the two classical models offered by analytic epistemology, *foundationalism* and *coherentism*. These models provide divergent accounts of both main dimensions of empirical knowledge distilled forth by analytic epistemology as it concerns the nature and source of justification. Foundationalism postulates a basic level of pure, theoretically untainted observations, from which inferences are made by various kinds of induction. Empirical cognition is depicted as a pyramid resting on a base of observational data and tapering towards an apex that comprises an extract of those data in the form of general theories. Coherentism denies the existence of pure observation data, insisting that all data are theoretically laden. As for the inferential links, coherentism typically opts for some relation of explainability: Evidential items cohere by forming a network within which they explain one another. Thus the model of cognition offered by coherentism is that of a network of beliefs, the outer edges of which border on a "reality" that is however only known through the way it impinges upon our total belief structure.

These models are abstract and idealized, being "rational reconstructions" that do not deal with actual cognition and all its imperfections. They are by that very fact exemplary and normative, but in a rather weak sense, as Fuller has stressed (Fuller 1993, 8; Fuller 1988/2002, 24f; Fuller 2012, 272f). They are not part of a programme of improving actual human cognition but merely put an ideal before us that can be invoked in *post hoc* evaluation of existing cognitive practices.

Fuller has aptly compared this kind of abstract epistemology with the kind of physics that operates with models defined within highly idealized conditions. To use a metaphor from the world of physics, analytic epistemology operates in a vacuum, where no friction or other obstacles hinder the mind's operations. But perhaps another analogy, one which Fuller preferentially adopts, is more apt: Put in terms of economics rather than physics, epistemic processes are construed as processes that consume no resources.

The sources of traditional analytic epistemology

There are several reasons why analytic epistemology adopts this characteristic stance. One is its familiar *a priorism:* Analytic epistemology deals in conceptual analysis, and its special concerns are such key terms as "knowledge", "evidence", "inductive inference", and "explanation". The outcome of the analyses are abstract models of human cognition unencumbered by empirical detail, with little concern for the question of how and at which level these structures are implemented. What matters is the formal structure, which is tacitly assumed to be realized isomorphically at whatever level human knowledge is found, be it in science or in everyday cognition. The material, organizational and social substrates of the implementations are largely disregarded.

To the extent that traditional epistemology addresses the issue of implementation at all, it tends towards *individualism* and *idealism*. Cognition is considered to be an activity of individual minds, producing a body of knowledge that is the possession of individuals; knowledge is a "state of mind". This helps to render the problem of resources invisible: when cognition is construed as the activity of putting our individual mental representations in order, what is needed for its success is basically

just a particular mindset. Such a thing comes at no cost, beyond the moral one of adopting a certain intellectual discipline, it is independent of any costly material or external equipment. Even when the focus of epistemology is shifted from everyday cognition to science, as happened both in logical positivism and in Popper's critical rationalism, the normative recommendations still remain abstract slogans, without any suggestions concerning material or organizational implementation. For positivists, it is a matter of anchoring our scientific concepts in a solid empirical basis which is largely a problem of correct definition (Carnap's *Aufbau*, Carnap 1928, instructs us about the logical techniques to be used), followed by cautious inductive projection of the predicates thus certified. In Popper's works, we are admonished to make bold conjectures and subject them to severe testing, which is basically a matter of refraining from the use of *ad hoc* hypotheses, and in general avoiding the intellectual sloppiness displayed by Marxists and Freudians. Both Carnap and Popper are dealing in abstract principles, with no consideration of concrete institutional implementations with their inherent operational costs.

As a matter of fact, for analytic epistemologists, all this talk about cognition as a process, however ethereal, is really just a figure of speech. Cognition is not conceived of as a "process" at all; this is a considered a psychologistic misconstrual, from which analytic epistemology is at pains to distance itself. What we are talking about are really structures of data, captured in propositions, and the abstract inferential relationships holding between the latter. The "process" of drawing a conclusion from a set of premises cannot meaningfully be said to involve "costs", as it is simply a matter of the existence (or non-existence) of the proper inductive or deductive ties in a Fregean "Third World".

Analytic social epistemology

During the last couple of decades, however, analytic epistemology has undergone a number of significant and mutually related changes. First came a change towards naturalism. This move was heralded by Quine in a famous article that carried its revolutionary message in its brief title, "Epistemology Naturalized" (Quine 1969): Here, Quine bids farewell to traditional *a priori* methods and rational reconstructions in epistemology and urges a shift towards an empirical examination of how human cognition is actually accomplished. But epistemology is still stuck with an individualist stance: According to Quine, the science to which we entrust this task turns out to be psychology, and indeed of the strongly individualist Skinnerian variety.

This empirical turn might seem to sideline the traditional programme of epistemology with its normative agenda: henceforth, naturalistic epistemology must apparently resign itself to a purely descriptive role. In this situation, however, analytical philosophy found a way to save normative epistemology by devising an ingenious alternative to the traditional "internalistic" models of foundationalism and coherentism. This alternative was *reliabilism*: We can maintain that human cognition is indeed (sometimes) justified, hence it amounts to knowledge, even though the processes involved in its generation may not possess any inherent rationality and are indeed purely psychological (or neuro-physiological) phenomena. We may do so if the overall process is still a reliable producer of truth (Goldman 1986). Thus, focus

shifts from the processes involved in knowledge generation (however virtual) to its products. The task of epistemology is henceforth to establish which cognitive practices are reliable.

This opened up two different routes for analytic epistemology to go. One route was conservative, and would consist of conceptual analysis of the novel problems that had become visible within the new perspective, thus keeping faith with the classical *a priori* stance of analytic epistemology. One popular issue was the status of testimonial evidence as either epistemologically autonomous or derivative (Fricker 1995), another was the epistemic implications of disagreement between cognitive peers. A special case of the latter was the problems posed by disagreements between experts and the consequent challenge for lay people to decide which experts to trust (Goldman 2001). There would also be the familiar philosophical activity of rebutting criticisms of the new position, such as that reliabilist epistemology incurred a vicious regress (or circularity) in defining the epistemic merits of one source of knowledge in terms of the deliverances of another (or itself).

The other route would be empirical and naturalistic, consisting of the examination of various actual human cognitive powers and epistemic practices to assess their reliability. Goldman and others would take the first steps in this new direction after having first addressed some of the basic conceptual issues mentioned above so as to secure the fundamental soundness of the new approach (Goldman 1986).

Once the naturalistic road was taken, the fact would soon obtrude itself that many of man's most important cognitive practices are not private and individual, but communal and public. Hence, developments would move away from the individualist focus of Quinian epistemological naturalism, which made psychology the key cognitive science, and towards a more social approach (Goldman 1999). Epistemology at last broke free of the Cartesian heritage, perpetuated by the British empiricists and living on in modern analytic epistemology, of taking the individual as its unquestioned starting point. Instead, knowledge generation was seen as a communal enterprise.

Exploration of this territory will force the investigator to broaden analytic epistemology's traditional abstract and formalist approach. Once we leave the philosophical armchair and examine the way knowledge is actually generated, for the purpose of discovering the best ways, the scope for the formulation of abstract general models is greatly diminished. A naturalized epistemology must capture the messy and diverse real-life properties of actual institutions of knowledge production in society, to assess their veritistic properties. A distinction drawn by Goldman is useful here, however (Goldman 1999, 5; Goldman 2002, 198), between "generic" and "domain‑specific" epistemic practices — or, perhaps better, between generic and domain‑specific *aspects* of such practices. With respect to the former, there may be some scope for formal models, as illustrated by Goldman's own work on Bayesian models for general aspects of various epistemic practices (cf. Goldman 1999, 109ff). Yet epistemic practices will also always display domain-specific features that call for detailed empirical investigation and varied employment of expertise from a broad range of disciplines in the social and human sciences, such as sociology, organizational theory, information theory, anthropology, and psychology. As a social

science, social epistemology cannot aspire to producing a grand unified theory, but will always remain a cluster of complexly related specialties.

Goldman's social epistemology out-sources ethical and political issues

It is a constitutive feature of Goldman's version of social epistemology that although it is explicitly normative, all issues of an ethical or political nature still are declared to lie outside its domain. Policy issues of this nature are referred either to political agencies or, if they are of a theoretical nature, to neighbouring academic disciplines such as normative ethics or political philosophy.

The following quote articulates this position and reveals some key assumptions behind it (Goldman 2004, 204):

> Let us use the term *truth-possession profile* (TPP) to designate a profile or distribution of states of various individuals that include both their doxastic (psychological) states and the truth-values of the propositional contents of those states. What would still be common to social epistemology is its interest in tracing causal connections between various social practices and resulting TPPs. This would be perfectly analogous to the subject of civil engineering, which examines causal relationships between various design structures and performance properties of such structures - how such structures would react under hypothetical stresses. Social epistemology would not recommend social practices unconditionally; it would recommend them only hypothetically, relative to certain assumed evaluations of the envisioned TPPs. It would not be the business of social epistemology per se to make TPP evaluations. That would be society's responsibility, and in the end the province of ethics or political philosophy.

This reads very much like a classical Weberian understanding of science with its strict bifurcation into "factual" and "value-related" aspects; in brief, the conception of science as "value neutral". Social epistemology pronounces only hypothetically upon the consequences of possible ways of organizing societal knowledge production, leaving it to political and other societal authorities to decide whether or not those consequences are desirable and thus warrant the implementation of the organization in question.

This strict separation of factual and value issues might be enforceable if social epistemology were already in possession of a complete and authoritative body of knowledge, a toolbox of theories of knowledge production into which one could simply feed the crucial parameters of a projected knowledge institution to generate a prediction of its veritistic properties; or, conversely, tailor a knowledge institution to suit previously given veritistic desiderata on the part of a client. Goldman seems to embrace a picture of this sort in the above quote when he compares the social epistemologist with an engineer who may be commissioned to construct bridges according to highly diverse specifications, in so doing drawing upon an already established basis of physical laws.

But whatever the case may be in mechanical engineering, with its secure basis in theoretical physics, this is definitely not the situation in social epistemology. Social epistemology is, after all, in its early infancy, and generally accepted theories do not yet exist, even of a domain-specific kind, that the social epistemologist can use in the manner of a simple "knowledge engineer". Most theoretical work is programmatic, while the empirical aspect is exploratory and partial.[3] Hence, a chief concern for social epistemology will be the development of theory. This means that questions about research policy with obvious societal import will arise within the discipline, and must be decided there. They concern which areas of societal knowledge production would seem to offer promising opportunities for theory generation. Considerations about the social importance of the knowledge generated inevitably will play a role in this decision. For instance, there is little doubt that considerations about societal importance have motivated the strong focus of STS upon natural science. But such external considerations are inextricably intertwined with other, internal factors determining the trajectory of theory development. First, there is the sheer contingency and haphazardness of the scientific "process of discovery"; fruitful theoretical ideas may crop up in areas that are far removed from immediate usefulness, but they should nevertheless be pursued. Moreover, there are certain standing theoretical concerns within a discipline that must be balanced against the external calls for usefulness.

A scientific discipline must be considered flawed if substantial areas nominally belonging to it are left unexplored, or cannot even be captured in its theoretical vocabulary. In the case of social epistemology, this means that it must secure the development of a broad and diverse theoretical base and a comprehensive picture of knowledge production in society. It might plausibly be argued that STS's preoccupation with natural science has drawn it away from this ideal, resulting in considerable theoretical narrowness and impeding the development of concepts and methods applicable to the other areas such as the humanities (cf. Collin and Budtz Pedersen, forthcoming). But only the practitioners of a scientific discipline, in this case social epistemology, can make informed decisions about the proper balance between such contrary concerns.

Moreover, this is not merely a transitional phenomenon that will pass once social epistemology reaches maturity. Social epistemology is, in part, a social science, and no sense can be attached to the idea of it developing a "final theory", or in other ways ever being finished with its theoretical work. The social sciences deal with a moving target: society and its institutions are in constant flux, novel phenomena constantly appear and call for new theories and concepts to capture them. Social epistemology is no exception to the rule, and it will need constant development of new theoretical conceptions, of the domain-specific kind, if it is to keep pace with its object of study. (Think, for example, about the way that the appearance of the internet and the "social media" reliant upon it have prompted the appearance of new theoretical concerns in social epistemology). Social epistemology will always be engaged in the work of

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[3] The history of Sociology of Scientific Knowledge and of Science and Technology Studies — which are the most sustained efforts to date to understand societal knowledge production in empirical terms — is one of ever shifting and competing theoretical orientations. The currently dominant school is Latour's Actor Network Theory, to which analytic social epistemology would probably be loath to turn for sociological guidance.

theory development, and will face the challenge of deciding in which direction this work should proceed.

Goldman could of course adopt a hard line here, insisting that choices concerning theory development no less than about the employment of already established knowledge be made solely by social epistemology's clients and paymasters. This would amount to a radical version of Mode 2 science, in which research is conducted in close collaboration with the users of the results, and presumably, Goldman would not recommend this purely client-driven model of science in general. It would turn all scientific disciplines into pure engineering enterprises and make the process of theory generation completely dependent upon the expected utility of the results, to the detriment of the development of the discipline.

The issue of who makes policy decisions in practice is only one aspect of the policy issue. Another concerns the abstract normative values and principles in play in such decision making. Traditionally, this area is the topic of other philosophical specialties, that is, normative ethics and political philosophy. Hence, we are not surprised when we read, at the very end of the above quote, that Goldman proposes to refer such normative issues to these two neighbouring philosophical disciplines. And indeed, the questions raised above are seen to be close counterparts to the questions John Rawls attempted to answer in his celebrated theory of social justice (Rawls 1971). Knowledge is certainly one of Rawls's "primary goods", and its production and distribution might plausibly be taken to be governed by something akin to Rawls's Second Principle: In society, we should aim to produce as much knowledge as possible, even if this leads to inequalities in its distribution, as long as we choose the mode of production that optimizes the position of the less well off. But whatever the attractions of this approach, Goldman would see it as falling outside the scope of the kind of social epistemology he pursues.

Thus Goldman takes great pains to stake out, as his own domain, a kind of social epistemology for which issues of an ethical or political nature are out of bounds. They are either delegated to neighbouring academic specialties such as ethics or political philosophy or, in their more practical aspects, are referred to societal agencies that will decide them on purely political grounds.

Fuller prioritizes political issues

On the other hand, the territory typically shunned by analytic social epistemology is precisely where Fullerian social epistemology preferably operates. Fuller moves with virtuosity and relish in the borderland between epistemology, political philosophy and social theory. Here, he is very much in line with the overall ideological concern of STS, which was to make science more permeable to broad social interests and to social control (Collin 2011, chapter 10). Fuller is more explicit and precise on this point and sometimes berates STS work for its political impotency (an issue to which I will return). His fundamental normative premise is a commitment to cognitive democracy, to the effect that no one should be forced to live under a cognitive regime to which they have not consented; more specifically, it holds for universalized knowledge forms such as science that "any form of knowledge that purports to be universal in scope — that is, true for everyone everywhere — must be subject to

democratic governance" (Fuller 1988/2002, xiii). He adopts the name of "prolescience" for scientific knowledge produced in conformity to this rule (Fuller 1993, xviii). Consistent with his principle of epistemic self-governance, Fuller has testified for Intelligent Design in a US court case about the admissibility of this doctrine in the biology curriculum of a local school system. He stresses that he is not himself a believer in Intelligent Design; to Fuller, the key point is the democratic right of local communities to have such doctrines taught in their own schools (Fuller 2008).

In the same spirit, Fuller has examined a number of general structures in our societal system of knowledge generation, assessing their efficiency and democratic credentials. Among them is the tendency of academia to undergo ever finer division into disciplinary specialties, which he views as both an obstacle to the free circulation of knowledge in academia and in society at large, and an impediment to its practical use. Fuller points to *rhetoric* as a remedy against this problem (Fuller 1993), and has devoted much attention to developing ways in which the rhetorical element can be woven into the fabric of science – even sketching out a core curriculum for a graduate program of knowledge policy studies with a heavy emphasis upon rhetoric (*op. cit.*, 383f).

Another Fullerian concern is the increasing commodification of knowledge as a part of the neo-liberal takeover of universities by the business world. After an initial sympathy with the dismantling of the universities' privileges as autonomous knowledge-producing institutions, Fuller has increasingly come to distrust the potential of the neo-liberal turn in academia to serve broad democratic ends (Fuller 2000, Fuller 2001). Finally, like analytic social epistemologists, Fuller has also written about scientific expertise, but characteristically in a much more critical vein. Where Goldman is concerned to determine which experts to trust, based upon reliabilist principles (Goldman 2001), what matters to Fuller is the legitimacy of the institution of cognitive expertise as such. His answer tends towards the negative: the use of experts is a form of cognitive authoritarianism that militates against the basic democratic principles of an acceptable knowledge policy (Fuller 1988/2002, chapter 12).

Not surprisingly, this difference in ground-level topics and concerns has led Fuller to criticize analytic social epistemology at the level of methodology and theoretical perspective. To Fuller, analytic social epistemology is inherently conservative, despite its declared value neutrality. This is in part because an academic enterprise that explicitly renounces normative concerns will tend to leave everything as it is, thus protecting the *status quo*, in part because its reliabilist methodology limits it to examining extant cognitive regimes in order to asses their reliability, without the possibility of projecting hypothetical systems (Fuller 1988/2002, xvii).

Fuller expresses great impatience with this limited agenda, but on this point might have heeded a lesson from one of his declared heroes, namely Karl Popper: Analytic social epistemology's methodology conforms closely to what Popper advocates under the label of "piecemeal social engineering", while the strategy recommended by Fuller verges towards "utopian social engineering" which is the project of effecting large-scale and radical transformations of society (Popper 1945, Chapter 9). Popper warns strongly against the latter, and his reasons for so doing apply in full in the

sphere of knowledge production: We simply do not have the requisite social science knowledge to effect a large-scale and thorough reorganization of the knowledge-producing institutions of society with any reasonable prospect of success. We are forced to rely on tinkering, "piecemeal epistemic engineering", proceeding by the pedestrian comparison of existing epistemic regimes to establish their relative merits (which, of course, will include ethico-political effects such as epistemic justice as well as veritistic virtues).[4]

Vicarious debates between the two schools: the issue of truth

Above, I have tried to characterize a key point of doctrine still separating the two versions of social epistemology. In short, social epistemology of the Goldmanian variety draws a firm distinction between purely truth-related ("veritistic") and ethico-political aspects of social epistemology, declaring the latter to be outside of its scope. Fuller, on the other hand, cares little about the distinction as such (he takes it to reflect mainly a difference in time perspective, cf. Fuller 2012, 277f) and anyway insists that the truly important issues in societal knowledge production will straddle it. He proposes a radical, normatively committed version of social epistemology with the democratization of science as its fundamental goal.

Although these differences reflect genuine ideological divergencies, there would seem to be fairly little separating the two schools at the level of explicit doctrine. Indeed, the difference might seem to be mainly a matter of alternative ways of carving up the academic territory; different divisions of scientific labour. Goldman places ethical and political issues outside of social epistemology proper, but evidently recognizes their legitimacy; specifically, he would grant the legitimacy of Fuller's reformatory agenda, which, according to Goldman's way of mapping the field, would amount to a set of extrinsic normative ends which the (value-neutral) knowledge engineer might then be commissioned to realize through the construction of a suitable knowledge regime.

Thus there would seem to be a large scope for collaboration between the two wings of social epistemology, despite their tacit political differences (a topic to which I shall return at the end of the article). It is emblematic of the somewhat dysfunctional relationship between the two camps, however, that their (infrequent) debates rarely touch upon these ideological issues – at least on the analytic side. Instead, the debates have come to revolve around certain metaphysical issues that seem to serve as proxies for the real underlying disagreements. These are the debates about *truth* and about *realism*. This has sidetracked the debate, making it harder to make progress with the real disagreements.

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[4] As it happens, analytic social epistemology actually moves beyond such piecemeal or "domain-specific" work by sketching out more abstract general models of knowledge production; what Goldman terms "generic" theories. But instead of welcoming this activity — which would after all constitute a necessary first step in any attempt at projecting novel kinds of knowledge producing institutions — Fuller construes it as an attempt to hang on to analytic epistemology's classical practice of conceptual analysis. He mockingly labels it "Tychism", after Tycho Brahe's retrograde and failed attempt to salvage the basic, and ideology-laden, components of the Ptolemaic world picture with only minimal accommodation of the elements of the new Copernican world view (Fuller 1993, 70 ff.).

This is particularly clear with respect to truth. STS proponents have been berated as "veriphobic" by Goldman (Goldman 1999, 7ff), an allegation Fuller dismisses as far as his own position is concerned (Fuller 2012, 269). And indeed, this allegation is hardly justified with respect to Fuller or to STSers in general: although STSers do indeed harbour suspicions about the notion of truth and generally avoid using it, it would seem that, at the purely technical level, what they are against is not truth as such but a particular conception of it, *i.e.* truth as correspondence. This is sometimes rather crudely conceived by STSers as implying that true statements are likenesses of reality. A criticism along these lines is expressed very eloquently by Latour (1999, chapter 2), a criticism that would appear to be telling only against correspondence as it was construed in classical British empiricism, where truth was indeed conceived as a matter of likeness between reality and "ideas" in our minds. It has little relevance to correspondence theories of truth conceived after philosophy's linguistic turn in the 20th century; for example Austin's classical statements from the 1950s. But there are other and more sophisticated criticisms of the correspondence theory — for instance the one presented by David Bloor (1976/1991, chapter 2).

There seems to be a lack of interpretative charity on the analytic side in this debate and little willingness to re-articulate the opposite side's scruples in terms that might make them recognizable to analytical philosophers. It is characteristic that, in Goldman's treatment of these issues in *Knowledge in a Social World*, he disposes of post-modernists and constructivists already in the first chapter on the grounds that they are "veriphobes" who allegedly reject truth as such; hence, they have no place in a treatise on cognition as a truth-seeking enterprise. Next, in a separate discussion (*op. cit.*, chapter 2), he goes on to examine the nature of truth (or the meaning of "truth") and the numerous controversies surrounding it. This discussion involves an entirely different cast of characters, consisting largely of recent anglophone authors. Some of these have serious scruples concerning truth and have proposed highly heterodox analyses of the concept. Goldman subjects these rival notions to critical examination, and it would have been an obvious move for him to combine the discussions of the two chapters: He could have reinterpreted the postmodernists' supposed rejection of truth as really an expression, however imprecise, of the same concerns that motivate the heterodox conceptions of truth on the analytic side.

One need not go very far in the direction of heterodoxy to find a conception of truth that would suit postmodernists rather well. This is *coherentism*, which defines truth as a property that accrues to a belief to the extent that it coheres with the rest of our beliefs. Much of postmodernist work on science adopts a sociologized version of Quine's network conception of knowledge (see, for instance, Collins 1985/1992). STSers' stabs in this direction are sometimes read as a crude consensus theory of truth, which is then disposed of in summary terms (Goldman 1999, 10f). Goldman examines other alternative conceptions of truth, such as minimalist, "deflationary" or "disquotational" accounts with which STS advocates would probably have little quarrel. (Bloor's discussion of truth in chapter 2 (1976/1992) has affinities with several of these alternative theories.) True, Goldman ends up rejecting both coherentism and the more radically non-standard conceptions of truth, but he grants that they deserve serious consideration and that their authors are worthy discussion partners. The same courtesy might have been extended to (some of) the STSers and

their constructivist stance: their positions might appear less absurd in analytic epistemologists' eyes if reinterpreted in terms of alternative conceptions of truth.

The issue of realism

Even so, the technicalities involved in defining truth remain a distraction from the real issues separating the two sides. By contrast, discussions of *realism* are closer to the heart of the matter. STSers tend to be constructivists and anti-realists, whereas representatives of the analytic side lean in the opposite direction. Some of the criticisms raised against the correspondence theory are really directed against realism. Yet even here, I would contend that we have not reached the heart of the disagreement and that the constructivism debate has to some extent served as a proxy for the real underlying issues.

On the STS side, this proxy has emerged in the following manner: STSers are minimally committed to some kind of *epistemic* constructivism. They deny the existence of universal, supra-cultural and trans-historical criteria of theory choice, of the kind traditional philosophy of science has tried to articulate. Instead, they suggest that various societal factors instead bring about the "closure" of scientific debates, that is, decide which theory will emerge victorious from a scientific controversy. Thus in their view, science is a *social construction*.

Notice that this is a dual claim, of which the second part is the more controversial one. It says that the slack left by the lack of universal criteria of theory choice is taken up by social determinants that secure the "closure" of scientific controversies. Analytic epistemologists typically oppose this view. With respect to the first part of the dual claim, concerning the criteria of theory choice, the STS view may indeed be controversial; still the line of disagreement here does not run between STS and analytic epistemology (or philosophy of science) as such, but divides the latter side as well. Such prominent analytic authors as Paul Feyerabend, Larry Laudan and others have raised similar points; much of this work uses ideas from the late Wittgenstein. It is a striking fact that STS draws very heavily upon these figures and other important analytic philosophers such as Quine, Nelson Goodman and Ian Hacking.

In this somewhat muddled dialectical situation, some STSers have found it useful to sharpen their rhetorical profile by emphasizing the points on which they most palpably differ from analytic epistemology, namely *ontological* constructivism. This is the claim that not just scientific theories but even their objects are social constructions, *i.e.* do not exist in their own right but are created and sustained by societal practices (on pain of reflexivity paradoxes, this claim is normally restricted to the theoretical entities of natural science). Some of the players in the game clearly revel in provocative statements of this view; the most notable is Latour, who adds insult to injury in suggesting that his opponents' failure to embrace the same view springs from certain elementary misunderstandings concerning the language in which scientific results are articulated. So rhetorically successful has this strategy been that it has been hard for STSers who reject ontological constructivism to put their position on the map at all — this is the case, for instance, of the Strong Programme (cf. Bloor 1999).

Analytic epistemologists, on their part, have only been too happy to adopt this arena for a confrontation with their adversaries. They believe they are on firm ground and consider this a battle easily won. This confidence, however, rests on a rather shaky foundation. Analytic epistemologists tend to forget that, even within their own camp, there are authors who have adopted positions not too distant from that of their STS opponents. There is a powerful challenge to scientific realism at a very principled level in Michael Dummett's writings in philosophy of language (1978, chapter 10; some very radical implications are drawn in Dummett 2006), and Hilary Putnam has experimented with various unorthodox forms of anti-realism (e.g. Putnam 1983).

More to the point, there are positions available in analytic philosophy of science that seek to accommodate some of the constructivists' insights, such as those proposed by Bas van Fraassen (1980) and by Ronald Giere (1999, 2006). According to Giere, scientific theories do not directly refer to the real world but to *models* serving as intermediaries between reality and our theories, considered as sets of general laws of mathematical form. Such models are never accurate depictions of reality but involve all sorts of idealization, selection, and so on. To the extent that Giere holds that the sciences have such models, which are obviously of the scientists' making, as their object, it would only be a slight exaggeration to say that for him, atoms and similar theoretical entities are human constructions — meaning that they are humanly constructed conceptual artefacts. It would have to be added, at the same time, that the models used in established theories such as quantum mechanics possess robust ties to reality, according to Giere, among them certain relations of similarity. Hence, it is still correct to say that the sciences deal with an independent reality, although indirectly. Still, Giere's position is far removed from a straightforward realism, and might be thought to constitute a step towards a compromise position, or at least a common conceptual ground for further discussion.

Neither side seems very interested in compromise, however. Analytic philosophers of science conveniently forget about the dissidents within their own ranks when confronting post-modern constructivists. As for STSers, they could easily defuse many of its opponents' arguments by making explicit the instrumentalist interpretation of science that is implicit in almost all of STS work. In terms of this conception, saying that we construct this-or-that element of the physical world simply means that when doing science, we impose upon our data a simplified model that does not correspond exactly to reality and that has alternatives that we could have adopted instead. But this would blunt the rhetoric edge of the STS position, and might not elicit the desired effect of scandalized shock among analytic epistemologists.

As for Fuller, an instrumentalist stance (at least with respect to natural science) is implicit in his stated commitment to what he terms a "shallow" conception of science (Fuller 1993, 12). According to this conception, the shared discursive surface of a scientific discipline (as presented for example in textbooks) will typically hide a great variation in the way the key terms are actually understood. Fuller holds that this heterogeneity matters only little for the actual practice of the discipline, and for the employment of its results. Considering that the key theoretical terms in a discipline define its ontology, on a realist reading of science, this "shallow" conception implies that the ontology of a scientific theory matters little in actual day-to-day scientific

work, or in the theory's practical applications. In other words, it comes to much the same thing as an antirealist (instrumentalist) interpretation of science.

Some key underlying disagreements

I have argued that these vicarious debates between the analytic and the constructivist version of social epistemology hide other issues that define what is really at stake in the opposition between the two. One important concern has already been mentioned, which is the conservatism inherent in reliabilist social epistemology versus the reformatory impulse characteristic of STS. Analytic social epistemologists define all such political issues as external to their agenda, and will refuse to address them in their capacity as knowledge engineers. This is the familiar concern of analytic philosophy to keep science strictly separate from politics, an urge all-dominant in logical positivism but still operative in later generations of analytic philosophers.

But there is also, I believe, another more diffuse ideological issue. Traditional epistemology's insistence on truth and on metaphysical realism reflects a commitment to a goal for knowledge that goes beyond mere instrumentality. Getting to understand the universe in which we live is a romantic and noble goal. Traditional epistemologists miss the pathos of Popper's talk about science as an "unending quest" for truth. Popper's early work reveals his initial metaphysical worries about the concept and his relief at Tarski's demonstration of its logical coherency (Popper 1959, 274). Later, Popper would start to worry over the inevitable gap between even our best theories and the final truth, and would struggle to develop a notion of "verisimilitude" that would allow him to hold on to Truth as the goal of the scientific enterprise, in the sense of something we aim at and move ever closer to, although we might never reach it (Popper 1963).

This noble ambition will not be satisfied by the meticulous recording of mere *truths*, in the plural, which will add up to nothing more than a huge inventory of disjointed facts about the universe. Nor is it satisfied by realist truth as such, which is simply truth that is determinate despite being beyond human cognitive reach. Such truths may be quite trivial in themselves (a typical example from the literature might be "On 2:45 p.m. 12 February 1969 Michael Dummett was in his College room", cf. Dummett 1978, 363). Rather, what analytic epistemology is after are "deep truths", located at the bottom of the cosmic order and explaining everything in it. Often, the ambition to know the universe is connected with the idea of a "Theory of Everything", in the sense of a general theory about the basic physical facts of the world, upon which everything else supervenes. What is wanted is a kind of knowledge that will somehow provide a synoptic vision, the celebrated "view from nowhere" or, in the logical positivist's somewhat more modest version, a "unified science" the unity of which consists largely in a communality of a basic observational language and shared methods.

All such ideals of unity are contradicted in the instrumentalist conception, according to which science falls apart in separate sectors using different conceptual models, each with only local validity and motivated largely by local societal concerns. Even though such practices may together cover every corner of reality, they do so in the manner of a large quilt composed of pieces of widely different shapes and colours,

without providing the kind of synoptic knowledge for which analytic philosophers of science yearn.

As for the STS side, it not only fails to provide such a picture, but actively opposes it. Any talk of a "final theory", beloved by philosophers and by such elder statesmen of science as Steven Weinberg (Weinberg 1992), are regarded by STS with deep suspicion, as the thinly veiled bid for a position of social privilege for those who claim to provide such knowledge (a good example is Latour's tirade in 1999, chapters 7 and 8, against Weinberg). The "view from nowhere" supposedly delivered by some such theory is really the view from a privileged social perch. Here we have what I think is the fundamental ideological concern of STSers: the desire to break up the oppressive monopolistic enterprise which, to them, science represents.

Fuller shares this egalitarian stance, as we have seen, but his version of it is at once more explicit and more fully developed, and at the same time also more moderate: Nothing similar to Latour's hostile rhetoric is to be found. Moreover, while being far distant from analytic epistemology's worship of Theories of Everything, Fuller still diverges from the STS mainstream in recommending that science strive to develop theories that are "universal" in the sociologized sense of being globally (and not merely locally) useful. He even declares that this recommendation places him in the lineage of such great synthesizers in the history of thought as Hegel and Comte (Fuller 2012, 269). Analytic epistemologists, however, are not likely to accept Fuller's sociological simulacrum of Comprehensive Truth as the real thing.

Metaphysical anti-realism turns into economic instrumentalism

In the perspective of analytic epistemology, metaphysical anti-realism (instrumentalism) has another insidious feature. It all too easily metamorphoses into an economic instrumentalism of the crudest sort. STS talk merges smoothly into the kind of science policy discourse familiar from a steady stream of government reports on the organization of science in modern society. It is all about speeding up the transition from research to practical applications. STS has become a key part of the science policy establishment in western societies, with the commodification of scientific knowledge and universities being transformed into businesses. STS's contribution to this development is somewhat paradoxical, given its history as a critical movement and its anti-establishment self-image. How did this happen? The answer will help to explain the opposition between the two wings of social epistemology.

Part of the answer lies in a number of simultaneous and linked changes in STS and in its object of study that have generated an ever more intimate interaction between the two. At its inception, STS, in the form of the Sociology of Scientific Knowledge (SSK), would largely examine societal macro-factors that were thought to impinge upon science in subtle and intangible ways, discernable only by the trained eye of the sociologist. Typically, they would operate upon the minds of the producers and consumers of scientific results, guiding the development of science in directions that would suit the interests of those groups, or would fit some dominant collective mindset. A typical case of this kind of analysis is Forman's celebrated account of physics in the Weimar republic (Forman 1971), which traces this back to the

operation of a particular kind of *Weltanschauung* in the German population. Or the operative factor would be class interests, as in Shapin's famous study of phrenology (Shapin 1975).

Today, the societal forces that impinge upon science are no longer subtle and hidden, but are all too obvious. A huge apparatus of science administration has been constructed, with the declared aim of directing research towards the satisfaction of various societal interests. Even the least reflective working scientists are aware of its existence, for example, through the constant pressure emanating from it to apply for funding for strategic projects, and through the time they are forced to spend upon research assessment exercises and other productivity measurements. This apparatus of science management is now a favourite object of study of SSK: instead of divining subtle influences upon science from abstract class interests, SSK can now examine the way that science is openly influenced by politics by tracing the workings of the ever-multiplying organizations of knowledge management.

The role of STS in societal knowledge management

It is an inevitable consequence of this that STSers have found their way into this knowledge management apparatus, in the form of committee memberships, consultancy jobs, and so on. Not only do they possess an insight into the workings of science that is highly valuable to policy makers, they are especially useful because of their constructivist and instrumentalist stance. This delivers a strong counterpoint to the classical argument from scientists against strong political governance of science, to the effect that basic science, when left alone as scientists' preserve, will in the end deliver much more substantial goods, even of a practically useful kind, than applied research commissioned by governmental science managers. This argument is undermined once the very distinction between basic and applied science vanishes on a strong instrumentalist interpretation of science.

The positive feedback loop between the role of STSers as observers of science and as agents for its management has become so tight that the distinction between the two functions is virtually disappearing. Given the intense interaction between observer and object, the role of observer in the ordinary sense must yield to that of the "participant observer" in the institutions of science management. This has also produced a curious new genre of academic writing: a report of factual changes in the organization of science that at the same time subtly praises those very changes. (Gibbon's report in the change from Mode 1 science to Mode 2 (Gibbons *et al.* 1994) is a good example.)

Yet only a few STSers have faced this dilemma explicitly, not to mention accommodating it in their theories. One of them is Latour, who welcomes the new development and explicitly states that the role of social scientists is to participate in the creation and strengthening of social networks, including those in science. It is all a matter of "reassembling the social", as the title of one of his recent books has it (Latour 2005, especially, 253ff). After all, this is a natural extension of Mode 2 science: Not only is scientific knowledge today produced in close collaboration with users, it is also produced under close monitoring by STSers, who report to and

counsel the financers of science. It betrays a curious blindness in most STSers not to accommodate this fact in their theories about science.

My analysis is thus that STSers have been so flattered by the interests in their services on the part of powerful clients that they have quietly adopted the latter's view of science as an instrument for economic growth. In step with their ever-growing complicity in the science policy process, the metaphysical instrumentalism (anti-realism) of STS has been transformed into the economic-technological instrumentalism of the modern research regime.

At the same time, STS has abandoned its original concern with the broader macro-social forces that were thought to covertly direct the course of science; the defining agenda of the original Strong Programme in the sociology of science (Bloor 1976/1991). This shift may be rationalized in terms of the assumption that now everything is out in the open, STS may directly study political influence upon science in terms of the way that officially declared goals of science policy are formed and implemented. By the same token, many of the original suspicions as far as science is concerned have fallen away. Since science policy has now been made transparent and a matter of public scrutiny and debate (at least in democratic societies) it may safely be left to the political process.

I read analytic social epistemologists' heavy rhetorical investment in the notion of truth as (in part) a reaction against this development in recent science policy, and against STS's role in it. In modern science management "newspeak", the word "truth" never occurs: an endless line of government reports about science policy manage to talk about the future of the university without ever mentioning it. Instead, focus is upon economic growth, the protection of market shares in the global economy, and lately also upon coping with certain so-called Grand Challenges to modern societies. Against this, analytic social epistemology bravely upholds a veritistic ideal: the purpose of the university and similar "cognitive" social institutions remains the discovery of *truth*.

Fuller's critique of STS and its role in knowledge management

What is Fuller's attitude to the managerial turn in academia and STS's role in it? Here we come to the second front in Fuller's participation in current debates, viz. his clashes with fellow STSers. The issues involved are less central to his concerns, and consequently there have been fewer engagements on this front. Hence we may be briefer here.

We may start by observing that in engaging in debate on this front, Fuller simply applies the theoretical tools developed within Science Studies to Science Studies themselves (cf. Fuller 1999) — not, as some analytic philosophers have done, to prove the enterprise to be fundamentally self-refuting, but just to show that it manifests the same blindness to its own conditions of knowledge production that it demonstrates to such good effect in other sciences. Such reflexive application indeed constitutes an important litmus test for any STS view (one that Bloor famously invited in the last of his four principles of the Strong Programme in Bloor 1976/1991, p. 7). While STS has applied reflexitivity at the level of methodology and literary form −

for a collection of such work, see Woolgar 1988 — it has curiously failed to apply it where it really matters, *i.e.* with respect to the actual societal role of STS in knowledge production.

As Fuller sees it, STSers here are the victims of a kind of sensory illusion. They mistake the revolutionary nature of their metaphysics and epistemology for a feature of their politics which, on closer inspection, turns out to be anything but revolutionary and indeed is quite establishment-friendly (Fuller 1999). The point holds specifically for Latour's highly influential Actor Network Theory (ANT): the societal networking that ANT officially supports and which it regards as some radical form of democracy, extended even to non-human actants (Latour 2004), may easily be seen as just an ameliorated description of the market. As Fuller points out, there are striking similarities in the description of science given by Latour and that provided by von Hayek (under the label of "catallactics") of the distributed and emerging character of societal formations (Fuller 2007, 212-13). For both, the crucial point is the self-organizing power of the social sphere. For Hayek and other free market advocates, a society based upon self-organization is the ideal habitat of human beings as free and deliberative agents. To critics of capitalism, on the other hand, it is an arena in which the strong are at liberty to exploit the weak.

Thus Fuller's charge is that Latour unwittingly, and despite his frequent calls for a radically democratic social order, actually abets an organization of society that will inevitably privilege those who have the resources to gather a network around themselves, including members that are material products such as machines and gear.

True to the spirit of the sociology of knowledge, Fuller proceeds to explain this fact in terms of the socio-economic context of ANT activities (Fuller 1999). For the larger part of his career, Latour was associated with the Ecole Nationale Superieure des Mines in Paris. This is one of the French "Grandes Ecoles", the institutions of higher learning founded by Napoleon in order to break the back of the traditional educational system with its emphasis upon humanistic learning. The new Ecoles would focus upon practical matters, and would be closely tied to their industrial clients' needs. This tradition persists to this day, and Fuller takes Latour's work to be an example of it. He diagnoses its covert political conservatism as a strategy to secure the economical basis of its existence, disguised behind a smokescreen of intellectually radical talk that makes it palatable also to the academic elite.

In responding to such criticisms, Latour will invoke his heterodox ontology. In presenting a picture of society as offering an unequal paying field to its members, the critics forget that the rich and powerful are not in control of their resources; rather, they have to *recruit* them along with other actants by a process of *interessement*. Put in traditional sociological terms (which Latour tries to transcend), this involves a development and articulation of the projected enterprise so as to persuade potential collaborators that it is in their interest to join in. In facing this challenge, the rich and powerful are no better off than anybody else: to get by, we all must solicit the aid of others, be they human or non-human actants. Thus the "flat" basic ontology of featureless actants translates into a social world that is completely equitable – that is, where nobody enjoys a position of superior power and influence.

In opposing Latour on this point, however, Fuller shows some vacillation. This betrays an ambivalence in his normative stance which in fact has been in it from the start and was remarked upon already by early commentators upon the programme (Rouse 1991, Roth 1991). They referred to it as the Machiavellian vs. the democratic side of Fuller. I do not think that these terms quite capture the nature of the tension, which is better expressed by the distinction drawn by Fuller himself between a *methodology* of science and an *axiology* of science (1993 p. xviii, 1988/2002 p. xii).

Methodology of science is directed at optimizing its efficiency, given specified goals. Axiology of science, on the other hand, is the articulation of those very goals. Fuller himself stresses that traditional philosophy of science has concerned itself with the former to the almost complete neglect of the latter; we have just observed how this is perpetuated in current analytic social epistemology. What we need is a careful reflection upon what goals should be pursued. The problem is, however, that Fuller himself never really delivers upon this promise, for a reason that I shall further explore below: the absence of a genuine *anthropology* in Fuller's system. The values animating an axiology of science must somehow derive from a reflection, at the most general level, about the human needs that science is to serve. But Fuller shies away from thinking along these lines.

Instead, the axiological strand in Fuller's system is basically a commitment to *democracy*. This is somewhat minimalistic and opens him to the charge of Machiavellianism, in the true sense: that is, endorsement of — or at least a failure to preclude — a political system that allows large-scale manipulation of the electorate in favour of policies that are not necessarily in their interest. For instance, politicians in western democracies have had little difficulty in rallying voters behind a science policy that massively favours natural and technical disciplines, on the promise of never-ending economic growth and with a tacit understanding that the distribution of goods would not be radically unequal (and with ecological concerns left entirely out of the picture). These last mentioned interests have routinely been violated, often in dramatic ways; this has led to sporadic opposition to this scheme, in terms of anti-globalist movements and protests from ecologically concerned groups against exploitation of natural resources, the introduction of gene-modified foods, etc. But such oppositional movements are easily suppressed by a strong coalition of government, big industry, and international finance capital, operating against the backdrop of an indifferent and silent majority. Fuller's critique of Latour and Actor Network Theory is that they surreptitiously assist this scheme.

True, Fuller's conception of democracy is not a purely formal one. As mentioned previously, he is always concerned that the political game of resource distribution be played at a level field and is fully aware that this is not always the case. For instance, he has given some attention to the obstacles in the way of full participation of women in political life. Yet, in the absence of a fully-fledged axiology, this effort may appear somewhat haphazard and unsystematic and may, in the end, largely benefit those groups that have already managed to establish a rhetorical platform for themselves (such as feminists).

Fuller's critique of the anti-psychologism of STS

Another dispute concerns the anti-psychologistic tendency of STS proponents, who often gravitate towards post-humanism (Latour 2004, Pickering 2006, Pickering 2008). This is a point on which Fuller differs from most STSers.

Anti-psychologism was part of the STS profile from the start. It was manifested, *inter alia*, in Latour's notorious call for a "moratorium" on psychological explanations in science (Latour & Woolgar 1979/1986, 280). Only social explanations would be acceptable, while the individual level of explanation was rejected, in particular such as would appeal to the "rationality" of the individual scientist.

Latour's methodology and ontology support each other nicely on this point. Famously, Latour argues that we should expand the notion of an agent into the broader category of an *actant* that, apart from human beings, also encompasses natural objects. What has attracted less attention is that, at the same time, Latour also challenges the status of *Homo sapiens* as an indisputable exemplar of an agent (actant). For many purposes, we should deconstruct the human agent. This heretic move was anticipated in Latour's general metaphysics, as laid out in *Irreductions* (in Latour 1988), in which he dissolved the philosophers' unitary "subject" (mind, *res cogitans*, and so on) into a network of more elementary actants. In *Reassembling the Social*, Latour adds some concrete detail to this abstract metaphysical argument:

> You don't have to imagine a 'wholesale' human being having intentionality, making rational calculations, feeling responsible for his sins, or agonizing over his mortal soul. Rather, you realize that to obtain 'complete' human actors, you have to compose them out of many successive *layers*, each of which is empirically distinct from the next. Being a fully competent actor comes in *pellets*, or, to borrow from cyberspace, *patches* or *applets*, whose precise origin can be 'Googled' before they are downloaded and saved one by one (*Op. cit.* 207).

Fuller does not share the general anti-psychological stance behind this attitude. Notably, he has tried to integrate insights from psychology into his version of social epistemology (Fuller 1989/1993). Still, Fuller sometimes verges towards a metaphysical post-humanism — or, as he prefers to call it, trans-humanism — that threatens to render psychology irrelevant (Fuller 2011, Fuller 2013). This is not based upon the radial deconstruction of the notion of a human subject as in Latour, but rather springs from doubts about the robustness of our standard distinction between humans and non-humans; it is the familiar kind of "slippery slope" argument. (Fuller often invokes Peter Singer's argument concerning animal ethics here, but tends to draw the opposite conclusion with respect to the ethical implications.)

This conceptual argument is intermingled with an argument of a completely different kind. Regardless of the alleged uniqueness of *Homo sapiens* among related species, the sciences of biology and medicine are approaching a point where they enable us to change the very nature of the species. Fuller displays an evident enthusiasm for the things that science can accomplish with respect to an enhancement of the powers of

the human species, an attitude that sometimes makes him veer towards trans-humanism (which is Fuller's version of post-humanism, cf. Fuller 2011, Fuller 2013). This may explain why Fuller has not invoked psychology in order to identify those human needs that science should help to satisfy, despite his favourable attitude towards psychology: Fuller is more interested in *transforming* man than in delineating his current profile of needs and interests — in the manner this is done for instance in "humanistic" psychology of the Maslowian type.

Where should social epistemology go in the future?

So much for an account of what Fullerian social epistemology has accomplished in its first 25 years of existence and of the academic skirmishes in which Fuller has engaged. It is impossible to guess that will happen during the next 25 years. Instead, we may be allowed to express wishes for where Fuller's social epistemology might go in the future. What follows below is one particular (sympathetic) onlooker's views, which are here merely stated rather than argued for since space does not allow me to make a careful case for them.

First, there is a need for collaboration between these two schools that go by the same name. As I have argued above, the two versions of social epistemology have by now come quite close in their views, and their academic insulation from each other, in institutional terms, is artificial and unfortunate. As a matter of fact, their relationship is a good example of the kind of disciplinary division in academia, with its attendant obstacles to the free flow of ideas, which Fuller begrudges; thus he should be concerned to eliminate it. Admittedly, collaboration would call for an effort mainly from the side of analytic epistemology which has neglected Fullerian social epistemology and has instead focused upon more radical versions of STS, such as Latour's ANT. This has led to vicarious and unfruitful arguments, as I argued above. Fuller's contribution to overcoming this impasse would largely consist in an effort to control his penchant for provocation and rhetorical exaggeration.[5] However playful and good-natured these pranks are, they may yet be counterproductive. Fuller has a keen appreciation of the powers of rhetoric, and should be sensitive to the danger that when used injudiciously, it may generate interdisciplinary estrangement rather than the collaboration he desires.

Such collaboration might hopefully result in a more integrated discipline of social epistemology, and the general formula for its formation would be fairly simple, at least as a first step: the input from Goldmanian social epistemology would concern the *production* of knowledge, or more precisely the local optimization of knowledge production, while Fuller's contribution would chiefly pertain to its global *distribution* and *use*, with all their attendant ethical and political problems. The two elements would soon fuse into a seamless whole, since the distribution of knowledge at a given point in time is obviously a crucial parameter in determining how knowledge is produced at the next moment; production and distribution are two faces of the same

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[5] For an example, read Fuller on "Science as Superstition" in Fuller 1997, chapter 4. Fuller's attitude to Intelligent Design has not been helpful either, nor has his general tendency to construe science and monotheistic religion as closely related ways of understanding the world (cf. Fuller 2013, chapter 3).

coin. Likewise, the ethico-political issues pertain to both aspects as well, not only to the side of distribution. [6]

The new unified approach within social epistemology would need a clarification of its ethico-normative basis. As we have seen, Goldmanian social epistemology has little to contribute here since it takes great pains to distance itself from such ethical issues. As for Fuller, he officially advocated the formulation of an axiology of science, but never really fulfilled its promise. Early on, he expressed an explicit commitment to rule utilitarianism (Fuller 1998/2002, xvi), but egalitarianism is an equally if not more important component of his position, mainly in the form of a strong commitment to democratic rule in matters epistemic. As it happens, Fuller's utilitarianism quickly gets redefined as a democratic political principle (*Ibid*).

There is a particular danger in the lack of a firm axiological basis when utilitarianism is combined with a post-humanistic stance. The utilitarian basis of a social epistemology will supply normative guidance only as long the subject of moral action is a human being equipped with definite preferences. It will tend to go astray, however, once we are in a position to manipulate the human genome and produce a "Homo version 2.0" who will derive pleasure and utility from the activities on which we decide. But which kind of human being should we create? Should it be one suitable for the global economy with its insatiable need for growth, which calls for a human being with unlimited capacity for hard work? Or a compassionate Christian? Or a hippie with great capacity for love and a low aggression level? There are no directions to be had from a purely utilitarian ethics. There is a need for an anthropological base here.

But worse is to come: what does follow from the utilitarian argument is an obligation to create a "utility monster" that generates much more utility than *Homo sapiens*.[7] It is obligatory, that is, to replace a species with a moderate capacity for utility production – such as us – by one with a superior capacity. In other words, we are committed to work to replace *Homo sapiens* with a successor. In Fuller, this conception is encapsulated in his recent move from utilitarianism to what he terms *superutilitarianism*, which is roughly utilitarianism operating with a very long time perspective, within which time *Homo sapiens* will have been transformed into *Homo version 2.0* (Fuller 2013, chapter 4). Here, one would like a normative social epistemology to offer us an alternative way.

As a first step towards developing such a new way, social epistemology should pay more attention to the humanities and their contribution to the overall knowledge basis of modern societies; this remains a sore spot in both versions of social epistemology. Perhaps an increased research effort in the humanities (which I would define broadly as encompassing psychology and anthropology) would help us, among other things, to draw a map of basic human capacities and needs and would provide a sound

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[6] Fuller 2002 offers a careful analysis of the relationship between naturalistic and "classical" (i.e., non-naturalistic) elements in a hybrid position of the kind I advocate here. Unfortunately, I cannot go into these issues here. But note that what I propose above is not a sketch for a standardised and uniform methodology for a future social epistemology, but just the terms under which the two wings of social epistemology could begin to talk productively with each other.

[7] I borrow the term "utility monster" from Nozick 1974, but use it in a slightly different sense.

naturalistic basis for the axiology of science as an instrument for the satisfaction of those needs. Hence, for several reasons, the humanities would deserve careful attention from a future reformed discipline of social epistemology. [8]

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