

The X-Phi(les): Unusual Insights into the Nature of Inquiry*

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Abstract: *Experimental philosophy is often regarded as a category mistake. Even those that reject that view typically see it as irrelevant to standard philosophic projects. We argue that neither of these claims can be sustained and illustrate our view with a sketch of the rich interconnections with philosophy of science.*

1. Introduction: Experimental philosophy (henceforth “XΦ”) takes seriously the idea that philosophical inquiry may benefit directly from quantitative empirical research. That view strikes many as deeply misguided, perhaps oxymoronic: experimentation is simply the wrong *kind* of investigatory technique for solving philosophical puzzles. But to think XΦ an oxymoron is to have an opinion about the relationship between scientific and philosophical inquiry – in particular, that philosophy and science are distinct, independent enterprises each pursuable on its own terms. We argue that this ‘separate but equal’ view of science and philosophy cannot be maintained.

We begin by surveying the range of possible relations between scientific and philosophical inquiry and arguing for continuity, not separation, and thereupon argue that XΦ itself cannot be separated off from philosophy more generally. We then consider how XΦ might ‘play well with others’, with a brief discussion of the possibilities for developing a relationship between XΦ and philosophy of science.

2. A Variety of Relations: Following Kornblith (1994) we identify three possible relations between scientific and philosophical inquiry. First, the two approaches may be seen as direct competitors, that is, science and philosophy constitute alternative approaches to a single explanatory task. Since at most one such alternative can be correct with the other discarded as a failure we label this view the *replacement* thesis. A second approach, hinted at above, sees science and philosophy as pursuing distinct explanatory goals with distinct methodologies. This ‘separate but equal’ approach we will label the *traditional* approach. The third option is that science and philosophy constitute generally overlapping areas of inquiry beneath the umbrella of inquiry in general. This view we call the *continuity* hypothesis.

In the balance of this section we locate various philosophical practices within this framework and argue that enough philosophy takes place on the ‘continuity’ plan to make XΦ, at a minimum, non-oxymoronic.

The replacement thesis has rarely had much currency. The version which suggests that philosophical inquiry ought/does supplant scientific inquiry has probably not had a serious advocate since Cardinal Bellarmine refused to look into Galileo’s telescope. The opposite view – that science ought to replace philosophy – possesses a more recent pedigree in Quine’s version of naturalism (Quine 1969). Despite this advocacy of its redundancy from within, few philosophers have adopted more scientific forms of inquiry.

In contrast to the replacement thesis, the ‘separate but equal’ approach of the traditional view is popular with philosophers. Richard Feldman exemplifies this attitude when he claims that, “... there’s not much reason to think that psychological results will play any significant role in the efforts to construct general abstract theories about or analyses of knowledge and justification.” (Feldman 1999 p. 184) While it’s unsurprising to see this view espoused by analytically trained practitioners of metaphysics, epistemology and ethics, it is something of a shock to realize that this is the view of many philosophers of science too. But this is the consequence of the ‘the scientist decides and we report’ attitude adopted by current philosophy of science (e.g., Giere 1988). While we strongly endorse the humility of such an approach we will suggest (section 4) that a traditional view of the relation of science and philosophy is *not* a necessary corollary of ‘taking the science seriously’.

The final alternative is that philosophical and scientific inquiry or at least some parts of both are really not so different after all. That, in short, there is *no* firm distinction between the modes and practices of inquiry deployed by the two communities. This practice can be found amongst both philosophers of psychology and linguistics. In both areas scientific research blends seamlessly into philosophical research: the theorists (aka philosophers) pay careful attention to empirical work and their theoretical discussions in turn provide the spur for further investigations by the empirically minded (aka scientists). Continuity of forms of inquiry is also espoused by some epistemologists (e.g. Goldman 1986 and Kornblith 2002) although in this case a commitment to the relevant perspective is clearer than how, in practice, the forms of inquiry relate. Finally, XΦ itself clearly supports the continuity hypothesis – if we are successful in our arguments below that it is properly part of philosophy.

Our view is that the continuity hypothesis provides the best account of the relation of scientific and philosophical inquiry. It alone of the three views has the resources to make sense of the results of the survey just completed. After all, the claim that the two forms of inquiry overlap to a greater or lesser degree is consistent with both the possibility of substantial overlap in some areas and minimal overlap in others. If there is enough overlap pursuing both forms of inquiry becomes otiose suggesting something like replacement. Conversely if the overlap is slight then the independent pursuit of both projects is mandated as the traditional view suggests. Thus both the replacement hypothesis and the traditional view constitute appropriate responses, within the continuity framework, to particular inquisitorial circumstances. Since a similar ability to incorporate the insights of the other perspectives is *not* part of either the replacement hypothesis or the traditional view we take the continuity hypothesis to be the best account of the actual distribution of philosophical inquiries.

One way to short circuit debates about whether or not the various positions possess (or lack) appropriate explanatory resources is to observe that adopting the ‘oxymoronic’ approach is to endorse the view that the traditional perspective is the *only* appropriate perspective on the relation between philosophic and scientific inquiry. But that approach must be incorrect because it would imply that obviously philosophical work (e.g., Goldman and Kornblith in epistemology) conducted from the continuity perspective was oxymoronic – wildly implausible, whether or not you agree with the conclusions of those authors.

3. Can we substantively demarcate XΦ from ‘traditional’ philosophy?

The development of XΦ could be seen as challenging more traditional forms of intuition-based philosophy, much as the development of careful quantitative methods challenged purely observational ‘naturalistic’ methods in biology centuries ago. One way traditional philosophers might seek to preserve their investigatory methods would be to show that one can cleanly partition off XΦ from standard analytic philosophy, as simply doing something different that need not be viewed as a competitor any more than formal mathematics need view empirical science as a competitor. Although we do not seek to push this challenge here,¹ we are concerned that this defensive maneuver is based on a mistaken view of the relations between XΦ and philosophy more generally. We consider three different strategies by means of which a practitioner of standard analytic philosophy (henceforth “SAP”) might seek to draw a bright line between SAP and XΦ, and argue that none are successful.

First, and perhaps most obviously, it might be suggested that the two are, after all, robustly distinct methods, or at least families of methods. They wear this difference on their nomenclatural sleeves: the one is *analytic*, and the other is *experimental*. Once we identify the core set of methodological norms that give them their identities, then we can keep them cleanly separated.

However, on close inspection the various researchers working under the XΦ umbrella have less in common than might be suspected. One can locate differences between their methods along a number of fundamental dimensions. We will articulate a few suggestive examples here, indicating their variation along such dimensions as subject population, explanatory orientation, and investigatory goals.

At one extreme, we find Joshua Knobe attempting to explore the content of various folk-psychological concepts such as intentional action (Knobe (forthcoming a)) and valuing (Knobe and Roedder (2006)). One of his chief investigatory interests has been to argue that our folk-psychological concepts are not solely structured for the purposes of prediction and explanation, but also for moral evaluation and the assignment of praise and blame. His target population, therefore, is ‘the folk’ at large, frequently college undergraduates because they are cheap and plentiful, but also on occasion people in the street (e.g., Knobe (2003)). His inferential strategy for getting at the content of our folk concepts is what we will call *ethological*: the relevant facts are more or less read off of the behavior itself, without an ambition to articulate the underlying psychological mechanisms that produce that behavior². The relevant categorical behavior is the expressed categorical decisions of the subjects to hypothetical cases – in many ways a paper-and-pencil version of the fundamental evidential tool of SAP. This basic model applies to several other practitioners of XΦ such as Thomas Nadelhoffer and Eddy Nahmias (Nahmias et al. 2005; Nadelhoffer and Nahmias forthcoming), though their interest has generally been more to determine what the intuitions of ‘the folk’ are for the purposes of refereeing burden-tennis matches in philosophy than about the predictive or moral components of folk psychological concepts more generally.

Karola Stotz and Paul Griffiths (see Stotz and Griffiths (2004) and Stotz et al. (2004)) are also trying to map the content of concepts – the various gene concepts at play in different communities of biologists – and as such their methods share some similarities with Knobe’s. They also present their subjects with surveys about hypothetical cases,

albeit ones that have significant real-world plausibility, and they also have an ethological approach to the data so gathered, where the interest is more in the distribution of different patterns of categorizations than in the underlying psychological mechanisms. But their larger theoretical goals are different. Stotz and Griffiths are pursuing the concepts of different communities of experts, not the folk, and where the others work from a starting presupposition that the categories of interest to them are broadly, even universally shared, Stotz and Griffiths are particularly interested in documenting the *differences* across those communities. Indeed, they bring to bear not just the survey methods of social psychology, but the tools of population ecology as well, to help map out the ‘intellectual ecology’ of the different ‘subspecies’ of gene concepts.

Not all X Φ practitioners are ethological, however. Stephen Stich, Shaun Nichols, Ron Mallon (among others) have pursued the *psychological* questions of the processing architecture – the ‘boxology’ – of various of our cognitive capacities (Kelly et al. 2007; Nichols & Mallon 2006). This work often enters into direct dialogue with research conducted by psychologists and published in psychological forums (Turiel 1979; Nucci 2001).

A further X Φ project, one which does not share the basically explanatory orientation of the research just mentioned, has more pointedly polemical ambitions. This project aims to demonstrate that intuitions of the sorts typically appealed to in SAP diverge across various demographic lines, and indeed may not even be stable within individual persons. (Weinberg, Nichols and Stich (2001); Swain, Alexander and Weinberg (forthcoming); Machery, Mallon, Nichols and Stich (2004); Nichols and Ulatowski (forthcoming); for an overview, see Alexander and Weinberg (2007)) This work is mostly predicated on the plausibility of generalizing from the results with their subjects (again, mostly undergraduates) to the patterns of intuitive judgments of philosophers, although some researchers are carrying out direct studies of professional philosophers as well.

So there is nothing like a rich, deeply shared method that holds across the whole X Φ community. At best one might suggest that they all use surveys, but that will not really serve to distinguish it from SAP, when SAP relies on fundamentally the same kind of data: namely, the categorical judgments of their ‘subjects’. And this appearance of a minimally shared method across all X Φ is perhaps more misleading than revealing; there is important work being done using brain imagining technologies as well, e.g., Green and Haidt 2002; and with historical/genealogical approaches, e.g., Nichols 2006. Furthermore, different parts of X Φ share substantive goals with SAP, such as coming to understand the structure and nature of various philosophically interesting concepts, or evaluating the kind of justification that intuition may (or may not) provide. There are *some* differences between the methods X Φ and SAP, of course, but they do not appear sufficient to establish a stark discontinuity between them.

Nonetheless, it might be contended that there is another important difference between SAP and X Φ . The latter has *empirical commitments* while the former is unsullied by the empirical world. For SAP is meant to be modally neutral, presupposing nothing about the particular possible world we are in; whereas X-Phi, by its very nature, draws upon specific empirically-ascertained – and hence contingent – data. In addition to its *prima facie* plausibility, this move also has the benefit of not seeming entirely ad hoc: SAP has ambitions of providing deliverances with necessary modal force (e.g., Jackson

1998), but $X\Phi$'s contingent premises doom it to merely contingent conclusions.³ Given SAP's goals, then, its practitioners may have good reason to hold $X\Phi$ at arm's length, or longer.

Now, this tactic only works if the methods of SAP are indeed devoid of entanglements with contingency. But a lack of empirical *premises* is not the same as a lack of empirical *commitments*. We know that scientific instruments embody empirical information about the world (e.g., Baird 2004). And this principle can be extended from material instruments to practices more generally. For example, the practice in Chomskyan linguistics of preferencing the intuitions of linguistically naïve native speakers is only sensible given a broad set of presuppositions about the general modularity of syntactic knowledge; the thinness of how that knowledge can be made manifest to consciousness (and hence reportable); and the possibility that explicit linguistic belief can interfere with such knowledge.

Furthermore, although practitioners are often aware of some of their most important presuppositions, this is not always so; Timberlake (2002) demonstrates that the learning paradigms of traditional behaviorists, without their realizing it, embody significant information about the niches that their animal subjects function well in, and very particular facts about how they so function. The training apparatus for any given type of animal will be 'tuned' to that animal, so that the researcher can produce enough appropriate behavior from the subjects to do their intended study. As a result of this process of bit-by-bit conforming the apparatus to the animal:

“...it is possible to infer from their standard Skinner boxes that pigeons forage for grain based primarily on pecking related to visual stimuli, do not like to move about in the dark, and are not inclined to feed out of holes they cannot see into. Rats ... use manipulation responses, attend to movement and sound, are less fond of brightly lighted areas, and have no compunction about poking into dark holes for food” (Timberlake 2002, p. 357).

The behaviorists, of course, do not take themselves to be including so much species- and niche-specific information in their studies – they are after general laws of learning, and it is inconsistent with their theoretical goals to explain animal behaviors in terms of individual species-natures and suitability to distinct environments and behaviors. This information became embodied, rather, over the course of trying to get a working apparatus. Particular empirical information therefore can be built in to a set of practices or instruments not only without the practitioners' awareness of it, but even against their express desires.

We contend that SAP, too, has its share of empirical commitments. To escape Meno's paradox, analytic philosophers must treat the sources of intuition as being cognitively distinct from our explicitly held theories. Also, one's explicit theory can sometimes interfere with one's intuitions, which is why occasionally a rival theorist's intuitions may be explained away as being 'contaminated' by their preferred theory. (It would be a nice piece of history to determine to what extent this move in philosophical practice antedated or postdated the rise of concerns about theory-ladenness of observation.)

Moreover, SAP is committed to the well-functioning of intuition across arbitrary stipulation. If you have two rival analyses – say, *that all As are F* and *that all As are G* – then the strategy is to devise a hypothetical situation, in which there is a relevant presence of F and absence of G (or vice versa), and check our intuitions for A-ness. If F and G tend overwhelmingly to travel together in our ordinary lives, then the target scenario may need to be rather bizarre or esoteric. And frequently, they are. (See, e.g., Lehrer’s gypsy lawyer (1971) or Lehrer and Cohen’s new evil demon (1983) or Lackey’s double-lesioned truth-telling liar (2006).)

A third contingent component of SAP is that the intuitions cited as evidence are widely shared, intra- and inter-personally. One linguistic sign of this commitment is the common deployment of either a definite article (“the intuition”) or the first-person plural possessive (“our intuition”). Moreover, they *need* to have this commitment, for it is unclear what evidential force such intuitions could have, if they varied with philosophically-irrelevant factors like the ethnicity of the intuiter, or the order in which cases are considered. Sometimes philosophers seem to require the agreement of the folk at large; other philosophers only seem to demand the agreement of other well-trained philosophers, but then they take on the additional commitment that philosophical training produces persons whose intuitions partake of a greater degree of truth-tracking oomph.

There may be further empirical commitments than these, but these three are enough to secure our basic point. SAP is not empirically pure, and cannot be shielded from $X\Phi$ by an appeal to the latter’s mucking about in the scientific world of observable contingencies.

Now, even while granting that SAP has empirical *presuppositions*, one may still object that SAP has no empirical *premises*; and the same cannot be said about $X\Phi$. This objection’s main claim is not obviously true – claims about “the” or “our” intuition about a certain case may be best understood as an empirical claim about how subjects at large, or at least philosophers, will treat that case. Yet even if it is true, it will not suffice to insulate SAP from an $X\Phi$ invasion, for empirical presuppositions may still be scientifically evaluated and, if the science goes the wrong way, invalidated. Moreover, being a responsible investigator surely includes appropriate openness to evidence concerning the state of one’s apparatuses and practices, and whether their commitments are satisfied by the world, or not. So, although the premise/presupposition distinction might be a good *epistemological* one, it is not clearly a useful *methodological* one.

So SAP and $X\Phi$ cannot be seen as discontinuous either in terms of their methods or of their having empirical commitments. One last possible source of discontinuity that we will briefly consider is historical. Perhaps they belong to different ‘intellectual clades’ – the philosophical on the one hand, the scientific on the other, for example – and thus are informed by and responsible to different traditions, having inherited different sets of problems that they seek to engage in and advance. It would thus be reasonable for SAP and $X\Phi$ to operate in parallel, non-competing, separate spheres.

Yet such an argument would get the particular history of $X\Phi$ wrong, as well as the broader history of the relation between philosophy and science. The aforementioned different research groups in $X\Phi$ all come from varying traditions of inquiry; all of them recognizably philosophical. The first, empirical conceptual analysis group, descends from analytic metaphysics itself; the second such group, from philosophy of science (see below); the third, psychological group from the kind of empirically-informed philosophy

of cognitive science that has been active at least since the origins of cognitive science about a half-century ago; the fourth, critical group from the empiricist, anti-metaphysical tradition in epistemology that stretches honorably back at least to Hume. The similarities that are observed between the different forms of X Φ are perhaps better seen as analogies, not homologies. Moreover, there is a long philosophical tradition of entwining one's philosophy with one's psychology, and as Knobe has observed, X Φ largely examines "the sorts of questions one finds in the work of Plato, Aristotle, Spinoza, Hume, Nietzsche, and so many others" (Knobe forthcoming b). It will thus be impossible to find an ancestor on philosophy's evolutionary tree such all of X Φ is found along one descending branch, without that branch including much of SAP itself.

4. Intersections: X Φ and Philosophy of Science: Having made the case for X Φ as a member in good standing of the philosophical community we can now consider how it might 'play with others' to the benefit of both parties. We illustrate this possibility with a brief discussion of how we see the relationship between X Φ and philosophy of science developing.

The broad outlines of philosophy of science's contribution to X Φ are clear. To pursue X Φ is to deploy scientific methods in the service of philosophy. As a result, philosophy of science constitutes a source of expertise that experimental philosophers may draw upon in the pursuit of their inquiries.⁴ For example, important progress in X Φ is sometimes made when a team of researchers notices that a key norm of experimental design has not been scrupulously observed by rival philosophers or psychologists. One can see Nichols and Ulatowski (forthcoming) on the importance of within-subject designs and the danger of over-reliance on population means; or Kelly et al. (2007) on not letting one's instruments dictate the form that one's theories should take.

Since X Φ has relied heavily on survey instruments, philosophy of science can be of service in helping X Φ discern the appropriate domain for using such tools. While much information of interest to philosophers may be accessed using survey techniques that need not always be the case. For example, if you think emotions are important parts of moral judgments (e.g., Nichols 2004), then the use of surveys may well be 'ecologically' inappropriate – that is, fail to generate the circumstances that allow for an accurate report of moral judgments.

Even more important than philosophy of science's contributions to X Φ , though, is X Φ 's potential to expand the tools available to philosophy of science itself. The work of Stotz and Griffiths on the variety of gene concepts has been mentioned already (see above Stotz and Griffiths (2004), and also Stotz et al. (2004)). Their methodology has much to offer other philosophers of science. Current philosophy of science cannot be faulted for the careful attention its practitioners pay to the details of the sciences they study. The insights of individual practitioners however are rarely quantified to even a superficial degree. These circumstances might with justice be compared to the glory days of Natural History where rural curates the length and breadth of Great Britain published richly detailed, insightful accounts of their local flora and fauna. Nonetheless this outpouring of detail led to little intellectual progress until the practices of quantification began to provide a framework within which the individual accounts could be compared and contrasted. The quantificational methodology of Stotz and Griffiths, we suggest,

holds out the hope of similar progress within the philosophy of science. Surely this is a prospect desirable, exciting and worth working for.

A quite different direction is which XΦ may be taking philosophy of science is illustrated by the work of Stich and Kelly (Kelly et al. 2007). The point we wish to focus on here is the degree to which Stich and Kelly's work can be seen as not merely observing the psychologists' debate about moral/conventional distinction, but actually participating in that debate. As we noted earlier (see section 2 above) much current philosophy of science supports, by default, the separate but equal doctrine regarding scientific and philosophic inquiry. It does so because it cedes ultimate intellectual authority to the science being studied. That is, although the science can (and does) impact the philosophy, the philosophy does not likewise impact the science. That Stich and Kelly's work is violating this stricture (as is not uncommon in the philosophy of psychology) is, we submit, both further evidence for the continuity hypothesis (see section 2 above) *and* indicative of a future for philosophy of science as a participant rather than a mere observer in scientific debates. Such a future need not be for everyone, but its existence surely provides a new and genuinely exciting opportunity for philosophers of science.

The possibilities highlighted by the work of Stotz, Griffiths, Stich and Kelly should not be taken to exhaust XΦ's capacity to contribute to philosophy of science. XΦ is a research project in its infancy and we are convinced that its contributions to philosophy of science (and philosophy more generally) will be limited only by the imaginations of its practitioners for the foreseeable future.

4. Briefly Polemical Conclusion

The moral of our story is that XΦ should not be seen as some alien invader, but as an organic outgrowth of philosophy itself, with significant promise for philosophy at large and also for the philosophy of science. One might well wonder, then, if there is *anything* that distinguishes XΦ from the kinds of traditional philosophy practiced by proponents of SAP. Although we think there is no deep philosophical difference, especially as both do have (we argued) significant empirical commitments, they nonetheless have *different* empirical commitments. One consequence of this fact is that it is possible for the empirical commitments of one to be falsified, even while those of the other remain intact. And the aim of at least one camp of experimental philosophers has been to show, indeed, that there is a growing body of worrisome evidence for the falsity of SAP's presuppositions. Just because we have all descended from the same intellectual ancestors, it doesn't mean that we're all viewing the world from equally high points of the contemporary evidentiary fitness landscape. But that is perhaps an argument for another time.⁵

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¹ Except for *very* briefly in our concluding section.

² Although a certain amount of psychological theorizing is required to help distinguish between some relevant hypotheses (e.g., to fend off attempts to explain the data away as the artifact of the pragmatics of the survey items) the main focus remains descriptive.

³ We note that XΦ has not shown any interest in Kripke's necessary a priori.

⁴ We note in passing that the deployment of these methods by XΦ provides additional data on features of these methods for philosophers of science. Space precludes further discussion of this point here.

⁵ For a review, see Alexander and Weinberg 2007.