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Method against method: swarm and interdisciplinary research methodology

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Part of a special issue on “swarm methodology,” this paper, written by a swarm participant, reflects upon the purpose and value of this kind of interdisciplinary research methodology. First, by way of a recognition of the interdisciplinary status of this paper itself, the question of what we hope to accomplish when we engage in conversations across disciplinary boundaries is broached. Second, a discussion of the practice of peer-review provides an approximate view of one paradigmatic understanding of how we produce a “conversation” within a given established research methodology. We are then, third, able to consider a number of possible related ways in which we might understand the value of a conversation between research methodologies. Finally, the common intuition that there is a concrete value specifically within a “holistic” or “synergistic approach” is addressed, and the swarm methodology put forth as a very likely place for such a value to emerge, if it is to emerge anywhere.

Keywords: swarm; methodology; interdisciplinary; Consumer Electronics Show

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Preface

I run the risk of being absurdly self-reflexive in the following, most especially in my preemptory reflection on self-reflectivity. I am a philosopher writing for a journal oriented more towards the social sciences than the humanities. Further, I am writing for a journal “for the Study of Race, Nation and Culture,” and I will not be discussing race, nation, or culture. What am I doing here?

That is the primary question: What am I doing here – what conversation can we have; what can I offer to you, my reader, in this venue? This is what I can offer: my struggle with this very question. What is my social identity qua the community of *Social Identities*? By speaking in this venue, do I inevitably assert the foundational primacy of philosophy as a discipline? This is, after all, not an uncommon form of arrogance in my field. If not, how do I speak across this boundary? What do we have in common?

This is a special issue on a particular interdisciplinary research project which *is* oriented towards the social sciences, and *does* deal with race and culture. As a participant in that project, I was always an outlier – my concerns different and more abstract than those of others. The research which I have conducted based upon this interdisciplinary project has concerned an attempt to extend Adorno and Horkheimer’s discussion of the “culture industry” (2002 [1947]) from content to form; from entertainment media to consumer electronics themselves, and to begin to construct a kind of “phenomenology of the end-user.” In the process of our collaborative work, I always held my view on process slightly under erasure. The gap between my field and others was wider, and there was a constant question of whether my interests, concerns, and views were really valid for others, and worth bringing to the table as an equal partner.

This, I still feel, is basically right. My thoughts about consumer electronics in the context of Schopenhauer’s comparison between boredom and a bird of prey¹ might be *interesting* to my colleagues in the social sciences, but it just simply isn’t *useful* to them, as the data and reflections on the data that they shared amongst one another clearly was. So, a very real question throughout this process has been what, exactly, I was doing there – or, more generally, what do we think we can accomplish in truly interdisciplinary research? Traditional research is *deeply*

grounded in disciplinarity – it seems that perhaps it is *only* because we focus both on topics of discussion and on a relatively few number of relevant approaches that we can, as a community of scholars, get work done without simply finding ourselves at sea every time we approach an object of study anew. Disciplinarity being enforced through various methods of in-group definition and definitional exclusion, what *research* goal could be served by a purposeful transgression of these important functions of societies of researchers?

So, while, in a way, I do want to make the philosopher’s claim of privileged knowledge – “I’m talking about philosophy of science, I can tell you something new about your own method” – I also intend to claim a sociological privilege: insofar as I am a philosopher, I am a scholar; insofar as I am a scholar, I am a member of a community of research; insofar as I am part of a *community* of research, research is governed by social functions and norms; and, finally, insofar as research is governed by social functions and norms, the sociologist can rightly make the same claim of privileged knowledge. Research is social, and my disciplinary identity is something which this project, in its interdisciplinarity, must make problematic.

Now, I do not intend to try my hand at being a sociologist. I will remain a philosopher, and I will take up my position as a philosopher by speaking about knowledge and methodology. But, in the following, I mean to also perform that transgression of which I will be, in the following, speaking. My participation in this interdisciplinary research was always a challenge to disciplinary boundaries, and speaking of this challenge across disciplinary lines, I hope to illustrate the possible value of interdisciplinary conversations as much by the very process of my writing, and your reading, as by the actual argument which forms the content which I write, and you read.

But this is more than enough of these Ouroborean considerations. I have said the following is a performance of productive transgression of scholarly identity. I can do no more than start the performance, and find out, in the process, whether I will have succeeded.

Introduction

The swarm methodology is, to the best of my knowledge, a monotypic genus – that is, it has been only attempted once. We who are writing on this are doing so because we believe the method to be of a more general value, both as a particular method and as a case study that illustrates more general aspects of research methodology. In this consideration, I hope to present to the reader this example as a possible paradigm case for a possible interdisciplinary methodology. Whether or not this succeeds, a larger goal applies as well: the consideration of this unusual case as telling us something about the goals, function, and value of interdisciplinary work in general.

While writing on the subject in this very early stage of development has very clear disadvantages, this can be said in favor of writing on it at this point: our example is at least easy to decide upon. In January 2007, an interdisciplinary group of scholars gathered in Las Vegas to attend the annual Consumer Electronics Show. The group consisted of scholars housed in Departments of Sociology, History, Journalism, Communications, Philosophy, and Science and Technology Studies. We gained access to the industry-only trade show by obtaining passes as press, industry analysts, or purchasers for our firms – all of which were technically correct according to the CES definitions. We descended upon the show, spreading out and blending into the expected social roles to varying degrees – this process of negotiating identity being part of the process of study itself. Over the course of the day, groups of scholars met to compare notes

and reflections, and in the evenings the scholars met as a whole in order to compare data collected, and preliminary conclusions that we felt those data were pointing us towards. These conversations varied in a number of ways, and their diversity and range can be illustrated by these examples:

- (a) recommendations of particular events and locations of general interest, e.g. the strikingly nostalgic “archaeological dig” display put on by Qualcomm;
- (b) reporting of data known to be of interest to the research of others, e.g. a report, from a scholar who is not pursuing data on representations of gender to a scholar who is, of the presence of a pink Tazer, described as “smaller, to fit in a purse;”
- (c) mention of data of interest, pursuant of seeking confirmation of a trend in the observations of others;
- (d) discussion of possible trends, following ‘hunches’ for which one scholar did not have sufficient data;
- (e) comparing notes on methods of gathering data, and notes on the applicability of data collected to quantitative or qualitative analytic methodologies; and
- (f) making plans for coordinating and organizing future data collection.

A general method of “swarm research” is by no means established through this experimental attempt, but for the purposes of this paper, let’s describe it as follows. A group of scholars from diverse theoretical perspectives and/or disciplines congregates on the location of an object of study. They avoid a theoretically pre-determined perspective on the object of study as much as is possible, although the assumption is that the diversity of theoretical frameworks will result in different – perhaps radically different – conceptualizations of the object of study. They immerse themselves in the object of study, and then convene in order to share reflections upon the object of study, this two-stage process being repeated for several iterations.

What would be the advantage of such a collective method? To embark upon a path towards an answer, let us first look into a particular prominent and well-respected example of collective research methodology within both the social sciences and the humanities: the peer review process.

Peer Review and the Intradisciplinary Conversation

Peer review seems relatively straightforward in the “hard” sciences, in one way at least: peer review seems like an intuitive extension of the basic idea that valid experimental results must be replicable by different scientists in different locations at different times, as long as testing conditions are kept constant. While the peer review process itself may not involve the attempt to verify or falsify experimental results, one important function of peer review may be understood as an enforcement of procedural norms which ensure that further studies can be relevant to the study in such a way that the results of further studies could count as either tending to confirm or to falsify the results of the study under review. In making this claim, I am broadly, and perhaps boldly, ignoring all manner of controversy about the exact process by which “falsification” or “confirmation” occurs in the hard sciences, including the question of whether this is primarily a rational or systematic process at all. My point here is very limited: I mean only to claim that the view above seems to be a commonsense understanding of one function of the peer review

process from within a commonsense understanding of method in the hard sciences more generally.

Now, in the social sciences, although this intuitive understanding is clearly relevant, its direct applicability is less obvious. It is, for example, on the one hand, not clear that the Marxist sociologist will have something to say to the ethnomethodologist that will help her on her own terms, or vice versa; and, on the other hand, it is also not clear that it is epistemically responsible to conduct peer review only *within* rather than *between* such theoretical approaches to research in the social sciences. In the humanities, the account of peer review that places its epistemic value upon the making possible of verification or falsification of results seems even further from the case.

In saying this, I mean in no way to impugn the value of such peer review, but only to argue that to make sense of the value of this process, we must consider additional factors. Most simply, we can provide a fuller explanation of the function of peer review by considering the policing of social norms within scientific communities as a means to exclude unacceptable deviations from definitive elements of group identity. To some extent, this is a deflationary and merely descriptive account, but it clearly applies, for example, minimally, in the preservation of group identity through the exclusion of terminology considered by the profession to be unprofessional, racist, or sexist; or, more robustly, through the partitioning of kinds of discourse and subject matter, where, for example, explanatory accounts such as

we are no longer living in a culture dominated by the image because we are the pure image (Kroker, 2002)

will occur in some journals, and those such as

As in the case of power, severity is evaluated at a point $\mu_1 = (\mu_0 + \gamma)$, for some $\gamma \geq 0$; yet the above holds because for values $\mu > \mu_1$ the severity increases (Mayo & Spanos, 2006)

will occur in others, despite that either of these claims may be relevant to the work being done by a researcher housed in a Department of Philosophy, depending upon her area of research.

In line with this observation, let us say that among the functions of peer review is the enforcement of institutional expectations regarding subject matter, investigative methodology, basic interpretive schemes, and so forth. We may rightly ask what the epistemic value of such a process might be. The most obvious explanation of the value of such enforcement of norms is that, by regulating the boundaries of the research program, we can ensure that the data collected are in a theoretical context sufficiently narrow to allow findings by one scholar to be *relevant* to the research of another scholar within the same program, thereby approaching the possible intuitive purpose of peer review within the hard sciences previously discussed. In other words, even in the humanities, it can be asserted that the enforcing of these norms allows for the kind of relevance and consistency from one study to the next *requisite* for one article to be able to support or oppose another. While falsification and verification are, in many cases, inappropriate terms here even on a loose interpretation; nevertheless, without *some* constancy of framework, we cannot even make sense of successive articles as being in dialog with one another.

Yet this constancy of framework must be rather variable, for it is clear that the idea of an interdisciplinary peer-reviewed journal is at least not incoherent. Let us then say merely that this

constancy can take place at differing levels, or in differing kinds of frames. Differing views can be profitably compared

- (i) within a research program;
- (ii) between research programs but within disciplines; or
- (iii) between disciplines but within a range of subject matter.

It is in this last case where I believe the special value of swarm methodology is to be found. To explain this, however, we'll need to make clear how we can make sense of this vague talk, above about "constancy at differing levels or frames."

To illustrate this, consider a variety of methods of surveilling an area. With a number of standardized, say, ceiling-mounted cameras, the kind of information provided at each point is constant, and the data can be combined in a purely meriological way, assembling a large picture through the spatial continuity of information. Here, the cameras are only numerically distinct, and a greater or lesser number of them determines the granulation of information resultant. That is, since there is no significant increase of information provided through overlapping fields of view, the more cameras we have, the more each can focus in on a small area, and thereby provide more detail. Alternately, redundancy may be desirable, perhaps due to the untrustworthiness of the cameras, or their tendency to malfunction. Here, where we have assumed that the devices have been standardized, this could be the only reason why overlapping coverage would be desired.

Secondly, we might imagine a different approach, wherein we use different methods of recording the same information. We might imagine using a number of different brands of camera or film, or, perhaps, different kinds of sound recording (for example, a hidden microphone vs. a boom microphone vs. a shotgun microphone). In this case, there would be a different obvious reason for overlapping coverage – namely, that preference for such an arrangement, rather than the fully standardized devices in the first example, above, might be the result of an uncertainty regarding the best way of gathering the data of interest.

Thirdly, we might have several entirely different kinds of information-collecting devices – that is, devices collecting data not just using different methods, but collecting different *kinds* of data; whose connection is overlapping, but incommensurable. For example, if we have normal video cameras, infrared cameras, and audio recorders, each will provide data related through a common referent rather than a common informational structure.

Among studies within a given theoretical approach we (roughly) want to gather data using investigative tools that are calibrated to one another, in order to ensure that there is a contiguity of data, and a clear applicability of results in one study to expected results in a neighboring or overlapping study. Thus, the first example above can be used as an analogy. When engaging in a study spanning rival theoretical approaches, a primary question of interest may be one of the relative fidelity of rival methods. Given that we are interested, for example, in questions of the social meaning of gender, what differences do we see through the lenses of structuralism vs. constructionism? This corresponds to the second example above.

In interdisciplinary research, the situation seems to be more in line with the third example, for the question of interest is not clearly one of fidelity, but more likely is a question of what shows up in a given general spectrum, and whether and to what extent those data correspond to data in another general spectrum. The different "lenses" of rival approaches are relevant to one another *as rivals* insofar as we assume that they frame the object of inquiry in the

same manner, and, hence, that they offer necessarily mutually exclusive accounts. Hence, I may ask, “As a sociologist, which account ought I to prefer in this kind of investigation?” in the same way that I might ask, “As a photographer, which film speed ought I to prefer in this kind of shoot?” The incommensurability of audio vs. video recording consists in their non-rivalrous relation, namely, that no matter how excellent my video, and how poor my audio, the excellence of the one cannot replace or replicate the data from the other. Each has its own sphere or spectrum of data, and we usually are interested in such incommensurate approaches, not in order to maximize accuracy, but to maximize comprehensivity. After all, if I seek accurate information, I likely know what *kind* of information I seek, and, thus, the relevant question is what kind of e.g. camera to use, not what kind of recording *simpliciter*. The question is one akin, instead, to “As a sociologist, I wonder what biology, psychology, or economics can tell me?” The answer may be challenging, confusing, or unexpected, but it will not be rivalrous – it will not be a direct challenge; there will always be a process of comparison and translation before obtaining any results which can validate or question any particular prior claim.

Now, this picture is, of course, an oversimplification, and, indeed, so much so that I must remedy this to some extent before moving any farther. The picture above is predicated upon a view of the disciplines that assigns a singular and unitary object of inquiry to each. Sociology studies society. Physics studies . . . physical objects. Chemistry studies, well, chemicals. Obviously such a picture is absurdly simplistic – one of the things at issue in rivalrous approaches within the disciplines is the definition of the proper object of study, and the circularity of each of these simplistic definitions is unhelpful at best. To use an example from above, the difference between the understanding of gender in structuralism versus in constructionism is not merely one of method, but, indeed, the object of inquiry is depicted by each as an entirely different kind of thing. Furthermore, this is not merely an idiosyncrasy of the social sciences – hearkening back to Kuhn’s discussion in Chapter X of *The Structure of Scientific Revolutions*, we might just as well ask whether Aristotelian physics or impetus theory better describes the motion of the pendulum; the pendulum, as an object of inquiry, only exists in one of the two systems (1970, p. 120).

So, to correct the oversimplified view above, we should have to keep in mind that some different perspectives lumped under the same disciplinary heading may have an incommensurate relation to one another. Such intradisciplinary breaks may, indeed, be more extreme than interdisciplinary differences – for example, a Continentalist philosopher may very likely have more in common, both in terms of methods and objects of inquiry, with a theorist in sociology than with another philosopher across a intradisciplinary divide – for example, one concerned with contemporary metalogic.

Thankfully, our goal here is not to describe the relationship between paradigms and/or between research programs, or to describe how those differences might correspond to disciplinary definitions. We embarked on this line of inquiry in order to delineate some ways in which research may be organized into different kinds of conversations. Despite the incomplete and problematic nature of the delineation above, I believe it will provide sufficient grounds to point out the possible benefits native to the swarm methodology, and to begin to consider the kind of circumstance in which this methodology may provide a distinct epistemic benefit.

The epistemic value of intermethodological conversations

Now, taking our admonishment, above, into account, let us say more modestly that it is at least coherent for us to differentiate between these different kinds of conversation between scholars:

- (1) Intramethodological studies, whose data are meriologically related
- (2) Rivalrous intermethodological studies, whose data are of a kind²
- (3) Non-rivalrous intermethodological studies, whose data are incommensurate

The first kind of conversation is simply the standard furtherance of a research program, broadly construed.³ The second kind of conversation is presumably primarily of interest where the most useful or appropriate approach to a given topic is unclear. What, then, is the purpose of the third kind of conversation, and when are we likely to benefit from it?

To find such a case, it seems at first that we must rule out any conversation whose goal is to choose one or more preferred perspectives over relevant alternatives, for this would imply a kind of rivalry between the views, and a kind of commensurability which would justify the sufficiency of one perspective to disregard another. It was said before that no matter how poor the sound quality and how excellent the video, the one cannot play the role of and replace the other. Yet, consider surveilling an unlit windowless space. Here, the video supplies only darkness. Now, admittedly, audio on its own could never present this information, but, still, we have grounds to say that the subject matter is such that audio is more relevant than video. Even though the results of the one method, indeed, cannot replace the results of the other, it is still the case that the one method is fecund, and the other sterile.

Parallel to this, let us now say that one kind of motivation towards this third kind of conversation can be described as

- (3)a. A conversation in which it is to be determined which perspectives are relevant to the object of inquiry; and to distinguish between perspectives from which the object is of interest, and from which is it not.

This kind of inquiry is one that seems to be engaged in infrequently, perhaps for good reason. We may rightly ask what kind of object of inquiry could be so indeterminate as to make such a conversation to be of value. We will return to this question after we have identified other varieties of this third kind of conversation.

To return to the audio/video example, we may imagine that a regular pattern becomes apparent in one data set, but not the other. In this case, the regularity in the one provides a clue relevant to the other, even though it does not tell us anything directly about any particular data present or absent. An occasional 60-cycle hum in the audio might lead us to look for slight changes in light levels consistent with a florescent light out of frame, which we may not have otherwise noted. In this case, the information from the one set does not provide us with anything that could replace data from the other, but indicates an area of potential interest within the other, thus constituting relevance without commensurability.

Parallel to this example, we might imagine

- (3)b. An intermethodological conversation in which it is hoped that the various perspectives will gain an advantage for their

independent, preexisting, and continuing intramethodological inquiry.

In addition to the two possibilities considered above, we should not ignore the possibility of a holistic value – in our example, that we may not be so much concerned with either audio or video, in particular, as with gathering as full a record as possible. Even though we might intuitively accept the value of this holism, it isn't immediately clear exactly what kind of epistemic value might be found in an intermethodological conversation that does not fall under either (3)a. or (3)b. above. Let's look at a couple of possibilities.

We might understand the epistemic value of a holistic perspective as reposing in further study at a later date, wherein we justify recording everything we can through the future value of the archive. This, however, is a variety of (3)a., for the implicit basis of this potential future epistemic value is an assessment such as: "I think I know what's interesting here, but I'll keep everything, just in case I'm wrong."

Perhaps we can turn to this example instead: it has been well documented that persons watching debates on television assess them differently from those listening on the radio. There may be some emergent epistemic value in the multiple modes of access – in this example, visual as well as auditory. The version of holism in this example, however, may be a variety of (3)b., as we may account for the differences in perception not through emergent knowledge, but the mere highlighting of patterns within one mode of access through observation of a pattern within the other mode of access, as, for example, the viewer's observation of the sweat on Nixon's brow may lead her to interpret his speech as containing uncertain rather than qualified claims, or anger instead of conviction.⁴

Although our use of analogies seems to be failing us here, nevertheless, it is a commonly held belief that there is some kind of holistic, synergistic, or emergent benefit to interdisciplinary or intermethodological engagement that we should include this as a *possible* goal of non-rivalrous conversation between incommensurate perspectives, even though, at this point, we can give no substantive account of wherein that epistemic value might repose. So, for the time being, we will give a third option only as

(3)c. A conversation the aim of which is neither within the compass of any of the methodologies employed, nor within them all in aggregate.

Despite the purely negative nature of this definition, we will leave it as it is for now, and will return shortly to the question of how we can give determinate content to this possibility.

Before we move forward, however, it may be of value to summarize the discussion thus far. First, we considered a commonsense intuition of a function of peer review within the hard sciences, and established more firmly a parallel function of peer review within the social sciences and humanities: to enforce norms which allow for relevance of work requisite for what, minimally, we might call a 'conversation' between scholars. This discussion being predicated upon a shared methodology or research program, we then turned to the question of what this consideration of epistemic value might tell us about the epistemic value of 'conversations' *between* methodologies which are either rivalrous and of a kind, or are neither able to share data sets in a straightforward way, nor able to be clearly in contention with one another about the proper depiction of the object of study. Finally, we then considered three ways in which we

might find epistemic value in this latter kind of conversation – one that employs diverse methodologies which are incommensurate and non-rivalrous. Forms of such conversations which may be of epistemic value include: (3)a. a study which determines which methodology is a good fit for the object of study, (3)b. a study which determines what one clues methodologies might offer one another in pursuit of their provincial goals, or (3)c. a study which brings about some emergent value, of an as yet undetermined nature.

Having gone through all of this as a propaedeutic, we can now ask whether and to what extent this account of possible epistemic values of interdisciplinary or intermethodological study helps us offer an account of the possible epistemic value of swarm methodology.

First, let us consider the possible epistemic value of swarm methodology in terms of (3)a. determining the relative values of methodologies and/or research programs regarding a particular object of study. While we noted above that it seems like it would have to be an unusual circumstance in which the object of study would be so indeterminate as to find a value in the mere determination of the relevance of various methods or approaches, it seems like our particular case study is one such example. When stepping onto the show floor, we, of course, each carried our own background and training with us, and this gave for each of us a certain range of objects and categories of interest. In addition, while we tried to avoid approaching the show with a particular study or thesis in mind, we all of course carried with us our own preoccupations with certain subject matter; one scholar looking at the rhetoric of expertise in the marketing of high-end audio equipment, another looking at the structures of interface design which construct the user as consumer, and a third looking at representations of domestic life as related to consumer electronics.

I do not think it is controversial to claim that it is a general expectation that, although a scholar may come to a given object of study with a perspective partially predetermined by her training and by her particular interests in some subject matters over others, nevertheless, there ought to remain a certain amount of responsiveness to the peculiar nature of the object of study itself. The purely quantitative macroeconomist who looks at cologne advertisements and refuses to notice the importance of constructions of sexuality and symbolic exchange, we may rightly say, has failed to be appropriately responsive to the particularities of the object of study. We each, however, cannot be maximally responsive, for a legitimate and important aspect of our professional training is the sharpening of focus in certain areas to the exclusion of others.

Given these hopefully non-controversial assumptions, a clear account of a possible epistemic value of swarm methodology can be put forth under (3)a. above. Namely, that by increasing the number of scholars approaching a given object and the diversity of methods and subjects of interest within that group – assuming they are able to retain enough interconnection in order to find one another's work relevant – we can expect an increased responsiveness to the object of study.

In order to make sense of this claim, however, we must take seriously the fact that, while these approaches may be in a wide sense 'in conversation' with one another, we have assumed that this conversation is truly intermethodological insofar as the respective data and analyses are incommensurable. This assumption forecloses on the possibility that a straightforward comparison will be possible; that one might offer a representation simply wider in scope or accuracy than the other. As noted above, this kind of study may produce clear results only in limit cases – cases parallel to video and audio surveillance of an unlit windowless room – in which case we can conclude that one mode of access seems to be sterile by comparison to another. But, in this case, it is still far from clear what the epistemic value of this conclusion

might be. If these are incommensurable in that the excellence of the one, no matter how great, cannot replace the data from the other, then there is still an irreducible value in each.

Indeed, to return to our primary example, nobody was under any illusions that philosophy might turn out to be a “better fit” to the object of study than sociology. In a way, this might provide an added interest to the results of the presumptively more sterile mode of access, for when we expect interesting results from a study, it is fine and good to receive them, but when we do not expect interesting results, any results of interest could be considered as of greater value merely on account of their unexpected nature.

So, with regard to the possible epistemic value in (3)a., we may conclude that, while the swarm methodology might be a good way of answering this kind of question, it remains unclear when this question would be worth asking, and when an answer to it would be of any consequence, and to whom; but, nonetheless, that there is still some intuitive value in this notion of an increased responsiveness with an increase of methodologies and research foci.

Let us now consider whether we can articulate this intuition in terms of (3)b. In this case, we would suggest that an epistemic value in the swarm methodology is that the increased modes of encountering the object of study would result in an increased overall responsiveness to the object, due to the fact that differences in the fecundity of diverse modes of study allow for each mode, independently, to refine its own intramethodological approach to the object as informed by resonances found in alternate, incommensurate approaches. Here, we can make sense of the seeming contradiction above: in the one case, the fecund results from sociology can help philosophy orient its investigation, and, at the same time, the few but perhaps surprising results from philosophy might provide an invaluable clue to discovering an inobvious pattern within the purview of sociology.

This is, I think, a coherent way of making sense of the value which interdisciplinary and intermethodological conversations may have, and is also a view of the kind of epistemic value which swarm methodology may hope to present. And, with this, I hope to have accomplished the basic goals of this article: to show that swarm methodology presents valuable possibilities as a research method, and, further, to provide a more general analysis of the possible epistemic value of interdisciplinary research.

Despite that I hope these goals to have been reached, I still have more to say on the considerations we have begun here. Namely: we still have left to consider whether (3)c. is an empty set, and if not, what the nature of these conversations might be, and what kind of epistemic value is to be found within them.

Emergent and Hybrid Intermethodological Epistemic Value

A primary worry in the philosophy of science has been the question of to what extent, and in what manner, our methodology predetermines our results. It is this which led Karl Popper to claim that Freudian psychology and Marxism were not properly speaking “scientific” because their concepts and methods allowed researchers to avoid any interpretation of data which would call the basic theory into question (1957 [1945], pp. 81–84; 1968, pp. 33–37). Kant held that the non-optionality of the perception of objects within space meant that we had no grounds to claim that space was real outside of our perception, for if it were not real, we would never know it (1990 [1781], p. 33). Popper, in turn, warned of the pitfalls of theories which, if they are false, can despite this avoid recognizing any evidence of that falsehood; and which are therefore dogmatic metaphysics dressed as science (1961 [1934], pp. 34–42). To extend this concern to a

different kind of case, we see a parallel concern in Negroponte's concept of "The Daily Me," (1995, pp. 152–4) further developed by Cass Sunstein in his *Republic.com* (2002, pp. 3–22).

Here, the worry is that, through the use of personal filters, we can control the information we receive in order to ensure that the news we read every day, and our informational diet in general, is geared towards our personal interests, and is presented from a perspective with which we are comfortable. In this way, we can encase ourselves within informational cocoons, neither being challenged by different viewpoints or uncomfortable evidence, nor being able to reach out to and discuss things with our ideological opponents.

We can take these examples as limit cases for non-responsivity to objects of study. The worry in each case is that, no matter how terribly wrong we might be, we might never know it, and, thus, our opinion formed using a method or research program which is non-responsive in this manner is in an important way a mere reflection of the method itself, rather than of the object it purports to describe.

Both Negroponte and Sunstein also discuss a "Daily Us," but the discussion of it, in each case, is rather more brief than that of the "Daily Me," and it does not seem to be the same kind of thing for both authors. Negroponte uses the example of traditional print news as a kind of Daily Us, for it is a kind of broadcasting rather than the narrowcast which I receive at my favored pundit's blog (1995, p. 154). Sunstein, in *Infotopia*, locates the voice of the Daily Us in some wikis (2006, pp. 10–11), in the wisdom of markets (2006, p. 15), and offers as an implicit definition of "the emergence of a Daily Us;" "a situation in which people can obtain immediate access to information held by all or at least most, and in which each person can instantly add to that knowledge" (2006, p. 219). Negroponte's Daily Us is a mass-market aggregate representation of various interests, as understood by a few experts (journalists and editors, in this case). Sunstein's Daily Us is an emergent, bottom-up representation of an object of study or interest, as understood not by any agents in particular, but, as it were, by the system qua complex system.

Sunstein's explanation of the success of many parts of Wikipedia is the same as Hayek's explanation of the predictive success of futures markets: that in situations where each of us has incomplete but, so to speak, better-than-chance perspectives on something, and where we are allowed to freely offer this information, the perspective emergent will converge on accuracy (Sunstein 2006, pp. 14–16). In other words, Sunstein's Daily Us has the advantage, described above, of increased responsivity through use of a diverse multiplicity of narrow perspectives.

Now, as noted above, we cannot straightforwardly apply this kind of account to the question of the value of interdisciplinary and intermethodological study – after all, in the futures market, whatever the different methodology, the outcome is always either "buy" or "sell," and in the Wikipedia article, the goal is uniform – viz. encyclopaedic – and individual additions of information are always either meriological or rivalrous. Still, let us ask what kind of parallel *can* be made.

The value of 'many minds' when their relation is merely additive and meriological is obvious, so we will turn immediately to consideration of a rivalrous interrelation. Here, even if we admit to the worst possibilities of the insularity of theoretical perspectives and the theory-ladenness of observation, we can see a kind of value in running such theories up against one another in a rivalrous fashion. Namely, that, as Kuhn argues in the closing pages of *The Structure of Scientific Revolutions*, at the end of such a competitive process, theories which are in some sense well-adapted to their subject matter are likely, overall, to win out (1970, pp. 171–3). Kuhn depicts a period of crisis in a science – i.e. a period in which the paradigm for research

in that science is unclear, and where there may be many rival possible paradigms – in a way that I will describe as a kind of hybrid swarm; a term from biology which refers to the variation within the offspring of hybrids between overlapping interfertile species. Great diversity may be seen, especially among hybrids of hybrids. Selective pressures, however, limit the success of these varied phenotypes, and the hybrid swarm undergoes speciation. Similarly, returning to Kuhn, after some time in a period of crisis, one of the paradigms in contention emerges victorious. This, Kuhn takes pains to point out, does not necessarily mean that progress in a teleological sense has taken place; just as the lineage emerging from a hybrid swarm is simply the one best adapted to its local environment, not necessarily being the “best” or “fittest” in *any* wider sense, so also the new paradigm emerging from a period of crisis is the solution best adapted to the local crisis, and is not necessarily the most truth-conducive.

This allows us to give a picture of a kind of artificial or induced crisis: imagine that, in approaching a research question within a given paradigm, we apply a variety of different analytic methods, and then utilize whichever gives the most clear and/or useful data set. In this case, while departing from Kuhn somewhat, we can still make out some relevant parallels. The overall meta-methodology is similar to the function of natural selection within a hybrid swarm; we simply employ methods across the diversity of relevant alternatives, and see which is most adapted to the object of inquiry. There is also a similar limitation in the strength of the conclusion about methodological preference that can be drawn from this meta-method: all we know is that, given this object, this particular method yielded particularly *easily interpretable* results, or results particularly *relevant to the hypothesis* being pursued. In the end, we have no real evidence about which method was most accurate *to the object*, nor do we know which is to be preferred more generally.

If we remove this rivalrous relationship between methodologies, we have left in our analogy only the hybrid swarm, without the selective pressure of the environment. This picture corresponds to interdisciplinary and intermethodological conversations, as defined above, for when we are between methodologies or research programs, there is no presumption of what counts as a result, and no presumption of what the primary object of study might be, just as, in the hybrid swarm, there is only a diversity of differing attributes which might or might not be adaptive, depending upon the circumstances in which it is placed. The hybrid swarm may have one member with redder flowers, another more fragrant, and a third more numerous. The swarm on its own has only diversity, and it is only once we see, in the environment, that it is e.g. bird-pollinated, that we discover redness, being more easily perceived by the pollinator, to be more adaptive than the other traits. Similarly, as in (3)b. above, the conversation between disciplines and methodologies – the hybrid swarm – reaches results only once we return to the local selective goals of a particular coherent method or program.

To make sense of a different kind of value to be found in the hybrid swarm, let’s imagine, not a particular environment with selective pressures, but instead a nursery. Instead of allowing a local environment to select the most locally adaptive, we will allow a plant breeder free reign in determining which attributes she wishes the resulting plant to exhibit. What might she value? Well, likely these values might have considerable overlap with adaptive traits in various environments, but her concerns may be quite different. If developing a variety for laboratory work, she may wish to select for a quick life cycle, and high rate of germination. If developing a variety for retail sales, she may wish to select for some other attribute, such as bright, attractive blooms. But, the most interesting possibility, she may choose to preserve what Darwin referred to as a monster or a sport – some phenotypic variation that would not be adaptive under the

selective pressure of virtually any natural environment. This, in fact, has happened often in the history of plant and animal breeding. Selecting a weak and sickly specimen, but one which exhibits some otherwise unseen trait or combination of traits, is a longstanding practice. Examples include the extra retrices which Darwin points to as that minor phenotypic variation that was selected for by pigeon breeders in the origination of the fan-tailed pigeon, or the distinctive dark blotches on Phalaenopsis blooms which gave rise to the breeding of ‘Harlequin’ varieties. The sport or monster, once the trait is stabilized, can be bred back with or grafted on to standard stock in order to increase the vigor of the offspring, while retaining the unusual characteristic.

It is this which is finally able to give us a kind of image of a value peculiar to (3)c. An interdisciplinary and intermethodological exchange may be valuable for the particular new clues for future research, or ‘hybrid vigor,’ which may be brought back into a particular methodology or research program, or ‘local selective pressure.’ But, *if* there is any peculiar value in the ‘hybrid swarm’ which is *not* to be found in the return to disciplinarity, it *must* be something like a sport – only something like a sport could offer a value which is neither within the compass of any of the methodologies employed, nor within them all in aggregate. The sport is not adapted to any particular natural environment, nor is it the compromise of the breeder – i.e. vigorous *and* vibrant; fragrant *and* fecund. Instead it is a new and emergent possibility, native to the hybrid swarm itself, and not merely to the sum of its parts.

This monster must be crossed back onto more typical plant stock in order to preserve the unusual trait while regaining the health and strength of a more generally fit member of the species – or, alternately, the sport must be grafted onto a more typical and robust rootstock. Similarly, the result of interdisciplinary and intermethodological research – if this value is really emergent from a synergy of incommensurate approaches, cannot be maintained in isolation. At the same time, this is *ex hypothesi* not a case of the use of the hybrid in order to pursue pre-established goals – this would correspond to (3)b., not (3)c. Instead, just as we preserve the sport or monster through grafting or crossing onto parent stock, so in finding a *new* value in interdisciplinary and intermethodological research and choosing to preserve that emergent value, we must alter the parent stock; we must change our disciplinary environment, and change research norms used as a selective pressure. In other words, if there is a lasting value to be found in the swarm, that value would repose in the possibility of alteration of the basic objects and methods used within a given research program.

Thus we can offer this alternate account of the possible epistemic value of certain kinds of interdisciplinary and intermethodological work, and swarm methodology in particular: through taking a diversity of approaches to an object of study, we may discover some constellation of categories, across disciplines, which is of value not because it allows each disciplinary approach to better pursue its own object of study as methodologically defined, but rather because it allows each disciplinary approach the possibility to modify its own basic interpretive structure, through which it encounters its object.

Let’s now return to the parallel with Cass Sunstein’s account of the emergence of knowledge from many minds which is superior to the knowledge which each has individually. Sunstein defends this, referring to the Condorcet Jury Theorem, in cases where each of us individually have *some* method or other of providing an answer which is better than chance (Sunstein 2006, pp. 25–29). In this kind of case, when we look at the answer which emerges from the aggregation and negotiation between these methods in forming the voice of the Daily Us, we find that we, taken together, are actually far more reliable than each of us taken

separately. This, I suggest, is the kind of benefit which we, ultimately, might hope to see emerge out of a swarm methodology. That all of us, together, might be able to, in our intermethodological and mutual casting about and intermingling informational feedback loops, encounter the object of study in a way which is methodologically superior to any of our individual methodologies. This “method against method” – more precisely, this methodology of productive transgression of methodological boundaries, limits, and conceptual prefiguring – may reveal regional ontologies or conceptual categories more peculiar and proper to the object of study than would be possible through either any of the individual methods participating in the process, or all of these methods in aggregate.

With this, I can summarize and restate the results of this last and additional subsection. I have here claimed that if there is anything of epistemic value which emerges from an intermethodological approach which is not to be found in the sum of its parts, it must be found not in the value which an approach can find within its own methodology with the help of an alternate approach – for this would not be this kind of synergistic emergent value – but must be an epistemic value constructed between approaches, the value of which resides in an opening or modification of methodological approaches. I will not here claim that any such value is to be found. I will claim only that if there is such an emergent value, its value would inhere in methodological and conceptual alteration and revision, and that insofar as we think that such a goal is of value, the swarm methodology represents a relevant approach. As to whether this particular intermethodological engagement – the engagement which this paper itself has, as a performance, attempted – this is something I cannot judge.

Notes

1. “Life presents itself chiefly as a task -- the task, I mean, of subsisting at all, *gagner sa vie*. If this is accomplished, life is a burden, and then there comes the second task of doing something with that which has been won -- of warding off boredom, which, like a bird of prey, hovers over us, ready to fall wherever it sees a life secure from need. The first task is to win something; the second, to banish the feeling that it has been won; otherwise it is a burden.” (Schopenhauer, 2004 [1892])
2. I mean “of a kind” in a loose sense here. In this use of terminology, I hope to make my argument more apparent to a general audience, even though it does a certain amount of conceptual violence to certain technical uses of these terms. While, e.g. the Kuhnian will claim that the rivalrous aspect of different paradigms consists in part in their incommensurability, the Kuhnian will nonetheless concede that the fact that this incommensurability is an *issue* for these rivalrous paradigms indicates that they are of a kind in a larger sense. So, Einsteinian physics is of a kind with Newtonian physics in a sense, for Einsteinian physics is concerned with objects of inquiry sufficiently similar to those of Newtonian physics to force a choice between them. By comparison, Einsteinian physics is not in such a close relation to phlogiston theory, Freudian psychology, or French cooking. To adapt this to Gestalt perception, the duck and the rabbit in the famous “duck-rabbit” (Wittgenstein, 1958, p. 194) are of a kind in this larger sense – in that their very mutual exclusivity results from shared constituent elements, even if those constituent elements appear under differing categories (e.g. pendulum vs. constrained falling) in each.
3. From here forward, in keeping with the recognition of these inter- and intradisciplinary boundaries of varying strengths, I will be speaking of methodologies and research programs rather than of disciplinary norms or disciplinary objects of study. In doing so, I will also be trying to navigate between various perspectives in the philosophy of science. I intend the following argument to be applicable regardless of how one divides up the fundamental organizational units of scientific practice. In speaking variously of methodologies, research programs, paradigms, and theoretical perspectives, I am trying to make an argument that will avoid siding with e.g. Popper, Kuhn, Lakatos, etc. In the end, however, the attempt to negotiate between these views may be a prefiguring of my eventual view, which is closer to Feyerabend

than the rest, at least insofar as the epistemic value of what is usually called 'interdisciplinarity' is concerned. Most of these views, most of the time, are really viewed as being about the so-called "hard" sciences anyhow, so my taking some liberties is skirting around their debates will, I hope, be more easily forgiven than it might otherwise.

4. This example, it should be noted, is unusual due to our prejudice towards attributing more importance to audio than visual in this case. Whatever the matter of fact may be, tend to claim that appearance ought not to be terribly relevant to political office.

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