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On Gunn on Boundary Work

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In "Reflections on Boundary Work on Social Epistemology", Hanna Kiri Gunn offers an analysis of the pros and cons of academic boundary work. I argue that this is an aspect of a larger issue, i.e. specifying the most productive organizational structure of scientific research. In addressing this issue, we must keep in mind that scientific research is carried out on various organizational scales. We must also recognize that the optimal organization may not be achievable by design, but will emerge from the scientific practice itself. Kuhn's paradigm theory offered a picture of natural science as a self-organizing process, and I argue that elements of Kuhn's analysis are relevant also to social science and humanities, as well as to scientific activities on a smaller scale than paradigms. This is the scale at which the debates between different branches of social epistemology are played out, and I use items from the Kuhnian toolbox to analyze them. Finally, I address Gunn's remarks on boundary work in some earlier contributions of mine on social epistemology.

Boundary work and the organization of scientific research

In "Reflections on Boundary Work on Social Epistemology" [1], Hanna Kiri Gunn offers an interesting analysis of boundary work in science. Some stimulus for Gunn's reflections was provided by two articles of mine, to which I shall return towards the end of this reply. But since those articles did not address the issue of boundary work as such, I need to go a somewhat circuitous route before I can get back to them.

The question about the pros and cons of academic boundary work is an aspect of a larger issue, i.e. what is the most productive organization of scientific research? I shall henceforth treat it thus – at the risk of being suspected of imposing my own academic definitions and boundaries here. I do not of course intend to address the full scope of this large issue, which would among other things involve such diverse and complex questions as, productive of what, and productive for whom? Instead, I shall narrow it down to a fairly technical question which I believe resonates well with Gunn's agenda: Does science progress best in an inward-looking milieu, or rather in a more open, inclusive organization? We might talk about "introverted" versus "extraverted" modes of research practice and organization.

There is an issue of scale that has to be cleared up, however, before we can make progress with this question: are we dealing with research organization on the macro-, meso- or micro-scale? There are evidently no clear-cut divisions here, but some such distinction is useful, and it may be illustrated by examples. The macro-scale refers to the overall structural features of university organizations, the division between the chief scientific domains, and the distinction between individual disciplines within those domains. The meso-scale comprises particular debates within disciplines, and this is where I would locate the debates between the different branches of social epistemology. The micro-scale comprises the work of individual researchers, even down to single articles.

Gunn's text suggests that she has primarily the macro-scale in mind. Her list of positive effects of boundary work would mainly be pertinent to the general structure of universities

[1] *Social Epistemology Review and Reply Collective* 9 (8): 1-12.

and other research organizations. For instance, the advantage of knowing whom to consult with respect to a particular scientific question is a concern belonging to a high organizational level and can hardly play a role in the running debates between academic schools, or be on the mind of individual researchers (with the obvious exception of philosophers and sociologists of science). Gunn's recommendation of interdisciplinarity also suggests a focus upon the larger scales of research organization. Nevertheless, Gunn also addresses the meso-scale, as she uses the debates within social epistemology to illustrate the problems of boundary work. Finally, Gunn examines two texts of mine for boundary work, thereby shifting to the smallest of the three scales. So Gunn's analysis covers academic work across all the scales, and I will follow her lead in my reflections below.

Boundary work at the highest organizational level

Let us start at the highest organizational level, that of overall university organization. Here, a further issue immediately suggests itself: Are we engaged in a purely theoretical reflection upon boundary-drawing, or are we discussing guidelines for active institution-building in academia? For even though we might agree on the importance of felicitous organization of academic labor, this does not automatically translate into a call for university administrators to engage in efforts to establish the proper boundaries. For perhaps this aspect of scientific organization should be left to the internal dynamics of the scientific process itself. In this case, the message to eager Knowledge Managers would be, Hands off!

If the notion of a natural-grown division of academic work appears obscure, we may point to an obvious analogy within the economic sphere. Here, there is an optimal distribution of activities, defined as the one that will produce the highest satisfaction of consumer preferences, at least in the sense of Pareto-optimality. At the same time, it is a prevalent doctrine in the economic profession that the only way to achieve this is for government and other regulatory agencies to stand back and let the market decide.

There are indeed suggestions in the literature about a "natural" compartmentalization of research activities, at least on the large organizational scale. One is implicit in Kuhn's celebrated theory of paradigms. The paradigm is the "natural" organizational unit of scientific work, and the shifts between "normal science" and scientific revolutions that go on within paradigms is the natural way for science to progress. It is characteristic of successful paradigm work that it will often disrupt existing disciplinary borders; these are the Scientific Revolutions referred to in the title of Kuhn's book. Newtonian mechanics dissolved the difference between terrestrial and celestial dynamics, Dalton's atomic theory contributed to breaking down the barriers between physics and chemistry. From this flows a general corollary: Science progresses best in a bottom-up regime where divisions between disciplines and research areas emerge spontaneously along the way. Any attempt to decide ahead of time what item of research belongs where is detrimental to the growth of science.

Kuhn's work arose out of a Post-WW2 context in which science faced the threat of strong political direction, and his work may be understood as a reaction to this danger (cf. Fuller 2000). It may be argued that Kuhn's picture of science was unduly influenced by this concern, and that Lakatos' subsequent development of Kuhn's ideas in the theory of Research Programs redressed this and other peculiarities of Kuhn's analysis. Lakatos devised a metric that would allow a rational assessment of the merits of paradigms/research

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programs. He introduced the notion of "progressive" vs "regressive" problem shifts, including a comparative metric that would even allow assessment of the relative progressiveness, or regressiveness, of competing research programs. Thereby, room would be made for the interventions of a Knowledge Manager to declare one of the competing programs to be defunct and shift resources to its rival. Moreover, Lakatos observed that successful research efforts do not progress randomly but are typically governed by a script that anticipates future developments; hence the name of "Research *Programs*". Lakatos referred to this script as a "positive heuristic".

It has often been pointed out, however, that the exactitude and objectiveness of Lakatos' comparative metric is only apparent. He never seriously addressed Kuhn's point about the incommensurability of theoretical languages, and even introduced a new problem of his own, documented through convincing historical examples, namely the possibility that a degenerating problem shift may rebound after a long time of decline and apparent oblivion. This means that Knowledge Managers can never know for sure that the time has come to shift research resources elsewhere. [2]

Another way to put this conclusion is this: Although the distinction between legitimate and illegitimate boundary work – aka gate-keeping – points to an important issue of academic organization, it does not itself deliver the tools to properly distinguish between the two kinds. There are no purely organizational or sociological signs that will tell us the score when paradigms or Research Programs compete. One has to let the academic debate run its course and reach a natural endpoint. There is no knowing ahead of time when this will happen, and what the outcome will be.

Does this conclusion apply generally?

Does this conclusion apply to all levels of scientific work, or only to the high level at which paradigms and Research Programs are found? And does it apply across the main scientific domains, or to natural science only? In the Preface to *Structure*, Kuhn famously declared that there are no paradigms in "social science" – among which he included some humanities – as is supposedly shown by the typical disagreements about fundamentals in those disciplines. Kuhn secured the truth of this claim by definition, making hegemonic status in a discipline part of the meaning of the term "paradigm", but this was on over-hasty conclusion and a misapplication of his own analytic tools. The most interesting element of Kuhn's theory was the analysis of how research progresses under the power of its own internal resources and according to a characteristic dynamic pattern. This starts with an impressive exemplary result, which inspires an effort to expand its reach, and this effort proceeds in stubborn disregard of negative results, which are dismissed as mere "anomalies". It may be true that in the natural sciences, this practice of "normal science" is often so successful that a particular line of research becomes hegemonic. But monopoly is not a condition of efficient research.

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[2] The lack of a simple technical procedure for assessing the future returns on academic investment has of course not deterred administrators from trying to "pick winners". Modern science managers use a somewhat related technique in comparing the relative publication records of researchers.

Kuhn was of course right that continued disagreement over fundamentals is detrimental to the growth of a scientific discipline. The paradigm theory alerts us, however, to the existence of two different kinds of discussion about basics, one being a genuine debate about how to interpret or extend shared fundamentals, the other a pseudo-debate where people talk past each other because they actually represent different paradigms with their mutual "incommensurabilities". Both kinds of disagreement would be detrimental to progress, but the problem would be much more serious in the former case. Here, it would be necessary to reach consensus on fundamentals before scientific work could proceed, whereas in the second case, each side could return to the safety of its own paradigm and work there. The skirmishes would merely be distractions and a waste of time better spent elsewhere.

This analysis suggests that the mere existence of a rival is not in itself detrimental to productive work within a paradigm. The rival may just be ignored; indeed, this would be the recommended policy since interaction between paradigms is largely irrational, according to Kuhn. As long as the rivals have a critical mass of manpower and other resources at their disposal, the process of normal science can move forward within each of them, and the existence of a competitor is of little concern. Thus, Kuhn's ideas about "normal" science and about "natural" units of scientific work transfer to areas of science where there are more than one such unit at play, and hence potentially to social science and humanities.

Fundamentally, this is all just about the division of academic labor. For science to progress, individual researchers have to take a lot for granted when going about their business. They are hard at work on solving various particular "puzzles", and should not waste time looking up from their little corner to survey the overall field. The logical positivist picture of science against which Kuhn rebelled completely neglected the issue of research organization, and thus implied, by default, that scientific work is scale invariant: On any organizational level, science consists in the collection of data and their subsequent inspection for regularities that might suggest underlying laws. By contrast, division of intellectual labor and scale variance is at the very core of Kuhn's "normal science", and in Chapter 3 of *Structure*, "The Nature of Normal Science", Kuhn enumerates several kinds and sub-kinds of such work. But Kuhn's analysis also implies the existence of a higher organizational level where a broader perspective is adopted, assessing the overall progress of the paradigm. For although "anomalies" are systematically put aside at the level of "normal science", a tally is nevertheless kept, and if the total is forever increasing and no puzzle is ever solved, this is recognized as a sign of crisis.

Boundary work in humanities and social science

I believe that some of the structure of paradigmatic work, or of Lakatosian Research Programs, can be found in humanities and social science as well; at least enough for similar organizational implications to follow. It is basically a matter of division of academic labor within the regime of "normal science", and of the existence of a "positive heuristic" that maps out the course of future developments. In social science, neo-classical micro-economics might be an example, while structuralism might qualify in the humanities.

Obviously, my argument does not imply that all work within social science and humanities manifests a paradigmatic pattern – but then, nor does all work in natural science. There are

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also important methodological differences between paradigm-like work in those domains, primarily with respect to the precision with which anomalies can be identified and assessed for epistemic import. Famously, the battle between Newtonian and Relativistic mechanics was decided in favor of the latter by a minute but precisely quantifiable displacement of the observed location of a star. It is difficult to imagine that anything similar could occur in the humanities. I cannot pursue this question here, however, as it would take us too far away from the debate between two schools of social epistemology.

It must be admitted that the introduction of Kuhn's analytical tools into social science and humanities has sometimes been counterproductive: There have no doubt been attempts to create artificial "paradigms" in humanities and social science by means of strategic boundary work, i.e. by declaring one's own research efforts to constitute a distinctive paradigm, and refusing to respond to criticism from other "paradigms" by invoking "incommensurability". Such misuse does not, however, undermine Kuhn's general conclusion that the formation or elimination of academic schools or disciplines should not be decided from above. Academic discussions should largely be allowed to run their course, and only afterwards should one ponder any organizational implications. There are sufficient similarities between natural science paradigms and development patterns in social science and humanities for the arguments in favor of an "introverted" policy, and for science managers to step back.

Boundary work at the meso-level

The next question is whether or not this conclusion carries over to smaller organizational units of research. This would constitute the meso-level of the hierarchy mentioned at the beginning of this article. I believe that the answer is yes, and that paradigm-like features of research dynamics can indeed be found there. As a matter of fact, I believe that Analytic Social Epistemology possesses paradigm-like features, or perhaps more precisely something akin to the "positive heuristic" of Lakatos' analysis. Goldman laid out the steps of this heuristic with admirable clarity in Knowledge in a Social World (Goldman 1999), and I tried to trace them in a recent contribution to SERRC called "Neurath's Ship Meets Social Epistemology". The "exemplary" item of ASE is Bayesian inference, which is a well-established statistical method for adjusting the degree of credibility of a hypothesis on the basis of empirical data. As the next step in his "positive heuristic", Goldman adds testimonial data to the data pool, as is necessary if ASE is to move beyond individual epistemology and become genuinely social. Next, the methodology is augmented to handle disagreement among information receivers. A group of epistemic agents may assess the available evidence differently, even if all employ the Bayesian method; hence, they must work out their differences through argumentation. Thus, a theory of dialogical argumentation is needed, and Goldman sketches one out in the text. Goldman next moves on to institutional producers of knowledge, with special emphasis on science, and also analyzes the functioning of the media as disseminators of the knowledge produced. Finally, he examines democracy as a mechanism for marshalling knowledge and deploying it to promote the population's ends.

I mentioned earlier that successful paradigm work will often disrupt existing disciplinary borders. This feature is also found in ASE's research program, which dissolves one of the most hallowed boundaries surrounding philosophy itself, that between philosophy and empirical science. Room is made for this "naturalistic turn" by the *reliabilist* stance of ASE, i.e. the principle that belief in a proposition p (including a scientific belief) may count as justified if generated by a mechanism that reliably generates truths, even though that mechanism does not operate according to "rational" epistemic principles; it may be purely causal. This allows epistemology to be enriched with empirical data concerning the truth-generating propensities of alternative ways to organize epistemic social practices, including science. Organization theory has long since used empirical data to identify "best practice" in various fields, but a similar procedure was not recognized by analytic epistemology until the advent of reliabilism and the naturalistic turn. With this move on the part of ASE, the sociology of knowledge becomes a legitimate part of epistemology, and the sociology of science a legitimate part of the philosophy of science.

The above analysis was intended to show that some humanities research efforts display the same kind of internally determined development as paradigms, although on a smaller scale. This may even include the ability to break down well-established academic traditions and disciplinary boundaries. The similarities are strong enough for my original conclusion to carry over: Such research efforts should be allowed to shape their own organizational boundaries. There is no way to second-guess the development or to interfere with it, the discussion must be allowed to run its natural course.

Boundary work between ASE and CSE

My argument obviously does not imply that all work in humanities and social science is governed by a clear "positive heuristic". As a matter of fact, this label would hardly apply to CSE, for instance. It is not for nothing that Fuller labels his enterprise as *critical*. His criticism has two targets, one being inequities in the distribution of epistemic goods in society, the other being blind spots in such social epistemology enterprises as ASE and Latourian Actor Network Theory. Because of its critical, outward-facing stance, CSE does not show the same "inner-directed" history of development, and hence does not constitute a Research Program in Lakatos' technical sense.

This divergence between CSA and ASE helps explain certain features of their rather unproductive interaction. Analytic social epistemologists have shown little interest in engaging with Fuller's criticisms, in part because they consider CSE to be an entirely different kind of enterprise. Whereas ASE is engaged in determining the societal conditions propitious for generating truths (it is "veritistic", to use Goldman's term), Fuller is branded as a "veriphobe", an enemy of truth. Fuller has vehemently opposed this labelling (Fuller 2012).

There could a suspicion of gate-keeping here on the part of ASE, an attempt to dismiss the debate as a case of two paradigms talking past each other. My present aim is not to adjudicate this question, however, but rather to insist that it can only be decided by a detailed analysis of the contributions. No external examination of publication patterns, of who debates with whom, or similar bibliometric or micro-sociological data will do the job. It would be agreed on all sides that the development of truly novel ideas in science require a certain amount of insulation against criticism from the old guard, and avoidance of irrelevant and distracting debates. The new ideas need protection as long as they are at the fledgling

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stage. Only a detailed internal analysis can determine if the protection is excessive and amounts to academic gate-keeping, or perhaps better "gate-installation". Moreover, even though an analysis of the debate might reveal instances where arguments were mishandled on either side, such analysis should be considered as an "internal" input to the debate itself, not as some external, higher-level audit of the proceedings. The debate musts run its own natural course, without the benefit of some external authority to police the process.

Thus I draw the same conclusion here as with respect to the larger, macroscopic scale: the sociological notion of boundary work and the distinction between legitimate and illegitimate versions – aka gate-keeping – point towards important features of academic organization, but they do not themselves deliver the tools with which to determine the issue of legitimacy. The situation is the same as with Lakatos' notion of "progressive" and "regressive" problem shifts. There is no choice but to let the program run its course.

Boundary work on the smallest scale

This finally brings me to my own contributions, as reviewed by Gunn. They are my 2013 article, "Two Kinds of Social Epistemology", *Social Epistemology Review and Reply Collective* 2 (8): 79-104, and a follow-up piece from 2019, "Two Kinds of Social Epistemology Revisited", *Social Epistemology Review and Reply Collective* 8 (12): 29-38. With this, we move down to the smallest scale at which boundary work may be carried out, i.e. at the level of the individual researcher and indeed, as in the present case, in particular articles.

Let me first emphasize that a chief aim of my work on ASE and CSE has been to foster collaboration between these two branches of social epistemology and to dissolve artificial boundaries between them, not to fortify such boundaries. But that obviously does not exclude the occurrence of other instances of gatekeeping in my own article. Gunn believes she spots one in my apparent restriction of the debate to philosophy. But this is inaccurate since the debate that I review had already transgressed the boundary between philosophy and empirical science. This move was made in Fuller's CSE from the very beginning, and it was also gradually made by ASE through its reliabilist, naturalist and social turns. Thus my perspective is not narrowly philosophical, since it accommodates the naturalist and social turns in epistemology that is characteristic of both ASE and CSE.

In my articles cited by Gunn, I did not suggest that ASE and CSE are the only kinds of social epistemology, as Gunn indeed recognizes; after all, their titles mentioned "Two Kinds of Social Epistemology", not "*The* Two Kinds of Social Epistemology". But ASE and CSE are highly interesting objects of examination for reasons that I hope will have transpired above. Goldman's ASE is the most organized and well-manned enterprise in the business, revolving around a well-defined positive heuristic. It has inherent limitations, however, and CSE deserves examination because it pointed to these limitations from the start and indicated a way to overcome them. I took an early interest in these two representatives of social epistemology and have written a number of articles about their relationship. At this detailed scale of academic work, I do not think it would be fruitful if every contribution had to start with a section locating it in the broader academic field, and specifying alternative perspectives that *could have* been adopted by the author but were not. Division of academic labor is fruitful and legitimate also at this level, and when making an article-sized contribution to an academic debate, an author will necessarily have to take a lot of tacit premises for granted. Explication of these premises, and assessment of their fruitfulness, has a place on another scale of academic work.

Let me finally return to the general issue of boundary work in science. My conclusion is that application of this sociological notion within social epistemology and the philosophy of science has considerable eye-opening potential, as is frequently the case when sociological notions are thus applied: It is often sobering to compare the supposedly lofty and almost other-worldly activities of science with the struggle over resources and influence between social factions. One lesson to be learned is that, like all organized social practices, science can be examined at different levels of aggregation, and different conclusions will ensue for different levels. But the illumination from the sociological analysis reaches only so far. In particular, the distinction between legitimate and illegitimate boundary work in science cannot be drawn in sociological terms alone.

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