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***Ambivalence, equivocation and the politics of experimental knowledge: A transdisciplinary neuroscience encounter***

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**Des Fitzgerald***
Cardiff University

**Melissa M. Littlefield**
University of Illinois at Urbana-Champaign

**Kasper J. Knudsen**
Aarhus University

**James Tonks**
Dame Hannah Rogers Trust and University of Exeter

**Martin J. Dietz**
Aarhus University

*Corresponding author: FitzgeraldP@Cardiff.ac.uk

**Abstract**

This paper is about what it feels like to participate in a transdisciplinary project between the social, human and life sciences. 'Transdisciplinary' and 'interdisciplinary' research-modes have been the subject of much attention lately – especially as they cross boundaries between the social/humanistic and natural sciences. However, there has been less attention, from within STS, to what it is actually like to participate in such a research-space. This paper contributes to that literature through an empirical reflection on the progress of one collaborative and transdisciplinary project: a novel experiment in neuroscientific lie detection, entangling STS, literary studies, sociology, anthropology, clinical psychology, and cognitive neuroscience. Its central argument is two-fold: (1) that, in addition to ideal-type tropes of transdisciplinary conciliation or integration, such projects may also be organized around some more subterranean logics of ambivalence, reserve and critique; (2) that an account of the mundane *ressentiment* of collaboration allows for a more careful attention to the awkward forms of 'experimental politics' that may flow through, and indeed propel, collaborative work more broadly. Building on these claims, the paper concludes with a suggestion that such subterranean logics may be indissociable from some forms of collaboration, and it proposes an ethic of 'equivocal speech' as a way to live with and through these kinds of transdisciplinary experiences.

**Introduction**

The desire to work between disciplines is an emerging feature of the contemporary academy. The trend towards the dissolution of boundaries between different ways of producing knowledge can be seen in a variety of sites, including novel degree programmes (Worton, 2013), genre-busting research-management strategies (European Science Foundation, 2013), and centres and objects of research (Hadorn et al., 2008). For scholars within Science and Technology Studies (STS), such hybrid research projects have particular salience. Not only has STS scholarship developed tacit expertise that crosses different disciplinary knowledges (Collins and Evans, 2002), but also the very production and sustenance of disciplined knowledge-objects has itself been a longstanding interest within these literatures (Gieryn, 1983; Star and Griesemer, 1989). Moreover, having been trained to identify the gaps and continuities between others' intellectual practices, STS scholars are well placed to actually work within and through interdisciplinary research projects (Jasanoff, 2011). Unsurprisingly then, as interdisciplinarity has emerged as a potent rhetoric of contemporary knowledge production and as a way of enacting a hybridized STS knowledge, sustained attention to the collaborative logic of interdisciplinarity has appeared within STS (Gorman, 2010; Maasen, 2000; Rabinow and Bennett, 2012) and cognate literatures (Frodeman et al*.,* 2010; Schmidt, 2007).

This paper reflects on the progress of one collaborative and transdisciplinary project, a novel experiment in neuroscientific lie detection, in which the authors of this paper, who are from different disciplinary backgrounds – STS, literary studies, sociology, anthropology, clinical psychology, and cognitive neuroscience – were entangled. There are still relatively few accounts (Rabinow and Bennett, 2012, discussed just below, is an exception) of what it is actually like, in the most basic sense, to participate in such collaborations, and even fewer analyses of the broader logics of collaboration that root themselves in such experiences, or in the feelings engendered through them. This is potentially significant. The humanities and social science authors on this paper, for example, entered the collaboration with ideal-type accounts of the logic of cross-disciplinary labour, based on notions of trade (Galison, 1997), interaction (Collins and Evans, 2002), and integration (Huutoniemi et al.,

2010). And yet these accounts did not serve the group at all well when they encountered the more mundane realities of actually producing knowledge within a collaborative and transdisciplinary research space. In preparing this reflective paper, which tries to re-focus attention away from the ideal and the abstract, and towards the mundane and the tangible, we have come to the realization that the mundane pragmatics of this collaboration were not so much guided by logics of collaboration, shared interest, and mutual enrichment; that they took place, in fact, within a rather less transparent, rather less unified, and rather less propitious sphere of interaction and exchange – a field that was dominated, instead, by some more subterranean feelings of ambivalence, critique, reserve, and even dishonesty. This paper is thus an attempt to unfold the consequences of a realization that collaboration is sometimes not so much about dialogue or trade; that it can also be a much more ambiguous intertwinement of knowledge, affect, and power.

In their *Designing Human Practices* (2012), an account of a similarly collaborative effort within the emerging field of synthetic biology, the ethnographer-collaborators Paul Rabinow and Gaymon Bennett describe their interaction with molecular scientists as 'a productive experiment' (2012: 1). And yet, as with our collaboration, such productivity does not mean that Rabinow and Bennett's experimental experience was an unending joy. Indeed, reflecting on an experience that produced some acrimony, the ethnographers go on to explore, at some length, their scientific collaborators' 'wide-ranging lack of curiosity outside of their specialties' and the ways in which agreed understandings were, in practice, 'rarely addressed seriously and…easily neglected, when it [came] to the inclusions of the human sciences in bioscientific enterprises' (pp. 4, 7). They describe how their social-science questions were frequently met with 'perplexity, indifference, and/or hostility,' from their bioscientist colleagues, and they show how they sometimes even experienced 'active resistance' to their research priorities (pp. 8, 20). Amid this 'hierarchy of power and privilege,' they point out, it was taken as natural that Rabinow and Bennett 'were conversant with the molecular biology and eager to learn more' – whereas, on the part of the molecular biologists themselves, 'no reciprocity emerged,

nor was it encouraged' (p. 29). As we will show below (but we will depart markedly from Rabinow and Bennett's conclusion) this sense of discomfiture resonates with our experience.

In an account of ethnography and collaboration at Xerox Park, and drawing on Barry et al.'s (2008) account of a more 'agonistic' interdisciplinarity, Lucy Suchman asks: 'what could it mean…to treat [such] resonances and tensions as productive?' (2013: 156). This paper provides one answer. Its core argument is that attending to these experiences opens up a more expansive and compelling space for theorizing interdisciplinary work; more specifically, by reflecting on the mundane everyday of collaboration, and on the feelings and senses of *ressentiment*[1] entangled in it, it argues that we might more fruitfully explore the awkward forms of 'experimental politics' that sometimes animate collaborations like this one. Thus, in the first section, we describe the experiment in question, and our reasons and methods for approaching it. In the second, we reflect on what it was like to participate in this experiment, focusing on the presence of three unanticipated collaborative registers in our project: ambivalence, critique, and dishonesty. The third and fourth sections argue that these feelings might be interpreted as effects of the epistemological politics of collaboration, and here we attempt a more generalized account of the relationship between politics and experiment in such collaborations. In the conclusion, reflecting on the ambiguous, shifting and perhaps intractable nature of such a politics, we do not call for more genial or transparent modes of collaboration but for more attention to *equivocation* as a way to live with, and to work through, such modes of collaborative exchange.

It should be noted that when we reflect on our experience, the feelings and intentions reflected upon are not always admirable, nor are they flattering to the authors. But our commitment here is not to judge the moral content of our experience. The paper's aim, instead, is to unfold the roles played by some less savoury feelings and motivations in our collaboration, and to bring these into understanding within a wider attention to interdisciplinary work. Setting aside the desire for a normative or practice-based account of this experience, we explore how such feelings might actually *underpin* a collaboration, one, indeed, that might otherwise be understood as 'successful,' or even 'good.'

**The experiment in question**

The experiment at the heart of this paper came from a 'NeuroSchool' on 'social neuroscience and neuroimaging', run by the European Neuroscience and Society Network (ENSN).[2] Despite its name, the NeuroSchool was not designed as a straightforwardly pedagogical institution. Its goal was to look for more complex interactions between the neural and social sciences, not only by promoting cross-disciplinary knowledge-sharing, but by requiring attendees to think with and through the perspective of another science. As such, the programme declared:

> Training in the neurosciences is very often ahistorical and asocial... [whereas] students trained in the social studies of neuroscience do not always have a chance to be directly exposed to how rationales and questions in neuroscience experimentation are formulated. (ENSN, 2009)

The goal was not simply to pool neuroscientific and social-scientific expertise, but was to generate a space in which neuroscientists and social scientists might work together to conceptualize a neuroscience embedded in its own culture and history, as well as a social world worked through the structures and functions of the human brain.

The ENSN attempted to generate an infrastructure for scholars to think and collaborate in the space between a burgeoning neuro-discourse and the wide-open field of related ethical and social issues. In an era that did not lack for neuroscientists thinking about the societal consequences of their work (Iacoboni, 2008), or for social scientists keen to talk about the cultural *a prioris* of neurobiological knowledge (Martin, 2005), the ENSN was distinguished by its attempt to locate a specifically *trans*disciplinary space between neuro-biological and sociological interests:

> 'transdisciplinarity', unlike interdisciplinarity, does not simply mean laying two or more disciplines next to each other. Rather, it means to set about a question simultaneously taking into account visions and methods on the same topic from seemingly different perspectives. (ENSN, 2007)

Participants at the NeuroSchool not only exchanged ideas on social neuroscience and the role of neuroimaging technology; they also formed into teams to collaborate on the design of neuroimaging experiments that were expected to grant intellectual parity to the role of social context. The team that designed the most engaging proposal was invited to carry out their experiment at the Centre for Integrative Neuroscience, in Aarhus, Denmark. The design that became the experiment at the heart of this paper was the winning proposal.[3]

What united many of the participants at the NeuroSchool – and certainly the authors on this paper – was an inchoate sense that there was ground to be claimed by pushing at the edges of an emergent 'social neuroscience' (e.g., Lieberman, 2006). The transdisciplinary goal of both the workshop, and the present authors, was to ask if experimenting with a 'more social' social neuroscience could help us, in turn, to reimagine forms of engagement across the biological and social sciences. Participants worked together, as in an interdisciplinary model, but in addition they tried to reimagine (however partially) some of the fundamental questions and assumptions of the 'other' disciplines (see Thompson Klein [2010] for a careful parsing of forms of inter- and trans-disciplinarity).[4] Thus, the ostensible and public shared normative commitments both of the members of this collaboration, and of the NeuroSchool in general, were not so much to the critique of neuroscience as such, but to the emergence of a more expansive, transdiscplinary, (especially) social neuroscience, one that maintained a commitment to the rigours of experimental practice but that was also mindful of the plurality of disciplinary perspectives on, and approaches to, 'the social'. In addition to its ostensible goal, therefore, the 'winning' proposal would also be an experiment in collaboration itself. How would a truly transdisciplinary engagement between sociological and biological knowledge actually play out, in practice? This was a question that animated the NeuroSchool and our collaborative group.

It is worth noting that the methodology of this larger experiment in transdisciplinarity was ad hoc and informal. The groups were formed seemingly only with an eye to disciplinary spread; there was no obvious processes for on-going assessment or mediation (beyond the competition), nor did

there seem to be an accounting for the quality and process of collaboration. We note this not as a critique of the ENSN, but to stress that virtually all of our group's design energy went into the neuroimaging experiment, rather than to the formation of our own interaction. This meant that we lacked an explicit method or forum for addressing and recounting the kinds of feelings that we describe in this paper, or for associating them with particular moments or issues in the collaboration. Our collective experience is that informality and ad hoc procedures are more common than not in interdisciplinary assemblages; thus, there was a serendipitous 'ecological validity' both to the ENSN's procedure, as well as to the experiment that followed. We note also that this collaboration worked. Despite some apparently unusual features, and the ambivalence that at least partly resulted from them, we produced a finding worthy of submitting to a peer-reviewed journal. We will reflect more on this relationship between the 'success' of the experiment and the lack of specificity in our procedures in the conclusion.

Our group's experimental design centred on neuroimaging studies of lie detection (Langleben et al., 2005; Wolpe et al., 2010). While attempts to produce a scientific basis of lie detection have a long history, several scholars from within the neurosciences have recently wondered if the brain-imaging method, fMRI (functional Magnetic Resonance Imaging), might not finally provide a method for reliably locating valid acts of deception within the body and brain of an individual. However, historical and cultural critics of lie detection – including Littlefield (2009, 2011) – had drawn attention to the way in which such an endeavour recapitulated some of the most basic and problematic aspects of previous attempts at scientific lie detection. These included the assumption that 'truth' and 'lie' are, if not natural categories, robust kinds with solid biological correlates; they also included the tacit suggestion that deception, as an embodied phenomenon, might be understood as a departure from a biological norm; that truth, therefore, could be positioned as the natural condition of the body at rest. The experiment attempted to transform this fundamentally sociological and historical critique into a meaningful collaboration, and thus to rearticulate a well-established socio-critique through the very methods and assumptions of

neuroimaging itself. Could we collaborate across epistemic cultures (Knorr Cetina, 1999) while designing a more reflexive experiment in fMRI lie detection, one that would have regard for the rigour and robustness of neuroscientific experiment, but that would also reflexively integrate social and cultural questions into its basic paradigm?

There is not space here for a formal account (see Littlefield et al. under review), but the experiment drew on an expansive historical and literary tradition to argue that truth and lie might not be so distinctive as the neuroimaging literature assumes (we alighted on lie detection, particularly, because it was a literature with which the group, via Littlefield, had some prior expertise). The core goal of the experimental design was to generate an 'ecological' situation in which a 'socially-stressful' truth would be operationalized. In the final experiment, we recruited 27 participants from choirs in Aarhus, told them we were running a brain-imaging study on team evaluation in the context of a competitive environment, and invited them to a day-long series of choir-based team-building activities, culminating in a team singing competition.[5] On arrival, participants were randomly split into two teams, which were to individually bond, compete with one another, and practice singing together as separate teams, until a final singing competition. During the day, each participant was individually led to a separate room, and asked to record a short video, singing solo to a camera. They were told that when they each came for their subsequent individual fMRI scan, they would evaluate four randomly selected videos. It was stressed that the 'accuracy' of their responses would contribute to an overall team score and would be revealed at an unspecified future date, when everyone would get together again, and all results be revealed.

What the participants did not know, however, was that each team contained two actors, one female and one male, who were working with the experimenters. The actors had two core instructions: (1) to play the role of a very likeable, outgoing member of their team; and (2) to sing very, very badly. Of course, when the 'real' participants were taken to record their solo videos, nothing was recorded; only the actors made (deliberately awful) videos. And when participants came for their brain-imaging appointment, we fixed it so that everyone could only select the names of the

four actors. The point of all this subterfuge was to get the participants to tell an awkward truth about the terrible performance of a member of their own team, when that person was particularly likeable, and when they had just spent a day bonding, and when this would be made public to the teammate in the future [6] – and *also* when the accuracy of everyone's judgement was very much at stake. We defined this 'socially-stressful truth' as a truth elicited from participants, in the fMRI scanner, under conditions that render the teller unsure, or slightly ambivalent, or particularly attentive, or self-aware. We hypothesized that the cognitive activity involved in the socially-stressful truth would belie the use of truth as a baseline condition in deception studies, showing truth-telling as a complex, costly, and sometimes awkward activity in its own right. And indeed we found that truth, no less than lying, showed activity in areas associated with mentalizing, empathy, attention, decision-making, and so on. Thus, we tried to use humanistic knowledge to expand and problematize this area of neuroscience by using the language and methods of neuroimaging itself.

As novices in this kind of project we were well versed in 'ideal-type' accounts of cross-disciplinary interaction and collaboration (Aboelala et al., 2007; Collins and Evans, 2002; ESF, 2013; Nissani, 1997), as well as long-standing STS theories of boundary-working and boundary-crossing in scientific spaces (Gieryn, 1983; Star and Griesemer, 1989). This is a valuable corpus on how collaboration can or should be done i.e., with transparency, clarity, and a productive outcome; it sets out the instrumental justifications for pursuing these forms of collaboration, and the gains to be made through them; and it begins to characterize some of the forms of knowledge and action (both tacit and explicit) that might actually allow researchers to talk across borders.

But the truth is that we simply do not recognize our own collaboration in such descriptions. And we are increasingly convinced that these accounts are too conciliatory and too instrumental. In short, they are too distant from our own, more contrary, experience of working across similar boundaries. Our goal in what follows is to expand these accounts of collaboration and to call for more attention to the intensely ambivalent, transgressive and affective qualities of epistemic boundary-crossing. Our gambit is twofold: (1) that the *ressentiment* that characterizes much of the experience

described here is not unique, or even terribly unusual, despite its low prominence in a literature that tilts towards encouragement; and (2) that if we wish to understand possible registers of collaboration more widely, we need some account of the presence of negative feelings in these spaces, and, perhaps more importantly, an understanding of the relationship of such feelings to experimental outcomes. The remainder of this paper is an attempt to work through this initial gambit. In the next section, we reflect on the unspoken tensions and lurking resentments that may haunt the space of some contemporary collaboration, but whose role, up to now, has not been fully elaborated. In the following section, we elaborate on the politics of experiment demanded by this reflection, and we ask what such an attention might tell us about collaboration at the broader scale.

## Ambivalence, critique and dishonesty in collaborative practice

### *Ambivalent traders*

It seems intuitively obvious that some kind of shared interest would lie at the at the heart of any successful collaboration. In their discussion of how interdisciplinary work might be assessed, Huutoniemi and her colleagues (2010) argue that 'the common bond shared by integrative activities is the need to combine knowledge resources in order to develop an integrated product, either a conceptual advance, or a solution to a practical problem' (p. 313). Similarly, Chiao (2009) suggests that, 'the convergence of … tools enables unprecedented ability to investigate the mutual constitution of genes, brain, mind, and culture – hence the motivation for conjoinment' (p. 291). In opposition to a well-motivated 'conjoinment,' however, our collaboration was characterized by a sort of ongoing and collective ambivalence. In particular, and throughout the project, several of the experimenters maintained a decidedly fuzzy attitude to their own goals and desires. For Fitzgerald, for example, motivations for collaboration swung radically back and forth between a desire to undermine some aspect of neuroscience in some way, and a countervailing desire to provoke a complacently interpretive social science. But precisely *not* clarifying this issue, and thus never committing or resolving it either way, was a constant feature of Fitzgerald's ongoing participation in

the project. For Littlefield, the opportunity to work with a brain-scanner was intertwined with a different series of unvoiced ambivalences: how could the project be fully collaborative when only one member of the group knew how to collect and analyze fMRI data? For example, when the group presented its original idea during an intensive laboratory meeting in Aarhus, the design began to change rapidly and significantly, as local attendees chipped in from the floor: suddenly the experiment needed gender balance among the actors, it needed a popular Danish activity (choir singing, as it turned out), it needed a head coil, it needed visual versus auditory questions and so on. Especially for the social scientists and humanists, the steam-roller effect of these suggestions left them feeling very much out of their depth, concerned that the experiment was getting too far from the original intentions, but also feeling quite unprepared and under-qualified to intervene in the rapid-fire workshop format. As the design changed, so did a gap emerge between the original idea and the actual experiment – so too, and not least for Littlefield, did an ambivalence form around whether that actual experiment lived up to the expectations of the idea, or whether the final design was ultimately an experiment for experiment's sake.

But what is perhaps most retrospectively striking is that the group, which had been assembled around this experiment in brain imaging, and which shared a loose desire for some more potent claim on the space between neuroscience and the social, never actually discussed the range of ambivalences gathered under this loose unity. Littlefield kept the above ambivalences to herself, for example – with the effect that the experimental design shifted significantly from the original idea (which was hers). It is notable that while some members of this collaboration (Tonks, Dietz) were more-or-less confident in the ability of the MRI scanner to reveal something truthful about brain function, others (Fitzgerald, Littlefield) were fairly committed to a view that imaging neuroscience was vastly overhyped, that confidence in its procedures was the product of a widespread epistemological naivety, and that as a cultural and academic force, its growing power was not always a good thing (Choudhury et al., 2009; Vidal, 2010). Across this divide, the shared interest remained decidedly unclear.

Collaborations are often characterized through metaphors of 'trade' (Collins et al., 2007; Galison, 1997). Galison's (1997) notion of 'trading zones' has been particularly influential as a way of describing a collaboration in which two or more epistemic communities might 'agree on rules of exchange even if they ascribe utterly different significance to the things being exchanged … trading partners can hammer out a *local* coordination despite vast *global* differences' (p. 783). Galison (1997) invokes an anthropological notion of trade to stress that collaboration is possible, even between 'two vastly different symbolic and cultural systems – between which not even the significance of the objects of trade is agreed-upon' (p. 804). Thus, in Galison's account, collaboration is not dependent on a banal and homogenizing agreement. It may, in fact, be an edgy, temporary, and local procedure (Galison, 1997: 805-6). While this rubric can account for collective diversity, it seems less able to explain individual ambivalences. Whatever their different starting points, as Galison (1996: 151) reminds us elsewhere, each participant in the development of a successful collaboration 'has a view'. In this collaboration, researchers' individual uncertainties created a much less easily identifiable zone of exchangeable views around the object (cf. Calvert, 2010). Rather than creating a 'local configuration' that would enable particular kinds of (temporary) epistemic transaction, we did not talk about, resolve, or actually even share our differences and our ambiguities (Galison, 1997: 830). Knudsen, for example, the participants' research contact, was beset throughout the project by ill-defined feelings that despite being interested in the internalization of culture at the neurological level, he was unsure, as an anthropologist, how to contribute to a study whose purpose and validity were entangled in methods about which he knew little. Moreover, and given that participants would be in a team-building environment, fostering connections with others, Knudsen considered some of the broader social consequences from an anthropological angle. What if, having partaken in a staged social interaction, participants fostered friendships with the in-character actors? Indeed, this happened during a team-bonding dinner, when a participant wanted to swap contact information. Such moments gave Knudsen pause to consider how he had at least partially subordinated his anthropological instincts to the exigencies of the experiment But Knudsen never fully resolved these

feelings, let alone talked about them, or rationalized or traded them through a local configuration. The effect of this was that Knudsen stayed attached to the experiment by dwelling in this space of ambivalence, even if he remained at times uncertain of what he was doing there, what his interest was, or what this integrative project was actually doing for him.

This also helps us to understand how the collaboration did not fall apart. In one sense, of course, winning the competition, and having the rare opportunity to follow-through with an experiment, produced its own propulsive dynamic, whatever participants' individual motivations. But in interpreting the persistence of the process, we also draw attention to the dominance of *ambivalence* over either enthusiasm or despondency. Rheinberger (2011) has shown that experiments often make space for rather more digression, novelty, and serendipity, than is sometimes imagined. It strikes us, similarly, that the persistence and coherence of our experiment was not necessarily troubled by the fact that the experimenters often remained suspended between different motivations and feelings. Thus we draw attention to the fact that collaboration sometimes proceeds precisely because individuals do *not* have a fixed idea of what they're doing there, or of what the own view is. There remains a persistent idea that transdisciplinary scholars should be masters of their own domain, that they must enter collaboration knowing who they are, what they have to offer, and what they want to achieve (Bruce et al., 2004; Lyall and Meagher, 2012). Our collaboration was rooted in precisely the opposite strategy. We kept things vague.

***Undercover critics***

There is not yet an account of collaboration in which researchers interact with another epistemic culture with some sense that they want to deflate at least one part of it. For several of the experimenters who were part of this project, collaborating with neuroscientists was a way to re-articulate an already-existing critique of fMRI lie detection from the heart of the field itself. There were, of course, already convincing and cogently-articulated reasons to be wary of fMRI lie-detection in particular, and of the biologization of deception in general (Bunn, 2012; Littlefield, 2011). What the

collaboration achieved, in the end, was a more 'subversive' re-articulation of a point that had more-or-less already been made.

This tacitly subversive goal of the research became explicit when one participant, having been told the true purpose of the experiment during the debriefing, misconstrued the experimenters' intent, called his research contact later that day, and asked that his data not be used; due to political and ethical concerns, he explained, he did not want to contribute to the creation of a neuroscientific lie detector.[7] We immediately agreed to remove the participant's data, but we asked him to come and speak to us in person again anyway. When he did, we were at great pains to re-emphasize that our research was precisely about making it *harder* to make such a lie detector. Relieved, and even enthused, the participant withdrew his request.

This places our project at odds with most of the literature on transdisciplinary neuroscience. The foundational insight of 'neuroanthropology', for example, is that 'our brain and nervous system are our most cultural organs' (Lende and Downey, 2012: 23). Calling for an approach that can simultaneously account for, 'public symbol, evolutionary endowment, social scaffolding, and private neurological achievements,' one of the core tenets of neuranthropology is that neural activity cannot be well understood without a detailed and nuanced understanding of the cultural environment in which a given brain was sculpted, while cultural knowledge is only half understood if we do not follow its effects, repercussions, and re-articulations through the central nervous system (Lende and Downey, 2012: 23-24). But the model of practice remains one of 'partner[ship]' and of 'shar[ing] across disciplinary lines' (Lende and Downey, 2012: 49, 27). In search of 'mutual engagement,' the authors seek a space in which 'by coming together, we can achieve significant advances in understanding' (Lende and Downey, 2012: 24, 25, 51; see also Chiao, 2009; Roepstorff et al., 2010). But this account, while helpful, occludes some less admirable and upfront motivations for collaboration too – including, in the case of some of the authors on this paper, a half-understood desire to (however naively and simplistically) undermine some intellectual practice from the inside.

We draw attention to the fact that, amid a broader insistence on the generative and 'positive' nature of interdisciplinary work, this was a collaboration whose fundamental goal was not only to make something new, or to solve a novel question. It was, at least in part, an attempt to undo a knowledge that already existed. For at least some of us, one of the core purposes of collaborating with neuroscientists was to render a more potent critique of (one area of) neuroscience. One thrust of our transdisciplinarity was *subtraction,* not *addition*. The point was to trouble a novel finding, not to create one.

***Deceptive experimenters***

A third surprising feature of our experiment was a consistent sense of reserve, and even deception. This was apparent on several levels. Most obviously, it affected many of our collaborative interactions, where we didn't speak very freely to one another – or criticize one another – to the point where it became a bit detrimental. The first time we ran the experiment, for example, we didn't recruit anything like enough participants for a high-quality publication (maybe any publication at all). And yet still we turned up and proceeded, without anyone having called a halt, or without anyone remonstrating with anyone else, or without the group really conceding what a setback this was. This sense of tacit evasion, of not confronting things, even in the face of significant problems, was quite characteristic of our interactions. We will have much more to say about the effect of such equivocations below. But there was a more telling sense in which ours was a deceptive collaboration, which was manifested in some of the experimenters' relationship to the practice of neuroscience itself. The fact is, although this was an fMRI experiment, we recruited the scanner only to the extent that it might betray its own limits; for some of us, the original design was set up to show something that fMRI could *not* do – i.e. that it would be induced to produce results that might bolster a case for arguing against the reliability of its measures and procedures more generally. There was a strong sense, then, in which this neuroscientific experiment only set the neuroimaging apparatus up to fail,

and in which some of us found a space to collaborate with neuroscience only by establishing this dishonest relationship with it.

And there was yet another sense in which this collaboration worked through moments of reservation and deception. We have already suggested that one of the most striking aspects of our senses of ambivalence and critique is that neither was ever really discussed among the group. And, of course, these sensations were not experienced by all authors, nor always to the same degree (not least for our collaborating neuroscientist, Dietz). But while there is not space, here, to more comprehensively illuminate what was specifically at stake for each collaborator, here we want to show how even the unwillingness to elucidate these differences corresponded to a broader sense of reticence or reserve among the experimenters, a feeling which, at its outer edges, drifted into a kind of muted dishonesty. For example, it is only in preparing this paper that some of the authors began to own up to their senses of ambivalence about the experience, a move that took others by surprise. 'I've never felt any ambivalence in my involvement in [this] fMRI study', Dietz pointed out when presented with these after the study had been concluded, and pressed for his own reflection: 'All the work that goes into formulating the experimental design which embodies the scientific question we were asking, programming the stimulus sequence, analysing fMRI data, and, finally, drawing statistical inference to answer the questions we posed – it all offered new ways to extend my skills and rehearse the various aspects of cognitive neuroimaging.' Clearly, the sense of ambivalence was felt more keenly by the social scientists and humanists than the neuroscientist here. We might locate this difference in the fact that the contemporary neurosciences are already made up of a host of (sometimes competing) disciplines and perspectives; there is a kind of multi-disciplinary cosmopolitanism inherent to the formation of the 'new brain sciences' that may make the presence of epistmeic *difference*, for the typical neuroscientist, a lot less jarring, and a lot easier to live with (Rees and Rose, 2004).

In any event, our point is not that there was a right or wrong way to feel about the study. Rather, our point is that as much as we kept what we were really doing from the participants, and from the laboratory machinery, we also, in some sense, kept it from one another.

Such experiences are hardly unique to our situation, and might even be trivial parts of ordinary research. But they nonetheless seem to run counter to what we are consistently told is good practice between disciplines. As Collins and his colleagues remind us, one of the core purposes of a collaborative 'trading zone' is precisely to resolve problems of communication. Indeed, Collins et al. (2008) provide a general taxonomy of communicative strategies within trading zones, from the formation of pidgins and creoles, to the enforced dominance of one mode, to the use of an interactional expertise as a linguistic communication device, and so on (p. 658-661). 'The idea of a trading zone as a place where problems of communication and co-ordination are resolved,' they point out, 'can help us understand a wide range of styles of social and scientific collaboration and the ways in which they may evolve into one another' (Collins et al., 2008: 665). What, then, can one say about a scientific collaboration in which resolution takes the form of such reserve? What kind of collaborative and communicative zone is it, exactly, where researchers assemble concepts they don't fully believe in, and where they pretend to each other that everything is fine? We argue that transdisciplinary zones may not *only* be defined by creoles, pidgins, and trades, but also by forms of reserve, reticence and deception. Sometimes, people just want to keep things to themselves.

**The politics of experimental collaboration**

It is not our goal to unmask the transdisciplinary experiment. But we *are* interested in expanding the conversation about what is actually at stake in the daily experience of collaborative labour. Resisting *both* bureaucratic proclamations of why interdisciplinary is a good thing *and* technocratic attention to how it should be performed, our attention to the feeling of participation in these spaces brings another perspective into focus. In particular, our account of ambivalence and *ressentiment* leads us away from a straightforward epistemic or practice-based account of our experience, and towards an interpretation of the *politics* of experimental practice.

Clearly, a macro-politics of knowledge structures interactions between the neural and psychological sciences, on the one hand, and the humanities and social sciences on the other. Scholars

from the humanities and social sciences are frequently exhorted to seek connection with 'science'[8] but often with little reference to the political economies that have made humanistic and interpretive interests increasingly unsustainable. '[T]he humanities are being driven into defensive positions', wrote the vice-provost of University College London recently. 'Despite isolated counter-actions, they experience marginalisation as martyrdom and tend to look inwards rather than outwards to new possibilities, such as recovering their status and influence through interdisciplinary working' (Worton, 2013). In the background of this advice, of course, is the fact that humanities scholars are well advised to seek collaboration with the sciences because of many states' radically different financial and rhetorical commitment to the two area(Browne et al., 2010; cf. Holmwood, 2011; McGettigan, 2013). This politics of knowledge was an implicit part of the design of our experiment, to the extent that, in effect, it recapitulated biologically a point that we already knew historically. This recapitulation was founded precisely on the recognition that a neurobiological claim is more rhetorically and politically potent than a historical one, even where the fundamental argument remains the same. It seems inescapable that much collaboration is similarly underwritten by cross-disciplinary differences in institutional power and epistemic prestige.

Equally, in the ENSN's NeuroSchool, despite clear desires for mutuality, the 'neural' was often unconsciously positioned as the thing to be understood, and the 'social' a mildly querulous constraint upon it. Social scientists were introduced to magnetic resonance and the operation of the scanner; but there was no assumption built into the collaboration that social neuroscientists should learn something about science fiction, the machinations of power/knowledge, or anything similar. Littlefield, leader and originator of the study, was often asked about the history of lie dection's experimental designs, and how these might be reimagined in a novel fMRI paradigm. But the flow of the experimental situation left little space for the numerous socio-cultural critiques that she leveled against the pursuit of lie detection through mechanical intervention. When the group collaborated around the pragmatics of the experiment, knowledges and tools got aligned in very specific ways. The white board, for example, was filled with '2x2' factorial designs, 'x and y' axes, and 'vectors' of various

kinds; it never contained sociological or humanistic theories. There are, of course, straightforward organizational lessons to be drawn here (the humanists may well have simply disrupted such alignments). But we still draw attention to the fact that, even in avowedly transdisicplinary collaborations like ours, some knowledges have to interject, and to insist on their own usefulness; others have the privilege of taking their universal utility for granted.

Such dynamics of exchange are clearly governed by a larger-scale epistemological politics, which renders methodological and conceptual differences between the social, natural and humanistic sciences as a hierarchy of intellectual prestige. Although this politics is well described elsewhere (Benneworth and Jongbloed, 2010; Philpott et al., 2011), it is strangely absent from the more formal and instrumental descriptions of – and sometimes exhortations to – cross-disciplinary collaboration considered above (Collins and Evans, 2002; Huutoniemi et al., 2010). By contrast, the re-consideration of this kind of scholarship at the level of mundane experience, through registers of feeling, and in the everyday pragmatics of transdisciplinary labour, brings these contests, and the politics that governs them, inescapably to the fore.

**Another politics of experiment is possible** [9]

But there is more to be said about the relationship between experiment and politics here. Experiment is exhausted neither by the 'nitty-gritty' of laboratory life nor by its recognition as a 'very peculiar human practice'; we can also think of experiments as *aesthetic* practices, as 'trying out … novel forms of intervention' (Roepstorff and Frith, 2012: 105). We draw on this analysis of the experiment as a novel aesthetic gesture, in order to reposition the politics of experimental knowledge as it emerged within the ambivalent space of our collaboration. If the politics of our experiment derives from hidden machinations of disciplinary power and prestige in transdisciplinary collaboration, we are also in pursuit of some more generative 'experimental politics', understood as an ethic and a method that allows us to resist a straightforward account of disciplinary victimhood, and to re-state why we think it worth entering these spaces in the first place.

This experiment was not only an attempt at collaboration that proved (sometimes) surprisingly difficult. It was also a commitment to build on the work of other scholars already attempting to muddle through the sometimes troubling and awkward, but nonetheless ultimately productive, work of drawing an anthropological, sociological, literary-historical and neurobiological interest through one another (Roepstorff et al., 2010; Rose and Abi-Rached, 2013; Singh, 2012; Wilson, 2004). On the other side of the difficult relations of power and prestige that structure these kinds of collaborations, this experiment came from a gamble that there might nonetheless be an experimental space worth claiming, one in which novel political and epistemological frontiers would, at least, come into view. By insisting on such an 'experimental politics' in our collaboration, we try to grasp some of the ways in which this project was also an attempt to think *with* the experimental, in both its laboratory and aesthetic senses, as a mode of knowledge, one with particular kinds of constraints, effects and possibilities (Fitzgerald and Callard, under review). Drawing on feminist STS and allied areas, we root such a politics in a methodological refusal of sharp distinction between the objects that are given to, or the questions that can be asked by, scientific and non-scientific research-practices (Haraway, 1997; Latour, 1999). Precisely through such a refusal, we have pursued a transdisciplinary mode of intervention in which the neurobiological legibility of 'truth', for example, is not simply affirmed scientifically or critiqued sociologically; instead this legibility is expanded and complicated through more risky and generous imaginaries of cross-disciplinary connection, a method that works to figure, in this case, the traffic between lies, bodies, feelings, theories, situations, and laboratories.

Such an experimental politics relies on a sympathetic view of what neuroimaging either is or might be. In her *Psychosomatic*, Elizabeth Wilson (2008) reminds us that 'neurological material is more confident, flexible, resilient, and assertive than many critics have yet acknowledged' (p. 22). Such a material may even prove itself a 'resource for theoretical endeavour, rather than the dangerous and inert substance against which criticism launches itself' (Wilson, 2008: 29). By attending, here, to the flows of ambiguity, reserve, and even critique in our collaboration, and to their indivisibility from analyses of the experiment as such, we have similarly tried to show how a neuroimaging experiment

can be a more pliable, resourceful and self-aware mode of knowledge-production than many of its critics have yet realized (Choudhury et al., 2009; Martin, 2014). Several of us entered this experiment committed to a view that, beyond well-worn critiques of 'neuro-reductionism' and 'neuro-colonialism' (Cromby et al., 2011; Martin, 2004) the neuroimaging experiment may harbour an untapped potential, one that might even claim neuroimaging laboratory as a site for forging novel alliances between biological, sociological and humanistic knowledges. We now interpret our awkward, and yet also somehow 'successful' experience, as sign of how we have learned to live with such alliances – even where they are difficult, or unhappy – to assist more marginal modes of knowledge as they seek to become both say-able and witness-able.

This pursuit of novelty, the forging of alliance, and the commitment to aesthetically experimental technique, is the frame, ultimately, through which we have come to understand the logics of ambivalence and reserve that flowed through our collaboration. Situating the linkage between neurological, humanistic and neurological knowledge as neither inevitable nor impossible, this experiment sought a space in which such connection might be actively configured. Our focus on feelings of discomfort and dishonesty illuminates the way in which such an experimental politics may require a rethinking of the logic of interdisciplinary collaboration, and the modes of sometimes temporary and uneasy circulation through which it gets practiced. We attempted to mobilize, in the experimental mode, something out of different bits of history, sociology, anthropology and neuroscience. And we are reminded that if such a composition is a difficult and tenuous achievement, it is an achievement all the same (Latour, 2010). Transdisciplinary awkwardness is neither simply a subterranean logic of collaboration (although it is partly that), nor is it only a reason for despair (although it is sometimes that too). But the reservations and ambiguities of our collaboration help us to re-imagine what an experimental politics makes possible in a collaborative mode. We believe there is scope for other inhabitants, from STS and elsewhere, to draw on, and to share, their own experiences of both ambiguity and possibility in collaborative space.

**Conclusion: Practising equivocal speech**

Writing this paper brought our sense of ambivalence and reserve into the open. But even here we have not placed everything on the table. This paper itself, as an open process, was produced through a hesitant, electronically-mediated, iterative procedure in which one author, voicing their own reflections and feelings, asks others to comment, respond, add, or delete and so on. Such a process, which is not unique to us, is also governed by an ethic of reticent *politesse*. So we do not claim to have resolved the macro- and micro-politics of knowledge animated by that reticence. In this sense, the solidity of the connection between these reflections, and the deep-rooted thoughts, affects, and memories of the authors as such, must remain an open question. Conclusion, here, is not always closure.

There are also other ways to account for our experience, not least to wonder whether our ambivalence is not simply a result of poor organization, and whether this paper should be interpreted as a call for more attention to method in the composition of projects like this one. We tackle some of these more organizational issues in an accompanying publication (Littlefield et al., 2014). But our experiment was also *successful*. We made a transdisciplinary hypothesis that drew on a literary-historical insight in order to both trouble and expand a neurobiological literature; the experiment that we designed produced a positive publishable finding, in line with our hypothesis; we wrote that finding up together and we submitted it to a journal. So we do not want to lament the feelings that accompanied our progress; nor are we minded to recommend ways to avoid our fate. Quite the opposite: our interest in dwelling on these feelings has to do with the fact that, unlike many others, our transdisciplinary experiment more or less worked out. It is in this sense that we have suggested that good collaboration might be less a question of fair and transparent commerce, and much more a practice of learning to live with feelings of ambivalence and reserve.

Let us return to Rabinow and Bennett. They should have known, they conclude, that 'the price to be paid for the power and instrumental mastery of modern science was the abandonment of hermeneutical meaning, general cultural significance, enhanced moral practice, and political or ethical

spirituality. We underestimated the existential price to be paid.' (2012: 173). The scales and stakes are rather different between our collaborative endeavors and those of Rabinow and Bennett. In both cases, however, social scientists and natural scientists were trying to do an actual, real, non-figurative 'experiment'; collaborators found that one of their most significant results was a disappointing preponderance of disciplinary hierarchy. Rabinow and Bennett suggest a very particular kind of response; in the final pages of their monograph, they argue that their distinctively collaborative and critical orientation produces insights that need to be 'put into play in a serious and consequential manner', lest 'their salutary effects on the practice of thinking ... be deflated or distorted' (ibid.: 178). The ethnographers urge other would-be collaborators from the humans sciences 'to speak the truth frankly ... practicing frank speech in consequential situations actually makes one more capable of seeking the truth' (ibid.: 179).

We want to conclude by suggesting a different kind of response. If our collaboration was not a great deal more comfortable than that of Rabinow and Bennett, it was still 'successful' – at least to the extent that it ended with a more-or-less publishable shared result, within the loose remit of the ambitiously transdisciplinary framework it had set up for itself. Our proposal is that this success might have come precisely because we did *not* speak frankly; we did *not* seek the truth; we totally *failed* to acknowledge – let alone discuss – the consequences of our experimental situation. What we did, instead, was to try to work and live within a zone that was just about ambiguous enough to keep everything together – that was sufficiently averse to frank-speaking to keep the worst of the resentments at bay. We suggest that collaborators attend instead to an ethic of 'equivocal speech' – a mode that is attentive to the things that are better left unsaid, to the feelings that are as well off not articulated, and to the senses of awkwardness and ignorance that probably won't help anything if openly unacknowledged. Against Rabinow and Bennett's solution, and its imagination of a collaborative subjectivity founded on openness and speech, and precisely *against* the clarity and transparency demanded by frankness, our solution is rooted in the more nuanced equivocations of feeling. What we have tried to describe, here, is the capacity for, collaborating scholars, especially

from the social sciences and humanities, to feel ambiguity, and resentment, and subversion; but also, in particular, their willingness to go on feeling them; and their experimental desire to keep collaborating through them; and thus the commitment to, if not the enjoyment of, living with them anyway.

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**Author Biographies**

Des Fitzgerald is a Postdoctoral Researcher in the Department of Social Science health and Medicine at King's College, London

Melissa M. Littlefield is an Associate Professor of English and Kinesiology at the University of Illinois, Urbana-Champaign.

Kasper J. Knudsen is a PhD student in Anthropology at Aarhus University

James Tonks is a practising Clinical Psychologist with the Dane Hannah Rogers Trust and an Invited Lecturer at the Department of Psychology, University of Exeter.

Martin J. Dietz is a PhD student at the Centre for Functionally Integrative Neuroscience at Aarhus University

**Notes:**

[1] We are grateful to Felicity Callard for suggesting this term.

[2] For some archival material on the ENSN, whose programme ended in 2012, see: http://www.kcl.ac.uk/sspp/departments/sshm/research/ENSN/European-Neuroscience-and-Society-Network.aspx

[3] Present at the NeuroSchool were Fitzgerald, Littlefield and Tonks. A fourth member, Robin Pierce, also participated in the work in Vienna, and at a design meeting in Aarhus, but subsequently left the project. Martin Dietz and Andreas Revsbech joined from Aarhus as local experts. Revsbech also subsequently left the project, and was replaced by Knudsen. The experiment was actually conducted twice, due to data errors in the first run. The reflections contained here are based on the conduct of the two 'runs' of the experiment, and subsequent analysis, when the five authors (Fitzgerald, Littlefield, Knudsen, Tonks, Dietz) made up the core team.

[4] In common with much of the literature, and albeit confusingly, we use 'interdisciplinarity' as an umbrella term for all of these formations.

[5] The experiment, which underwent ethical review both at Aarhus University and the University of Illinois, relied on the authors withholding the ultimate purpose of the experiment from participants until after fMRI data had been collected. All participants were, however, fully and carefully debriefed at the conclusion of their scanning session.

[6] This, too, was subterfuge – of course we would never publicly reveal results. But we wanted to make the situation cognitively taxing and a bit socially pressured. Participants were all given an individual restaurant voucher in lieu of this third meet-up that never happened.

[7] See note 5 on de-briefing. In fact, this particular participant was debriefed in the earlier run of the experiment; for the subsequent run, and based on this experience of having been once misunderstood, we provided even more clear and detailed explanations of the experiment's purpose to each of the participants during the debriefing stage.

[8] See, for example, the intense desire for 'reciprocal relationships' in a current high-profile funding call form the UK's Arts and Humanities Research Council, 'Science in Culture' (AHRC, 2013) http://www.ahrc.ac.uk/Funding-Opportunities/Research-funding/Themes/Science-in-Culture/Pages/Science-in-Culture.aspx

[9] With apologies to Fassin (2009).