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What is This?

Continuous Variations: The Conceptual and the Empirical in STS

Casper Bruun Jensen[1]

Abstract
The dichotomy between the conceptual and the empirical is part of common sense, yet its organizing force also extends to intellectual life more generally, including the disciplinary life of science and technology studies (STS). This article problematizes this dichotomy as it operates in contemporary STS discussions, arguing instead that the conceptual and the empirical form unstable hybrids. Beginning with a discussion of the "discontents" with which the dominant theory methods packages in STS are viewed, it is suggested that STS has entered a phase resembling Kuhnian normal science. Based on a discussion of the making of cognitive dissonance theory, it is then argued conceptual–empirical mixtures are unavoidable in actual research practice. This situation can be taken as an encouragement for more sustained exploration of conceptual–empirical relations and their inventive potentials. Invoking Deleuze and Guattari's notion of "continuous variation," the article concludes that STS as a discipline is well served by promoting an ethos of empirical and conceptual experimentation.

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Introduction: The Conceptual and the Empirical

> Conceptual: related to or based on ideas.
>
> Empirical: based on experiments or experience rather than ideas or the-
>   ories (*Oxford Advanced Learner's Dictionary*).

If the relation between the conceptual and the empirical remains of
interest for science and technology studies (STS) today, one reason is
to do with the strength of the dichotomy that both binds together and
separates the two. Consider the two opening quotations from *Oxford
Advanced Learner's Dictionary*. Whereas the conceptual is defined as
belonging to the realm of ideas, the empirical has to do with experi-
ments or experience *rather than* ideas. Thus, the two are defined dichot-
omously and this is reinforced through colloquial as well as more
formal usage.

If the dichotomy is part of common sense, its organizing force also
extends to intellectual life more generally. For one thing, it is vividly exem-
plified in STS. It is common, for example, to say that on one hand we *apply
concepts and theories* in order to *interpret and analyze empirical material*,
whereas on the other hand we *observe practices empirically* in order to
build concepts and theories. In this sense, the terms complement each other.
One can develop no interesting theory without proper data, but one will find
no proper data unless guided by relevant theory.

However, complementarity can also give way to opposition, as when
ethnographers insist that their task is to describe the world as it really is
(without taint from theory) or when cultural critics wince at what they see
as the naive empiricism of STS. Such worries or accusations are premised
on a differentiation in which the empirical world, reality, is located on one
side; and the realm of ideas, somehow not *really* reality (Lynch (2001, 46)
speaks of "grand theoretical delusion") on the other.

To give a homely example, the sociology of science emerging in the
1970s and 1980s made much of the fact that, contrary to armchair philoso-
phers and epistemologists, they studied empirical rather than idealized
science (e.g., Barnes 1977; Collins 1981; Mulkay and Gilbert 1984). Micro-
sociology, history, and ethnography provided more robust contact points

with the real (Gooding 1990; Knorr Cetina and Mulkay 1983; Latour and Woolgar 1979); science came to be viewed as practice and culture (Pickering 1992).

These are but a small subset of STS studies that have contributed to more nuanced descriptions and conceptualizations of sciences, technologies, and their varied relations to society. Approaches from sociology of scientific knowledge (SSK) to feminist studies and ethnomethodology have enriched such understandings, not least by showing the complexity, hybridity, and tangled relations between action and knowledge, practice and theory. Even so, I argue, to this day STS often retains the dualist model.

In the following, I invoke and problematize this model. I suggest that taking seriously the multiple ambiguities and hybrid forms of the conceptual and the empirical enables an expansion of current STS imaginaries and practices. If this article at times adopts a slightly provocative tone, this is an act of affection. In the present moment where STS is larger, more recognized and, in some senses, stronger than ever before, I suggest that there is no good reason to rest on the laurels or opt for a strategic retrenchment[1] based on already established theories and methods (case studies, more than anything).

I begin with a brief discussion of some theory methods packages in STS (Fujimura 1996) and the discontents to which they presently give rise (Geels 2007). I then suggest that STS has entered a stage that resembles Kuhnian ''normal science.'' I proceed to discuss the making of cognitive dissonance theory, where conceptual–empirical hybridization occurred in direct contrast to the intentions of the researchers. The importance of this example is twofold. First, it offers a vivid illustration of the inseparability and hybridity of the conceptual and the empirical and it exemplifies the complex trajectories through which such hybrids are formed. Second, cognitive dissonance theory was chosen by Merton as an illustration the very midrange theory that has recently been argued to be of assistance in moving STS beyond the conceptual–empirical impasse. However, as the cognitive dissonance case shows, it is quite unclear what either ''mid-range'' or ''theory'' can mean in a context where the dichotomy between the conceptual and the empirical ceases to hold. Rather than advocating midrange theory, I thus propose that the omnipresence of conceptual–empirical hybrids encourages sustained exploration of their forms of emergence, their relations, and their potentials for expanding STS's own research horizons.

This argument leads to a series of observations concerning what I call ''tastes of research'' and their consequences. The aim is not to replace or supersede present forms of analysis. Instead, it is to support multiple forms of conceptual and empirical work in STS. This seems to me important at a

time when the field is increasingly defined conceptually by a well-established core set of theories and empirically by the case study. Accordingly, I contend that a degree of experimentation might help keep STS vigorous. In conclusion, I turn to Gilles Deleuze and Felix Guattari's suggestive notion of "continuous variation" to argue for the importance of STS studies that experiment with conceptual–empirical relationships.

Theory Methods Packages and Their Discontents

STS is not lacking in theories in spite of its strong empirical bent. The empirical programme of relativism (EPOR; Collins 1981) provides an early example of a theory methods package (Fujimura 1996), the name of which literally advertises its commitment to empirical specificity while simultaneously offering a set of generic concepts for dealing with it.

Such packages have proliferated. Sally Wyatt and Brian Balmer, for example, point to the "grand theories" of actor-network theory (ANT) and the social construction of technology (SCOT), and "programmatic statements" that include the Strong Programme and "Third Wave" science studies (Wyatt and Balmer 2007, 620-21). Introducing the "Third Wave," Harry Collins and Robert Evans group together several of these packages as "wave two" studies to be superseded by their new wave (Collins and Evans 2002, 239).

Collins and Evans claim to offer a brand new alternative. Other researchers express unease about what is currently on offer. Their "discontents" (Geels 2007) center in particular on the uncertain relation between the conceptual and the empirical. For example, the recent issue of *Science, Technology, & Human Values* on "mid-range" theory expressed the concern that concepts are used and recombined in problematic or epistemologically naive ways. Wyatt and Balmer recount their "frustration" that "something was missing from the middle" of STS (Wyatt and Balmer 2007, 619). Indeed, they suggested that "even the best writing in the field—by students or peers, ourselves included—could provoke such uncharitable thoughts as: What is this a case study of? What does it add to our understanding of different concepts? Interesting as the story itself may be, how does it contribute to discussions of anything beyond itself?" (619-20). In the same issue, Geels (2007) argued that STS ought to spend more energy figuring out how mid-range theory can bridge from individual case studies to the broad theoretical vistas offered by social construction or ANT.

In the tellingly entitled "Not another case study" from the same issue (Beaulieu, Scharnhorst, and Wouters 2007), the authors express irritation with the many cases filling the pages of *Science, Technology, & Human*

Values and *Social Studies of Science* that carefully describe empirical material in ways that mainly apply theories to exemplify already established analytical points.[2] This echoes Andrew Barry's considerably earlier observation that "Science and technology studies have tended to be dominated by the study of 'cases' which become the object of theoretical argumentation, but whose significance for the study of politics is obscure" (Barry 2001, 12). These authors are dissatisfied with the relation between empirical cases and their conceptual elucidation. At one extreme, theory methods packages offer general theories that purport to capture the diversity and complexity of everything within the field (e.g., Pickering 1995). At another, ever-increasing case studies open themselves to Edmund Leach's famous critique of anthropology as "butterfly collection" (Leach 1961, 2-6).

Unsurprisingly, the situation is evaluated in conflicting ways. Some propose strategies of containment: STS as a field ought to delimit or circumscribe the undisciplined proliferation of concepts. If STS is to have an impact on the "real issues" brought to light by the voluminous case studies, these issues need to be adequately explained and this requires disciplined theorizing. The problem is that "concepts are not (yet) theory" (Geels 2007, 630). For Geels, this is precisely the importance of midrange theory.

Yet, this is not only an epistemological issue but also one of disciplinary politics. The introduction to the 2008 *Handbook of Science and Technology Studies* describes STS as "an interdisciplinary field that is creating an integrative understanding of the origins, dynamics, and consequences of science and technology"; a characterization immediately connected to the importance of a stable environment for STS ("we seek academic respectability and institutionalization and their accompanying resources"; Hackett et al. 2008, 1). Meanwhile Jasanoff's (2010, 204) recent "A Field of Its Own: The Emergence of Science and Technology Studies" argues for retrenchment on the grounds of disciplinary "survival and continuity," and worries about the hesitance with which many STS scholars greet the need to "demarcate" its "delightfully unruly territory" (203). Failing to do so "ratifies a status quo" (204) in which STS will continue to be seen as a second-grade field in the broader intellectual landscape, and lose the opportunity to gain disciplinary standing. The lack of order and coherence of STS, Jasanoff argues, "militates against the field's maturation as a self-defining, self-governing area of inquiry" (204).

STS as "Normal Science?"

At this juncture, it is possible to suggest with slight irony[3] that STS has entered a phase resembling what Kuhn (1962) called *normal science*, or, in Jasanoff's

case, aspires to do so. Kuhn identified this phase, which followed the more dramatic upheavals of scientific revolutions, with the important but mundane task of "puzzle-solving." The analogy to STS is the contrast between the "not another case study" syndrome noted by Barry, Beaulieu et al., and Wyatt and Balmer, and the liveliness and controversy (Jasanoff's "delightful unruliness") that characterized debates in previous decades.

With this diagnosis in mind, the question immediately arises: "what to do about it?" The answer is not straightforward, for Kuhn did not propose that anyone could simply decide to change the modus operandi of a discipline even should they want to. Further, as exemplified by the *Handbook*, it is not obvious that everyone would want to change the situation. After all, as the editors note, a more institutionalized STS undoubtedly brings significant benefits.

The editors of the *Handbook* resolve the issue by shifting between two views. On one hand, they say, STS provides an "integrated understanding" (Hackett et al. 2008, 2), but on the other hand it entails "theoretical eclecticism," "strategic crossovers, and melded ideas" (3). Meanwhile, eclecticism is precisely the worry of Wyatt and Balmer (2007, 620), who ask how it is possible to: "think it reasonable to use concepts from completely different normative and epistemological traditions?"[4]

In the following, I propose that a fresh look at the conceptual–empirical dichotomy facilitates a particular kind of response to these concerns. I argue that a careful consideration of the making of "concepts" out of "empirical" study shows that the two cannot be separated except post hoc, and even so only temporarily. Thus, we are always in Hackett et al.'s realm of "melded ideas" whether we want it or not. I further argue that this observation can be used to leverage STS in a form that strives to maintain its ability to reinvent and expand its analytical repertoires through engagement with multiple "conceptual–empirical" packages.[5]

At one level, this is an entirely nonprescriptive suggestion. It *disqualifies* nothing. Certainly there is sufficient room in STS for plenty of case studies, grand theory building, and bridging efforts, and the field would be poorer without each. However, these are still only a subset of the available options, and loosening the conceptual–empirical dichotomy may make other possibilities visible. The following discussion of the research that led to the formation of cognitive dissonance theory, offers an illustration of how the conceptual and the empirical blend in research practice, only to be subsequently filtered into theory (in this case: of cognitive dissonance) and supporting empirical evidence (never again questioned and now entirely inconsequential). Though this case is obviously specific, I suggest it can stand as a synecdoche for conceptual–empirical relations in general.

Insides and Outsides

In *Natural Reflections* (Smith 2009, 2-5), Barbara Herrnstein Smith explains how in the early 1950s Marion Keech, a resident of a town in the US Midwest, called on newspapers to let them know of a coming flood, which was going to be the first in a series of catastrophic events, leading to eventual worldwide cataclysm. Keech had been told about these forthcoming occurrences by aliens through the medium of automated writing. Along with a small set of devotees, she prepared herself in the countryside of Minnesota.

Shortly after this public announcement, Keech got five additional followers. Who were these newcomers? Neither journalists nor anthropologists nor secret agents, they were a group of psychologists. Alerted to the existence of the millenarians through newspaper reports, they had decided to conduct a "field test," an empirical experiment to test a theory. The theory, which subsequently became familiar as the theory of cognitive dissonance, aimed to explain the tendency for people to remain convinced of their beliefs even in spite of strong disconfirming evidence (Festinger, Rieken, and Schacter 1956). When the flood failed to materialize, the psychologists did indeed get their hands on a body of evidence that bore on the matter.

This example helps us to think about the relation between the conceptual and the empirical. The psychologists' approach certainly did not resemble approaches that are au courant in contemporary STS. Their assumptions about the relations between the conceptual and the empirical were traditional, not to say outmoded. Nevertheless, as we consider their ways of doing research, we find that these epistemological assumptions were practically undone in interesting ways. Indeed, the scientific end result defied the separation between the conceptual and the empirical that undergirded the research in the first place. Conceived as a field test, the experimental setup of the psychologists was guided by the epistemological and methodological assumption that scientists have to take an outside (observer's) perspective in order to produce objective and valid knowledge. Accordingly, their aim was to test a hypothesis derived from the available corpus of psychological knowledge. The purpose of study was to develop psychological *concepts* through neutral observation of *empirical reality*. The traffic between the conceptual and the empirical was thus assumed to be one way. Given this starting point it is not surprising that, although the psychologists went to live with the millenarians, they were not in the least interested in understanding their social contexts or practices.

Yet, in spite of this detached stance, the form of engagement actually adopted by the psychologists must be characterized as a kind of covert

"participant observation." Thus, although the psychologists' ambition was conceptualization facilitated by an outsider's neutral position, their mode of operation defied the traditional requirements of detached objectivity. Indeed, they established their outsider's vantage point by pretending to go native. The objectivity of the psychologists' observations was immediately troubled because they had to act as if they were part of the interpretive community they studied.

Much could be said about the analytical and ethical problems attending this particular "field test." Here it is sufficient to note that the method actually adopted located these psychologists in an epistemological space that was neither quite inside nor outside. Instead, the study mixed up outsider and insider genres of inquiry. This is why Smith (2009, 3) refers to it as a "methodological mare's nest." In consequence, also, the relations between the empirical *explanandum* and the conceptual *explanans* of the test blurred. As Smith documents, this *unplanned methodological mixture* created both opportunities and problems for conceptualizing the psychological makeup of the millenarians.

Moreover, this happened in spite of the fact that the psychological study had no reflexive aspiration whatsoever. Indeed, it occurred unnoticed and quite in spite of the psychologists' "stalwart" empiricism (Smith 2009, 152, n8). Simply as a consequence of doing research, clear-cut epistemological and methodological principles gave way to actual methodological and analytical hybridity.

This blending emerged from within a body of manifestly positivist research; however, I propose to read it as a synecdoche for conceptual–empirical hybridity more generally. Certainly, chances are that STS research, in which overt rather than covert interaction is the order of the day, and where mode 2 collaboration (Nowotny, Scott, and Gibbons 2001) and interactional expertise (Collins and Evans 2002) are promoted, would give rise to *much more diverse* forms of conceptual–empirical hybrids than what cognitive dissonance has on offer. This raises the question of what would happen if we took as a starting point conceptual–empirical mixtures and inside–outside hybrids? What if mixture was seen, not as instantiating potentially avoidable epistemological or methodological errors, but rather as an underlying condition of inventive research?

But is posing such questions to an STS audience not simply preaching to the converted? STS researchers would seem fully aware that they work with "fuzzy sets" and "blurred categories" (Hackett et al. 2008, 4). And, of course, in some sense they are. Yet, if we turn to STS journals, we routinely find forms of argumentation that adhere strictly to the conventions of the

conceptual–empirical dichotomy: we read that this article is a "case study of X" or this article is a "theoretical contribution to Y." As I indicate further on, much "high-end" STS debate likewise continues to revolve around "big theories," discussed as freestanding intellectual formations pitched against one another.

Thus, I suggest that the constructive implications of conceptual–empirical hybridity have not been sufficiently explored. The point is not primarily about encouraging STS researchers to explicate the conceptual–empirical packages with which they work. In any case, as the history of cognitive dissonance theory suggests, these packages are neither fully controllable nor fully explicable by researchers, no matter their degree of reflexivity. The point, rather, is that insofar as hybridization occurs *in any case*, it becomes clear that *there are always* many different versions of the conceptual–empirical dichotomy in play and that still others are possible.

Symmetry

In STS, the best-known procedure for ridding oneself of dichotomy goes by the name of symmetry. In basic form, symmetry means that the same form of explanatory causes should be adopted to account for both what is normally viewed as rational and irrational behavior (Bloor 1976): the same type of general explanation, for example, for millenarian "irrationality" and scientific "rationality." Generalized, it involves the expansion of agency (*in potentia*) to every conceivable entity (Latour 1993). This means that concepts, qua agents, are located in the world; that they operate in and on the empirical (e.g., Pickering and Stephanides 1992)—although of course not equally in all cases. At the same time, it implies that the empirical is itself conceptual in multiple ways.

It follows that "problematic" methods and "epistemologically naïve" assumptions may generate novel insight (as the psychological field test indeed did). Concepts may have surprising empirical effects (Jensen 2007), and the empirical may itself be mined for the multiplicity of concepts and methods that actors exploit (Maurer 2005; Verran 1998; Morita this issue). Symmetry loosens up the relation between the conceptual and empirical.

Barbara Herrnstein Smith's comments on the millenarians and the psychologists are explicitly symmetrical in Bloor's sense. Showing that the millenarians' behavior after the nonoccurrence of the flood did in fact support several of the psychologists' theses, she also notes that a number of their predictions failed to come through. For example, some of the millenarians

did abandon their convictions. Nevertheless, just as many millenarians were capable of offering creative accounts for why catastrophe had not occurred, the scientists also "exhibited considerable resourcefulness in explaining the relevant disparities between expectations and experience" (Smith 2009, 4).

If the psychological field test enabled the formulation of the theory of cognitive dissonance, it therefore did so in a situation where the guiding conceptualization *simultaneously matched and failed to match the empirical*. And this happened *both* in the cases of the millenarians *and* of the psychologists.

Thus, Smith hands us the tools to deconstruct cognitive dissonance. And yet, she suggests, critical dismissal would be an inadequate response. It would be inadequate because it would disable recognition of the constructive effects of cognitive dissonance, which came to be established as "one of the most firmly established, highly respected, and intellectually fertile theories in the history of social psychology" (4). Not only that, but, as in Hacking's (1995) looping effects of human kinds, cognitive dissonance has created new effects in the world, including the now widespread ability to experience such dissonance. In spite of all its problems, this theory has thus been tremendously generative, both "conceptually" and "empirically."

Cognitive dissonance, as successful theory and practice, has thus moved along a complex trajectory. Smith's account of this process is simply one more point on the trajectory, since she uses the parable of the millenarians to create further insights with and about this "theory." For the present argument, however, it is particularly salient that dissonance theory became one of Merton's *exemplars of midrange theory*, described by Brian Rappert (2007, 695, emphasis removed) as "neither a collection of empirical observations made of a certain culture at a certain time nor the sort of totalizing theories of behavior proffered by Parsons." Not a totalizing theory, then, but nevertheless a theory; certainly not a "collection of empirical observations." But where, then, did "the empirical" that so infused the making of cognitive dissonance theory go? It would seem that the contingent and highly particular "empirical bits" of the Minnesotan countryside and millenarians have fallen by the wayside.

Even if the concept of cognitive dissonance emerged from a process of blending, this "co-production" eventually turned into a "theory" later used in discussions about how STS might leave behind its "discontents." When Wyatt and Balmer urge us to return to this Mertonian scale in order to deal with contemporary discontents in STS, their discussion is thus already shaped by the conceptual–empirical dichotomy.[6]

Tastes of Research

I have argued that STS, as other social research, develops and changes through varied engagements with the conceptual and the empirical. As illustrated by the case of cognitive dissonance, conceptual–empirical blending is not a matter of epistemological preference. Nor is a stable relation between the conceptual and the empirical is a matter of choice or volition, because that very relation is at stake in concrete, unpredictable practices of research. It follows that what we usually call the conceptual and the empirical are relativized. They are produced in multiple versions, which depend on context and circumstance, as much as on predilection and rigor.

In their introduction to the midrange debate, Wyatt and Balmer (2007) expressed concern with theoretical eclecticism. But although conceptual inconsistency or empirical inadequacy may be common problems, the risk for STS with which I am concerned is not to do with representational inadequacy, as in a painting that fails to look like its object. Rather, the risk I have in mind is that applying the same theories to evermore cases lead to diminishing interest, with the consequences that the dishes presently on offer in STS too often have a bland taste. The attending question is how STS tastes of research might be diversified and the dishes enriched.

Now the reference to blandness will seem harsh. Even so, it is noteworthy that the arguments of scholars from Andrew Barry to Sally Wyatt point in the same direction. Nor are they alone. In his recent appraisal of the influence of *The Structures of Scientific Revolution* and *Science in Action*, Sismondo (2012, 417) remarks that "through the middle 1990s, it seemed that one could pick any section of *Science in Action* and build a project around it, matching a few key terms with a good case." Lynch (2012, 452) goes him one better, stating that "speaking as en editor who has read upwards of 200 new submissions per year over the past decade, I have been led to the sad conclusion that the volume of BADANT (Banal and Derivative Actor Network Theory) greatly exceeds the well-researched and broadly informative written work that rides under the ANT banner." He continues to emphasize that "ANT is by no means the only theoretical resource that has suffered this fate" (452). Both seem to hone in on a problem of blandness, connected to the "normal science" application of well-established theories.

When it comes to suggestions for what might be done to alleviate this state of affairs, however, arguments routinely fall back on the conceptual–empirical dichotomy, applied in favor of one or other of just the same core theories. For one recent example, consider Jasanoff's (2012, 493) argument that Dorothy Nelkin's controversy studies preceded Bruno Latour's

but were comparatively disregarded since they were "devoid of social theory and lack[ed] verbal pyrotechnics." On one hand, Latour's approach is presented as having gained advantage due to conceptual "pyrotechnics" in contrast with Nelkin's empirically grounded work, which "stayed pretty close to the surface of things" (493). On the other hand, Jasanoff goes on to argue for her own more "co-productionist project" (493).

For another example, consider Collins and Evans' (2002) claim that the way forward for STS is a "normative theory of expertise." As they write, "we now need to reconstruct knowledge and develop studies of expertise and experience" (2002, 270). In response, Jasanoff (2003, 397) focused on their "exceedingly narrow formulation." Collins and Evans (2003, 444) replied that Jasanoff "insists that descriptive analyses of the way expertise is established and the like are the only business of science studies." Notice that Collins and Evans' critique of Jasanoff is precisely that she is merely "descriptive," not sufficiently conceptual. It is just the inverse of Jasanoff's critique of Latourian pyrotechnics in defense of Nelkin's empirical "surface of things."

Even so, Collins and Evans' characterization is incorrect. For though Jasanoff certainly argues against *Collins and Evans'* theory, she is not averse to offering coproduction as a more comprehensive and satisfying recipe for STS. In other words, Jasanoff's characterization of Latour's purely conceptual pyrotechnics is just as misleading as Collins and Evans's depiction of Jasanoff as a theory-less empiricist. Contrary to what these combatants suggest, there is no battle of theory versus description here. Instead, we are witness to a fight, where the conceptual (theory) and the empirical (description) can in turn be wielded against each other. The conceptual–empirical dichotomy is left intact in these cases: what varies is simply what is slotted into each side of it.

Now insofar as taste is seen as a purely individual preference, the metaphor of tastes of research is obviously flawed. However, one needs to only consider the variety of cuisines worldwide (sushi and curry, burgers and tarantula) to realize that taste is never solely subjective. In the case of food, taste emerges at the intersection of individual preference, geographic location, history and culture, the availability of foodstuffs, and, centrally, the ability to see specific items as edible. As regards research, taste is an outcome, among other things, of research interests, social networks, institutional histories, battles over turf and prestige, and, prominently, what scholars are able to recognize as appropriate and adequate forms of conceptualization and analysis. When Latour, Jasanoff, and Collins carry out "high-end" STS discussion, we are witness to a competition among some of the most enduring and beloved tastes of research in STS. They also happen to be rather mutually distasteful.

However, other tastes are also on offer or emerging. They, too, are part of the ongoing history of the STS, which is presently embedded in increasingly varied institutional settings, its research related to increasingly heterogeneous concerns. It is not unreasonable to claim that the ontology of the field itself is in a blended state. Still, when it comes to general characterizations, as expressed in central discussion papers or state-of-the-art commentaries (e.g., the ones just discussed), we are presented with much narrower "core versions" of what STS is. We are witness to a set of well-established and canonical theories battling for turf. When Jasanoff or Collins and Evans offer prescriptions for what the field *should be*, they focus on imposing order and coherence (Jasanoff) or on adopting just the right kind of theory (Collins and Evans, namely their own). In comparison, we hear relatively few voices speaking up for the importance of expanding conceptual and empirical registers or experimenting with new resources. When I argue for loosening the conceptual–empirical dichotomy, it is with these latter interests in mind.

Dissolved Boundaries?

It is clear that inventive analyses continue to be made within STS. Some of these studies are created from disciplinary centers of STS, like Cornell, Harvard or Ecole des Mines but many others are not. Indeed, some of the most imaginative scholarship comes out of STS research conducted in institutional environments where, by choice or necessity, it is shaped through close encounters with other disciplines and concerns.[7] Close encounter with other disciplines entails undeniable risks, such as the flattening of important differences, instantiated, for example, in certain forms of conceptual eclecticism. At the same time, the interdisciplinarity of much STS research holds inventive potential, because it enables and requires the field to expand its areas of "empirical" concern and "conceptual" inspiration *at once*, and to create new versions of the empirical and the conceptual in the process.

In a sense this is simply a description of the operations of current STS (though, as the preceding discussions makes clear, no description is pure). As Lynch (2012, 453) argued with reference to ANT, its traveling to many new contexts has permitted and relied upon "drift and slippage from one node to another, with relatively weak and decentralized policing" (cf. Gad and Jensen 2010). Yet, one might also conclude that STS, rather than attempting to impose order and keeping at bay noncanonical approaches, might do better to welcome diverse forms of conceptual–empirical hybrids.

This runs directly opposite to Collins and Evans' (2002, 251) prescriptive argument. They note that "some writers," (unnamed), take "the

fuzziness of many boundaries as the empirical counterpart of a philosophical prescription: 'dissolve all boundaries.'" Whereas Collins and Evans claim to know what STS as a field must do ("we now need to reconstruct knowledge and develop studies of expertise and experience"), my "recommendation" does not entail any strong claim to knowing what "we" need. Indeed, it is purposefully nonspecific,[8] because I am convinced that STS benefits from too many different things to be specified by any agenda, no matter how encompassing.

Contrary to what Collins and Evans' might think, however, this is not due to a preference for dissolving all boundaries. It is rather premised on the recognition that STS research is already part of heterogeneous settings and that both its internal and external boundaries have already multiplied. The consequence of this situation is that STS's its knowledge-making practices are already thoroughly entwined with those of other disciplines. Though Collins and Evans suggest that certain researchers have a predilection for dissolving boundaries, the issue is by thus no means exclusively philosophical. STS is presently part of many overlapping intellectual and applied practices, and STS researchers are de facto subject to multiple, sometimes incongruent, forms of quality control. The complexity of the situation is not due to the fact that "boundaries" are being philosophically dissolved but that they have multiplied in practice. There is no one form of order and coherence in STS, because the fields' development is premised on the uneasy coexistence of many.

Perhaps we could stop with that relativist message. We could then follow Wyatt and Balmer who begin by invoking Merton's (1968) middle-range theory as the space between "descriptions and theories of everything" (Wyatt and Balmer 2007, 619) and ends by speaking of "to middle range" as "a process or performance, middling but not muddling" (622). We would have been reminded that STS, as other areas of inquiry, is a fundamentally hybrid space. We would not, however, have moved away from the conceptual–empirical dichotomy. Thus, I return to the "weirdly non-specific" prescription embedded in the present argument by engaging with Deleuze and Guattari's evocation concept of continuous variation.

Continuous Variation

In *A Thousand Plateaus*, Deleuze and Guattari (1987, 372-73) introduced the notion of the *following sciences*. Contrasting following sciences with what they called the *reproductive sciences*, Deleuze and Guattari defined the former as concerned with *singularities* and *continuous variation* rather

than law-like regularities. Following sciences are *itinerant* and *ambulant*, concerned with tracking flows in a "field across which singularities are scattered like so many 'accidents' (problems)" (372). Although these authors did not refer to the social sciences (cf. Brown 2010), it is noteworthy that the designation "following science" connects almost verbatim with the literalized version of ANT's injunction to "follow the actors" (Latour, 1987). STS might thus be characterized as the *following science dedicated to following science* (and technology).

It may seem as if STS automatically fits the bill of a "following science." After all, ethnomethodological studies of science (e.g., Garfinkel, Lynch, and Livingston 1981), laboratory studies (Knorr Cetina 1981; Latour and Woolgar 1979), and studies in the tradition of symbolic interactionism (e.g., Star 1989) were nothing if not interested in following actors and practices. They demonstrated, along with numerous other STS studies (e.g., Hackett et al. 2008), that actual scientists are by no means solely preoccupied with theory, concepts, or truth. Rather they spend their time gathering resources, laboriously tinkering with laboratory setups, enrolling ever more natural and technical entities, carefully working on the rhetoric of their articles, and fighting with other colleagues over intellectual capital and financial resources.

Devoting their efforts to the naturalistic study of all that scientists *did,* the early laboratory studies described scientific systems, networks, and so forth, from the *outside*. The characterization is somewhat unusual since one of the traits distinguishing laboratory studies from the philosophy of science was precisely that it moved *inside* laboratories, rather than defining epistemological principles in the abstract. Yet, even as Latour and Woolgar entered the laboratory, their aim was not to elucidate the self-understandings and motivations of the scientists. On the contrary, they took deliberate advantage of their position as "strangers." The result was that they could redescribe what scientists viewed as a search for facts and truth in quite different terms: as a practical matter of producing inscriptions. (Notice the striking similarity between this description and the making of cognitive dissonance theory. In both cases, analysts "came from the outside" and "went back to the outside" to make theory untainted by "indigenous conceptualization.")

This "mode of following," however, also had certain built-in limitations. By declining to query the quality of scientific concepts *as concepts*, STS scholars were prevented from coming to grips with the question of what makes science meaningful, important, lively, and exciting *to scientists* (Bowker 2010; Stengers 2011). This diminished the ability of STS to

elucidate the varied ontologies of science. Pithily stated, the issue was that whereas scientists were taken seriously as *practicing actors*, they were not addressed as *thinkers*. Though seemingly "following" the conceptual and the empirical wherever they went, classical laboratory studies garnered strength from the dichotomy: it was left to STS theorists to conceptualize science as exhibited empirically through the activities of scientists.

As I have argued throughout this article, however, the relations between "outside" and "inside," "the conceptual," and "the empirical" are unstable. This is the case for ANT as much as for cognitive dissonance theory or ethnomethodology. All are "blended products." All produce the conceptual and the empirical in specific versions. The histories of all exemplify the conceptual and the empirical in continuous variation, and variable form.

The notion of STS as a following science suggests several "non-specific" points. First, it highlights that no version of the conceptual–empirical can or should be taken for granted. Whether it is admitted or not, STS research is already involved in making blended products. Much research is likely to rely on *some version* of the dichotomy (since dualism is our "entirely necessary enemy"), but it is always, contingently, open for negotiation and translation. Just as STS never ceases to find hybridity in the midst of other practices, "at home" we are also witness to a variable and shifting ontology in which the conceptual and the empirical move about in surprising ways.

Second, experimentation might be actively pursued by bringing more varied conceptual resources into play in STS (e.g., Bowker 2005; Jensen 2008, 2010). It might also be pursued through engagement with much more varied empirical sites and resources than are found in the "core set." The opportunities engendered by this kind of empirical experimentation are exemplified by various kinds of contemporary interdisciplinary STS research (e.g., at the intersection of STS and the arts), and by research from outside the Euro-American centers of the field (e.g., in East Asia or Africa). As Jasanoff (2012) recently emphasized, STS ought to connect scientific laboratories to many more social arenas. I would only add that we should also engage seriously with all the "conceptual" laboratories located in these diverse "empirical" arenas.

Finally, bringing out this inventive potential requires both individual interest and systemic support. As regards the former, I have argued that insofar as the conceptual–empirical dichotomy is always unstable this can be taken as an encouragement to experiment rather than a reason to protect the canon. The latter point returns us to my sweeping claim that STS has entered a phase that resembles Kuhnian normal science. Recent discussions concerning the relation between STS, policy, and society at large (e.g., Nowotny 2007; Webster 2007), and the institutionalization of the field (Jasanoff 2012)

seem to indicate a diminishing interest in, or support for, the inventiveness that characterized the ''delightfully unruly'' STS of yore.

While a degree of stabilization unavoidably accompanies the creation of a field, and is also welcome to a degree, it nevertheless carries the risk of normalization. I read this as an implicit but central aspect of the ''frustration'' (Wyatt and Balmer), ''discontent'' (Geels), and ''sadness'' (Lynch) with which recent scholars view the application of canonical theory methods packages currently on offer. However, there is only reason to be discontented if those packages are henceforth unchanging monuments in the STS landscape. In contrast, I have argued that insofar as conceptual–empirical relations are *in any case* recreated on an ongoing basis, they might as well be actively engaged, challenged, and transformed.

In this case, there is still much for STS to learn, not only from the varied people, cases, and practices we study but also from the diverse concepts, methods, and approaches put to varied use by scholarship both *in* and *around* STS. They provide opportunities for lateral learning: the combination and mutual translation of knowledge across domains. As Jasanoff wrote, the sea is indeed still uncharted.

It has not been the aspiration of this article to disqualify any of the outstanding work that researchers in STS already know and draw upon. Recognizing, however, that intellectual agility was precisely what made the field so exciting and relevant during previous decades, it has argued that it is in the best interest of STS to continue promoting an ethos of empirical and conceptual experimentation.

Notes

1. In her contribution to *The Oxford Handbook of Interdisciplinary Studies*, "A Field of Its Own: The Emergence of Science and Technology Studies," Sheila Jasanoff (2010) likens the task of STS to that of charting unknown seas for new islands of insight and learning. This is a very appealing metaphor. However, she continues by expressing regret that "that many scholars who see themselves as members of the STS community are hesitant to support disciplining in either sense of that term: either importing order and coherence ... or constituting STS as what some have dismissively called a '*high-church*,' and therefore an elitist and exclusionary academic discipline" (Jasanoff 2010, 203). Both forms of discipline are needed, in Jasanoff's estimation, to fully constitute STS *as* a discipline. The imposition of order and coherence in order to gain disciplinary leverage exemplifies what I mean by strategic retrenchment.

2. Thus, Beaulieu, Scharnhorst, and Wouters (2007, 673) ask: "Case studies are good for deconstructing claims of universality (i.e., the scientific method). But how can they be used to inspire other work, without explicitly or implicitly falling into a universalizing fallacy?"

3. The irony is twofold. First, Kuhn's main distinction between normal science and paradigm shifts can no longer be taken at face value (cf. Smith 1999; Jasanoff 2012; Lynch 2012). Second, I do not claim to have studied the history of STS in sufficient detail to make a straight-faced argument that STS *has* entered normal science. I invoke the notion of "normal science" to pinpoint a certain tendency, subsequently discussed in more detail.

4. Wyatt and Balmer (2007, 4) refer to eclecticism within a single case study, but amplifying eclecticism to the level of the whole discipline would not seem to diminish the problem. The editors of the *Handbook* go on to characterize STS as particularly attuned to: "fuzzy hybrids and ambiguities, tensions and ambivalences" seen as "processes of co-production." The notion of coproduction is developed and used in particular by Sheila Jasanoff (e.g., Jasanoff 2004; Jasanoff and Martello 2004), but here comes to represent STS's integrative efforts in general.

5. Though I follow Hackett et al. in talking of melded ideas, the previous sentences themselves obviously draw on the dichotomy between the "conceptual" and the "empirical." But this simply testifies to the strength of the dichotomy. As Deleuze and Guattari said, one has to work "via all the dualisms that are the enemy, an entirely necessary enemy, the furniture we are forever rearranging" (Deleuze and Guattari 1987, 23). Of course the same logic can be applied against the present argument: everyone in STS knows that the conceptual–empirical dichotomy does not hold, but they rely on it for practical, strategic, or conventional reasons. No doubt this is true. Yet, as I show below, the dichotomy also generates *specific*

argumentative effects that support established versions of STS and disfavor new experimental work.

6. This does not mean that either Wyatt and Balmer (2007) or Rappert (2007) adopt Merton's solution. For example, in an observation that resonates with the present text, Wyatt and Balmer (2007, 622, emphasis added) consider whether the middle range could be seen as a place where "the two [case studies and theory] already meet and mix *but which is not properly interrogated.*"

7. I do not offer specific examples, just as I chose not to illustrate what I take to be "another case study" (nor, probably for reasons of politesse, did Barry, Beaulieu et al., Lynch, or Wyatt and Balmer). The point is not about assignation of praise and blame but about a more general tendency and its implications.

8. In an excellent formulation, one reviewer characterized the argument as "weirdly non-specific."

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