Enacting the 'neuro' in practice: Translational research, adhesion and the promise of porosity

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Abstract

This article attends to the processes through which neuroscience and the neuro are enacted in a specific context: a translational neuroscience research group that was the setting of an ethnographic study. The article therefore provides a close-up perspective on the intersection of neuroscience and translational research. In the scientific setting we studied, the neuro was multiple and irreducible to any particular entity or set of practices across a laboratory and clinical divide. Despite this multiplicity, the group's work was held together through the 'promise of porosity' – that one day there would be translation of lab findings into clinically effective intervention. This promise was embodied in the figure of the Group Leader whose expertise spanned clinical and basic neurosciences. This is theorized in terms of a contrast between cohesion and adhesion in interdisciplinary groupings. We end by speculating on the role of 'vivification' – in our case mediated by the Group Leader – in rendering 'alive' the expectations of interdisciplinary collaboration.

Keywords

expectations, neuro, neuroscience, research groups, translation

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Introduction

The promise of neuroscience to reveal the inner workings of the human mind and brain has become a source of fascination for a range of publics, policymakers and researchers in recent years. The designation by the United States Congress of the 1990s as the ‘decade of the brain’ is often identified as capturing the zeitgeist of the recent history of neuroscience (Conrad and De Vries, 2011; Cromby et al., 2011; Rose and Abi-Rached, 2013; Williams, 2010). That designation resulted in the channelling of funding into neuroscience research and public understanding of science programmes in the United States, with knock-on effects around the world. The years since have seen neuroscience flourish, both in terms of research infrastructure and as a cultural trope, its momentum showing no sign of abating (Johnson and Littlefield, 2011; Ortega and Vidal, 2011; Pickersgill and Van Keulen, 2011; Rose and Abi-Rached, 2013; Vidal, 2009; Vrecko, 2010). The sociologist Simon Williams (2010) argues that

If the 1990s indeed were dubbed the decade of the brain, then the twenty-first century looks set to become, if not the century of the brain, then an era or age in which the neuro looms large in both scientific and popular culture. (p. 547)

At the same time as such predictions about the neuro proliferate, so too do scepticism towards what is sometimes regarded as ‘neurophilia’ (Trout, 2008) and calls for critical analysis of the very notion of ‘the neuro’.

This article attends to the processes through which neuroscience and the neuro are enacted in a specific context: a translational neuroscience research group which was the setting of an ethnographic study. The article therefore provides a close-up perspective on the intersection of neuroscience and translational research, a confluence that has received little analysis to date. Crucial here is ‘the promise of porosity’ – that is, the expectation that one day the disparate research practices and perspectives of laboratory-based and clinical researchers will be subject to translation, and the work of the lab will translate into clinically efficacious interventions. However, the promise of translational neuroscience rested most centrally on the Group Leader (GL) himself, a clinician-scientist without whom the group would have had no unifying principle. We suggest, then, that while the neuro was a resource for this group – a critical element in an imagined future of translation between lab and clinic – its significance cannot be understood outside of the group’s epistemic and organizational practices and the specific people involved.

The rise of the neuro

What has prompted Williams and others (Hacking, 2004; Littlefield and Johnson, 2012) to point to the neuro as this century’s cultural and scientific motif? First, there are recent well-publicized neuroscientific findings that seem to bear on longstanding philosophical questions about personhood, consciousness and empathy. Much of this research relies on advances in imaging technologies such as functional magnetic resonance imaging (fMRI) (Williams, 2010). Moreover, novel neurotechnologies suggest more immediate clinical application (Pickersgill and Van Keulen, 2011), and psychopharmaceuticals are finding a

rapidly expanding market (Williams, 2010). Beyond this, some argue that such developments have significant implications for our understandings of the 'human' (Giordano, 2013; Levy, 2008; Roskies, 2009).

Second, social scientists have pointed to the emergence of 'neurocultures' (Ortega and Vidal, 2011). Neuroscientific research is used popularly to explain gender differences, criminality and addiction or to enhance cognitive capacities (Littlefield and Johnson, 2012; Williams, 2010). One upshot, it is argued, is that we enact ourselves as, for example, 'neurochemical selves' (Rose, 2003; see also Ehrenberg, 2004; Vidal, 2009). Along with these scientific and cultural developments, there is a so-called neuro turn in the humanities and social sciences that seeks to integrate insights from neuroscience (Jack, 2010; Littlefield and Johnson, 2012; Rose and Abi-Rached, 2013; Vidal and Ortega, 2011). Examples include neuroethics, neuroeconomics, neurohistory, neurolaw, neuropsychanalysis, neuromarketing, neuroanthropology and neurosociology.

While asserting that we are now in a time of 'neuro-reality', neuroethicist Illes (2009) acknowledges that others have been more cautious about accepting the priority of the neuro. Thus, 'neuroskepticism' is one emergent discourse that questions the claims being made about the significance of neuroscience and its applications, as well as its popular representation (Klein, 2011; Marks, 2010; Rachul and Zarzeczny, 2012). There is also a distinct body of work, fitting broadly within social studies of science, that analyses the cultural authority of neuroscience as another iteration of biological reductionism (Dumit, 2004; Martin, 2004; Vidal and Ortega, 2011), links its rise to the cycle of expectations and hype now common in science (Brosnan, 2011; Conrad and De Vries, 2011; Pickersgill, 2011; Rose and Abi-Rached, 2013; Vidal, 2009; Vidal and Ortega, 2011), and queries whether the brain is really as culturally significant as neurophiles imply (Pickersgill, 2013; Pickersgill et al., 2011). Other commentators have promoted a 'Critical Neuroscience', calling for a reflexive 'neuro-reality check' and the introduction of greater critical input from the social sciences and humanities into neuroscience (Choudhury and Slaby, 2011; Choudhury and Stadler, 2011; Choudhury et al., 2009).

Of particular interest here are questions about the significance of the neuro as a category. Klein (2011; also Littlefield and Johnson, 2012) argues that there is little conceptual coherence in the neuro that is prefixed to the various neurodisciplines (the same applies to 'nano'; see Baird et al., 2004; Hayles, 2004). Indeed, Littlefield and Johnson (2012: 4) suggest that the use of the neuro prefix may serve to blackbox the complexities of neuroscientific work. It may also confer scientific legitimacy (Vidal, 2008: 22). In other words, such black-boxed renderings can be seen as performative, each serving to construct a particular 'definitive' version of the neuro that affords particular types of futures, expectations and actions (e.g. the establishment of a new field, the attraction of funding and regulatory support, etc.). The performativity of a reductionist neuro can also apply to critiques of the neuro.

We, too, are concerned with the variety of the enactments of the neuro, in particular with variation between laboratory and clinical enactments. Moreover, we are interested in that variety as it is manifested in an empirical setting where one might expect shared accounts about what the 'neuro' is, and what it applies to (let alone, whether it applies at all). However, before discussing our empirical analysis, we need first to situate our

empirical setting – a research unit marked by the expectation of translating lab findings into clinical interventions – in relation to the literature on translation and expectation.

Expectations and translational research

Translational research, also often known as ‘bench to bedside’ research, aims to develop and/or draw on ‘basic research’ in order to derive innovations that are of therapeutic (but also diagnostic and preventive) benefit. As Watts (2010) puts it, translational research has ‘shot up the (UK medical research) agenda’ (n.p.) being promoted by the ‘Medical Research Council, the Academy of Medical Science, and the Office for Strategic Co-ordination of Health Research’ (n.p.). It has also featured heavily in the European Commission’s 7th Framework Programme and is central to current National Institutes of Health (NIH) funding initiatives.

Translational research can be situated in the context of what has been called the ‘sociology of expectation’. This is because translational research is chronically uncertain: it is necessarily oriented towards a future prospect of some sort of clinically effective outcome. The sociology of expectations is concerned with the ways in which such expectations are enabled, structured and circulated. Crucial here is also a focus on the uses to which such expectations are put. Thus, the performativity of expectations can be explored in terms of the ways in which actors deploy some sort of representation of a future state of affairs in order to mobilize other actors, resources, regulations, and so on, which then, ideally, serve in the realization of that future state of affairs (e.g. Michael, 2000). Inevitably, the sociology of expectations encompasses a range of approaches, with some authors interested in a more ‘structural’ approach that addresses how expectations are embodied in socio-technical networks (regulatory frameworks, technologies, scientific fields), while others take a more microsociological view that focuses on, for instance, the rhetorical structure of expectations (see Borup et al., 2006; Brown and Michael, 2003; Hedgcock and Martin, 2003).

Of interest here is how the expectations attached to neuro research with translational aspirations (e.g. converting laboratory-based stem cell research into clinical interventions such as transplanting dopamine cells into patients with Parkinson’s Disease (PD)) vary among lab scientists and clinical practitioners, and how this variation – divergence, even – is managed in the context of an interdisciplinary research group. On this latter score, we can note that it has been pointed out that there is a tension between the different cultures and goals of medicine and science (e.g. Cribb et al., 2008; Wainwright et al., 2006; Wilson-Kovacs and Hauskeller, 2012). In their analysis of human embryonic stem cell research, for example, Michael et al. (2005) suggested that scientists and clinicians were regarded as so different that translational research was, at the very least, fraught. The viability of translation rested on a ‘hybrid professional’ (p. 386) who straddled the laboratory/clinic divide.

Indeed, clinician-scientists, with expertise in both basic scientific research and clinical medicine, have become seen as the key facilitators of bench-bedside translation (Wilson-Kovacs and Hauskeller, 2012). This role is then imbued with expectations inseparable from the translational paradigm. Wilson-Kovacs and Hauskeller (2012), examining the pressures felt by clinician-scientists who must meet demands in both

medical and academic scientific domains, argue that randomized control trials, which are central to the translational process, are a key tool by which clinician-scientists can exert authority and consolidate their professional status. Our study sheds further light on the dynamics of translational research by exploring how translational expectations are configured within a single research group and how they intersect with another performative category, the neuro.

Studying the neuro in practice

The empirical study drawn on here set out to explore how 'ethics' intersects with day-to-day practice in neuroscience and what it means to neuroscientists (see Brosnan et al., 2013). The study was one of several projects in a larger programme of research that explored how the social sciences can contribute to studying bioethics empirically, focusing in part on translational bioscience settings (the Wellcome Trust-funded London and Brighton Translational Ethics Centre). This particular project employed ethnographic methods to examine how ethics was manifested in the daily experiences of scientists in a UK university-based neuroscience research group whose work focused primarily on degenerative diseases such as PD. The research site was selected because the group's work was limited neither to the laboratory nor to clinical neurosciences, but encompassed both under the same roof. On one floor were the open plan labs where 'wet' science involving tissue and cells was carried out on benches and under hoods and microscopes. On another was a carpeted waiting room and a suite of offices and consultation rooms where patients came one by one for assessment (free under the UK's National Health Service) and to participate in a range of university studies. A major goal of the group's work was to translate techniques from the laboratory into clinical treatments, and at the time of fieldwork, the group's preparation for a clinical trial involving a cell-based treatment for Parkinson's epitomized this approach.

Our findings, then, are based on a study of a specific group at a particular point in time, with the group members themselves noting that dynamics might change as the trial progresses. The study therefore provides a snapshot of a particular iteration of neuroscience. Rather than aiming to generalize to other neuroscientific contexts, our goal here is to carefully explore the processes through which the neuro was enacted, in order to develop a conceptual framework that can be tested in other settings.

Between September 2010 and April 2011, author C.B. carried out observation in the group and interviewed 13 out of 18 group members, including the Group Leader (GL1), a clinician-scientist whose dual training in neurology and neurobiology enabled him to direct research in both the lab and the clinic. A 14th interview with a Group Leader (GL2) from an associated group was conducted in order to gain another senior neuroscientist's viewpoint. The remaining interviewees – PhD students, postdoctoral researchers, lab technicians, research assistants and trial managers – worked almost exclusively in either the lab or the clinic and are referred to here as 'lab researcher' (LR) or 'clinic researcher' (CR).

The focus on practice was designed to move on from previous work which to date has examined the ethics of neuroscience through philosophical reflection (e.g. Farisco, 2011; Räikkä, 2010), questionnaires (e.g. Illes et al., 2010; Robillard et al., 2011) or focus groups

(Pickersgill, 2012). Although interviews were central to this study, they were centred on discussion of the scientists' everyday practice and drew crucially on observation of that practice. In examining how the neuro was enacted in research practice, we uncovered a multiplicity that has not typically featured in the accounts of the neuro described above.

Useful here is a comparison with Annemarie Mol's (2002) central premise in *The Body Multiple*, her ethnography of atherosclerosis in a Dutch hospital, where she asserts that by studying the material practices from which disease categories emerge, rather than their construction in medical/patient discourse, the multiplicity of objects such as diseases can be revealed. Mol's work did not directly inform data collection but we take it as a useful framework with which to interpret our findings.

Observing the different ways that the disease manifests itself in the different areas of the hospital (the clinic, the pathology lab, the haematology lab, the waiting room, etc.), Mol concludes that atherosclerosis is not a stable entity that can be separated from the context of its observation. What atherosclerosis is when it is studied under a microscope in a pathology lab is not what it is when a doctor in the clinic is diagnosing 'atherosclerosis' by touching the feet of a living patient to test for temperature. In fact, the two enactments exclude each other, for one concerns a communicative patient while the other requires an amputated leg (Mol, 2002: p. 35). In each case, atherosclerosis is 'being done' differently, dependent on other objects in the setting to render a particular enactment possible (Mol, 2002: pp. 31–32).

Despite the multiplicity of practices through which a disease is enacted in the hospital, they are also made to cohere: vascular surgeons can talk to pathologists about atherosclerosis, for instance. However, this is not always possible or necessary:

The various atheroscleroses enacted in hospital Z are sometimes coordinated and jointly form a single disease that somehow hangs together. But not always. Sometimes the incoherences between different ways of enacting atherosclerosis aren't smoothed away. (Mol, 2002: 87)

Different enactments of atherosclerosis may be 'distributed', rather than coordinated, for example in the 'two worlds' of the haematology lab and the department of vascular surgery (p. 111). As well as the former concerning blood and the latter the vessels of living patients, the two were separated architecturally within the hospital building and also socially into biologists and surgeons (pp. 108–114).

One difference between our study and Mol's is how we have identified our object of study, the discipline of neuroscience (as opposed to the disease atherosclerosis). While Mol entered a hospital and then set about following the disease through the various disciplinary areas of the building, we, in wanting to find out about everyday ethics in neuroscience research, found that our choice of setting was guided by a university administrative structure that had already designated the research group as being part of 'neuroscience'. Our exploration of the neuro therefore takes place within this predetermined category and the circumscribed space of the group. We look to see what meaning the neuro has here, what practices are enacted under the neuroscience rubric and what it is that binds the group together, rather than following the neuro elsewhere.

In the following sections, we draw first on Mol to show how the various practices in the research group under study revealed the neuro of neuroscience, and the brain itself,

to be irreducible to a single entity, instead being enacted in different and sometimes incommensurable ways across and within the spaces of the lab and clinic. We then go on to explain how this 'incommensurability' was managed and the group's work made to cohere – or rather, as we argue, *adhere* – not through reference to an overarching neuro-discipline, but via the GL, both through his vision of the group's future and through the way he himself was positioned by group members as embodying several neuro-domains simultaneously. We interpret this form of *adhesion* as an instance of interdisciplinarity – one structured by a logic of accountability (Barry et al., 2008) within and between the lab and clinic and to the GL himself – but one in which the lab–clinic boundaries remained unbreached, translation across the two spaces still being an aspiration.

The multiple realities of the neuroscience lab and clinic

By attending to daily practice in this neuroscience setting, the stability of the diseases under study, and the brain itself, was troubled. What was striking upon entering the research site as an observer was the vast disparity between the different practices being undertaken across the group. Although all the scientists were working on 'the brain' or on 'Parkinson's disease' or in 'neuroscience', they were actually doing a wide array of very different activities on a day-to-day basis. LR2, for example, was working on a project to test the effect of a protein on the proliferation of neural stem cells taken from fetal rat brains. Upstairs at the bench, dressed in a white lab coat, he spent many an hour painstakingly dissecting the tiny rat fetuses to extract the necessary part of the developing brain, needing to work rapidly before the cells died. These minuscule sections of tissue were then broken down with enzymes in a solution and the experimental protein added to some samples. Days later, LR2 would spend many more hours separating the cells out again by pipetting them into different media in different containers, over and over again with different samples. Eventually, tiny drops of the final solution were placed on a slide and a dye added to stain the particular cells of interest. The slides were then placed in a machine that scanned them and counted the concentration of different cell types, which generated a digital image. The image and the quantitative data were the final precious result. Lynch (1988) noted the same kind of transformations in his ethnography of a neuroscience laboratory:

For neuroscientists, it is not enough to show what brains look like. Instead, brain materials are organized into graphic demonstrations. The squishy stuff of the brain becomes a subject of graphic comparison, sequential analysis, numerical measure, and statistical summary. (p. 273)

Downstairs in the clinic part of the group, however, the 'squishy stuff' was nowhere to be seen. Data collection in this part of the group took quite different forms. A major part of CR1's job was to assess cognitive function in patients taking part in long-term studies of PD. Meeting each patient in the waiting room, she would bring them through to one of the consultation rooms, furnished with a bed, desk and several chairs. Sitting together at the desk, CR1 would run the patient through a number of standardised cognitive tests. Patients would be asked to recall sets of words and an address read out by the researcher, to identify various objects depicted on the test paper, to write a sentence, to

fold a piece of paper and place it on the floor. CR1 recorded the results in pen on her test sheet, nodding and encouraging the patient all the while. Then the patient would be asked to perform various computer-based tasks, testing motor skills as well as cognition. All these data would also be quantified and later might be compared to genetic data or brain imaging taken from the same patient.

It is difficult to pinpoint just what is being enacted in the work of CR1 and LR2. If CR1's work was enacting the disease of PD, it was a particular version of PD which relied on the combined actions of the patient, the researcher, the test paper and the computer to come into being. This PD was not found upstairs in the lab, where, as in Mol's pathology lab, PD could only be located in tissue extracted from brains that were no longer part of a living being. In fact, although a long-term potential application of LR2's research was the optimization of cell-based therapies for diseases such as PD, he was not working within a disease model at the time, so no disease was actually 'being done'. It might be said that in each domain 'the brain' was enacted, but the brain too takes disparate forms – it is not a stable object, but rather a combination of different practices and entities. How, then, did these neuroscientists make sense of this multiplicity? As we will show, the objects enacted by researchers in the clinic and lab areas were more distributed than coordinated, and this was reflected and reinforced by the distribution of the researchers themselves.

In many ways, the lab and the clinic researchers functioned as two separate groups. They were spatially separated and only really saw each other at the weekly group meeting. CR1 explained,

[At the group meeting] every week someone presents their own work, but you tend to find that lab people will comment on lab techniques, because the clinic people won't know any of the lab techniques, obviously they can't really help that much. And then when clinic people present, it's other clinic people who comment on it. I think in terms of Group Leader, he's got a, obviously a great sense of both, and he links the two in his own mind. But I wouldn't say there's a great link really. I mean you don't even get to know people's names upstairs [in the lab].

This comment, echoed by many other group members, and observed by CB, suggests that the very different practices employed in the lab and clinic were a barrier to their coordination. Group members did not recognize the objects being enacted in the other part of the group and did not readily perceive a coherent category or entity that held their work together.

According to Mol (2002), incompatibility in terms of the different objects being enacted is not necessarily problematic, 'for the simple reason that there is not always a necessity to search for common ground' (p. 87). This was echoed in this LR's comments that suggested that up to this point, the 'two groups' had been able to function perfectly well on their own, working within their own epistemological frameworks, or better still, their epistemic cultures (Knorr-Cetina, 1999):

LR2: I don't think there are many actual research links between the two groups at the moment. I think the [clinical trial] thing is making it, you know, it's making a link between them. But beyond that, I think it's quite separated.

CB: And why do you think it's separated?

LR2: I just think the questions they're asking really, are quite different. It's difficult to link them together. And the fact that we're not actually performing at the moment, any kind of transplantation itself, means that the patients that come into the lab for testing, they're being tested for, you know, neuropsychological things, which aren't necessarily related to the lab-based stuff. So I think the patients and the lab work, there's no real reason to connect them at the moment.

In this neuroscience group then, rather than a shared neuro-reality (see Illes, 2009) defining their work, multiple versions of reality coexisted.

However, let us consider in a little more detail the parallel we have drawn between Mol's case study and our own. The coherence or coordination that Mol documents takes place within the routines of a hospital where temporally proximate diagnosis and treatment are paramount. The point is to intervene, and the sooner, the better. By contrast, in our case study, as we have seen, this is not yet possible – or at least, not in a way that can be informed by the findings that emerge from the laboratory. However, there is an explicit expectation that this will happen at some stage in the future. This is indicated when LR2 initially states, 'I don't think there are many actual research links between the two groups *at the moment*' and reinforced when a little later he says 'So I think the patients and the lab work, there's no real reason to connect them *at the moment*'. The point we are making here, and that we will elaborate below, is that coherence and coordination (and for that matter non-coherence and non-coordination) are inextricably intertwined with enactments of temporality. Here, by introducing temporality, we aim to complement Mol's primary focus on spatiality – in the form of architectural separation of surgeons and biologists in her case study, and in the form of the different floors on which wet labs and consultation offices are located in ours.

We can further contribute to Mol's analytic by expanding on the participants' 'object' of study (e.g. arterioscleroses, PD). Specifically, and unlike Mol's case study, we have seen already how the 'neurosciences' is a category that circulates beyond the clinic and the laboratory, or the sciences, and through social scientific and broader cultural media. 'Neuro' might thus be posited as a category that can serve as an additional resource in the enactment of coordination or coherence across laboratory and clinic.

However, on first pass, the concepts of neuroscience and the neuro were problematized by the group members. Some explicitly denied a shared neuro that gave their own practices special neuro-ethical status, for instance. Both LR1 and LR2 emphasized that they were simply working on cells, indicating this complexity:

LR1: To me, what we do is cell work, and cells are cells and [ethically] it doesn't matter whether they're neurons or liver cells.

LR2: I mean they could essentially be any cells from animals or humans. ... the [ethical] questions that I have, or the issues that I have to feel comfortable with, is just the use of animal tissue. If I was using, you know, skin cells or muscle cells, I think I'd feel entirely the same as I do now. I don't think the fact that it's neurons makes any difference really.

LR5 was reluctant to recognize neuroscience as a discipline, reflecting the construction of the category back on to the interviewer:

- CB: Do you think neuroscience as a whole, raises any particular ethical issues?
LR5: Neuroscience? What do you mean by neuroscience?
CB: Well I mean, as opposed to other areas of bioscience.
LR5: What do you mean? What do you mean? Neurobiology, or, neurology? I mean, that's two different concepts to me. I mean, neurology is to deal with neurological conditions in patients, but neurobiology is to deal with neurological symptoms in experimental models in cells or in animals or whatever. So I mean, what do you mean by neuroscience?

CR1 also resisted the idea of a coherent neuroscience or a shared disciplinary gaze (cf. Abi-Rached and Rose, 2010):

- CB: Do you see yourself as working in neuroscience? How would you describe your work?
CR1: I guess I am working in neuroscience. But, for me, the most important part of it is meeting the patients. ... so although it is neuroscience, I wouldn't really be cell based, more kind of psychology based, neuropsychology.

Even when the various types of neuroscience are assembled in an apparently unified way, such as in the *Journal of Neuroscience*, this coherence is not necessarily shared by those doing everyday neuroscientific work. Talking generally about working in experimental science, GL2 commented,

And there's no time to read. Like you open the *Journal of Neuroscience*, you have cellular neuroscience, disease neurobiology, behaviour, cognitive systems biology, systems neuroscience, and you see how you skim through, and you say, 'Oh well in this section, cognitive neuroscience, I'm not going to bother to read any of this because, I don't know, it's not in my frame of thinking'.

LR5 in fact saw neuroscience as a category constructed by or for non-experts. This excerpt followed on from his previous comment that contrasted neurobiology and neurology:

- CB: So, would you describe yourself as working in neuroscience or do you tell people that you work in neurobiology?
LR5: I mean to the lay public, I think neuroscience is a term like that. But I mean if I'm talking to scientists, I may describe myself as a neurobiologist as opposed to a neuroscientist, or neurologist. I'm not a neurologist.

While interviewees were willing to affiliate themselves to 'the neuro' in some form then, this was not usually without qualifying the term by linking it to their specific domain of practice (biology or psychology). Littlefield and Johnson (2012) raise the question of

whether, for instance, neurohistory, neuropolitics, neurotheology and neurology are predicated on the same version of the ‘neuro’ (p. 8). Our data suggest that at least for some neuroscientists, the neuro only really takes on a specific meaning when it is conjoined with a more specific discipline: it is the stem word that provides the neuro prefix its particular meaning, as much as the reverse.

As we have illustrated so far, in this research group, both neuroscience and the brain were multiple, enacted through distinct practices in the lab and clinic, and seemingly more distributed than coordinated (at least for a tacitly demarcated ‘present’ – see below).

This did not mean that the group was unable to function, however. Clearly there was an overarching logic that enabled the group to exist within the university and to attract funding. Such multiplicity in objects and perspectives also does not necessarily mean that we cannot regard neuroscience as a discipline. Barry et al. (2008) point out that

the existence of a discipline does not always imply the acceptance of an agreed set of problems, objects, practices, theories or methods, or even of a shared language or common institutional structures. ... They may develop ways of translating across and negotiating internal boundaries; or chronic internal intellectual divisions may persist unaddressed through pragmatic working arrangements, or may even be collectively denied. (pp. 26–27)

What is clear in our case is that ‘neuroscience’ was not enough to bridge the two cultures of the lab and clinic on its own. The possibility of translation could not rest solely on the existence of this overarching category – further work was needed at a local level. We turn now to examine some of the ways in which translation was variously enacted within the group. In large part, accounts of translation focused on several versions of the future – not least as represented by the figure of the GL. This deferred version of translation was concerned less with the coherence or cohesion that Mol describes than with ‘adhesion’. As such, instead of an interdisciplinary whole that encompassed lab and clinic and through which could flow knowledge, objects and practices, there was a simple mosaic of two adhering parts, stuck together by the glue that was the Group Leader (GL1) and, crucially, the promise that he embodied.

Enacting the GL

We begin with GL1’s own account of the team’s research. It was clear that he had a particular vision of translation, embedded within a developmental narrative of his own interests and career and his own efforts to build a team that cut across the laboratory–clinic divide.

GL1 described the group as having two distinct subareas:

[since undertaking a PhD twenty years ago] I’ve really been involved with the field, trying to better understand how we can take the cell therapies from the lab to the clinic, what the clinical trials have told us so far about using cell therapies in patients with neurological disease. And from that I’ve built up a group, over a number of years, which specifically addresses both of those issues – a lab-based approach to brain repair as well as a clinical approach on how better to define disease types and how you can best match therapies to patient.

However, he privileged the overall goal of clinical translation and saw the diseases as being the key focus of the group's work:

Yeah I mean we're basically a sort of clinically led group of researchers. We don't tend to do a lot of basic research unless it's got a very clear therapeutic end, if you like. So most of our work is around trying to better understand how to repair the brain either intrinsically or extrinsically, using transplants or innate repair mechanisms. We try and understand the different types of disease, so we can marry novel therapies to these subtypes. But by understanding what underlies these different types of disease, this will open up a therapeutic approach which we can then take back to the clinic. So our work is really all designed around trying to treat the condition. Sometimes it's a little more obscure if you like, because it seems much more basic. But ultimately it's all designed around disease. So right at the centre of it are the diseases themselves. So even the basic questions we ask are related to why do you get that cell loss in Parkinson's Disease?

At the same time, he recognized that the diseases the team was dealing with were multiple:

In the 1980s, Parkinson's was a single disorder – there was a loss of dopamine cells; that was pretty much it. You take out the developing mid-brain, everyone just says, 'I'm taking out the developing dopamine cells of the nigra to transplant in.' So when you think about that you think, well, Parkinson's Disease – single disorder, loss of dopamine cells, I'm transplanting in dopamine cells. It's a bit of a no-brainer, you think, 'it should work'. But of course then when you start thinking, 'Well actually Parkinson's Disease isn't really one disorder, it's several disorders. And actually it isn't just the dopamine cells, it's actually much more complicated than that. And actually 90% of what I'm transplanting are not dopamine cells, what on earth are they doing?'

His vision for the future was for the group to move further along the translational pathway from bench to bedside. The clinical trial was seen as something that would facilitate this:

historically, what we've done is we've spent a decade describing aspects of disease, both Parkinson's and Huntington's Disease, and we've spent a decade in the lab trying to better understand behaviour of transplants and innate repair mechanisms. And I think, for me, the future is now how can we make that into a, a more unified approach. So experimental therapeutics, I think. So, for me, the next ten years will be much more about not just describing what goes on in the disease, but trying to alter it, so actually trying to push for therapies more to the clinic, and to try and think of what we can do in the lab which will have a much clearer input into the clinic. So I think having described, we now have to explain. And once we've explained, we can then interfere. And I think that's what we need to do really. So, I feel some of the things we do in the lab now I can really see running to the clinic. And that's what I want to do. I want to do more of those intervention studies. And that's why [the clinical trial] is so important to me, because it's a template by which to think through the issues of experimental therapeutics.

Here the brain (and more broadly the neuro) is enacted as something that straddles the divide between laboratory and clinic in complex ways. The movement across the divide is prospective; it is cumulatively an object that invites description, explanation

and ultimately intervention. At the same time, it displays a manifest multiplicity (the multiplicity of PD disorders once thought to be singular). This movement towards translation is captured in the promissory term ‘experimental therapeutics’ (tellingly used only by GL1 – it was only he who seemed to be able to articulate this hoped-for translatability in this hybridic and encompassing way) that in its two component parts points both towards the laboratory and the clinic. In contrast to Mol’s ethnography of a hospital environment, in this neuroscience research setting, the possibility of coordination remains in question.

When we turn to consider the accounts of members of the research team, whether in the laboratory or the clinic, the vision of how translation might be accomplished was less well articulated. For example, while CR5 describes a translational trajectory of sorts, she seems less certain about the steps involved:

CB: What is it that the work has in common?

CR5: Oh, I suppose it’s all to do with like neurodegenerative, like Parkinson’s and Huntington’s Disease. And so the scientific, the lab based work dealing with the kind of, the cellular and the neurodegenerative proteins and genes and cells involved and then kind of then experimentation in, with animal experiments, animal models of the disease. And then in the clinic with the actual patients with the disease. And so I think it’s all to do with working towards increasing knowledge in Parkinson’s and Huntington’s Disease mainly.

Similarly, we can detect a degree of tentativeness in the following quote from LR2:

It will essentially be trying to mimic cell therapy in patients in rats. So you will, you create a lesion in the striatum, which is kind of mirroring what you see in Parkinson’s Disease, and then transplanting rat cells into that lesion, which is analogous to transplanting the cells into Parkinson’s patients. So it’s using the technology in animal models, but essentially trying to see, you know, it’s answering the question, ‘Will it work in humans?’ but using animal models. So it’s the first step in answering the clinical question, I suppose.

In both of these extracts, we can see the outline of the potential translation but it seems rather uncertain and not a little vague. This is not surprising given that again and again members of the team turn to the GL as the person who ‘really knows’ about the translation. LR5, for instance, thought the group’s work was too broad, but that nevertheless the GL would somehow manage this diversity to enable translation:

And the other thing is, I think one slight concern of our group is that it diversifies too widely. For example, I’m working on neurodegeneration and neural protection of Parkinson’s. And there are some other people working on cognitive function in Huntington’s, and there’s other people working on harvesting cells – I mean, regardless of the context of PD or HD – and there are people working only on the clinical aspects of Huntington’s and there are people working only on the clinical aspects of PD. And there are people working on sleep, for example, and there are people working on apathy and biomarkers. I mean it’s just, it’s just too many, too many directions. I think so far, up to this stage, Group Leader copes with it well. But I mean if the diversity keeps widening, I don’t know, I mean, yes I mean you have to talk to Group Leader about it!

The following quotes expand on this perception of the GL who copes with the multiple threads of the research. LR5 also depicts the GL as having the capacity to grasp what is important in the laboratory experiment and situate this within a broader vision, presumably tied to the production of therapeutic interventions:

LR5: I can't recall when was the last time Group Leader stepped into the lab. ... he probably can't remember all the very fine details about experiments. But he has a very, very good ability to just grasp the concept of your project and to point out the weaknesses about it. So I mean I think that is very, very helpful, because I mean, when you're doing a project, I'm not sure about other people, but I have the tendency to work deeper and deeper into that and then ignore everything that happens around me. He has the big vision to kind of provide you direction about where you should be going. I think that's very, very helpful.

CR5 nuances this figure of the GL by noting that he is both clinician and scientist who enables the movement of 'lab techniques and issues to the clinic'. Interestingly, this is situated, albeit rather nebulously, in the context of other initiatives in the University, which has started a new 'translational medicine course, and things':

CR5: [something that facilitates translation is] having people like, I mean like Group Leader, he's a clinician who works on the ward as well as being a scientist, so having the kind of links in both areas. And I mean I think in general, developing lab techniques and issues to the clinic, is that there's a lot of focus now, especially, I think in [University] they've started a new like translational medicine course, and things. And there's a lot of focus on trying to kind of help that process. So there's definitely a lot of, in [University] there's a lot of focus towards that.

Finally, we can note that the GL is constructed as the one who can take into account both sides of the group:

CB: Is there anything that you think facilitates the translation between the lab and the clinic in this area? Or in the group?

LR3: I suppose again it comes down to Group Leader. He's the kind of – you know, we talk about things in the lab meetings, and it's, generally the people in the clinic don't ever suggest lab work that could help and vice versa. ... But I think Group Leader is probably the one who can see both sides of it best and is most likely to facilitate influencing one or the other. ... He's probably the only one who has [an overview] to be honest.

Here the GL is further performed as a sort of conduit that facilitates the influence of 'one side or the other'.

In the foregoing, we have seen how the GL is enacted in relation to the doing of translation across the laboratory–clinic divide. He has several guises – from his own

developmental self-portrayal with its cumulative orientation towards translation, through a vague sense of translational process among group members, to the enactment of him as a ‘translator’ who possesses the appropriate vision, who can make lab work relevant to the clinic, or who can facilitate mutual influence across the divide. This scrappy picture is not totally unexpected when we think of the GL as someone to whom translation is deferred: if it is the GL who is uniquely positioned to ‘do’ translation, there is no need for others to invest too much thought in how it proceeds or, indeed, what it practically entails. Having noted this, translation remains very much a prospect, something that will be actualized sometime in the future. Here, the ‘neuro’ – disparately manifested across the divide – is nevertheless held to *become* coherent in due course. In the following section, we analyse this dynamic more formally.

Accountability and the promise of porosity

At the time of fieldwork, translation remained very much a prospect. Mediation across clinic and laboratory thus amounts to what is at base a *promise of porosity* that is embodied by the ‘hybrid professional’ (Michael et al., 2005) GL and his expertise in both neurobiology and neurology. Let us unpack this a little further.

According to Pinch and Trocco (2002),

We need to describe, for instance, how actors can slip the anchors that keep them tied to just one identity and how new identities themselves come to be. Not only do people change identities, transgress boundaries, and move from one world to the other – say, from engineering to music – but they also apply the knowledge, skill, and experience gained in one world to transform the other. ... We call such people ‘boundary shifters’ – people who cross boundaries and in so doing produce a transformation. (p. 314)

As yet, within our research group there has been relatively little traffic across the laboratory/clinic divide. The boundaries remain by and large intact, and any ‘boundary shifting’ is still mainly a prospect. If anything, the figure of the GL, rather than shifting the boundaries, is seen to ensure their stability (even as he is the conduit that will breach the boundary and is the embodiment of the promise of its eventual porosity). This does not mean that nothing new emerges, only that the new will most likely be ‘discipline specific’ – in either neurobiology or neurology. Moreover, to ensure a boundary’s stability is itself no less an accomplishment. As such, in contrast to coherence with its connotation of some level of integration across the two domains, and in equal contrast to incoherence with its connotation of disintegration and the lack of any contact across the two domains, we might say that there is *adhesion* – there is continued ‘surface contact’ between clinic and laboratory and a commitment to an idea of future translation. Unlike Mol’s case, here there is a ‘hanging together’ but without much practical outcome other than its continuation – a hanging together which is marked by a coordination and some level of coherence that remains in the future. Here, a cohesive ‘neuro’ is practically enacted as a prospect that recursively serves to bind together adhesively the disparate neuros of lab and clinic in the present (Michael, 2000).

We can also approach this dynamic from the perspective of interdisciplinarity and in particular Barry et al.’s (2008; also see Born and Barry, 2010) recent influential

rendering. Barry et al. (2008) draw on empirical work in a range of fields to map out the various forms that interdisciplinarity can take, and there are resonances here with translation that we find illuminative. On Barry et al.'s account, there are three 'logics' that govern interdisciplinarity: a logic of ontology (mutual change in different parts of the group), a logic of innovation (solutions arising from interdisciplinary relationships), and a logic of accountability. While we find little of the first two at our site, we find the 'logic of accountability' to be particularly suggestive. Within the logic of accountability, 'interdisciplinarity is guided by the idea that it helps to foster a culture of accountability, breaking down the barriers between science and society, leading to greater interaction, for instance, between scientists and various publics and stakeholders' (Barry et al., 2008: 31). This might entail the legitimization of the science or, conversely, its scrutiny. In Barry et al.'s formulation, accountability seems to be directed to constituencies outside science – to publics or policymakers, say. However, we might reframe accountability as operating *within* a purportedly interdisciplinary research group, as scientists and clinicians make themselves accountable to each other. As such, laboratory scientists might make their work accountable to the constituency marked by clinical problems as identified by the clinicians; meanwhile, clinicians can make their work accountable to the scientists who will illuminate the organic basis of the neurological conditions and provide the technical framework for their treatment. However, as we have shown, at the time of our fieldwork, this was happening in a rather inchoate way. Instead, it would seem that accountability is oriented towards the GL.

Over and above the obvious forms of accountability to the GL as the institutional head of the team, the GL's position as straddling laboratory and clinic affords him a particularly important role in the process of accountability. Simultaneously, the GL stands for each epistemic domain's self and other. In interdisciplinary work, despite the institutional impetus towards collaboration across disciplines, there are nevertheless potent forces that reinforce disciplinary boundaries. While research councils encourage interdisciplinary projects, refereeing and auditing processes can militate against these (e.g. Griffin et al., 2006). Arguably, academic status remains, by and large, tied to reputation within a discipline. The GL as 'epistemic self' can be said to mediate, at least in part, accountability to one's usual disciplinary constituency. In parallel, as the representative of the 'epistemic other', the GL mediates accountability to one's 'other colleagues' – to those disciplines that support work by offering the prospect of the warrant of either clinical application or biological grounding. The GL embodies the elusive 'neuroscience' that is enacted in so fragmentary a way in both lab and clinic at present.

This stands in contrast to Mol's case, where accountability was rather less 'symmetrical'. It seems that in Mol's hospital the lab serviced, and was accountable to, the clinic (in Barry et al.'s terms, the mode of interdisciplinarity was one of service-subordination). Wilson-Kovacs and Hauskeller (2012: 507) also suggest that translational research ultimately subsumes basic science into medicine. By comparison, in our group, not least through the interdisciplinary figure of the GL, there was accountability both to the disciplines of self and other. Furthermore, given the GL's embodiment of what we have called the promise of porosity, there is also an accountability grounded in a collective future of translation. The divergent neuros of lab and clinic are held together by the conjoint

prospects of mutual accountability (currently mediated by the GL) and translatability (again embodied in the figure of the GL).

Concluding remarks

In this article, we have attempted to trace the ways in which the 'neuro' has been enacted across domains within a research group engaged in translational research. Within the research group, there was little evidence of coordination similar to what Mol identifies in her hospital case study. However, by adding a temporal dimension to the 'collaborative' process across lab and clinic, we suggest that there is a 'promise of porosity' across institutional and disciplinary boundaries, and it is this promise that 'binds' lab and clinic. We can tentatively theorize this 'binding' by drawing out a contrast between cohesion and adhesion.

Cohesion might imply some form of 'integration', however, less grandly, we take it to connote participants' commitment to a set of existing and immediate institutional goals, a 'shared' accountability to an 'external' but present constituency, an ongoing traffic of materials and signs across different disciplinary practices, and a practical management of the different ontologies enacted through those discipline-based materials and signs. By comparison, *adhesion* implies pursuit of disparate institutional (disciplinary) goals, but also deferral to a future goal (of translation); accountability that is 'internal' to the group – and in the present case study, oriented towards the figure of the GL (who embodies that future, and epistemic self and other); no or little traffic of materials and signs across ontological or disciplinary divides; yet a 'together but apart' adhesive form of organization whose glue takes the form of a 'promise of porosity', embodied, in our case study, by the GL.

To be sure, our distinction between cohesion and adhesion is one in need of further empirical illustration, conceptual elaboration and nuance, not least in relation to concepts that similarly attempt to address coordination across disparate actors, notably boundary objects (Star and Greisemer, 1989). Nevertheless, the role of the GL in the adhesion of our study group has some suggestive implications. The centrality of the GL rests on his (imputed) capacity to straddle laboratory and clinical neuro-domains, and on the translational neuroscience vision to which he has privileged, if not exclusive, access. In this setting, 'neuroscience' was a promissory category towards which the group's work was directed through the GL – a blending of the currently distinct practices of neurology, (neuro)psychology and (neuro)biology. At the same time, rather than bringing with it an essential meaning, this particular vision of neuroscience was produced through the specific, future-oriented dynamics of the group.

While our argument derives from a study situated at the intersection of the enactment of the neuro, translation and expectations, it can be extended to other settings. In those empirical cases where 'interdisciplinary teams' initially share few common points of reference and where the traffic of materials and signs (including boundary objects) is minimal, the promise of porosity might be key to their ongoing adhesion. This promise might take various forms, not least documentary – grant applications, policy documents, 'sociotechnical roadmaps'. However, on the basis of our analysis, we might suggest that the promises or expectations that attach to an interdisciplinary or translational programme of research might need

‘vivification’ – to be rendered organizationally alive, so to speak. In our case study, as we have seen, this sense of promise among most of our participants was sketchy at best. It was the GL who vivified it. The question thus arises, what other entities or events can serve in the ‘vivification’ of a technoscientific promise? While we can imagine a number of such moments of vivification – the invention of a group brand, a Christmas party, the visit of an esteemed dignity – this is an empirical matter that merits further investigation.

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