The London School of Economics and Political Science

‘Tracing Autism’

Ambiguity and difference in a neuroscientific research practice

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Declaration

I certify that the thesis I have presented for examination for the PhD degree of the London School of Economics and Political Science is solely my own work other than where I have clearly indicated that it is the work of others (in which case the extent of any work carried out jointly by me and any other person is clearly identified in it).

I warrant that this authorisation does not, to the best of my belief, infringe the rights of any third party.

I declare that my thesis consists of 88,543 words.

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Abstract

Tracing Autism is about neuroscientists' on-going search for a brain-based biomarker for autism. While much recent sociological work has looked at the 'cerebralization' of such diverse diagnostic categories as depression, bipolar disorder, psychopathy, addiction, and even autism itself, surprisingly little light has yet been shed on the mundane ways that researchers in the new brain sciences actually think about, reason through, and *hold together* neurological accounts of complex and emerging diagnostic entities. Situating itself within a series of interviews with neuroscientists who work on the autism spectrum, one of the most enigmatic, recalcitrant and unresolved categories of contemporary neuroscience, *Tracing Autism* is an attempt to fill this gap.

The key argument is that while this work might be seen as a process of gradual 'neurobiologization' or 'neuromedicalization,' talking to autism neuroscientists reveals a practice much more complex, much more ambiguous, much less monolithic, and also much less certain, than the sociological literature yet fully realizes. The thesis shows how autism neuroscience works by *tracing* its way across some very different and ambiguous commitments – carefully negotiating the space between the biological and diagnostic definitions of autism, the hope and disappointment of neuroimaging technology, as well as the intellectual and visceral commitments of laboratory research.

Locating itself within a recent turn to theorising the entanglement of cultural and biological phenomena within scientific spaces (Barad, 2007), and joining with a growing literature that wants to take neuroscience seriously (Wilson, 2004), *Tracing Autism* shows how the complex work of autism neuroscience picks its way across social deficits, neurobiological substrates, psychological theories, disappointing machines, and loving scientists. *Tracing Autism* is the story of an intellectual and affective complexity that has come to define autism neuroscience; but it is also the story of the care, seriousness and novelty with which neuroscientists talk about their work.

Acknowledgements

As much as a thesis emerges from its author's own desires, struggles and failings, it is also deeply entangled in the intellectual communities in which the author was lucky (or unlucky) enough to find themselves.

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Acronyms and shortened words used in the thesis

ABA	Applied Behaviour Analysis
ADHD	Attention Deficit Hyperactivity Disorder
ADI-R	Autism Diagnostic Interview - Revised
ADOS	Autism Diagnostic Observation Schedule
ANT	Actor-Network Theory
APA	American Psychiatric Association
ASD	Autism Spectrum Disorder(s)
CDC	Centers for Disease Control (US)
DSM	Diagnostic and Statistical Manual of Mental Disorder. Also DSM-IV, DSM-IV-TR and DSM-V for different editions
ECG	Electrocardiogram
EEG	Electroencephalography, or an Electroencephalogram
fMRI	functional Magnetic Resonance Imaging
MEG	Magnetoencephalography, or a Magnetoencephalogram
MRI	Magnetic Resonance Imaging
NHS	National Health Service (UK)
NIMH	National Institutes of Mental Health (US)
NINDS	National Institute of Neurological Disorders and Stroke (US)
HFA	High-functioning autism
PI	Principal Investigator
PKU	Phenylketonuria
Postdoc	Postdoctoral researcher
RCT	Randomised Control Trial
SPECT	Single-Photon Emission Computed Tomography
STS	Science and Technology Studies
WHO	World Health Organisation

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Introduction

Outline of the argument

At the end of June 2012, *New Scientist* carried a story about a potential new brain-based biological marker – or 'biomarker' – for the neurodevelopmental disorder, autism (Coghlan, 2012). The tone of the piece was uncharacteristically low-key – and yet, if the central proposal proved correct (that patterns of brain activity could be used to diagnose autism), it would be quite a breakthrough. Autism was first described by the child psychiatrist, Leo Kanner, in 1943, when Kanner noticed increasing numbers of children coming to his clinic with symptoms that could be formed into a consistent profile – and one that was not well-characterised within the existing psychiatric literature (Kanner, 1968 [1943]). Often bright, and sometimes with real intellectual strengths in specific areas, these were children who nonetheless did not communicate typically, if at all; who often showed little interest in the company of their parents, or in anyone else's company; who sometimes did not seem to make strong distinctions, in interaction, between other people and inanimate objects; who relied on repetitive and stereotyped movements, and often had very specific, narrow-looking interests; who had deeply idiosyncratic speech patterns, and sometimes seemed happiest when left alone – so, on and on, went the description (Kanner, 1968 [1943]). Building his account, in particular, around a characteristic sense of *aloneness*, and on the degree to which, in his view, 'these children had never engaged with the social world' (Grinker, 2007: 53), Kanner characterized his syndrome as an 'autistic disorder of affective contact' – borrowing, a term coined by Bleuler to describe (in schizophrenia) 'an active turning away from the external world' (1951 [1913]: 399).

Despite the debates and controversies that have sprung up in the intervening decades, Kanner's 'autistic disorder of affective contact' is still recognisably the syndrome that today we call autism – or, in recognition of the symptoms' variability, autism spectrum disorders [ASD[1]]. In contemporary clinical accounts, autism is usually described as a complex developmental disorder, with characteristic 'deficits' in three core domains: social interaction, communication, and range of interests

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[1] Throughout the thesis, I will usually just refer to 'autism' – which remains the most commonly-used descriptor, despite widespread recognition of the range of underlying symptoms. I will have more to say about the 'heterogeneity' and the 'spectrum' of what are sometimes called 'the autisms' in the next chapter (Coleman and Gillberg, 2011)

(American Psychiatric Association [APA], 2000: 75; World Health Organization [WHO], 2007). In the soon-to-be superseded fourth edition[2] of the APA's Diagnostic and Statistical Manual of Mental Disorder [DSM-IV], symptoms include 'a lack of spontaneous seeking to share enjoyment, interests, or achievements with other people,' 'a lack of emotional reciprocity,' 'stereotyped and repetitive use of language or idiosyncratic language,' and 'encompassing preoccupation with one or more stereotyped and restricted patterns of interest' (APA, 2000: 75). In the decades immediately following Kanner's identification, autism research, unattached to any obvious neurological or other biological cause, remained a beguiling if niche interest (see Evans, 2010, for the history of autism research in the UK). What is perhaps most remarkable about autism, though, is that, in many countries, including the UK – growing slowly, but starting really in the 1980s and 1990s – autism became very suddenly visible, and very suddenly present. In 1976, autism had an estimated prevalence rate of between 4 and 5 per 10,000 children under 15 – or about 1 child in 2,000/2,500 (Wing *et al.*, 1976). By 2009, and in a study of 5-9 year-olds only, autism was estimated to be diagnosable in about 1 child in 64 (Baron-Cohen *et al.*, 2009; Powell *et al.*, 2000).[3] Although many have argued for an environmental cause for this upsurge, the most common (clinical and research) view is that the increase in diagnosis reflects some combination of better recognition of autistic symptoms, and also 'diagnostic substitution' of autism for older categories like 'mental retardation' (Frith, 2003; Eyal *et al.*, 2011)[4]. In any event, with this surge in diagnosed prevalence came a quite sudden growth in popular awareness and concern about autism – marked by its growing visibility in politics (in the UK, the All-Party Parliamentary Group on Autism was established in in 2000), the arts (Mark Haddon's *The Curious Incidence of the Dog in the Night-time* was published in 2003), the charitable sector (the research funder, *Autistica*, was set up in 2004),

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[2] The fifth edition of the APA's definitive diagnostic tome (DSM-V) is due to be published in 2013. The list of proposed changes is available online, with autism among those diagnoses potentially set to be significantly altered (APA, 2011; Mahjouri and Lord, 2012.).

[3] The same pattern is seen in many other countries – including the USA (Centers for Disease Control [CDC], 2012a), Sweden (Gillberg *et al*, 2006), and Israel (Gal *et al.*, 2012).

[4] Although I will not spend a great deal of time on it, the causality of autism, and discussions of an autism 'epidemic,' remain complicated and sometimes bitterly-divided topics (CDC, 2012b). The issue is particularly vexed by a history of 'parent-blaming' in an older psychiatric culture (Bettelheim, 1967; See Feinstein, 2010, for a history). This pushed many groups of parents to focus on extrinsic causes, and into attempts to identify and treat increasingly esoteric 'environmental' or 'biomedical' causes (Rimland, 2004.). The 'vaccine controversy' is the most well-known dividing-line here: see Offit (2010) for an account.

centrally-driven research strategies (the UK Government's Autism Research Co-ordination Group first met in 2005), and, finally, the law (in 2009, the UK Parliament enacted an 'Autism Act,' mandating a countrywide strategy for the disorder, and requiring guidance to be produced for local authorities on delivering services) (Great Britain, 2009).

And yet, as Stuart Murray has pointed out, as much as autism has emerged as a 'pressing issue of current concern' – so it also remains a 'somewhat abstracted, unsourced, alien phenomenon' (2008: 2). What Murray means by this is that despite the increasing visibility of autism, and in particular despite concerted efforts to strategize research activity around it (Charman and Clare, 2004), there is still no known single, identified biological cause, marker or descriptor of autism's core symptoms. Among other things, this means that diagnosis is done using a quite broad behavioural assessment only. Which in turn, raises questions about what, exactly, it *is* that's being diagnosed in such growing numbers, and even whether 'autism' describes a biologically coherent phenomenon in the first place (Geschwind and Levitt, 2007; Happé and Ronald, 2008). This has sometimes made autism research frustrating for scientists at the coal-face: 'the field of autism,' says the psychologist, Laura Schreibman, 'is littered with the debris of dead ends, crushed hopes, ineffective treatments and false starts…we are dealing with a devastating disorder for which we have few answers to date' (2005: 7). It is striking that even as autism has emerged as a focus of popular concern, and even as it is located within both the bodies and habits of an ever-larger number of people – so it has continued to resist any sort of easy clinical or biological definition. And for a diagnosis that seems most likely to mark a disorder of brain development, particularly, this absence has been felt with special keenness by neuroscientific researchers on autism: in a recent review, Lord and Jones noted that while there had been great hopes for both structural and functional neuroimaging findings, for example, the truth is that 'these approaches seldom provide data on an individual level, do not yet have well-accepted standards or replicability across time or site…and have rarely addressed questions of specificity of findings to ASD' (2012: 491).

This is the context in which *New Scientist* was interested in a paper published on a potential brain biomarker for autism (Duffy and Als, 2012). 'At present,' the article pointed out, 'autism is diagnosed by carefully observing a child's behaviour and assessing their clinical history' (Coghlan, 2012). Now, however,

> Frank Duffy of Boston Children's Hospital and colleague Heidelise Als have developed an electroencephalogram (EEG)[5] test that involves placing 24 electrodes on a child's scalp… Duffy says that the patterns seen were so consistent in so many of the children with typical autism that they could represent the first neuro-physiological "phenotype" of the disorder (ibid.)

'The findings are remarkable, real and they work,' Duffy told *Time* Magazine: 'it seems to establish that no matter how you come to have autism, you have a distinct pattern of abnormality' (Sifferlin, 2012). As a sociologist of the new brain sciences, with a particular interest in the autism spectrum, I have been keeping an eye on these kinds of claims for a few years now: in 2010, Timothy Roberts and his colleagues at the Children's Hospital of Philadelphia tentatively proposed a brain-imaging biomarker for autism, using Magnetoencephalography (MEG)[6], to distinguish autistic participants from controls on the basis of a brain-measured 'latency' in processing sound (Roberts *et al.*, 2010). Finally, claimed the science correspondent of the *Daily Telegraph,* 'researchers believe they have discovered a potential way of spotting the disorder in early infancy by scanning the brainwaves' (Alleyne, 2010). At the tail end of the same year, Christine Ecker and her colleagues at the Institute of Psychiatry, London, published a paper describing the use of another brain-imaging technology, structural Magnetic Resonance Imaging (MRI)[7], which they allied to a complex system of multiparameter classification to distinguish people with autism from controls across five morphological dimensions – a paper that got its lead author invited onto the *BBC Breakfast* television programme (Ecker *et al.*, 2010). The following year, Michael Spencer, a psychiatrist based at the Autism Research Centre at the University of Cambridge, led a team arguing that using yet another brain-imaging technology, this time a *functional* Magnetic Resonance Imaging (fMRI)[8] measure of the response to seeing others' emotional expressions, could distinguish between 'unaffected' siblings of children with autism versus

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[5] EEG is one of the more long-established forms of brain-imaging; it measures electrical currents in the brain. See Borck, 2008, for a history.

[6] A form of brain-imaging related to EEG, but based on measuring the magnetic fields given off by electrical currents. See Hari and Salmelin, 2011, for an anniversary appraisal.

[7] MRI is a form of medical imaging that is also used for structural scanning of the brain, based on the relationship between electromagnetic fields and the alignment of atomic nuclei in the body. See Joyce, 2008, for an ethnographic account.

[8] The specific claim of fMRI is to image the brain while it's actually doing something – using a measure of blood oxygenation as an index of brain activity. Hence the iconic status of fMRI within cognitive psychology particularly, where it can scan the brain, and produce neural correlation, while a participant performs some cognitive task. See Beaulieu, 2002, for an ethnography of fMRI imaging.

controls with no family history of the disorder – and ultimately form the basis of a brain-based autism biomarker for autism (Spencer *et al.*, 2011). *The Daily Mail* was moved to describe this as 'ground-breaking research' that 'could pave the way for treatments or even a cure for sufferers of autism and Asperger's syndrome[9]' (Daily Mail Reporter, 2011).

I have been following these attempts because I am interested in the ways that neuroscientists may – or may not, as the case may be – put together a convincing neurological account of a particularly complex psychological or psychiatric diagnostic category. With its deepening cultural presence, combined with an emerging sense of heterogeneity, and also an on-going history of biological recalcitrance, autism seems like a particularly rich site for thinking about this aspect of neurobiological research. It is true that attention to the growing relationship between the sciences of the brain and longstanding categories of 'mental disorder'[10] is not new in sociology: we have already had detailed attention to the complex somaticization and 'cerebralization'[11] of such diverse diagnostic categories as depression (Helén, 2011), bipolar disorder (Martin, 2007), psychopathy (Pickersgill, 2009), addiction (Vrecko, 2010a), and even autism itself (Ortega and Choudhury, 2009) – as well as similar identifications of an emerging neurological basis to adolescence (Choudhury, 2010), old age (Williams *et al.*, 2011) childhood difference (Rapp, 2011) and, in fact, human subjectivity in general (Ortega and Vidal, 2007). This forms a valuable and growing literature on an emerging set of methods, institutions and bodies of knowledge that could loosely be called neuroscience (or 'the new brain sciences') – at least as neuroscience attends to psychiatric and psychological categories. And yet surprisingly little light has been shed, so far, on

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[9] Asperger's Syndrome is usually considered an 'autism spectrum disorder,' but is a distinct diagnosis, reflecting a 'high-functioning' part of the autism spectrum, and one particularly distinguished by the absence of problems in language development, and 'normal, cognitive development in the first three years of life (APA, 2000: 80; *cf.* Wing, 1981). The distinction between 'high-functioning' autism and Asperger's as distinct diagnoses remains an open question; indeed, current proposals for DSM-V would remove Asperger's as a separate diagnosis entirely (Baron-Cohen, 2009; Wing *et al.*, 2011).

[10] I will not keep putting words like 'disorder' in scare quotes during what follows, but will rely on the general *ethos* of the thesis to make clear that, working through psychological accounts, I adopt that discipline's language as a shorthand only – and I do not do so in ignorance of the politics and contest that still surround this language, especially in discussions of autism (Singer, 1999; Bumiller, 2008).

[11] In general, I follow British spelling throughout the thesis. However, I have retained US spelling where it is used in the original (such as in 'Centers for Disease Control') and I have not attempted to convert academic neologisms that are more typically spelled in an American style (such as 'neurobiologization' or 'biomedicalization').

the basic conceptual labour that enables groups of neuroscientists to locate or *trace* difficult diagnostic entities through the practice and epistemology of the new brain sciences. When I say the 'conceptual labour' of neuroscientists here, I draw attention to the way that neuroscientists have to engage in hard, on-going work (and not all of this work 'intellectual') on definitions, theories, methods, categories, and so on, in order to create convincing and sustainable accounts of the brain basis of complex mental disorder. I am interested in what it is that neuroscientists find so challenging about the attempt to locate a specific neurobiological marker or explanation of autism. I am trying to think about what we can learn from the issues involved – and from neuroscientists' attempts to resolve those issues – about the ways that convincing neurobiological accounts of tricky and variable mental disorders are made and sustained. And I am trying to establish whether thinking with neuroscientists' own accounts of such attempts will tell us anything about the new brain sciences that we do not already know. These were the basic interventions around which the research was first thought and organised.

The space around these interventions marks a significant gap. If we know, now, to put it very broadly, that psychology and psychiatry are entering a neurological age (Andreasen, 2001; Rose and Abi-Rached, In Press); and we know that this involves re-locating more and more mental disorders (and other perceived deficiencies) to the level of 'brain disease' (Vrecko, 2010a); and also we know that this process may actually radically re-cast how we think about, and intervene upon, categories of mental distress (Trimble, 1996; Pickersgill, 2011) – still we do not have a full sociological account of the ways that researchers in the new brain sciences actually think about, reason through, and conceptually (as well as affectively) *hold together*, tricky neurological accounts of complex and emerging diagnostic entities. There are many valid ways to think sociologically about the new brain sciences. But the key argument of the thesis is that attention to *this* level, hitherto neglected, suggests something that we do not yet know about the putting-together of neurobiological accounts of mental disorder. And what it suggests, in short, is that as much as neurobiological accounts can be seen as processes of careful and accretive neuro-biologization and neuro-medicalization (Ehrenberg, 2011; Clarke *et al.*, 2003); as much as they can be characterised as the techno-somatic whittling-away of the complexity of human life (Pickersgill, 2011; Cohn, 2004); as much as they can be theorised as an emerging and increasingly powerful neuro-reductive ‘vortex,’ sucking in all possible alternatives (Martin, 2000; Gergen, 2010) – nonetheless, when you talk to the neuroscientists who are working on the neurobiology of a disorder, you often get a sense of the practice of neuroscientists as something much more complex, much more ambiguous, much less monolithic, and also much less certain, than a great deal of the sociological literature yet fully realizes.

This, then, is a thesis about neuroscientists accounting for their own practice, in tones that are beset by ambiguity, and uncertainty, and complexity, and even some anxiety. It is a portrait that therefore pre-empts a sometimes-dominant sense of sociological critique or censure of the new brain sciences (Choudhury *et al.*, 2009; Ortega and Vidal, 2007). In her *Psychosomatic*, Elizabeth Wilson proposes an image of the new brain sciences that is ‘different from those we have become used to in feminist, antipsychiatric, and social constructionist literatures’ – one, indeed, in which we can begin to see an ‘articulate, obligated, libidinized’ neuroscience as both ally and ‘resource’ for social-science theorizing (2004: 27-29). In what follows, I want to associate my account of a careful, ambiguous, uncertain neuroscience with this image. The story of the thesis is the story of a cutting-edge neuroscientific practice that is both attentive to, and mindful of, the differences and limitations that run across its own practice; it is the story of an intellectual, practical and affective complexity that has come to define the work of autism neuroscientists; it is also the story of the care, seriousness and novelty with which many neuroscientists pick their way through that complexity.

With this in mind, the thesis proceeds in terms of a double gambit. First, while there are many different and equally valid levels on which to locate an image of the contemporary brain sciences, from intellectual history (Vidal, 2009), to laboratory ethnography (Dumit, 2004) to artistic and cultural analysis (Frazzetto and Anker, 2009), these tend to give a slightly Whiggish image of neuroscience as an intellectual and cultural practice. By this, I mean that they circulate around an image of the new brain sciences in terms of its growing prominence and certainty (however troubling and ill-founded this certainty may be). Neuroscience emerges, from these discussions, as a confident discourse of the biology of everything. It looks like a practice that is raising ever-more money, that is getting more and more entangled with its suite of high-tech accoutrements, and that is thereof producing more carefully black-boxed and impenetrable results – generating an ever-growing sense of organic and molecular certainty about the relationship between our brains and any number of facets of human social life. (These are of course broad brush-strokes. I will discuss the complexities of the sociological literature in much greater depth in the next chapter). But would a different picture of neuroscience emerge from a level not yet fully exploited, i.e. conversations with neuroscientists about what it is they have to do to create neurological accounts of a particularly tricky mental disorder, such as autism? This question directed my attention to the specific issues and problems that trouble neuroscientists of autism. It led me to focus on the conceptual issues they really care about and work on, and what that work actually consists of. It invited me to think about what it is that neuroscientists actually think they're doing, and which parts of their practice they really struggle with. And it helped me to think about where it is they want to get to, and what it is they're willing to stake to get there. Gradually, through this attention, a different picture of neuroscience emerged – one that suggested a much more complex and ambiguous kind of practice than many of those described previously.

Second, while we have had some rich close-up accounts of what is actually involved in a neuroscientific – and, in particular, a neuroimaging – practice in general (Beaulieu, 2000a; Cohn, 2008), there has also been a natural tendency for these works to focus on the complex technical work and epistemological ground of the neuroimaging laboratory *as such*. But would close attention show us something new if it focused less on the work of 'the new brain sciences' or even just on 'neuroimaging,' and if instead it oriented itself to researchers' accounts of doing the neuroscience of just one particularly tricky, and still unresolved, category of neurodevelopmental disorder, such as autism? Following this intuition forced me to think about what was so tricky in figuring the relationship between neuroscience and autism particularly. I began to see that, from the point of view of the practitioners, doing a neuroscience of autism actually meant thinking through, and living with, many kinds of differences and uncertainties – like the difference between knowing something and feeling it, the difference between biology and symptom-sets, or the difference between good neuroscience and troubling neuroscience. The thesis is thus based around a double intuition: (1) that there is still much to be gained from thinking about the ways that neuroscientists actually figure neurobiological accounts of disorder, and from focusing on the things that neuroscientists struggle with and labour over in that figuring; (2) that one potential way to get at this level of analysis is to engage individual neuroscientists about their work on a particularly complex, varied and still-emerging diagnostic category – and one which actually has a long history of resistance to neurological calculation.

My working hypothesis at the outset of the project was that the definition of autism as a disorder rooted in specific patterns of social interaction and communication (among many other things) had created a category that, from the outset, and however well-constructed the assessment tools, would inevitably be very heterogeneous and varied, loaded with symbolism and meaning, marked by culture and history, and so on. I had guessed that these drifting, expansive social categories would not sit easily against the narrowly organic requirements of a new brain science – and that, therefore, the neuroscience of autism might be a doomed project from the outset (Nadesan, 2005; Happé and Ronald, 2008). If this is not entirely wrong, my interviews nonetheless suggested the possibility of a related argument, but a more complicated one. What we actually see amid the practice of autism neuroscientists is not a misguided attempt to put some very different categories of thing together (for example, the constraints of an fMRI paradigm, and the affective subtlety of a child's filial commitment). What we see, instead, is a series of intellectual, practical and somatic strategies – and not all of these admirable – for producing, living with, working through, and even locating some scraps of scientific positivity in, the complex and entangled ambiguities that run across neurobiological autism research. This practice is what that I have begun to describe as 'tracing autism.'

The image of autism neuroscience as a practice of 'tracing' actually comes directly from my interviews. I first heard the word used by a mid-career psychiatrist and brain-imager, who I had met towards the end of this process, and who was trying to get me to understand the difficulty, but also the sense of possibility, surrounding image-based neurobiological work on something like autism. He described how a person might have a very 'pure,' identifiable and innate genetic lesion that disturbed their language functioning. Because of the person's language problems, he pointed out, people in their environment would react differently to them – and so this very small and innate molecular difference would radically alter that person's social surroundings. This environmental input, in turn, might lead to a measurable biological difference elsewhere in their brain, and so on. This structure of looping interactions between biological lesions and the social world, which were somehow distinct *from* one another, but also irretrievably entangled *in* one another, was exactly what I had been trying to comprehend about the neuroscience of autism. For this psychiatrist, these looping entanglements were not a cause for despair – but they did lead him to a particular way of thinking about his work. 'You can,' he said, 'without doubt, trace it up – now, not necessarily very easily. But you can.'

At the time, I was struck by his use of the metaphor of 'trace' to describe the way that he worked through this complexity. In one sense, to trace something is simply to doggedly follow its contours and turns: to trace autism, in this sense, would simply be to pursue it through its various neurogenetic and environmental manifestations. But to trace something can mean independently drawing an outline or a figure of it too: tracing autism, in this sense, would be an attempt to actively put together an image of autism, to sketch a close copy of an only half-seen original. What this borrowed image of 'tracing' allows me to do, then – and quite unlike a metaphor such as 'making' – is to describe the active way that these neuroscientists work to establish lines of connection within a complex and often ambiguous research-area, but also to recognise the way that they do this without relinquishing the sense of distinctive and singular neurobiology of autism, an organic phenomenon that is actually quite independent of this labour.

In what follows, I highlight the fundamental ambiguities, uncertainties and differences that surround the doing of neurobiological autism research – whether this is ambiguity about the scientific nature of psychology, a difference between the biological and diagnostic essences of autism, or an uncertainty about the efficacy of brain-imaging technology. Framing the chapters as different ways of *tracing* autism allows me to be faithful to the complexity and ambiguity of the intellectual labour that is involved in each, while also recognising that the neuroscientists in question are usually quite aware of this quality, and that they work through it nonetheless. Ultimately, leaning on the image of *tracing* helps me to show that what makes these scientists so interesting, and so worth thinking with, is that they do not take this complexity, or their own work of making sense of it, as an indication that one cannot talk about a more-or-less singular neurobiological disorder called autism. Perhaps, in fact, quite the opposite. There is clearly some affinity, here, with the strange quality that Karen Barad calls 'intra-action' – a neologism that Barad has coined from her engagement with the philosophy-physics of Niels Bohr (2007: 33).

For Barad, 'intra-action' avoids a commonsensical assumption about independent things *inter*acting with one another, and describes, instead, a world in which 'distinct agencies do not precede, but rather emerge through, their interaction' (ibid.). The fundamental insight is that just because things are complex and ambiguous, and sometimes only held up by their entanglement with other things, it does not follow that one has to deny them their agency or their distinction. Just because the neurobiological account of autism seems inseparable from the interactions of (and not the distinction between) biological truths and diagnostic conveniences, or just because an account of autism's brain-basis only comes *after* the difficult weaving of psychological concepts into a physical science – this does not require us to concede that there is no such thing as a neurological, brain-based autism, distinct in itself. Barad's warning is that just because a scientific entity is clearly emergent from a particular arrangement of apparatuses, and people, and concepts, and definitions, this is no excuse for reducing it to the status of the 'made-up.' However awkward, 'tracing autism' is my attempt to be faithful to the way that some neuroscientists seem to work in a space that is more complex and ambiguous than previously thought. But it also marks my attempt to say that this complexity and ambiguity may not stand in opposition to a good and efficacious neurobiological account of autism. Setting out some small aspect of that essential insight is the task of each of the six chapters that follow.

Method

Having established a concern with the ways that neuroscientists work through, and think about, some of the problems that run across neurobiological autism research, I decided to pursue my own research through a series of qualitative interviews with neuroscientists who were working, in some way, to locate the neurological areas or processes at stake in autism. Below, I discuss why I went about it this way, what I actually did, and what this method produced exactly; I conclude by outlining some of the core problems to bear in mind about my approach.

Why interviews?

From a preliminary reading of the literature, it was clear that the neurobiological account of autism was – indeed, is – still far from settled (Herbert, 2006; Happé *et al.*, 2006; Charman *et al.*, 2011). This seemed to be true in the prosaic sense that no neurobiological marker (whether structural or functional) for autism had yet been discovered, although this wasn't for want of trying. But it was also true in the sense of a 'science and technology studies' (STS) approach to the active and processual aspects of scientific claims, which directs attention to the on-going work of scientific accounting (Latour, 1987; Shapin and Schaffer, 1989). Mindful of the degree to which the neurobiology of autism was still very much in question, it seemed reasonable to assume that neuroscientists' own understandings of what was actually required for the putting-together of a neurobiological account of autism were likely to still be in a state of flux. Moreover, because of the on-going and sometimes intractable-looking difficulties of doing a neuroscience of autism, I proceeded on the basis that researchers in this area might be quite open about the concepts and processes that formed the ground on which they were working. If autism neuroscientists are still – and very much on the surface – working out some of the basic issues implicated in forming and holding-together convincing accounts of neurobiological disorder, then they might be well positioned to reflect on that process. They also might do so in a way that is distinctive from the more bounded suppositions of a journal article or a textbook, *and* from the more black-boxed daily practice of doing neurobiological autism research.

In other words, if there is interesting sociological data to be gathered from neuroscientists on what is actually at stake in autism neuroscience, and on why this research seems so difficult, such data might not be most easily accessible in either: (1) the broad and 'public' world of neuroscientific discourse, or (2) the close-up world of the laboratory's daily workings. In the first case, several scholars have drawn on public statements of neuroscientists, on the broad discursive claims that structure the field, and on the ways that neurobiological assumptions are read into public policy, education, and other arenas (Martin, 2000, 2004; Rose, 2001a, 2004). This work is distinguished by its ability to grasp a broad field of 'neuroscientific truth,' and to follow, assess, historicize and qualify the emergence of that truth. But what is interesting about the data that I sought is that they were not yet 'in the true' (Rose, 1991). My focus on the difficulties of autism neuroscience, and on the kind of work still necessary for finding a way through those difficulties, positions the neurobiology of autism somehow 'below' the general structure of this field. In the second case, many scholars have drawn on close-up ethnographic encounters with neuroscience, and with the laboratories, conferences, workshops and meetings that make up its normal practice (Beaulieu, 2000a; Roepstorff, 2004; Dumit, 2004; Cohn, 2008; Joyce, 2008). This work is marked by its attention to the unspoken or non-obvious assumptions and partialities that are built into (and produced by) neuroscientific practice. But I am interested in issues and problems that are very much obvious and known – and whose consideration, and working-over, is part of the surface discourse of this space. Thus, in a similar way to my positioning of the neurobiology of autism 'below' the field of neuroscientific truth, I also want to say that it remains suspended 'above' neuroscience's 'black box' (Latour, 1987). This led me to semi-structured qualitative interviewing.

The data that were best suited to my interest were most likely to be located in neuroscientists' own reflections on what was actually involved in neuroscientific autism research – reflections that were still being compiled and thought-through. My 'field,' then, was to be scientists' own accounts of the conceptual and practical labour inherent in doing a neuroscience of autism; it was to be made up of their thoughts on their successes and failures, their on-going, articulated working-out of the connection between autism and neurobiology, their still-in-progress ideas about what needed to be done for this connection to be established more firmly – and also their speculation about how this might re-cast our ideas about the nature of a diagnosis like autism, and the practice of a discipline like neuroscience. Because my interests were thematic, and not fixed to central questions, I maintained a loose, semi-structured interview format, which allowed my key themes (see below) to circulate through whatever aspect of the research that the interviewee was most open to talking about. This kind of interviewing is of course a standard tool of empirical sociological research (Kvale, 1996; Gaskell, 2000; Wengraf, 2001; Hopf, 2004). But the capacity of qualitative interviewing to work with the specifically processual nature of ideas and understandings, has also made this a frequently-used tool in social studies of science, including in recent studies of neuroscience (Beaulieu, 2002; Joyce, 2005; Schüll and Zaloom, 2011; Pickersgill, 2011). There were also several additional advantages to using qualitative interviews for this study: (1) data from neuroscientists' reflections on difficult neurobiological disorders are cheap and readily available, but are still not widely gathered; (2) because autism neuroscience is a mobile and flexible field (in terms of people's interests and careers; but also in the sense of an absent fixed 'centre' of research activity), interview data allowed me to stitch together the varied commitments of dispersed individuals; (3)

perhaps most significantly, for issues that the scientists that were still thinking-through, and that were still not obvious to them, interviewing created a space for me to use dialogue, and my own disposition, to actually generate data in the midst of conversation. As I will note in the substantive material, there were several key moments during this process where interviewees seemed to articulate ideas that were either surprising or troubling to themselves; I claim this less as a victory of my own interviewing skill, and more an indicator of the use of dialogue as a generative tool in research such as mine (Mills *et al.*, 2006; Doucet and Mauthner, 2008).

What I did

An initial web-based trawl for potential interviewees produced a long-list of sixty people. My initial search criteria were: UK-based researchers, who had a declared interest in or record of publishing on autism, and who were either identifiably 'neuroscientists,' or whose work-practice had a strongly neuroscientific component (in fact, the border of what counts as neuroscience is not always clear. See the discussion below). I also included an additional number of non-neuroscientists who might have a relevant view on this research. These included some neurogeneticists, biostatisticians, clinicians, and representatives from related fields. It also included a small number of representatives from the 'third sector' (i.e. not-for-profit autism funding or advocacy organizations). On the basis of their core training, publication history, or current declared interests, forty of these initial sixty could be called 'autism neuroscientists' or 'neuroscientific researches with a particular interest in autism' without strong fear of contradiction.

I approached fifty-six[12] people in total for interview. This produced thirty-seven interviews, thirty of them with identified 'autism neuroscientists' (i.e. researchers who either worked in neuroscience centres, or frequently contributed to neuroscientific projects, who listed autism among their core interests, and who had a publication history of neurobiological autism studies). Of the remaining seven interviewees, three were people from the third sector (i.e. autism charities or advocacy groups), three were psychologists who had not worked directly in neuroscience but were well-known autism researchers; and one was a psychiatric researcher, best known for contributions to genetic research. It is worth noting,

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[12] The four I did not approach had either moved away, or did not fit my interview criteria strongly enough on a second look.

here, that early in the process, I was still tentatively interested in the way that neurobiological accounts of autism might filter out of the labs, and be taken up or re-articulated by people in the broader autistic field – and I conducted some exploratory interviews in this area (e.g. with the three people from the 'third sector'). However, as the research progressed, and the internal story of an 'autism neuroscience' narrative started to unfold in ways that were more confusing and rich than I had anticipated, I narrowed my focus, and I stopped approaching people from 'outside' neuroscience[13]. To my disappointment, this shift in focus included letting go of plans to interview people who had been diagnosed with autism (I discuss this further in the conclusion). It also meant not pursuing some of my early questions which were more about autism, and autistic experience, than they were about neuroscience as such. These were, for example, questions of how neurobiological research on autism may change autistic people's ways of accounting for themselves; or political questions about the relationship between neuroscience and autistic identity, as well as rights-based claims made on the basis of this identity.

Not including the three interviewees from the third sector, the remaining thirty-four interviewees were all based at large research universities in the UK. In terms of discipline, they had a remarkable diversity of job titles and affiliations. When I separated interviewees by either training, departmental location, publication history, or other potentially determining mark of affiliation, twenty-six had some strong affiliation to 'psychology,' four to 'psychiatry,' three to 'neuroimaging methods' (which meant a background in physics; notably, all three of these interviewees were based in departments of psychiatry), and there was one rogue molecular neurologist. Strikingly, twenty-three of these thirty-four were women – perhaps not unexpected in a field with such close affiliations to psychology, and something I discuss in chapter six (Willyard, 2011). These interviewees were also spread across a wide range of age and experience: I interviewed two PhD students, twelve researchers at the postdoctoral or research-fellow level, nine at the lecturer level, and eleven at the level of professor or principal investigator. The low numbers of PhD students reflects two things: one is the absence of any major funded autism-

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[13] However, I have still included those three third-sector voices here: one interviewee had worked in a funding organisation, one sometimes consulted for autism funding organisations, and one came from an autism charity. I also include the voices of the four autism scientists (three psychologists and one psychiatric geneticist) who nonetheless do not really meet the criteria of 'autism neuroscientist' or 'neuroscientific researcher with a particular interest in autism.'

neuroscience project in the UK during the period that I was conducting my interviews. Another is the relative intellectual invisibility of PhD students in institutional or discipline-specific web-spaces – a source that I relied upon quite strongly in my search.

Aside from a small number of pilot interviews in early 2010, most interviews took place between October 2010 and April 2011. As the relatively high number of interviewees shows, I opted for breadth over depth. This partly reflects the mobility of many scientific careers. In fact, most 'autism neuroscientists' had interests other than autism, and intellectual commitments external to neuroscience. I took this as an argument against in-depth interviews of a smaller number of people – and instead compiled a picture of 'autism neuroscience' from smaller pieces of different researchers' careers. I asked for 'around half an hour' from each interviewee, although the majority of interviews went longer than this – some of them quite a bit longer. In total, I conducted just under twenty-six hours of interview, with the average interview lasting about forty-two minutes (a figure that did not change significantly according to the seniority of the interviewee). Social scientists sometimes talk about achieving 'saturation' in the interview process – although it remained unclear to me, throughout the research, how this concept could actually be operationalized (Mason, 2010). Nonetheless, I aimed at between twenty and forty interviews, and I found that about thirty interviews produced a small number of consistent themes, whose basic contours and parameters were relatively solid and unchanging by the end (Baker and Edwards, 2012).

Within each interview, my method was to proceed inductively, using semi-structured interviewing around the initial key themes of: (1) why was it important to 'find' disorder in the brain?; (2) what were the barriers or problems to this?; (3) how did neuroscientific research affect people with autism; (4) why was there so little consistency in the autism-neuroscience literature?; (5) how did the social deficits of autism relate to the brain?; (6) what kind of 'thing' was autism? While I did not pursue a specifically 'grounded theory' approach (Glaser and Strauss, 1967; Charmaz, 2006), I did proceed without a strong hypothesis to test, and I worked under the auspices of a common-sense intuition that the data from interviews were dialogic, that they were being generated as I went along, and that much of the data likely did not precede my intervention in any significant way. Thus, I re-worked and revised my basic questions and themes as I progressed, and I proceeded without a strong distinction (either temporal or conceptual) between the empirical work of conducting interviews, the analysis of the interviews, and the formation and reformation of my basic research questions (an approach that many associate with 'grounded theory.' See Charmaz, 2003, for a contemporary discussion). This meant that these initial themes, and my way of approaching the themes, ebbed and flowed as the research progressed. For example, I quickly dropped the explicit theme on 'what kind of thing autism was,' because it made little sense to interviewees (and ontological questions naturally came up anyway, during discussion of the biological basis of autistic symptoms. See Chapter Two for a discussion). I also relegated explicit questions on the neuroscience of specifically social deficits of autism, and the effect of neuroscientific research on the lives of people with autism (although the former, for instance, came up when interviewees reflected on their status as 'scientists.' See the discussion in Chapter Five. The latter, more a question for people diagnosed with autism, ultimately never produced interesting data in these conversations). Similarly, after it emerged in conversation a couple of times unexpectedly, I added a theme of 'emotion' and 'affective relations' to my general scheme (and this forms the basis of Chapter Four).

All interviewees were provided with an information sheet and consent form (see Appendices One and Two), and all interviews were recorded with permission. Initially, the consent form offered interviewees a choice of anonymity or of being quoted by name; when none opted for the latter, and after it had produce some awkward interactions, I re-cast my consent form to guarantee anonymity only. I did subsequently offer the 'real name' option to some of the more prominent interviewees – and these all agreed. In the writing of the thesis, however, to remove a jarring inconsistency between named prominent scientists and an unnamed group below, I ultimately anonymised all interviews. All but three interviews were conducted on site in the interviewee's office or laboratory. Of the outstanding three, one was conducted via Skype, while two interviewees (both from the third sector) came to my office.

After an initial experiment with full transcription, I opted for partial and thematic transcription for reasons of time, working inductively through the recordings, and sometimes moving back and forth across them, as themes emerged and receded. The average resulting transcript was around 2,200 words long, producing a total interview-transcript word-count of just over 80,000 words.

Although I had already worked with a series of developing themes throughout the interviews, I formally coded the interviews using the qualitative analysis software, NVivo 8 (QSR International Pty Ltd., 2008). Although this re-produced the major themes I was already interested in, it revealed issues that were in the data but not prominent, and that may point to openings for future research (such as the place of gender, the role of families, and autism politics). It also arranged the major themes into sub-themes, which I then used as interpretive tools when compiling the arguments for each chapter.

At Appendix Three is a list of all interviewees. It arranges them according to short codes instead of names, and also giving the background discipline of each interviewee, an idea of their broad research area, their position, their gender, and the date and length of each interview. The individual code for each anonymous interviewee begins either with '3S' (for 'Third Sector'), 'P' for ('PhD Student'), 'L' (for 'Lecturer') or 'SS' (for 'Senior Scientist'). Each code also contains a number to mark out individual participants within each category (e.g. '3S02' or 'SS09'). Throughout the substantive material that follows, I will attach the relevant code to each interview-extract. This allows the reader, if they so want, to refer to Appendix Three, and to get some idea of the background and interest of the speaker. But it also allows the reader to work across my (necessary) tactic of isolating small extracts from interviews, and of building these into a coherent theme for each chapter or section. Using the codes, the reader can also trace contributions from the same interviewee through different parts of the thesis.

The specificity of the site

In the introduction, and at a couple of points during the thesis, I have positioned myself in terms of a desire to get close to, and take seriously, what it is that neuroscientists think they're doing when they're doing a neurosciences of autism. I have claimed that this gives a particular kind of view, otherwise not well recognised in the social sciences (of neuroscientific complexity, ambiguity, uncertainty, and so on). However, it needs to be stressed that mine is nonetheless a very specific 'site,' and not always the ideal one for a sociology of neuroscience. In particular, I want to flag a problem about the relationship between my site and where 'neuroscientific truth' is usually located. It seems reasonable to assume – and my data support this – that a neuroscientist in an anonymised conversation with an unthreatening sociology

PhD student might be unusually given to speculation, and to a relatively expansive view of their own practice. What people say about 'neuroscience' or 'autism' in my interviews is different to what they would say in an invited talk to the Royal Society, or to the National Autistic Society. It is also something different to what they would say in a journal submission, or in a textbook, or in an undergraduate lecture. While it is tempting to interpret my interview data as a 'backroom' view of neuroscience, the hard fact remains that neuroscientific truths are mostly made in public, and also that sometimes those truths bear only a tangential relationship to the kinds of expansive and intimate moments to which I was sometimes privy. In fact, far from being simply made in the laboratory, or resolved in smoky backrooms, neuroscientific truths are subject to much longer and more varied chains of negotiation – chains in which, of course, talks at the Royal Society play a major role, and idle conversations with precarious, half-sociologists rather less so (Callon *et al.*, 2011).

This is important, because throughout the thesis I am going to lean quite heavily on a view of ambiguity and uncertainty in neuroscientific research that is not, I claim, well-elaborated elsewhere. But it might be argued that these ambiguities and uncertainties do not, in fact, play a large role in the broader field of neuroscientific truth. It might also be suggested that sociologists of the new brain sciences who attend to these more grandiose and monolithic claims, and who diagnose 'an age of neurobiology' on this basis, may have a stronger grasp of 'where neuroscience is' than those insisting on attention to the complexity of research, and to the intellectual generosity of neuroscientists, and so on (Rose and Abi-Rached, In Press). This has a bearing on what follows: in Chapter Three, for example, I will criticise a group of scholars who take a 'critical' view of neuroscience (e.g. Choudhury *et al.*, 2009), based on their reading of neuroscientific journal articles, policy statements and so on. I will argue that this public literature is at odds with the tentativeness and care that are revealed in my interviews, and that, as an image of neuroscience, it should be treated with caution. All of this is true – but it is still unclear who, precisely, has gotten closer to the heart of 'neuroscientific truth.' Proximity to neuroscientists, and conversations about difficult research, which I rely on here, may prove to be a red herring. Moreover, even if it is a minority pursuit, neuro-reductive chauvinism is indeed a 'neuroscientific truth,' as is the more ambivalent and self-conscious 'neuroscientific truth' that I am in conversation with

– and the former, indeed, may prove more consequential in the long term (Churchland, 1986; Changeux, 1997).

This is not a fatal problem, but the reader nonetheless needs to be aware of the specificity of my site, and of the limitations that this specificity presents. The kinds of issues and concerns that I present here make sense within my site, and certainly have consequences beyond it. Still, I will try to be attentive, nonetheless, to aspects of these conversations that are invitingly frank, but that would nonetheless have little bearing on how the wider neuroscientific field is structured. I also hope to show that even where I cannot claim to talk about 'neuroscience,' the richness of my interview material, which gives new insight into an area of neuroscience not yet fully explored by sociologists, has nonetheless justified my remaining within this limited area – but this is a limitation that needs to be recalled all the same.

Disciplining neuroscience

Perhaps the most significant issue for a thesis proceeding on the basis of interviews with neuroscientists is that it was not always clear who was a neuroscientist, or what a neuroscientist was, or what a neuroscientist did. This problem appeared in two different ways, both of which need to be mentioned here.

First is the question of the different practices, methods, and histories, that are gathered together under 'the new brain sciences,' or simply 'neuroscience.' In the next chapter, I offer a very brief history of the emergence of what actually gets called 'neuroscience' in 2012. Here, though, I want to point out that I searched for interviewees among people who were doing autism research using any recognisable method of the new brain sciences. In retrospect, however, I was not either sufficiently alive to the range of things that *can* get called neuroscience, or to the narrowness of what usually *does* get called neuroscience within psychological and psychiatric research. In fact to the extent that about thirty people within my interview sample can fairly be called neuroscientists, a considerable majority are associated with 'cognitive neuroscience.' This is the branch of neuroscience most closely aligned to the history of psychology (as opposed to, for instance, either psychiatry or neurology), whose founding goal is the establishment of biological substrates for mental states (Gazzaniga and Bizzi, 1995). Moreover, cognitive neuroscientists are almost always neuroscientists only to the extent that they use brain-imaging technology (rarely pursuing, for example, studies of neuronal function in living animals, or molecular-level study of post-mortem human brains) – which allows the cognitive neuroscientist to correlate a measure of the brain simultaneous with the subject's experience of a specific mental state.

This is a limitation: my study on 'neuroscience,' for example, has little to say about neurogenetic research on autism, which people are certainly doing in the UK (Bolton *et al.,* 2004*;* Yates, 2012), and which has strong intellectual affinities with psychiatry[14]. In some ways, this limitation is unavoidable. For a disorder, like autism, still defined by behaviour and cognition, with no known genetic substrate of any of its core symptoms and with no available biochemical therapies, it is no surprise that neuroscientific research has up to now overwhelmingly taken the form of brain-imaging research. Nor is it a surprise that psychologists and cognitive neuroscientists – and not biological psychiatrists – have made up the majority of the field. Moreover, although brain-imaging and molecular neuroscience have many differences, and although they come from quite different epistemic cultures (Knorr Cetina, 1999; *cf.* Wilson, 2011), they do share a 'style of thought' (Abi-Rached and Rose, 2010) – which was marked, in my sample, by the disciplinary and methodological mobility of many of the people working in this field. But there is still a limitation on the ability of this thesis to generalise its discussion to an undifferentiated 'neuroscience.' And although I will frequently talk about 'neuroscience' and 'neuroscientists' in what follows, more often than not I am referring to cognitive psychologists who specialise in brain-imaging (often self-described 'cognitive neuroscientists') – and who may share a neuromolecular thought-style with neurogeneticists and neuropathologists, but perhaps not a great deal else.

The second disciplinary complication is the mobility just referred to: most of my interviewees had at least some nominal discrepancy between the field they trained in, the department that housed them, and the sub-area they saw themselves contributing to. It is entirely possible, for example, for an interviewee to have trained in psychology, to be affiliated to a department of psychiatry, and to describe themselves as a developmental cognitive neuroscientist. Indeed, to put some order on this, and in an early attempt to think about the difference, I tried to assign

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[14] In fact, neurogenetic research is currently moving into the vanguard of brain-based autism research (Ecker *et al*., 2012). See also Navon (2011) for a discussion of the way that genomic research may re-define the way diseases get distributed in the first place, with a special emphasis on autism.

interviewees both a ‘broad discipline’ (based on their training, or the name of their department) and a ‘specific area’ based on their job title or stated interest (see Appendix Three). But this needs to be taken with a pinch of salt: the most salient empirical fact is not who is a psychologist, who a psychiatrist, and who a ‘methods person’ – but that an all-encompassing neuroscience has dissolved much of the boundary between these categories, at least rhetorically. In what follows, I do not try to work against this tendency, conceding analysis to a generously-defined group of ‘neuroscientists,’ although the difference remains pertinent (see for example Chapter Five, which is about the struggle of psychology to achieve the status of a ‘hard science’). In practice, indeed, psychology was a dominant part of my overall interview-sample’s intellectual hinterland: again, leaving aside interviewees from the third sector, four of my interviewees could be associated unambiguously with psychiatry (three had training in psychiatry, two of whom still practiced; the fourth was housed in a department of psychiatry, and consistently described his work as ‘neuropsychiatry’). Three others could be unambiguously classed neither as psychologists nor psychiatrists, but as neuroimagers (in that their principle disciplinary affiliation was physics, and their affiliation was much more to the method than the disorder), and one was a molecular biologist. All other interviewees had some more-or-less defining affiliation with psychology.

There is certainly more work to be done on the relationship between these agglomerations, and on the intellectual and disciplinary mobilities that make them up. Not least, there is room for discussion of the politics of mobility in this space: quite a few interviewees affected indifference when I tried to establish the distinction between a psychiatric and a psychological approach (a question I eventually abandoned for want of data), and which I still find difficult to separate from the politics of health-research clustering in an increasingly neoliberalized and instrumentalized academy (Browne, 2010). Again, I contribute something to this discussion in Chapter Five, where I analyse the keenness of interviewees, and especially interviewees from psychology, to be identified as (‘hard’) scientists. But my general elision of the differences between disciplines remains a limitation to the study, and there are surely important tectonic differences that are papered over in my reluctant acceptance of the agglomerative term, ‘neuroscience.’

The Structure of the Thesis

The thesis that follows does not work towards its main argument in a linear fashion. I have instead pursued a more circular and descriptive strategy, layering different kinds of ambiguity and difference over one another, in order to paint an overall picture of an 'ambiguous' space of neuroscientific practice, and also to describe the labour of 'tracing' that works the neurobiology of autism across this field. Thus, the aim of each chapter is to set out, describe and theorise one of the key problems or ambiguities encountered by scientists working in this space, and to think about some of the ways that a neurobiological account of autism neuroscience is nonetheless traced through this sense of developing uncertainty.

Chapter One

In Chapter One, 'Public Issues/Private Bodies,' I preface the empirical discussions that follow by setting out some of the key over-arching theoretical commitments of the thesis. Instead of discussing these on their own, however, I try to work them through a discussion of some of the key features of both autism and neuroscience – which I draw from some of the more prominent sociological literature on these two. In relation to neuroscience, I first set out a commitment to seeing complexity and uncertainty in the translational imperative of the new brain sciences, particularly as these imperatives mediate the space between 'social' or 'mental' phenomena and those phenomena's biological substrates. Second, I set myself against 'critical' approaches to the new brain sciences, and I commit myself to 'thinking with' the neuroscientific practice under discussion. In the final third of the chapter, I turn to discussions of autism, where I argue against casting a judgment about whether autism is principally a social or biological entity, and also against conceding discussions of the emergence of autism, or the effects of that emergence, to either register on its own. Instead, I commit myself to thinking about autism as precisely a function of different entangled registers, even where those are sometimes quite contradictory, and hotly contested.

Chapter Two

In Chapter Two, 'What neuroscientists talk about when they talk about autism,' I begin my empirical discussion with the most basic issue still in question in this space, and this is the definition of what kind of thing autism actually *is*. I show that when I pursued this question with neuroscientists, I got two very different kinds of answer: on the one hand, autism was described as something that was 'biologically true' and that was composed of an 'unchanging core.' On the other hand, interviewees described autism as heterogeneous and separable, a 'symptom check-list' or an 'umbrella of convenience.' But rather than interviewees feeling the need to resolve this tension, I will show how the links between a biological singularity and a dispersed intangibility were traced together by thinking about autism through alternative registers. These were discursive registers that positioned autism as an 'enigma,' or that claimed it as a 'feeling' in an interaction, or that positioned it as something you 'just knew' when you see it. I will use my data to join with an emerging literature that sees 'neurobiologization' as a more various, complex and uncertain process than is sometimes allowed (Langlitz, 2010; Pickersgill, 2011) – and I will also argue that it shows how a broader process of 'biomedicalization' (Clarke *et al.*, 2003) may be made up of some surprisingly recursive and oscillatory moves.

Chapter Three

In Chapter Three, 'The trouble with brain-imaging,' I turn my attention to the ways that interviewees talk about neuroscience, and I show how this seemingly hard and reductive science, often associated with a kind of neurobiological chauvinism, is also sometimes deeply inflected by feelings of uncertainty, disappointment and even some anxiety. While there has been much written about the way that positive 'futures' and 'expectations' can both frame and organise scientific research programmes (Brown *et al.,* 2000; Borup *et al.*, 2006), I will use show that neuroscientific research on autism is sometimes structured by a kind of disappointment and ambivalence – a contradictory and hesitant attitude, in which research programmes are somehow assembled around a deflationary sense of *low* expectation. I will argue that a 'tracing' neuroscience, one that proceeds through a strange entanglement of confidence and ambiguity, may well be bilingual in discourses of both promise and failure – and that discussions of 'expectations' in neuroscience must take more account of the latter. To further elaborate on the co-presence of these ways of talking about neuroscience, I will draw particularly on the work of Karen Barad (2007), to argue for a kind of efficacy undergirding these different senses about the future of neuroscience.

Chapter Four

In Chapter Four, 'Then it hit me in the stomach,' I pay particular attention to the complex and careful ways that categories of 'knowing' and 'feeling' can get mixed up in the neuroscience of autism. I begin by showing how interviewees sometimes gave accounts of their own intellectual biographies that mingled a commitment to good science with the affective and emotional aspects of scientific work. In thinking about the ways that neurobiological research on autism is related through registers of upset and heartbreak, desire and excitement, and also a 'visceral' commitment to research, I draw on the work of Elizabeth Wilson (2004, 2011, 2012) and Natasha Myers (2012) to argue that autism neuroscience shows how categories of both thinking and feeling, of cognitive-work and body-work, can be carefully traced over one another in the space of a contemporary bioscience. To situate this claim more precisely, I go on to consider the argument in light of AN Whitehead's (1935, 1964, 1979) discussion of the role of emotion in bringing 'things' together. I argue that what we see, here, is an intellectual project that is, in some sense, carefully structured by the maintenance of ambiguous relationship between affective and cognitive labours.

Chapter Five

Chapter Five, 'Falling in the middle,' is slightly anomalous, in that it shows the discussion of tracing running into something of a limit. In this chapter, my interest is in the ways that my psychological interviewees, in particular, characterised psychology as a discipline once embroiled in 'Freud and faff,' but now, under the aegis of a physics-based cognitive neuroscience, become unambiguously scientific. Because it was so often related through either personal or disciplinary history, I approach this material via Georges Canguilhem's idea of 'recursive' history (Lecourt, 1975; Rose, 1996a) – and I suggest that these claims to science might be heard as forms of boundary-working within a contemporary psychology. I argue that what specifically troubles the newly 'scientific' border-lands of psychology is the re-emerging salience of something like 'social context' within the purified space of a cognitive neuroscience. This has come from an attention to epigenetics and to gene-environment interactions within psychological research, and also from the complex 'social' and 'cultural' claims that have to be accounted for in research on the autism spectrum. I argue that rather than being a limit to the careful work of 'tracing' already described, these incongruous claims to science are also ways for interviewees to think about the border-trouble produced by just this tracing neuroscience (Gieryn, 1983). I conclude with a suggestion that moving across these boundaries is not always an easy or a graceful procedure.

Chapter Six

Throughout these chapters, I have talked about autism neuroscience as a practice of ceaseless ambiguity – a way of putting and holding different things together, even where they are not obvious bedfellows. In Chapter Six, 'There's a unicorn,' I delve deeper into this claim, to think more concretely about the mechanics of actually tracing things together within neurobiological autism research. Starting with interviewees' insistence that autism is a 'multi-level thing' that has to be appreciated on quite different 'levels of understanding,' I consider a series of metaphors used by interviewees for their own neuroscientific practice – where they variously described it as a process of 'tangling,' 'assembling,' 'connecting' or 'shuffling.' Drawing on the work of Bruno Latour (2004, 2008), Karen Barad (2007), and Donna Haraway (1997), I argue that that these metaphors are ways of talking about a 'contingent stability' in autism neuroscience – and that they are ways of characterising a practice that actually allows autism to be traced across precisely these different levels. Leaning in particular on Latour's account of 'immutable mobiles' (1987) and also on Haraway's (1997) commitment to forms of 'relay' and 'exchange' in the spaces of technoscience, I argue that these explicit tracing practices go to the heart of the ambiguities and differences that I have been describing. My claim is that this is both a key activity of autism neuroscience in its own right, *and* an explanation for what lies beneath the sometimes fuzzy-looking commitment to difference and ambiguity that I have been trying to describe, otherwise, throughout the thesis.

Conclusion

I conclude the thesis by re-considering some of the underlying themes that emerge throughout these chapters. These themes are: the role of complexity and ambiguity in the space of neuroscience; the specific contribution of my account of 'trace'; the presence of 'neuropsychological naturecultures' that this tracing practice works through; the specificity of sociological space and the limitations of the thesis; and the figuration of autistic presence in the thesis as a whole.

1. Public Issues/Private Bodies – sociologies of neuroscience and autism

Introduction

While this thesis intervenes in a range of different literatures, its central contributions are aimed at two in particular. The foremost of these is the sociology of neuroscience – which can lay some claim to having been part of the first 'critical' swathe of sociological interest in science and technology (Lynch, 1982, 1985), but which is more strongly embedded in a complex literature that emerged somewhere between the twentieth and twenty-first centuries (Beaulieu, 2000a; Dumit, 2004; Roepstorff 2004). The other is a more recent, and more small-scale sociology of autism research – which, and really only in the last half-decade or so, has gradually emerged from the establishment of autism as both an increasingly visible fact of social life, and an increasingly fraught matter of public concern (Grinker, 2007; Eyal *et al.*, 2011; Silverman, 2011).

My goal for this chapter is two-fold: first, I want to introduce the reader to both neuroscience and autism, to do this primarily via a reading of current sociological debates around both, and then to situate the thesis within these debates. Second, however, I want to use this forum to set out the very broad conceptual framework that the thesis is working under, and also to make explicit its commitments within this framework. Gathering these two goals together, there are three basic claims in what follows: in the first section, 'Neuroscience,' I show that much sociological literature has concerned itself with a translational imperative within the new brain sciences, but this literature has not yet elaborated the complexity and uncertainty at the heart of these translations, and has sometimes missed the 'contingent stability' of neuroscientific objects (Haraway, 1997). In the second section, 'Critique,' I show that much of this literature has also worked on uncovering hidden technical or cultural biases within neuroscience, and has therefore missed the degree to which neuroscience might be seen as a 'resource' for social theory (Wilson, 2004). In the third section, 'Autism,' I show how much of the sociological literature has been concerned with parsing the sociological from the biological in autism research, and has thereby missed the achievement by which this research has actually managed to 'entangle' these different areas (Barad, 2007).

There is a theme common to all three of these, which I have tried to capture in the title of the chapter, a re-casting of C. Wright Mills' (1999 [1959]) famous programme for sociology – i.e. that private troubles not be held apart from public issues. By similarly doubting any meaningful disaggregation of 'public issues' from 'private *bodies*,' I draw attention to the overall gesture of the chapter, which is to seek affinity with an emerging sociological attention to the life sciences – one for which the public world of social and cultural issues is not placed in opposition to the biological workings of individual and collective bodies. A major presupposition of neurobiological autism research, this will also be a major theme of the thesis.

Neuroscience

The goal of this first section is to establish what I mean when I say 'neuroscience,' and to situate the thesis within some of the more well-known sociological literature that has already attended to it. In particular, when I use terms like 'neuroscience,' or 'the new brain sciences,' I am gesturing at quite a recent intellectual practice. In fact 'the new brain sciences,' only developed in the second half of the twentieth century (Abi-Rached and Rose, 2010). Moreover the essential features of this practice (the use of new molecular-level technologies, the formation of an interdisciplinary space, and an increase in funding, prestige and popular interest) can more likely be traced to the proliferation of low-cost brain-imaging technologies from the 1980s, and, in the United States, the declaration of the 'decade of the brain' in the 1990s (Jones and Mendell, 1999; Andreasen, 2001). In associating 'neuroscience' with 'the new brain sciences,' I follow Steven Rose's attention to two particular aspects of contemporary neuroscience (2004: 4). First is its novelty – in terms of methods (for cognitive neuroscience, primarily brain-imaging), aspirations (the neurogenetic explanation of human behaviour), and epistemic prestige (in both scholarly and popular discourse). Second is its plurality – as I noted in the methods discussion, today's coherent neuroscience is, at its core, a radical and sometimes uneasy series of agreements on problems and methods between sciences like anatomy, genetics, imaging physics, and psychology.

To Rose's account, I would add a third mark of distinction, and this is the deepening degree of financial and political investment in a mostly-undefined 'neuroscience' as such. Consider that in 2009/10, funding for neuroscience from the two major contributing UK Research Councils alone was about £163m – near enough the same allocation for everything joined under the Economic and Social Research Council (Medical Research Council, 2011; Biotechnology and Biological

Sciences Research Council, 2011; Economic and Social Research Council, 2010). These financial investments are equally matched by a staking of political capital from public institutions (Foresight Mental Capital and Wellbeing Project, 2008), and also an accruing neuroscientific cultural capital, seen in an emerging discourse of neuromarketing and neuroeconomics (Schneider and Woolgar, 2012), neuroaesthetics (Cinzia and Gallese, 2009), and neurophilosophy (Churchland, 1986). These developments and their implications are not central to my concern, but have been much discussed elsewhere (Illes, 2003; Rose, 2005). However this is the broad field that concerns the sociological literature that I am trying to isolate – a literature that has emerged at the very end of the twentieth century, and developed through the first decade of the twenty-first.

If I were to isolate one over-arching theme of this literature, I might say that it has often been concerned with the ways that the technologies and rhetorics of the new brain sciences have sought to *translate* some hard-to-define, qualitatively distinct, intangible social or cultural category, into a more organic and definable structure or function of the human brain. This has particularly been the case for sociologists who, like me, have engaged with the new brain sciences as they have emerged through new forms of biological psychiatry, neuropsychology, and psychopharmacology (Singh and Rose, 2009; Pickersgill *et al.*, 2011). In this first short section, I want to angle the thesis as a contribution to this discussion particularly. The thesis is an account of some of the ways that autism, clinically identifiable only in the behavioural and cognitive domains, might be located in the brains of autistic individuals. As I described it in the introduction, my attention to this on-going neurobiological concern has given me a particular insight into the complexity with which such a translation is sometimes pursued – i.e. that it is beset by uncertainty, ambiguity, contradiction and precarity. As the thesis progresses, I will argue that neurobiological translation can be a process of learning to live with complexity, and with the 'contingent stability' of neurobiological accounts (Haraway, 1997). This view is at odds with much of the sociological literature on similar translations, which lean towards the view that the neurosciences actually reduce complexity in the name of a narrow biological certainty (Martin, 2004; Ortega and Vidal, 2007). My first commitment, then, is to draw attention to the complexity of neurobiological translations, and to suggest that recognising this complexity involves re-thinking whether, for instance, words like 'essentialism' or 'reductionism' are adequate to describing this process (Williams and Cockerham, 2010; Cromby *et al.*, 2011;).

The emphasis on translation can be traced to the first generation of ethnographers who engaged with the new brain sciences. For example, Beaulieu (2000a, 2000b) suggests that what is truly at stake in the emergence of brain-imaging, in particular, is a move from thinking about things at the level of the mind, to actually 'looking' at them at the level of the brain. Accessing the mind,' she argues, 'is [now] presented as a matter of technology to look at the brain' – to the extent that, 'in the course of developing a mapping practice, a different object, the mind-in-the-brain, is constituted' (2000a: 7). Beaulieu's central point is that a translated 'mind-in-the brain' is constituted through a two-stage process of 'biologisation,' in which the mind is assumed to be a functional aspect of the body, and 'digitalisation,' a technological passage that produces facts through a particular configuration of numbers and images (ibid.: 12). Thus, neurobiological translation is heavily dependent on an imaging technology that fixes new material and organic configurations within the laboratory (ibid.: 63). It is precisely through this process, Beaulieu argues, that 'digital representations have served as the context into which mind (as differential blood flow) and brain (as anatomy) can be translated, coming to exist as two features of a voxel' (ibid.: 166-7; a voxel is a sort of three-dimensional pixel, used for the visual representation of fMRI data).

What clearly interests Beaulieu is the ease with which complexity gets hived off in the course of this translational imperative: in her account, producing an image of brain and mind is a relatively smooth, unidirectional process – and not one that admits of much uncertainty or ambiguity. Andreas Roepstorff, similarly, describes the way that neuroimaging data 'pass through long chains of mathematical transformations…at each step they lose locality, particularity and materiality while simultaneously gaining compatibility, standardisation, and relative universality' (2004: 1108). Simon Cohn, another early ethnographer of the new brain sciences, argues that the key development of brain scans is that they are 'evocative of a human existence in its totality, of the essence of what makes us conscious and social, of *bios*' (2004: 55). Neuroscience, suggests Cohn,

> potentially presents a new world of equivalence – in which it is impossible to distinguish life from information, or define the living as against the non-living. It correspondingly may be blurring the distinction between people and objects, forms and

> processes, commodities and moral principles…the reductionist trope of the biological and medical sciences thereby threatens our core conception of life itself' (ibid.: 53).

The most important quality of brain-scanning, for Cohn, is that 'all external signs of life must be eliminated' in order to produce a 'convincing, cinematic illusion' (ibid.: 62-3). Thus, 'life is effectively being recast,' as 'life devoid of context and sociality…from which humanness is abstracted, yet which nevertheless claims to represent it' (ibid.: 70). And this is not only a deliberate, but also a very successful, translational move: 'what really distinguishes the scan,' notes Cohn, 'is the success of its elision,' such that 'there are fewer and fewer alternative perspectives' and 'the traditional boundaries between science and the social sciences are regarded as redundant' (ibid.: 71).

I want to draw attention to two things here: one is the establishment of a translational imperative as a key dynamic of the new brain sciences (note also that, for all the authors just cited, this view is heavily embedded in a concern with brain-*imaging* technology particularly). Second is the description of this practice as a more-or-less straightforward and one-directional loss of complexity and richness. My concern, though, is that amid this early surprise and concern with the very presumptuousness of making digital images of categories like 'mind' and 'life,' we may have missed the degree to which this effort is, in fact, fairly difficult and precarious. For the first anthropologists of the brain-imaging laboratory, an imager's loosely-expressed ambiguities about whether, in fact, a voxel really does identify an element of a mental state, may have looked like poor consolation. But my worry is that the sociology of neuroscience is still too much in shock about all of this, and that it sometimes misses the intricacy of the labour that actually goes into pulling these images together. Dumit, for example, also points to the emergence of something new (in this case, a translated 'objective-self') – but he argues that personhood remains a 'lived categor[y],' and one dependant on the uneven making and acceptance of 'facts in the world' (2004: 7, 88). Thus, for Dumit, whatever the power of voxels to lose complexity, we remain in a world of 'multiple circuits of theory transfer from laypersons to experts and back again to laypersons via all kinds of mediators' (ibid.: 99). In other words, we are still in an era of 'contests over the true nature of human nature,' and not in a space of 'technoscientific domination' (ibid.: 12). While Dumit is not as focused on ambiguity as I am, his account is nonetheless an important stepping-stone towards putting the 'digital image of the category of the person' back into perspective (Dumit, 1997)

A second tranche of relatively early sociological writing on the new brain sciences has also characterised them as processes of turning one thing into another. This literature focuses more on a 'cultural' level of popular and journalistic discourse, legislative and policy networks, patterns of diagnosis and prescription over time, health-service re-structuring, and so on. What these sociologies have identified are not so much specific translations at the level of the brain-area and the voxel, but much broader movements – for example, at the level of the person and the subject. Nikolas Rose (2001b, 2003, 2004), in a series of contributions, has charted the emergence of what he calls a 'neurochemical self.' As part of a large-scale consideration of changes within biological psychiatry, and setting out in particular from an analysis of the growth and spread of psychopharmacology in the Anglo-American world (2004), Rose has argued that

> the thought style of contemporary psychiatry has become inextricably bound up with developments in neuroscience that posit a specific anomaly in the brain – most frequently in one of the neurotransmitter systems – as the ground of each specific anomaly in mood, cognition, affect or conduct (2001b: 4).

This thought-style is centred on 'a new objectivity' in which 'psychiatry claims to have overcome, at last, the Cartesian dualism of body and soul' – which has consequences for how those so marked come to think of themselves (ibid.: 9). Considering the moves from 'sadness,' to 'depression,' to 'chemical imbalance,' for example, Rose argues that a

> recoding of everyday affects and conducts in terms of their neurochemistry is only one element of a more widespread mutation in which we in the West, most especially in the United States, have come to understand our minds and selves in terms of our brains and bodies' (2003: 46).

This 'mutation' in everyday life is certainly another way of talking about the translational effects of the new brain science. At this level, there is least some thematic similarity (although also a considerable difference in perspective) between Rose's account, and what some other, more self-consciously critical scholars, have described as the 'cerebral subject' (Ehrenberg, 2004) or 'brainhood' (Ortega and Vidal, 2007) – the latter for example, a configuration 'that embodies the idea that the human being is essentially reducible to his or her brain,' and against which the analyst must 'sketch its history and map its multifarious presences in contemporary culture' (Ortega and Vidal, 2007: 255; *cf.* Ortega and Vidal, 2010; Ortega, 2009; Vidal, 2009).

For all their differences, these are all accounts of neurobiological translation: the self, once dispersed as a duality of body and soul, is now legitimately claimed to be all body. But again, what is passed over, here, is the labour – I am claiming, the difficult and uncertain labour – of compiling and tracing neurobiological accounts of the subject. In one sense, this may be a trivial observation. These accounts are trying to describe a broad field of emergence, and the descriptions are neither concerned with, nor necessarily contradictory to, whatever worries and uncertainties are happening at the level of neuroscientists' discussions of their own practices. But it is still worth worrying about the degree to which accounts like 'neurochemical self' or the 'cerebral subject' might actually scale down. Without sufficient attention to the disparity between neuroscientists' accounts and neuroscientific discourse, we risk a (semi-ironic) sociological and historical black-boxing of the very difficult work of neurobiological translation, and of the fuzzy logics of contradiction, uncertainty and anxiety that reside therein.

However, recent years have seen something like a 'second wave' of sociological interest in the neurosciences. Two things distinguish this scholarship: (1) with much of the broad field already sketched out, it tends to focus not on 'neuroscience' as such, but on specific, small-scale neuroscientific interventions; (2) with the eliminativist-materialist rhetoric of some of the earlier commentators on neuroscience (Churchland, 1986; Changeux, 1997) now more or less run aground, it has found scope to engage with neuroscientific practice without too much suspicion. Pickersgill, for example, in a discussion of the emerging neuroscience of psychopathy and the anti-social has identified a '"technosomatic" imperative…that privileges both the body as a locus for psychopathology and technology as a means of visualising it' (2011: 449). And he argues that this emerging privilege is animated by hopes of efficacy and therapeutic promise, which form part of a more large-scale 'rapprochement between neuroscience and psychiatry' (ibid.: 450). But in his interviews with neuroscientists, Pickersgill is also very much alive to the difficulty of pursuing both this imperative and this rapprochement, and of the precarity of its success: while there may exist a broader 'promissory discourse pertaining to the faculty of neuroscience to improve the clinical management of severe psychopathologies,' he argues,

> the embedding of this therapeutic promise within the talk and practice of scientists and mental health professionals is far from straightforward…

> neuroscience engenders considerable ambivalence, expressed both by clinicians and scientists themselves (ibid.: 460).

Similarly, in a study of neuroscientific research on hallucinogenic drugs, Langlitz has described his realisation that 'the prevalent objectivist image of cognitive neuroscience had to be qualified to apply to this particular case – and possibly not only to this case' (2011: 40). In her analysis of the neuroscience of Attention Deficit Hyperactivity Disorder [ADHD], Rapp shows how, for the neuroscientists in her study, 'data are highly abstract, provisional and negotiated' (2011: 16), while Vrecko has argued that 'analyses need to pay closer attention to developments arising within the context of the laboratory-based brain sciences…and should strive to "take biology seriously" (2010a: 54). All of these are quite distinct form earlier work on the translational imperative of neuroscience, which was keen to capture the alarmingly smooth-looking transition from complexity to simplification.

This renewed focus – all of these were published since 2010 – which emphasises that things may not always be so smooth, creates space to begin thinking about the intricacy of neuroscientific translations, and also the ambiguities and uncertainties that sometimes structure them. It also creates space to focus on the rich complexity of neurobiological work, and of what is involved in it: as Rose and Abi-Rached argue,

> at their most sophisticated, [the neurosciences] are struggling towards a way of thinking in which our corporeality is in constant transaction with its milieu, and the biological and the social are not distinct but inter-twined (In Press).

My contribution to this emerging discourse is to focus on the persistence of difference and ambiguity, particularly, within these nonetheless very sophisticated neuroscientific accounts. But all of this requires a particular attitude to the new brain sciences, and this is one that is not dominated by 'critique.' In fact, attitudes to critique mark a genuine emerging fault line in the sociological study of neuroscience. This is what I want to turn to in the next section.

Critique

When I refer to a sociology of neuroscience oriented to 'critique,' I am describing a mode of writing that looks for an unconscious or hidden bias within the new brain sciences, and one that particularly seeks to uncover some social, political and economic agendas within them. Of course, this view of science as being irretrievably embedded in cultural practices and norms, and of scientific knowledge as essentially a social product, is more or less the foundational insight of the social studies of science and technology, itself a discourse with much internal contestation – and I will not re-run those arguments here (see Pickering [ed.], 1992, for classic statements). In this section, I will discuss some of the specifically critical approaches of sociologists, and other social scientists, to the new brain sciences. Despite an emerging 'second wave' suggested above, these are still prominent, and perhaps even dominant, within a broadly-conceived 'sociology of neuroscience.' But what I also want to do, in this section, is to set out another of the over-arching conceptual commitments that undergirds the thesis – and this is a commitment to not seeing the new brain sciences as overwhelmingly 'cultural' practices. My goal, instead, is to consider them without a pre-determined recourse to explicitly sociological categories of power, or discipline, or political economy – or some other pre-eminent ideological fixation, in sore need of a sociological corrective. This is not because I think these categories unimportant. Rather it comes from a more basic recognition that such categories, being also deeply entangled with material and scientific agencies, cannot be deployed to 'explain' these practices, or their objects, from the outside. My goal, instead, is to remain faithful to the kinds of debates, arguments and positions (and even critiques) that make sense to my interviewees – positions, moreover, and as I hope to show, that emerge from their reflections on, and struggles with, precisely the differences and similarities wrought by the 'material-semiotic' entanglements that structure the new brain sciences (Haraway, 1988: 588).

There are several different ways to practice socio-critique within a sociology of neuroscience. In her ethnography of clinical Magnetic Resonance Imaging (MRI) suites, Kelly Joyce (2008) argues that this imaging practice, typically characterised as a transparent visualization that reveals the inner body, is, in fact, a cultural agglomeration – whose black-boxed techniques and calculations actually work to *produce* the very body that it claims to uncover. 'There is nothing natural or inevitable,' says Joyce, 'about the current form of MRI scans or the design of the technology itself' (2008: 20). Moreover, deciding to represent the body or the brain on the basis of 'how hydrogen nuclei absorb and release energy in response to particular frequencies' is neither obvious nor straightforward – being founded on both a cultural preference for visualization, and an economic-military imperative that drives research in atomic physics (ibid.: 25). Thus, Joyce argues, MRI images, ‘do not *reveal* the inner body, but instead *produce* the body’ (ibid.: 48. Emphasis in original).

Similarly, Burri describes the anatomical figure produced by MRI-imaging as a ‘sociotechnical anatomy’ (2007: 110). ‘Bodies,’ Burri argues, ‘are constituted in medical imaging [through] the sociotechnical practices and constellations in which the image production takes place’ (ibid.). Drawing on a powerful image from Elizabeth Grosz (1994), Joyce suggests that MRI images ‘“etch together” local decisions and priorities, technology, and aspects of the physical body to produce what is perceived as cutting-edge, authoritative knowledge’ (ibid.: 70). It is the gap marked by the word ‘perceived,’ here, that really drives Joyce’s account. Her position is not that MRI is intrinsically bad or wrong, but that rhetorics of transparency, driven by economic and medical-ideological priorities, obscure what is really implicated in the production of the image, and what can actually be seen. Joyce’s point is well taken – she is surely right to be worried about this gap, and to think about who wants to erase it, for what purpose and what the effects might be. But I have another worry, which is that in the rush to establish what MRI *isn’t*, there’s a risk of missing the force of what it *is*. And this is a technology that precisely *can* ‘etch together’ local politics, de-oxygenated blood, sick bodies, nuclear physics, and the clinical gaze, to produce a convincing image of a person, and a body (which are both, of course, themselves, and well prior to their engagement with the medical imaging suite, invariably shot through with political priorities, clinical desires, and material configurations). If this is a problematic achievement, it is an achievement all the same – certainly one that stands up well against the social theory produced in the same period.

A related mode of critique focuses less on these specific biases, and more on the larger cultural picture that the new brain sciences reflect and co-produce. For Burri, the emergence of brain-imaging is inseparable from the disciplining of bodies:

> the machine exclusively accepts precursorily disciplined, specifically formed bodies which have already been adapted prior to the MRI examination… the patient’s body is now fixed, stabilized and made immobile – that is, it is disciplined and made instrumental’ (2007: 113).

Similarly, for Vrecko, to talk meaningfully about neuroscience is to ‘analyse formations of knowledge, power and subjectivity associated with the discipline and its practical application’ (2010b: 1) – while Brennikmeijer (2010), urges attention to the emergence of the new brain sciences as a site for developing techniques of the self. For Meloni, meanwhile, we need an 'understanding of the anthropological consequences of the neuroscientific programme, its current circulation in society as a cultural-material narrative,' in order to 'problematize the current intellectual prestige of the neuroscientific vocabulary, manifested in our epoch's urge to neurobiologize previously 'cultural' or 'sociological' phenomena (2011: 310). While for Williams and his colleagues,

> developments in neuroscience and neurotechnology clearly raise a host of important issues for sociology and society at large which take us beyond the realms of medicine, not least of which concerns the very notion and nature of what it is to be 'human', including fundamental questions of human consciousness, will, intentionality, affect, selfhood, behaviour and so forth (2012: 249).

I don't claim that these authors offer any kind of simplistic or un-nuanced critique of the new brain sciences. But I *am* trying to locate a particular critical perspective on neuroscience, common to this literature, and this is a perspective that analyses these practices through the lens of a much broader series of social and cultural developments.

Emily Martin's engagement with the new brain sciences has been exemplary on this score (2000, 2004). Martin has identified, amidst the production of new knowledge throughout the brain sciences, the emergence of a figure, 'neuronal man,' whose levels 'begin with molecules, but go no farther than the central nervous system' (2000: 574). Through the emergence and promulgation of this figure, Martin argues, 'all of what anthropologists call culture has drained through the hole and dissolved in the realm of neural networks' (ibid.: 576). With cultural and social explanations for phenomena all but ruled out by definition, Martin argues that 'the brain becomes sovereign' and also 'generative of everything humans do' (ibid.: 574-577). But Martin locates the cultural and institutional desire for the 'restraining force' of this 'ahistorical concrete body' in unambiguously social developments: for example, in the need for a reaction to the mania and wildness of *fin de siècle* capitalism (ibid.: 576, 581), or in psychiatric-expert attempts to 'snare' both the 'criteria of rationality' and the 'meaning of language' (2004: 194). Thus, Martin's focus is on 'the environment we live in (and that scientific theories are produced in it) [which] had shifted so that a brain-centred view of a person began to make cultural sense' (ibid.: 200). There is an important point, here, i.e. that neuroscience cannot be isolated from the peculiar developments of capitalism in the late-twentieth and early-twentieth-first centuries, nor can it claim innocence of the systems of governance and intervention which are sometimes well-suited to its not-always-benign ends (Rose, 2001a). But, once again, my worry is that cultural critique has a tendency to over-state what is materially true; and if the brain is said to be generative of everything humans do, or if neurobiological discourse re-imagines what is culturally true about humans in general, still neither of these claims can be made with great fidelity to current neuroscientific research. If nothing else, and as I suggested earlier, the distinction between these modes needs to be made much more sharp.

But there is a deeper point embedded here, and this is about the tendency within some critiques of the new brain sciences to extend this observation into an argument for the ontological primacy of the cultural over the neurobiological – and thereof a demand that neuroscientists attend to the social and political biases with which their practices are badly entangled. This is the practice of critique, precisely as Bruno Latour describes it, as 'trying to detect the real prejudices hidden behind the appearance of objective statements' (2004: 227). Fernando Vidal, for example, has argued that cultural and rhetorical attempts to materialize personhood, or to locate some organic and naturalized account of the self, in fact long precede the emergence of the new brain sciences – that this is an ideology onto which neurobiology is mapped *post hoc*. For Vidal,

> whether ontological or methodological, the belief in brain-self consubstantiality seems to have impelled brain research. The idea that 'we are our brains' is not a corollary of neuroscientific advances, but a prerequisite of neuroscientific investigation (2009: 7).

This is an important distinction. In Vidal's account, what needs critique is less the weird after-life of research on the brain, but neuroscientific research *as such*. 'Brainhood' (Ortega and Vidal, 2007) actually comes before neuroscientific practice, and not after. A related argument has been made by a group of scholars who argue for what they call a 'critical neuroscience' (Choudhury *et al.*, 2009; Slaby and Choudhury, 2011; Choudhury and Slaby, 2012. See also my discussion of this literature in Chapter Three). The essence of the 'critical neuroscience' argument, which roots itself within a Frankfurt School tradition, is not to tear down neuroscience, but instead to inculcate among neuroscientists,

> self-critical practices, which aim to achieve reflective awareness of the standpoint-specific biases and constraints that enter into the production,

> interpretive framing and subsequent application of neuroscientific knowledge' (Choudhury *et al.*, 2009: 65).

In other words, neuroscience is *itself* to be reformed as a critical practice – insofar as it must become aware of its own political and economic standpoints and drivers. But neuroscience must *also* harness the 'emancipatory potential' for neuroscientific workers to reflexively work on the biases embedded in their own practices (ibid.: 65). The key, here, just as it is for Emily Martin, is to view 'neuroscience itself as a cultural activity' (ibid.: 62-64). By re-thinking neuroscience in this way, Choudhury and her colleagues re-situate neuroscience within a 'social structure,' and re-formulate it as a practice run through with economic drivers, political climates and cultural contexts (ibid.). 'Brain facts,' they point out, are not 'objectively given things-in-themselves but emerge from communities of scientists working collectively at a given time in a given context' (ibid.). Again, the distinction is important – the argument is not that brain-facts (on-the-loose) *become* cultural products, but that they *are* cultural productions, by definition, and that neuroscientists need to begin taking account of this.

I am concerned about the unwillingness, within this critical literature, to see the potential novelty and productivity in the awkward and sometimes troubling ways that 'culture' and 'neurobiology' can be traced together– and not as separate-but-equal domains of signification, but as deep entanglements of matter and meaning. This critique is founded on a requirement that the social somehow be *prior* to the claims of neurobiological research, in any instance of novel interaction between politics, culture, bodies and science. But such a view can only be premised on maintaining a strict separation between these realms, a position now long-since thrown into doubt (Whitehead, 1964; Haraway, 1991; Latour, 1993). I set myself against this kind of critique because I think it actually half-sees, but then significantly misinterprets, the complexity and the subtlety of what's truly at stake in the new brain sciences, i.e. a practice of carefully thinking through, and working on, the connections between bodies, brains, subjectivities, lives, machines, and so on. My claim is that trying to get a handle on this process, rather than re-asserting the boundary, may, in fact, open significant new vistas for scholars who are willing to dally at the margins.

This view has some direct and pertinent forebears in discussions of neuroscience. In particular, Elizabeth Wilson's *Psychosomatic* (2004), which is aimed at working the neurological back into feminist accounts of the body, and to feminist theory more generally, clearly sets out the empirical benefits of *not* trying to purify the space of either social theory or the new brain sciences. What is at stake for Wilson in a theoretical mingling with neurology is the 'potential in the neurosciences for reinvention and transformation' (2004: 13). She argues that between psychology and neurology 'forces of influence and determination are more mutually entangled than the critics of neurological determinism have hitherto acknowledged' (ibid.: 16). Taking as exemplary Freud's account of neurasthenia, which envisions a circuit of nerves-penis-cortex-psyche, Wilson argues that we do not find a relationship of simple causation, rather we have 'a system of mutual constitution from which no particular element emerges as the originary, predetermining term' (ibid.:19). If the psyche is nervous, Wilson reminds us, so too are the seminal vesicles psychic (ibid.: 19-20). Thus: 'neurological material is more confident, flexible, resilient, and assertive than many critics have yet acknowledged' (ibid.: 22). This is what social theory needs to re-discover; and this is what an ethic of socio-critique prevents us from seeing: 'by disconnecting biology from its constitutive relations with other ontological systems,' argues Wilson

> biology becomes isolated and destitute. The barriers behind which biology has been sequestered do not annul the secondary relations that biology has on other systems (e.g., the effect of neurotransmitter uptake on psychological mood), and it is these kinds of causal relations that neo-Darwinian commentaries seek to exploit. These barriers do, however, obstruct the operations of a more originary relational network…within which biology is constituted, animated and evolved' (ibid.: 70).

I am in pursuit of this network in what follows – and equally in pursuit of an 'articulate, obligated, libidinized' neuroscience, which 'may even be a resource for theoretical endeavour, rather than the dangerous and inert substance against which criticism launches itself' (ibid.: 29). Wilson is not alone, here: Catherine Malabou (2008), for example has also argued that recent developments in the neuroscience of plasticity, taken seriously, actually call for a renewed attention to theory and interpretation. Barbara Stafford (2007) has argued for a genuine co-constitution, at the level of theory, between ways of thinking about art and art-historical practices, and work within the new brain sciences. Andreas Roepstorff (2001) has tried to use his dual identity as a brain-imager and cultural anthropologist to re-animate the animalistic, world-experiencing 'biophilosophy' of Jacob von Uexküll. I am not claiming that these are all equally successful or above criticism. But they do all exemplify the kind of spirit in which this thesis is written, and in the name of which I have made a commitment precisely against a ‘critical’ view of the new brain sciences.

What follows is not a bold attempt to wring theory from the kinds of connections that are evident – and they are evident – in a neuroscience of autism. My central commitment here is more modest, and it is to a kind of negative methodology, or a resolution to approach this space without an ethic of socio-critique. This means that I will neither seek to reveal what these sciences cover over, nor to read them through the language of neuro-reduction – nor will I try to parse good biology from bad social influence. My basic hope is that the description made possible by this negative methodology will contribute to the burgeoning literature that takes neuroscience as a resource for interpretive social science. I do this mainly by providing an account of ‘tracing autism’ that would have been difficult to see without it.

Autism

In this final section, and to situate the discussions that follows, I want to first develop my earlier account of what autism is – at least to the extent that it has been figured by a (neuro)scientific research practice. In clinical and research settings, autism is typically described as a developmental disorder with characteristic features in three core domains: social interaction, communication, and range of interests – with a diagnosis requiring some element of each (APA, 2000; WHO, 2007). In typical case descriptions, ‘social interaction’ is often glossed as a tendency towards idiosyncratic, or 'atypical' forms of interaction with other people, seen in early-childhood symptoms such as not making eye contact with parents in infancy, not enjoying the sensation of being held as a baby, not trying to gain or share a parent's attention as a toddler, and so on (Kanner, 1968 [1943]; Wing and Gould, 1979). The ‘communicative’ features indicate anything from a delay in spoken language to developing no spoken language at all, to communication through inanimate objects, to 'atypical' patterns or styles of speech, such as a preference for echolalia and repetition (Baggs, 2007; Ochs & Solomon, 2010; Schopler and Mesibov [eds.], 1985). 'Repetitive interests' describes a tendency to become deeply interested (to the exclusion of more typical interests) in sometimes random-seeming aspects of life (Baron-Cohen, 1999; Turner, 1999).

Autism is usually diagnosed in childhood, but it is a lifelong disorder with a sometimes wavering course (Howlin, 2005). It is strongly heritable (Bailey *et al*., 1995), has some known neurogenetic components (Yates, 2012), and has been diagnosed in three-to-four times as many boys and men as girls and women, although it is still not clear why (Baird *et al*., 2006; Constantino and Charman, 2012). Autism has a prevalence rate of about 1% in the UK (Baron-Cohen *et al.,* 2009), but is notoriously heterogeneous: at the 'high-functioning' or Asperger's end of the spectrum, people with autism can lead 'normal'-looking independent lives; others will require lifelong care and support (Gillberg, 1998; Stevens *et al*., 2000; Howlin *et al*, 2004. This heterogeneity of autism will be discussed more in Chapter Two)

All of this seems clear enough. But autism has had a notoriously awkward clinical and research history (Feinstein, 2010; Silverman, 2011). Initially isolated as a nameable clinical entity in the United States in the 1940s, autism emerged more-or-less simultaneous with the high-point of American psychoanalysis (Luhrmann, 2001; Hobson, 2011), and although there was much debate in the early years about the degree to which autism was a 'neurological' condition, its genesis was for a long time associated with parental – and particularly, maternal – coldness (Kanner, 1949). Bruno Bettelheim's now-infamous account of autism is perhaps best-known here – *viz*. 'that the precipitating factor in infantile autism is the parent's wish that his child should not exist' (1967: 125). One of the most important side-effects of this association, however, was that it pushed autism parents, in particular, towards the creation and sustenance of a parallel world of biologically-oriented research and support – and this eventually emerged as the psychological mainstream, as the influence of psychoanalysis waned (Eyal and Hart, 2010). Perhaps most well-known, in this move, are Bernard Rimland, founder of the Autism Society of America, whose *Infantile Autism* (1964) propounded perhaps the first coherent 'neural theory' of autism; and, in the UK, Lorna Wing, founder of the National Autistic Society – who is perhaps most famous for re-discovering the research of Hans Asperger for an English-speaking audience (Wing, 1981).

With the waning of psychoanalytic prestige, but still with no clear biological marker, the 1980s saw the emergence of the major cognitive theories of autism. Hollin (2012) divides these into a big three of: 'theory of mind,' or a claim that the major 'deficit' in autism is in the attribution of mentation to other beings (Baron-Cohen *et al*, 1985); 'weak central coherence,' which argues that the major cognitive phenomenon in autism is to focus on detail at the expense of piecing together the 'whole picture' (Frith and Happé, 1994; Happé, 1996); and 'executive function,' which looks at autism in terms of global planning, initiating and inhibiting functions (Ozonoff *et* al., 1991; Ozonoff and Jensen, 1999). While all of these have been influential in their time, no single cognitive explanations has been widely accepted as an explanation for all of the 'deficits' associated with autism. In recent years, the centre of autism research has increasingly formed around different neurobiological accounts. Early studies tended to focus on the identification of one or more specific brain-areas, where either function or structure might be reliably correlated with autism – prominent examples include studies proposing differences in white matter and overall brain volume (Herbert *et al.,* 2004), problems with the amygdala (Adolphs *et al*., 2002), synaptogenesis (Persico and Bourgern*,* 2006), and problems in 'mirror neurons' (Dapretto *et al*., 2006). Again, none of these suggestions have ultimately gained traction as neurological bases for autism as such. Perhaps the current most prominent emerging theory of autism has moved from a concern with a problem in specific regions, to a problem in connection between regions (Belmonte *et al*, 2004; Just *et al.,* 2007, Kleinhans *et al*., 2008). Despite this sustained effort, the neurological basis of autism remains unexplained.

One of the over-arching outcomes of this wavering scientific history has been to create space for scholars, including social scientists, to wonder about the ways that autism actually gets differently figured as either a 'social' or a 'biological' phenomenon. Here, I am referring to the way that autism is both definable and claim-able as something that is biologically true, and a distinctive embodied phenomenon – and also as something that is more a product of changes in medical practice, and cultural anxieties about childhood. The tricky crossing of biology and culture in medical practice is hardly unique to autism. But as Chloe Silverman has pointed out, there is something about autism's range of interactions with 'the social' that make this a particularly vexed issue in autism research (2008: 325). In fact, there are at least three quite distinct ways that social scientists who are interested in autism research and the broader autistic field (my interest is in the former, but the borders are not always clearly marked) have taken up the question of how autism actually gets figured as either a sociological or a biological phenomenon. First, they have taken up the question of whether the 'epidemic' of autism (or of autism diagnoses) represents a biological change in the population, or whether it has its origins in changing diagnostic (or other cultural) practice (Liu *et al.*, 2010; Eyal *et al.*, 2011). Second, they have paid attention to whether the actual emergence and definition of autism in the first place was the discovery of a biological reality, or a re-definition of how society wanted to think about, and intervene upon, problematic childhoods (Nadesan, 2005; Grinker, 2007). Third, they have become interested in claims that autism should be thought of as legitimate form of 'culture' (usually through a 'neurodiversity' movement that claims autism as legitimate difference, on the basis of differently-configured neurology), and have wondered about the degree to which this overtly political claim is embedded in a kind of paradoxical biologization (Bumiller, 2008; Ortega, 2009).

My goal, here, is to work through some examples of each of these questions, and to the literature that attends to them. In particular, I will show how each is focused on the way that autism seems to be simultaneously embedded in both individual bodies, and also in social or cultural practices. But I also want to thread through this discussion my own contribution – which is to focus particularly on the way that autism is figured in scientific research, and, in particular, to direct sociological attention to its increasing entanglement with the theories, practices and technologies of the new brain sciences. But as I discussed above, I do not see neurobiological research as a mixture of social inputs and biological pre-dispositions that can be disaggregated. I see it instead as a careful practice of actually allowing the 'sociality' and the 'biology' of autism to tumble over one another. Here, then, I re-state my commitment to remaining faithful to the complexity of neuroscientific practice – and to setting aside questions of whether autism is a social or a biological phenomenon. Central to what follows, in fact, will be a sustained focus on the novel ways that autism may actually be traced through the sheer complexity of the bio-logical and socio-logical 'entanglements' that are so deeply embedded in neuroscientific practices.

Perhaps the field in which these concerns are most obviously in play is in sociological attention to the rising number of autism diagnoses. Given the increasing prevalence of autism (Centers for Disease Control, 2012a), and also the lack of clarity about a clear biological or environmental cause of the increase (Weintraub, 2011; Centers for Disease Control, 2012b), several sociologists have recently wondered if there isn't some 'social factor' in the background (Liu *et al.*, 2010; King and Bearman, 2011; Eyal *et al.*, 2011). One proposed theory is 'diagnostic substitution,' which suggests that people now receiving diagnoses of autism would simply have received a different diagnosis in another era (Bishop *et al.*, 2008; Coo *et al.*, 2008). In *The Autism Matrix* (2011), Gil Eyal and his co-authors identify the rise in autism diagnoses with a broad-scale shift in the institutional network that actually seeks to recognise, name, and intervene-upon, categories of childhood mental disorder. The authors argue that the 'real earthquake' is not the rise in diagnoses of autism, but actually 'the deinstitutionalization of mental retardation that began in the 1960s' (2011: 3). With the dismantlement of 'custodial institutions,' they argue, a new 'institutional matrix' emerged, composed of 'community treatment, special education, and early intervention programs,' forming an 'ecological niche' for the emergence of autism (ibid.:3, 39). This is diagnostic substitution – the practice through which (in this case) 'children who in the past would have been diagnosed as mentally retarded are now given the diagnosis of autism at increasing rates' (ibid.: 47).

Note that this is not a straightforward social-constructionist argument – underlying the analysis is Hacking's (1995) notion of 'looping,' and a claim that 'classifying and naming autism set in motion processes that act on the phenomena classified and possibly change them in ways that, in turn, react back on the classification' (Eyal *et al.*, 2011: 209). Nonetheless, the authors situate their account exactly on the fault-line of biological and cultural explanation (i.e. between more cases of autism, or just more diagnoses of autism) – and they make a clear argument that behind the emerging visibility of autism, and underneath its increasing presence in clinical spaces, is a cultural change in medical practice. Similarly, Marissa King and Peter Bearman have correlated changes in autism prevalence with periods of change in diagnostic practice in California, to argue that 'one-quarter of the increase in measured autism prevalence is the result of diagnostic change' (2009: 1231). In another paper, they use the same data-set to look at other 'social' factors that may undergird changes in the rate of diagnosis in California – noting, for example, that rates of diagnosis in California are correlated with zip-code, with neighbourhood wealth, with educational attainment of the parents, and so on (2011: 9, 12, 22; *cf.* Liu *et al.*, 2010).

Again, what is most apparently at stake in sociological discussions of the autism epidemic like these is the question of whether an autism diagnosis is really identifying something biologically distinct, or whether it is more a question of groups of psychiatrists, psychologists and social workers differentially parsing particular kinds of behaviour. I am in sympathy with much of the argument from 'diagnostic substitution.' But I am nonetheless resistant to using the tools of sociology to carve out a specific and distinctive space for 'social origins,' as a phenomenon distinct from biological change. This is partly from a philosophical commitment to not seeing 'the social' as a kind of substance that explains worldly phenomena, as if it didn't have its own history (Donzelot, 1988; Rose, 1996b; Latour, 2005). But it also comes from an empirical realization, drawn from my conversations with autism neuroscientists, that what might be most interesting, here, is precisely the way that these qualities actually get theorised *together* by scientists and others working on autism (Barad, 2007: 25)

A similar set of distinctions can be seen in sociological accounts of the emergence of autism as a diagnostic category in the first place. Here, the question is less about diagnostic practice, and more about the role of the broad-based social and cultural anxieties to which the emergence of autism as a diagnostic category might be traced. Roy Richard Grinker (2007), for example, who is both a cultural anthropologist and the parent of an autistic child, argues that autism 'is a brain disorder that can affect anyone in any culture,' but also that 'culture affects the way we view autism' (2007: 13). In this account, autism is a real biological thing, but its emergence and visibility are cultural phenomena too – and it is the job of the anthropologist to think about the different contributions of each: 'scientists,' Grinker argues, 'belong to different cultures,' and 'their research is often a product of their time and place their community's interests and values' (ibid.: 16). On the recent visibility of autism, Grinker suggests that

> we have hundreds of diagnoses, but it's not because people have hundreds of psychiatric symptoms they never had before. It's because the science of the mind, brain, and behaviours – and our educational systems more attuned than ever to individual differences – demand specificity (ibid.: 75).

Nadesan makes a similar claim, arguing that,

> over time socially constructed ideas change in response to changes in institutional arrangements and changes in the forms and rhythms of everyday

> life. To argue we will definitely *know* autism elides the fact that our ways of knowing are always/already socially and historically situated' (2005: 134).

Even more starkly, Nadesan argues that 'autism is not a thing unto itself, and it is not a disease with specific biological markers. Autism is a term used to group people exhibiting a wide range of cognitive and behavioural deficits' (ibid.: 172-177). For Nadesan, as for Grinker, thinking about these basically social decisions to name and isolate an 'autism' does not require us to 'reject a biological basis for the conditions that come to be labelled as "autistic"' (ibid.: 2). Nonetheless: 'the condition called autism does not stand outside of the symbolic awaiting discovery. Rather, the symbolic inscribes and produces autism' (ibid.: 214).

Again, the phenomenon of interest here is the difficult mingling of an unambiguously biological bodily phenomenon with a set of historically-situated diagnostic and surveillance processes. But what is sometimes missed, in the attempt to separate the two, is the strange achievement of a clinical and research practice, gathered around autism, which is able to name and work on a diagnosable condition *without* necessarily pulling these apart. What interests me, then, is the way that autism research seems to be a practice of gathering-together – and what differentiates my account from these authors is my commitment to thinking about how this is actually done.

Chloe Silverman's *Understanding Autism* (2011) is perhaps a closer precursor of my approach. Although Silverman also tries to follow 'the evolution of the diagnostic category of autism as people have understood it in different places and times,' what sets her account apart is her commitment to seeing autism as simultaneously embroiled in, and inseparable from, a wide variety of inquiries and interests (2011: 2). And at the heart of Silverman's historical understanding is not an attempt to disaggregate different lines of research and knowledge, but precisely to weave and hold together the complicated big picture. Silverman even manages to entertain twin empathies for both the psychoanalytic years in autism research and care (noting, whatever the problems, a commitment to 'sympathetic and individualized interpretation') *and* the role of expert amateur parents (who, in autism research, continue to make clear that 'expert knowledge and private life have continually intersected in the families of autistic children') (ibid.: 88, 118). In the same way, Silverman is attentive to the tricky simultaneity of the ways in which, for example, 'genetics provides an effective vocabulary for expressing responsibility and experiences of membership that develop out of love, friendship, and loyalty,' while retaining a sensitivity to the intricacies of the vaccine controversy around autism, declaring an unwillingness 'to dismiss the claims of parents who are so specific in their delineation of the physical symptoms that affect their children, the gut infections, the gut symptoms the allergies, and the insensitivity to pain ' (ibid.: 142, 228).

What allows Silverman to do this is her commitment to the use of 'love' as an 'analytical tool' for the social researcher (ibid.: 1). Love is a 'a technique as well as an emotion,' says Silverman; it is also 'a way of knowing about bodies, persons and relationships in biomedicine'; and if it has been sometimes seen 'as a liability or a barrier to reliable knowledge,' it has also been 'the source of specific, focused and committed knowledge' (ibid.: 3-4). Focusing on, and employing love, allows Silverman to hold together parents, genes, brains, politics, and psychoanalysts. In this thesis, I am similarly committed to not thinking about the differently scientific and cultural commitments of researchers – but in paying attention to the delicate ways that researchers work across these registers. With the literary critic and autism parent, Stuart Murray, I resist a conception of autism as 'being *only* subject to the workings of metaphor and fascination,' even as I remain committed to 'an understanding of the condition that moves beyond a scientific or medical categorization' (2008: 50, 8; *cf.* Osteen [ed.], 2007)

The final part of the autistic field that has directed sociologists' attention to how autism gets thought of as either a social or a biological phenomenon is the emerging politics of autistic difference. While the association of autism with social movements, and in particular with forms of parent advocacy, has been widely noted (Silverman, 2008; Chamak, 2008), recent discussions have begun to include the claims to particular kinds of recognition and citizenship from autistic people themselves (Bumiller, 2008; Ortega, 2009). In particular, many researchers have noted a growing claim based on 'neurodiversity' (Singer, 1999). 'Neurodiveristy' is a name for a broad-based 'movement,' or collective identity among some autistic people, that stakes a rights-based claim to respect for autistic diversity, on the basis of an ineffable neurological difference. As one advocate told the *New York Times*, 'we don't have a disease…so we can't be "cured." This is just the way we are' (Harmon, 2004). Neurodiversity is presented as a challenge to a 'deficit' model of autism, focusing on both the validity and the advantage of difference (Robertson, 2009), as well as to the distinct modes and meanings of an autistic culture (Davidson, 2008; Chamak *et al.*, 2008. I will discuss these claims in more detail in Chapter Five).

This is also a political declaration that rests on a claim about the firm biology of autism – and, via the literature on 'biological citizenship' (Novas and Rose, 2005), it has formed another route for sociologists to think about the degree to which autism should be thought of as either social or biological, and what the difference is either way (Orsini, 2009). Francisco Ortega, for example, has argued that the claim to neurodiversity

> must be understood within the context of the diffusion of neuroscientific claims beyond the laboratory and their penetration in different domains of life in contemporary biomedicalized societies' (2009: 425).

What Ortega wants to stress, then, is that the price of neurodiverse freedom might be the deeper penetration of neurobiological discourse into social and political life – that neurodiversity is, at heart, a neurobiologization of what might otherwise be a social claim (Ortega and Choudhury, 2011). But as Kristin Bumiller (2008) points out, what may be ultimately at stake, here, is less whether it is good to create subjectivity through the brain – it is the recognition that autism is not always or obviously disabling, and that there is space, therefore, to create 'new understandings of people with disabilities as fully human and capable of all major life activities' (2008: 971). What remains interesting to Bumiller about the autistic claim to neurodiversity is not the rights and wrongs of rooting autism in either the brain or in social processes, but the way that the complexity of autism's appearances can disrupt the politics of disability, by creating novel relations with the (essentialized) biology of the brain. Amid a political landscape that crosses disability, gender, cosmopolitanism, and individual rights, neurodiveristy makes an intervention that is simultaneously 'progressive, creative, unique, and often vehement,' which, despite a sometime-alliance with, e.g., forms of brain-based gender essentialism, contributes to the 'antinormalizing efforts of feminists supporting diverse causes such as the rights of intersexed persons, support for alternative family forms, and genetic diversity' (ibid.: 985-986).

As with the two literatures discussed above, it is clear that the sociological literature on neurodiversity has also situated itself between different claims about the socio-logical or bio-logical definitions of autism – although, here, focusing more on social movements than spaces of research. What distinguishes Bumiller's account is that she shows how attention to this field does not require the parsing of social and biological claims – but rather a commitment to precisely the awkward or 'quirky' way that political claims to citizenship are traced through a very particular kind of biological essentialism. This has strong affinities with what follows: for my account of neuroscience, as it is for Bumiller's analysis of neurodiversity, the question is not whether this is a reasonable claim, or even what its effects are. It is about the 'creative' ways that such claims are made, and also the 'unique' possibilities that are actually visible therein – both of which help us to significantly re-cast the very premises of such questions.

Conclusion

In this chapter, I have tried to lay the foundations for the empirical discussion that follows, by setting out a three-way, inter-locking commitment to: seeing complexity and uncertainty in neuroscientific research; nonetheless resisting an ethic of critique; and trying to come to terms with, and to understand, the strange scientific configuration of autism as, at once, both a social and a biological phenomenon. I have also tried to begin the elaboration of my own commitment to not seeing complexity, mixture or difference within scientific practice as a sign of error, or as a problematic aggregation in need of the hygienic attentions of a critical sociology. Instead, I have claimed an affiliation with a literature that is inclined to refuse a difference in kind between natural and social phenomena – and that also sees the awkward complexity of neurobiological research as a reason to re-think social theory as a force, and not to wield it as a weapon (Latour, 1993; Haraway, 1997; Wilson, 2004). Threaded throughout the empirical discussion to come, therefore, is an argument for a sociology that sees living-with-difference as an achievement of sorts – or at least as something not to be dismissed out of hand, and maybe even as something to be partly-emulated. Although this commitment undergirds most of the description and interpretation that follows, my explicit elaboration of this commitment will be relatively low-key in the empirical sections. I will return to these themes more directly in the conclusion.

2. What neuroscientists talk about when they talk about autism

Introduction

On the face of it, asking a group of autism neuroscientists, 'what is autism to you, exactly?' may not seem the most brilliant research tactic. Autism, as everyone knows, is a neurodevelopmental spectrum disorder with characteristic deficits in three domains (known as the autism 'triad'): social interaction, communication, and repetitive behaviour (APA, 2000). We know that autism is strongly heritable (Bailey *et al.*, 1995), is diagnosable in about one in one-hundred school-age children in the UK (Baron-Cohen *et al.*, 2009), and that it costs the UK economy about £34 billion every year (Knapp, 2012).

The problem, of course, is that none of this is as straightforward as it looks. For one thing, proposed changes to the Diagnostic and Statistical Manual of Mental Disorder [DSM] would remove communication from the three domains entirely, and would also collapse the distinction between autism and cognate diagnoses like Asperger's Syndrome (APA, 2011).[15] The one in one-hundred figure may have to be revised too – a March 2012 report from the Centers for Disease Control increased this to one in eighty-eight in the United States (Centers for Disease Control, 2012), while the £34-billion cost estimate, already a notable increase from a 2009 estimate (Knapp *et al.*, 2009), has still to be finalised, and is described as 'tentative' (Knapp, 2012). More to the point, perhaps, there is now on-going disagreement about whether the triad, causally, actually composes a specific disorder at all (Happé *et al.*, 2006), while the 'specific genetic etiology' of autism, after more than a decade of research, 'remains largely unknown' (Gupta and State, 2007). And skating across all of this is a series of political contests over whether we might characterize autism as a 'disorder' in the first place – and even if the diagnosis might not mark a 'cognitive style,' or even a talent (Happé, 1999; Frith and Happé (eds.), 2010). 'We know more about autism now than at any point in history,' says the cultural historian, Stuart Murray, 'yet, at the same time, if we're honest, the foundational observation that we might make, the "central fact" about autism with which we should probably start, is that we don't know very much about it at all' (2011: 1).

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[15] One paper has suggested that this revision would ultimately remove one-quarter of people with autism from the ASD category – and fully three-quarters of people currently diagnosed with Asperger's (McPartland *et al.,* 2012). Publication of the proposed DSM changes came too late to be significantly incorporated into my interviews or analysis, but see Brock (2012a) or Robison (2012) for a flavour of the on-going online discussion.

This is the context in which I became interested in the question of the kinds of things that neuroscientists would talk about, if you asked them what autism was. And the gist of the answer I discovered was this: some say that autism is a discrete and 'true' biological entity – something that is organic, timeless, and complete in itself (even if, as yet, we do not know exactly how). Other people, though, and also sometimes the same people at different times, say that autism is irredeemably heterogeneous, something impossible to pin down, a biologically-incoherent umbrella of convenience. As I discussed in the introduction, my over-arching goal for the thesis is to think through some of the ambiguities and uncertainties that are thrown up by neuroscientific research on autism. Of course it is well known, now, that the practices and objects of both the 'psy' and 'neuro' sciences are rather less stable, and much more strangely inflected, than the public cultures of those disciplines usually suggest (Dumit, 1999; Blackman, 2007). But my goal in this chapter is not only to say that there is a large definitional ambiguity at the heart of autism neuroscience. I also want to begin thinking about the ways that neuroscientists actually work across such ambiguities. In particular, I want to show how neurobiological research on autism might progress, not despite these differences, but somehow with them. Doing a neuroscience of autism that proceeds with difference and entanglement, and not despite it, is at the core of the what I have called 'tracing autism,' i.e. a neurobiological practice that does not always have to pull things apart (biology, idiosyncratic children, diagnostic check-lists, clinical pragmatism) in order to seek singularity and distinction (a well-described and isolated neurodevelopmental disorder called autism). In this chapter, my focus is on some of the ways that autism neuroscientists seem to work within a research-space that contains the idea of autism both as a 'biological truth' *and* as a 'symptom checklist.'

The first two sections of the chapter will establish the presence of these two wavering positions within my interviews. In the first half of the chapter, I will work through the different emphases and inflections of each. In the second half of the chapter, I move to thinking about what it is that helps these two to co-mingle within these discussions. To do this, I turn to two cross-cutting ways that neuroscientists also talked about autism – and these were references to autism as an 'enigma,' or as something with which these scientists were intrinsically 'fascinated,' and also descriptions of autism as something that they just 'knew' when they saw it, or a 'feeling' within an interaction. My argument is that thinking about autism through the registers of 'fascination' and 'feeling' is precisely a way of tracing autism through a field of considerable uncertainty. It holds together these neuroscientists' commitments both to an idea of an independent autism, on its own 'out there,' but also to an entangled autism, one not so easily divisible from the webs of research, experience and affect that surround it.

This argument makes possible a number of interventions in existing literature, and I will try to thread these through the discussion. First, several scholars have recently paid attention to a phenomenon of 'neurobiologization,' or the process of locating some otherwise 'fuzzy' categories of human and social life within the solid space of the skull (Abi-Rached, 2008; Williams *et al.*, 2012). Amid a literature often framed by a concern about reduction, I want to contribute to a conversation that is more focused on the widespread vagaries, and the complexities, and the sheer theoretical and practical idiosyncrasies, which are sometimes visible in the midst of these projects (Pickersgill, 2011). This argument can also be extended into a larger conversation within medical sociology about 'biomedicalization,' i.e. a move towards thinking about the emergence of a widespread, industrialised and pre-emptive space of physical and ethical intervention within contemporary biomedicine (Rose, 2001a; Clarke *et al.*, 2003). My argument is that while the emergence of autism as a neurobiological category offers a clear window onto the potential biomedicalization of a tricky diagnosis, as this literature proposes, it also shows this process to be far from unilinear. Indeed, it shows how a process of biomedicalization can take the forms of a series of recursive and oscillatory movements. It also shows how, even in the twenty-first century, neurodevelopmental diagnosis can maintain a loose and promiscuous relationship to the ever-tightening loops of biology and technology.

The Unchanging Core

Early on in this project, I would quite directly, and very naively, ask researchers whether or not there was a real sense of 'biological truth' to autism. At the time, by 'biological truth,' I meant an autism that was not only a concatenation of symptoms with a more-or-less varied neurogenetic core, but an autism that was in some way 'biologically discrete.' And by this, I meant that autism would ultimately be located in, malfunction in a specific brain area, such as the mirror neuron system (Dapretto *et al.*, 2006), or in a series of specific genetic mutations, such as the 22q11.2 deletion from chromosome 22 (Fine *et al.*, 2005). I was interested in the idea that if you could once have answered the question, 'what is autism?', by saying that it was a product of over-educated parents (Kanner, 1968 [1943]), or a reaction to maternal rejection (Bettelheim, 1967), or a symptom of childhood schizophrenia (Rutter, 1968), or an outcome of mindblindness (Baron-Cohen, 1995) – today, you would more likely talk about something like the ratio of white-to-grey matter in the brain (Herbert *et al.*, 2004), or the thickness of the corpus callosum (Just *et al.*, 2007), or something similar. This is basically the view that, as one interviewee (R11) put it,

> ultimately [autism is] a brain thing, because social interaction comes from the brain, so our social tendencies come from the brain, along with all the other things in autism that are wrong, like repetitive behaviours. So it has to be a brain thing, in that, if you could fix that thing in the brain, if you knew what it was, then you could, I guess, cure it.

'The question is, of course,' said one senior professor (SS06), perhaps growing tired with my attempts to dance around the topic, 'if there's a kind of natural entity that's autism.'

'Yes,' I conceded.

'Who knows?' she said eventually:

> I think that there is. But I sometimes doubt it. […] I have that view [that autism is a natural kind]. Why do I have that view? I think it dates back probably to the sixties, when I was no doubt educated to see it like that, and it has in a sense stood me in good stead. And it was possible even to have that narrow view that we had at first about autism to expand it, and to embrace a whole spectrum of conditions. That's pretty good, you know – it sorts of seems a strong, robust sort of concept.

The committed subscription to a model of autism-as-natural-entity is striking here, particularly given how aware this researcher is of the relationship between her view and a particular historical way of thinking about things ('I was no doubt educated to see it like that'), and also how this view is, for her, basically pragmatic ('it has in a sense stood me in good stead'). And when I asked her whether there had not been a lot of changes in how we thought about autism over time, she readily agreed that there had been 'huge changes' – but then said: 'it is a wonder that we can still recognise the same thing, but I think we can, which is remarkable.' This 'same thing' of autism is basically the image that I was trying to illuminate with these kinds of questions. And indeed I frequently found that people were committed to this view of autism as a kind of 'thing' – singular, persistent and biologically discreet.

For interviewees who had this view, however, their conviction was often less rooted in a commitment to a specific neurological or genetic theory of autism, than it was bound up with a sense of the disorder's timelessness. Where people were committed to the biological truth of autism, they would often point to its persistence through history. Talking about her early work in the field, another senior person (SS08) said:

> And people whom we saw then, what, 40 years ago, we still have contact with now and they still meet criteria – I've just done, finished a long-term follow-up and those people still meet criteria for autism [...] The actual criteria haven't changed all that much, although people say they have.

But this view went beyond recent and personal senses of time. 'Historically,' said another interviewee (L03),

> you can go back and find cases of people who clearly had autism 200 years ago before anyone knew the concept of autism – so, as an organic, developmental brain disorder it exists in the same sense that schizophrenia exists, cerebral palsy exists, down's syndrome exists, you know, and has always existed.

This is a popular view: in his part-memoir/part-history, *Not Even Wrong* (2006), the author and autism-parent, Paul Collins, splits the story of his own autistic son's diagnosis, with the story of Peter, the nineteenth-century 'Wild Boy' of Berkhamsted – through whom Collins gradually draws parallels with the autistic children of today, even enough to diagnose, in the book's subtitle, a 'lost history' of the disorder. In another example, the social historian Rab Houston and the psychologist Uta Frith tell the story of Hugh Blair of Borgue (2000) a well-known 'natural fool' of eighteenth-century Scotland – in whom the authors are inclined to make a tentative retrospective diagnosis of autism. Indeed, the alignment of Houston and Frith's project with arguments about the essential nature of autism is remarkably explicit:

> We believe that it is important to separate the existence of labels and explanatory theories from the existence of pathological conditions. The syndrome now called autism was not categorized until the 1940s...however that does not mean that [this condition] did not exist before then – just as there were presumably germs around before germ theory was promulgated to explain disease...through studying Hugh

> Blair's condition in its historical context we hope to be able to expose *the unchanging core of autism*' (2000: 4; my emphasis).

This 'unchanging core' is precisely what I mean when I talk about the 'biological truth' of autism. Certainly, no one that I spoke to expressed this view easily or flippantly. But there certainly were plenty for whom autism had some kind of organic and timeless singularity at its heart: 'Well, said another interviewee (R08), when I broached the topic:

> I think if you go to a certain…if you break it down enough you would find biological markers in common. As you say, I doubt it would be at this whole-umbrella level. It would be at the sub-type level. But I'm sure there are…there is a biological truth. But I think it…yeah, I think it's difficult.

What I find interesting about this statement – and I don't mean this at all in a critical way – is how unconvincing it is, even to itself. 'There is a biological truth,' she says, almost like an invocation – a faithful quality that precisely explains the concession that comes almost immediately after: 'it's difficult.' The commitment to 'biological truth' is indeed just that – a commitment. And, as we shall see, one not always easy to maintain.

The sense of how unconvincing this view is, even to its own adherents, is among the most remarkable things about it. I had come into this research at least part-prepared to tell a story about 'biomedicalization' – an analytical framework established by Adele Clarke and her colleagues (2003). Biomedicalization rests on another concept, 'medicalization,' a now-classic concept in sociology, in which a sometime social phenomenon (often some kind of deviant behaviour) is brought discursively within the purview of a medical science (often psychology or psychiatry), creating the potential for a reconfiguration of forms of expertise, intervention and government (Conrad, 1992; Lupton 1997; Williams *et al.*, 2012). But with this view becoming something of a sociological cliché (Hedgecoe, 1998), Clarke and her colleagues have proposed a series of important contemporary additions. These include the emergence of radically novel medical technologies, and new fields based on these technologies; the shift in medical focus to the maintenance of health over the cure of illness; a medical-institutional bureaucratic concern with prevention and risk assessment; the emergence of novel corporeal possibilities and social movements extending from these; and the situation of contemporary medicine within a hyper-capitalist political and economic complex

(2003: 62-63; see also the subsequent volume edited by the same authors – Clarke *et al.*, 2010).

This is a powerful frame, and it does indeed capture much of what is happening between neuroscience and autism. But it nonetheless seems to predict (rhetorically if not directly) an ever-tighter entanglement between autism and these emergent biomedical forms – precisely on the basis of autism's deepening subscription to new 'bio'-technologies, and new 'bio'-fields. As Orr puts it for the field of US psychiatry more generally, biomedicalization describes 'an uncanny generalization of the techno-structures of a 1960s mental hospital into a widening assemblage of medical and social spaces' (2010: 379). As I have shown in this section, this kind of bio-essentialism and generalization – 'it has to be a brain thing' – was certainly sometimes prevalent in my conversations with autism researchers. But also, and as I outline below, these same categories were also very much in question within this research. Certainly, I have seen people content with the emerging biological truth of autism, and also people who were quite content to generalise the 'techno-structures' of neuropsychiatry into this space. But these accounts were also heavily diluted, as I describe in the next section, by a sense of autism as something that too often manages to *evade* new technologies in the biosciences, and also as something which *resists* the widening field of biomedicalized psychiatric and psychological generalization. In these accounts, autism is *not* obviously in the process of becoming a more stringently biomedical concern. Sometimes, indeed, quite the opposite.

Let me stress – I am not attempting a counter-example to the frame of Clarke and her colleagues. I choose 'biomedicalization,' here, only because it provides a series of analytic tools that illuminate what is and isn't possible to say about autism. Moreover Clarke and colleagues are very much alive to the 'contradictions and unanticipated outcomes' with which this vast, ambiguous shifting complex is inevitably associated, and there may well be room in their account for the way that my interviewees talked about autism (2003: 184). What I am trying to show here, nonetheless, through the case of autism, and at the coalface of neuropsychiatric biomedicalization, is the complexity and ambivalence of the process through which a contemporary diagnosis actually becomes entangled in the machinations of bioscience and biomedicine. And I am trying to say that, such is the degree of this complexity, we may at some stage wonder who, after all, is wrapped-up in whom. Mine is only an argument against simple accounts of medicalization and biomedicalization – it looks for disorders that become *more* complex and *less* clear, the further they are brought into a biological framework.

It's not clear that there's just one condition.

One of the most striking things for an outsider entering the field of autism research is simply how much is missing from our knowledge of autism – knowledge, for example, about things like cause, course, the degree of heritability, the likelihood of therapy, and so on. But perhaps even more remarkable than these is the extent and proliferation of explanatory gaps between the things we do know. In other words, not only do we not really know what causes autism, or how it manifests in the brain of an individual, or how it affects cognition, or even what it will look like in any given diagnosed person – but it is also often unclear how the few things that we might know, at any such level, will connect or interact with what we know at any the other level. You and I may manifest similar measurable deficits in communication and interaction – but this is no predictor of a shared cognitive or neurological problem in the end.

This, in essence, is what researchers usually lament as the 'heterogeneity' of autism (Ronald *et al.*, 2006; Geschwind and Levitt, 2007). But autism is heterogeneous in a number of distinct and important ways. Most strikingly, autism is heterogeneous in terms of its symptom-set: the famous diagnostic triad of communication, language and repetition can appear to very different degrees within equally-diagnosed people (Ring *et al.*, 2008). There is a related heterogeneity in how well a diagnosed person will 'function' in what some advocates for neurodiversity call the 'neurotypical' world (i.e. the world of 'typically-developing' people). Some people will lead independent, typical-looking lives; others will never speak, or might require round-the-clock care (Howlin *et al.*, 2004). And there is heterogeneity of causal pathway – autism is highly heritable, although specific mutations for more than a minority of diagnoses continue to elude researchers (Betancur, 2011). Thus, it is likely that people who share broad symptomology in late adolescence – enough to diagnose each with autism – may have arrived there by very different developmental routes. All of this is common knowledge, and scholars have tried to deal with it in a number of ways. Mary Coleman and Christopher Gillberg, for instance, have started a trend for referring to as 'the autisms' or 'the autistic syndromes' (2011). Daniel

Geschwind, a prominent autism neurogeneticist, describes this view of autism as 'a collection of dozens or perhaps hundreds of etiologic forms that converge on common behavioural and cognitive phenotypes (2009: 372). Indeed, such is the degree of disparity between the presence of the core symptoms, some have suggested that there will likely be *no* overlap in genetic causes between the different components (however big the sample-size) and the best outcome might be to abandon the idea of a single 'autism' altogether (Happé *et al.*, 2006).

As I emphasise throughout the chapter, none of this is terribly controversial or astonishing: 'no one thinks of a unitary thing called autism,' said one interviewee (SS10) to me, rather bluntly, when I first raised this question with him:

> you've got a crazy mind if you think there's a unitary thing called autism…It's a diagnostic category just like any other diagnostic category. And you can draw the line on that category where you wish to draw the line.

This is, for many, the more-or-less public way of talking about autistic heterogeneity – and also the way that it is often reconciled with the 'biological truth' view, discussed above. But I also found that interviewees often had less sanguine views of autistic heterogeneity – at least as it related to the causality of autism, irrespective of biomarkers. And this was because not only was research failing to reduce the causal and explanatory gaps between its different elements – but even, sometimes, it seemed to drive those gaps further apart: 'I think my personal take on that,' said one interviewee (3S02), 'is that the more the science tells us, the more complicated the picture gets, and the less likely we are to find simplistic solutions.' Or as another (SS07) put it:

> my current beliefs about autism, my intellectual beliefs about autism, based on the data we have, is that the underlying basis of the social and communication difficulties is going to be different from the underlying basis of the [inaudible] repetitive behaviours and the special interests and to some extent the detail focus […] if there ever comes a time when we can intervene, we'll be able to intervene separately on those different components.

This was a common position. Another (L04) said:

> I think that there won't be one single cause for autism, or for any other disorder probably. That might mean that there is a constellation of biological markers that, if you're born with those, you're likely to develop autism under certain conditions – and those conditions may well be completely unmeasurable and out of our grasp. I don't think they'll be as straightforward as, I don't know, you were exposed to x substance at x point in your

> development or anything. I think it'll be a very complex interaction of things. And that complex interaction will be different for many, if not all, individuals. So I think this search for a single cause at whatever level you're looking – whether you're looking at a cognitive level or a more biological level, I can't see how that is possible. I think if it was possible, we would be much clearer as to what it is. And I don't think we would see so much heterogeneity.

In short, these researchers are arguing that, far from research bringing us closer to autism's biological truth, it is increasingly clear that what looks like a unitary thing called autism may actually be the result of multiple-level interactions between genes, environments, and behaviours – and these are very unlikely to have a single biological identity. Causality is 'probably going to be a sort of a multi-level thing' said another interviewee (SS09) –

> so genetically you'll be able to describe the different gene variants that can contribute to it, but to me the final causal pathway if you want to use that sort of language is most likely to be a neurodevelopmental story […] we already know there are a lot of factors that can lead to this causal change, so genetic, environmental and so forth, so it's not going to be a simple causal story – you will have multiple different possible factors going into one final common pathway which will be a neurodevelopmental story, and then that'll have multiple widespread consequences subsequently that's my guess.

I will return to this quote, and to a fuller discussion of causality across 'multiple levels,' in Chapter Seven. Here, I only want to emphasise the complex, contingent views of 'what autism is' that ran through my interviews – even among interviewees who began from a position of basic scientific certainty: 'Certainly a few decades ago,' said one person (L07),

> people didn't think it was a biological condition at all. They thought it was a response to a particular parenting style, or who know what else. But now there's very good evidence that there is a genetic basis.

Then she said:

> – although it's actually a complicated genetic basis – so one problem is that although it seems to have a genetic basis, it's not a single genetic basis, it's not clear that there's just one condition. We don't have clear marker the way we might do for sickle cell disease, we don't have a clear genetic way of defining the condition in terms of identifying particular genes. So it may be one condition that varies in intensity. Or it may be a variety of conditions that share a common genetic load.

I am particularly interested in this contribution because the interviewee begins with a 'biological truth' position, but then immediately, and once she starts to talk in detail about the biology, she starts to qualify this commitment, and instead emphasises the knowledge that, in fact, according to her, 'we don't have.' This is exactly the kind of oscillation that I am concerned with in this chapter, i.e. the shift between a fairly straightforward idea of 'biological truth,' and a more problematic and complex view of autistic 'heterogeneity.' Again, I stress, I am not saying that that the latter is inimical or hostile to biology or to neurobiological research. But it nonetheless gently proposes that autism may be too variable, too contingent, too time-bound for some final biological substrate to be the subject of a research endeavour.

And yet 'an autism' persists. When I asked one interviewee, a young lecturer (L09), what a final 'neuroscience of autism' would actually look like, she said 'it's not going to be a point change in the brain…

> it's going to be the influence of a small change here on a small change here. Individually, you won't be able to observe those changes, they'll be too small. But their interaction will be observable. And in fact it is observable – it's called autism [*laughs*].

I love this joke – or at least I love the observation that of course we can already 'see' the thing that's apparently so hard to find. Mindful of the perspectives outlined in the first section, I am trying to thread this sense of the stubborn persistence of sameness ('it's called autism') through the discussion of difference. And I am trying to hold onto this nonetheless-observable 'autism' – just as several of my interviewees traced it through their own discussions. In a short primer aimed at an interested lay public, Uta Frith recollects how:

> when I first saw autistic children I was only dimly aware that autism comes in degrees, form mild to severe. Actually all the cases I saw were severe…[now,] autism is no longer a narrow category but has widened enormously to embrace a whole range of autistic conditions (2008: 4).

But then, after reflecting on the implications of this realisation for a little bit, she goes on:

> every individual is unique in a multitude of ways, but they also resemble each other in some fundamental preferences and characteristics…no one has yet given up the idea that there is a common pattern behind the kaleidoscope of individual behaviours. I will therefore frequently use the familiar terms autism

> and autistic, as a reminder that there is a central idea behind the spectrum (ibid.: 4-5).

Here we see the same move: enormous width, on the one hand; fundamental characteristics, on the other. When I spoke to one very eminent professor (SS06) about these issues, she said to me – referring to her general philosophical orientation, I think, and only half seriously – 'Probably…I'm a bit naïve…

> I think that we are each of us individuals in the sense that we are incredibly different from each other, through our genes, our history, all sorts of factors that shape us – the physical environment. And, yet, you can forget about all this and say, 'well, here is a man [*laughs a bit*] or a woman.' And even though your hands and my hands are very, very different I'm sure, they have five fingers [*I interject to point out that I have notably small hands – which is true*]. Never mind – you know, there is a hand…so I suppose philosophers would call that essentialism. Anyway, I am sort of quite, you know, I see that all the time in my own looking – I'm not particularly worried about the differences. I can see the differences and I think they are what makes life interesting and everything, very nice. But I am not detained by them to such an extent. And that applies even to autism.

I am struck, upon re-reading this, by the ability to scientifically reconcile questions of difference with some kind of identity-nonetheless. And I very much like this interviewee's response to my own – genuine – qualms about the smallness of my own hands, i.e. the straightforward reassurance of 'never mind…there is a hand.'

I have said, in these two sections, that when a group of neuroscientists talk about autism, they sometimes talk about the degree to which it is a more-or-less unitary and distinctive disorder, recognisable even in history, and with some sort of basic, biological underpinning. But entangled with this biological view is a sense of autism as a heterogeneous and separable phenomenon. This latter autism has a biological pathway so radically unclear that it seems likely any biological truth will lie within a series of quite dispersed and contingent events – events which are perhaps even distinct from one another, and which we only diagnose as autism when they (perhaps arbitrarily) co-occur. But I have also said that 'an autism' is held together, distinctly, through these two registers. In a minute, I will have much more to say about the way that an autism neuroscience traces its object across these two registers. But before moving on, I want to push the discussion of these two narratives a little bit, and also delve a bit deeper into the consequences for the novel scientific practices that are mutually – and constantly – formed and re-formed around them.

The only thing people agree on

The whole discussion related below is taken from an interview that I did with a young postdoctoral autism researcher (R04), who had trained in psychology in continental Europe, but who was now housed in a psychiatry department in the UK – two facts to which she steadfastly refused to attribute any epistemological significance whatsoever. It was an odd interview, conducted in a regional psychiatric hospital, and in the course of what became, for me, a long and melancholy day spent loitering in the hospital's grounds. I was about to write that it was one of my longest interviews, but having just checked, I see that it was actually a notably brief encounter – just over 35 minutes. Certainly, I remember this interviewee as being a small bit combative, and challenging, and also, like many, somehow torn between suspicion of my motives, and indifference to my inanities ('oh my God,' she said at one point, entirely exasperated, 'I mean, if I would be able to answer these questions…'). But it was also weird because by the end of our conversation she sounds (on the tape, and also in my memory) quite exhausted, and even a bit despondent about where we had gotten to. It was as if this was the first time (and I say this with no sense of self-aggrandisement at all; indeed, I take this as evidence of my blundering only) that she had talked out loud about some of the trickier aspects of her own practice. I relate the conversation here, in some depth, because the critical parts centre on precisely the questions we have been outlining – questions about what exactly autism is, but also, more importantly, how a neurobiological account of autism gets traced through some very different registers.

We had started off talking about different ways to think about and approach autism, and she had defined her own method as natural-scientific, and specifically neuro-scientific – saying that she was interested in the 'potential to answer questions in a not so fluffy way.' We danced around this for a bit, and after a while I said to her: 'can you give me your most useful definition, off the top of your head, of autism – that you work with? Not the textbook definition but the definition that's practically useful to you.' 'Well,' she said,

> I would always give, and I usually do in talks, the definition that is given in the diagnostic process – which is that autism is diagnosed based on a triad of symptoms based on social interaction communication and restricted interests.

> And I'm using that because it's very convenient. Nobody's questioning it because if you really start thinking about it then it's very difficult – *really* defining autism. Because it is not only diagnostically defined as a spectrum, for example, of abilities – low-functioning people and high-functioning people, they are all within the spectrum – but also that between individual differences are so large that I find it sometimes quite difficult to put all those people into one umbrella term. And so far it's a very convenient way of defining it by just going back to the DSM-IV.

'Is that how you think of it? I asked – 'just a kind of umbrella of convenience?'

> Yeah, of course – I think many colleagues are also questioning….well, I think there is a certain agreement, but there is not one autism, yeah. There are certain things which are shared between people on the spectrum, but there is definitely not one sort of autism.

This gap between 'a certain agreement' and 'definitely not one sort of autism' closely mirrors what I talked about already. But I am also interested in another gap that opens up here. This is the gap between this researcher's earlier commitment to an uncomplicated science that reduces and quantifies to reduce the fluffiness – and then the fact that her actual research object might be a bit of an arbitrarily cut-off concatenation of quite different things; and anyway, no one could even agree whether it existed or not, beyond some 'certain agreement' that nonetheless remained undefined. This marks the kind of ambiguity about 'what autism is' that I am trying to outline in this chapter. 'So,' I said, happily mixing metaphors, 'the umbrella is really a flag of convenience for research purposes, for diagnostic purposes – but really, we think underneath this, there's a series of different things, or…' I let the question peter out. 'There's a series of different disorders,' she replied:

> Different – and, again, it's very difficult to differentiate them because basically that's what I'm trying to criticise in a differentiation between, let's say autism spectrum disorders and other developmental disorders. It's of course an artificial differentiation. So it would be equally wrong to say 'autism spectrum disorders are made up of five or let's say six different disorders' – because then you just re-introduce an artificial differentiation. I think there are spectra of abilities and dimensional domains and you tick some boxes and some you don't as an individual, and if you are on the spectrum or not, even that, if you are looking at the whole population, if you are diagnosed with autism or not, is sometimes a matter of degree.

‘Okay,’ I said, ‘that’s interesting. I guess…..’ – here, I am tentatively trying to come to the point – ‘I mean something that’s interesting about that is that is sounds kind of *fluffy* to me…’

> It does, it does.

‘So, I’m intrigued…’

> It’s not that I like it.

‘Okay, um…so how do you deal with that then?’

> I think what you do – what like, constantly, that’s the whole purpose of being a neuroscientist I think, is to try to overcome the fluffiness and just try to formulate something that can be regarded as temporary truth, and you work with them.

‘But it seems to me that as the neuroscience of autism progresses…there’s less certainty. That it’s….more answers become possible rather than fewer.’

> Yes, that’s quite difficult to cope with, it’s true. Yes, like, there are other theories that it’s a theory of mind issue, theories like weak central coherence and so on, and they all have their niche in autism spectrum disorders…but there is nothing so far which really [*inaudible*] the potential to cover it all. Yeah, and we go further away from it.

Here, a really interesting tension opens up around this question and this is the tension between, on the one hand ’the whole purpose of being a neuroscientist’ and, on the other, the fact that, as research progresses, ‘we go further away’ from autism. ‘Yeah,’ she said again, when I pushed it,

> I think we are not working towards one final truth. I think that’s too challenging for us. We will never get there. Like neuroscience in general and with autism spectrum disorders, let’s say, well what would that truth – there are *sometimes* final truths like with, um, let’s say Down’s Syndrome has a certain truth behind it. Well then again that’s maybe, I don’t know, maybe questionable – there are differences between individuals with Down’s Syndrome.

‘Speaking very generally, that’s an interesting distinction to draw,’ I said – ‘the distinction between Down’s Syndrome and autism spectrum disorders […] I guess what’s interesting about Down’s Syndrome is that you can point to a particular chromosomal problem and say ‘this is it.’ You don’t think we’ll ever do that with autism?

> ‘There is many, many genes which are involved, it seems. There’s no universality.’

'Not just on the basis of genes, though. Do you think we could ever point to a brain area or a function or a something and say "this is autism?"'

> Well I think the only truth that people agree on is that that's not the case.

I like this exchange, because not only does it give us this 'no universality' view of autism in a very clear way, but this researcher clearly relates how the lack of 'final truth' is both a property of 'neuroscience in general' and something that's quite 'difficult to cope with.' This is important: in 2011, neurobiologization is still something that tends to be hard, and confusing, and exasperating. In his discussion of personality disorder, Martyn Pickersgill has shown how neuroscientists manifest an ambivalence about their own efficacy, rooted in the intractability of this diagnosis. For Pickersgill, such ambivalences might

> direct our attention to practical issues surrounding the potential of neuroscience to translate into and enhance clinical practice, as well as theoretical concerns revolving around the place and role of the biological within contemporary neuroscience and psychiatry (2011: 460).

Something very similar is manifest in this conversation – in which the story of autism's putative neurobiologization is neither straightforwardly one of victory nor failure. It comes, instead, in the form of a rolling account, in which the researcher's commitment to neuroscience sometimes runs up against the daily reality of being an autism researcher.

Later on in this interview, we got talking about gender and its relationship to the brain, and my interviewee was markedly hostile to neurologically-determinist accounts of gender. Socialisation, she explained, was a big issue for developmental neuroscience in general – because it was impossible for the researcher to isolate where or when, exactly, the almost infinite constellation of possible interactions between genes and environmental inputs, were woven just tightly enough to forum the kinds of culturally identifiable and interesting shapes that we might tentatively name 'autism' or 'male.' 'Yeah,' I said, 'so I mean – so it's a particularly difficult science, I guess, is what's interesting about it.' There was a bit of a pause: 'a very fluffy one,' she eventually said.

Let me stress that it is not my purpose at all here to enter a critique of developmental cognitive neuroscience as a particularly 'fluffy' pursuit (which, at any rate, would be a short-sighted move for an interpretive sociologist). But I am interested in what has happened in the space of a conversation that lasted only a little over half an hour, and in the context of a discussion only about the definitions that were at stake in neuroscience research on autism. This young researcher has moved from an uncomplicated self-narrative in which she becomes a neuroscientist precisely to distance herself from the fluffy behaviourists and psychoanalysts who dominated the discipline in her home country, to an account of developmental cognitive neuroscience, her chosen area, as, now, itself, a decidedly complex and compromised intellectual pursuit. What I am trying to get at, in this chapter, is the way that talking about 'what autism is,' to a neuroscientist, brings these things to the fore. My suggestion is that working on autism, and talking about it, and then trying to describe your efforts to someone else – that this can reveal something important about the complex entanglements that are at stake in neurobiological labour. It begins to show just how close to the surface are the concessions, and the confusions, and the ambivalences, that can sometimes make up its daily practice. But how do people deal with this uncertainty? And how does a more-or-less stable and researchable autism persist across it?

If you've got Down's Syndrome, you're not fascinating

I have said when neuroscientists talk about autism, they sometimes rely on a register of autism as plain biological truth. But also, and perhaps more often, they talk about the complexity and heterogeneity, and even the non-existence of autism as a natural category. But what is striking to me about all of this is that no-one throws up their hands in despair. Even if no fact about it is uncontested, still my interviewees seemed to trace a coherent autism neuroscience through both the categories of 'biological truth' and 'umbrella of convenience.' In order to begin thinking about how this is done, I want to introduce another category that was consistently deployed in the course of discussions about 'what autism is.' This is the category of 'enigma,' and, extending out from it, the characterization of autism as an object of particular 'fascination.' My argument is that focusing on these 'enigmatic' qualities allows interviewees to trace a neurobiological account of autism across some very different discursive registers.

From a monograph by Uta Frith (2003) (*Autism: explaining the enigma*), to the blog of neuroscientist Jon Brock (2012b) (*Cracking the enigma: an autism research blog*), to a recent special issue of *Nature* (2011) (*The autism enigma*), the specific term, 'enigma,' is strikingly common in discussions of the neurobiology of autism. An interviewee (R08) once said to me, explaining how she had altered her research trajectory post-PhD, that she had

> literally brainstormed on a piece of paper the different things that I was interested in, and out of that came that I had this real intellectual interest in autism that had always captured my imagination, the kind of 'the enigma' and all that.

'And all that,' is telling here– it suggests a shared understanding that the word is now even a bit of a cliché in autism research, and that just by using it, the researcher can gesture at, and assume that I'll instantly understand, the long chain of associations that she's referring to. 'Enigma' itself is an interesting word– because it doesn't just suggest mystery; it specifically implies a puzzle with a ludic element to it, something 'to afford an exercise for the ingenuity of the reader or hearer' (Oxford English Dictionary, 2012). This quality of the research was also frequently expressed by interviewees in terms of their 'fascination' with autism. 'There's a certain element to people with autism that is intrinsically fascinating,' said one (R12) – 'that sparks your interests and makes you ask: why are they the way they are? What is going on inside them?' Another (SS07) said of her first encounter with autistic children in a special school: 'it's just so different… I found it completely captivating.'

But what is it about autism, specifically, that gives it this fascinating, enigmatic, captivating quality? 'I think the fact that autism is essentially about social engagement makes it a different disorder,' a lecturer (L04) suggested,

> or makes it stand out and makes it rather unique, that as a society we…social contact is absolutely crucial, and we can't really comprehend that you wouldn't want to have that contact for example, and our world doesn't work without that social interaction at varying levels. So I think there's something just quintessentially different about autism, that's to do with this social instinct, if you like, that we must have, that people with autism either don't really have at all, or have in a really unusual way.

A PhD student (P02) said:

> I think it's, it's really an interesting disorder, because it's kind of everything that's sort of dysfunctional in autism is kind of what makes us kind of human, if you like […] I think it can tell us a lot about, sort of, how we are as humans generally, as well as the actual autistic conditions.

The suggestion in both of these contributions is that autism is fascinating because 'what autism is' is no less than a privileged microcosm of human development in general. Indeed, my argument is that the researchers are figuring autism as a mysterious gateway to something much larger – and that this makes all of the ineffability, and the weirdness, and the complexity, much easier to live with. In these descriptions, the contradictions of autism are configured less as barriers to knowledge, and more as hints of the great secrets that lie behind. This is how, I think, an association with some enigmatic – maybe even an unknowable – humanness allows researchers to bridge these thorny questions of biological essence and distributed heterogeneity.

'The first time I met these guys,' said a more senior person (SS03) to me, 'I thought, "wow," you know,'

> they embody kind of all the things that puzzle me about psychology. Because it's a developmental disorder – in other words, there's something different about their constitution, which doesn't determine, but constrains the way they develop, so they develop differently, along a range of different trajectories, which have certain things in common. And so it touched all the buttons, I was just, you know, 'what's it like to be you?' I mean, I've always been fascinated by that about, you know, anybody, any other person you meet: 'what's it like to be you?' But these guys, being so different. They *are* so different. And yet so similar, you know?

This sense of 'so different….and yet so similar' is a critical link for connecting the diagnosis of autism to the question of the human. People with autism are enough like neurotypicals to be considered as some kind natural laboratory; but also odd enough, and odd in the right ways, for the distinctions to hint at something larger and more interesting[16]. By drawing attention to these registers, I do not want to focus too much on the solidity of these links. I draw attention to them, here, only to show how this sense of intrinsic and on-going fascination might partly explains how researchers persevere with neurobiological studies of autism, even amid the inevitable difficulties, and confusions, and setbacks. I am arguing that this conception of autism, in which a vision of radical human alterity lurks just out of sight, helps autism neuroscience to trace connections delicately across some

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[16] There is a tantalising hint, here, of the way that autism might be used as a laboratory, or a 'biomedical platform' (Keating and Cambrosio, 2000; Gillis-Buck and Richardson, Under Review), for thinking about 'social cognition' as a neurobiological marker of human distinction in general (Adolphs, 1999; Saxe, 2006). This was something I pursued without great success among some of my early interviewees, and which I eventually had to let go. In fact, it is likely that this thought is not so well-developed among autism researchers themselves – and anyone researching it would likely have to seek data elsewhere. Still, several referred to it as part of their 'fascination' with autism – although none was seriously pursuing it as part of their research programme.

different, and in fact quite contradictory, definitions – here, a biological truth; there, a simple check-list.

Consider the following extract, which is taken from an interview conducted with a senior professor (SS01) who had trained at the Maudsley Hospital in the 1960s. 'Yes,' she said when I brought the topic up, 'it is intrinsically fascinating…

> I mean, like these early children that my colleagues talked about at the Institute of Psychiatry, we would be talking about, over coffee, this child that might, you know – we were very young in these days; you wouldn't get on a course as young as it [today]. We were totally naïve and in our early twenties, and didn't know anything about anything. And then there would be these children that would do really weird things. It was the mismatch between often the appearance of being quite bright, and then not really having any common sense. But also occasionally you'd get children who had these, um, you know strange behaviours, repetitive behaviours or obsessions, or knew strange things. I remember this one child I was asked to assess with autism, sat there saying, 'you don't want to do this.' I sort of thought 'no, I don't.' And then I thought no, actually, what he's….[*words swallowed by laughter. What she's talking about is the sudden and unexpected acuity of the child with autism*]. And I thought it was great, you know - woah!! So it is very…but again I think I'm partly driven by the fact that I'm such a neurobiologically-oriented person. I wanted to know, you know, what in the brain generates some of these behaviours.

In this extract, she, like many of the others that went before, describes this mysterious quality of being somehow bright and lacking common sense – of autistic people being somehow the same as herself, but also somehow very different, an almost 'knockout' quality that (she thinks now) initially drew her in. But then – as is a common feature of these interviews – she launched quite suddenly into self-description of herself as 'such a neurobiologically-oriented person.' Originally, I heard this as the re-erection of a boundary, against the rather human and personal turn the interview had taken. Anne Harrington (2005), in her work on Oliver Sacks, has written about the ways that the romance of the individual case history can trouble the border-line between the humanistic and the strictly-neurobiological: 'the particular, the emotional, the value-laden, the meaningful, and the relational aspects of human experience,' Harrington suggests,

> functions to remind us that being a human being—a human brain—is still a more complex and richer thing than can be contained in the spare and reductionistic vocabulary and frameworks of our sciences (ibid.).

Adding to this observation, and inflecting it a bit differently, I also now read my interviewee's coda ('I'm such a neurobiologically-oriented person') as a deliberate mingling of the relational and the scientific. I hear it as a way of bringing some neuroscience back into this conversation about fascination: neurobiological research, she was emphasising, was not outside of, or inimical to, or something that had to be disentangled from, her fascination with the enigmatic world of human difference.

Indeed, it seems to me that, far from marking a boundary-point, neuroscience emerges here as a home for working through the relays between her fond memories of clinical process and human difference. And it was the study of autism, in particular, that allowed these qualities to co-mingle. 'I mean, this is the sad thing,' the same interviewee said later,

> if you've got Down's Syndrome, you're not fascinating. […] everything is just deficit. You're like a younger child. So, in autism, you get behaviours that are not just like a younger child, you get behaviours that are unexpected – and the other thing, when I first started to work on language, and I still remain absolutely gripped by it, is that what they emphasise is what is weird, and often that is language, if we're not autistic…so, if you get these children who are very over-literal, so, em, there's some wonderful, wonderful examples – one being a teacher saying to a child at school just sort of 'just go into the toilet and get yourself a glass of water,' and the child is sort of getting the water out of the toilet bowl. But then you think, 'well, why don't we all do that, you.' […] You feel that what the autistic child is doing is actually more logical than the rest of us. They're taking a literal interpretation of what you've said, and then it forces you to think, 'how the hell do we operate and not do that?' And I regard that as still a huge question of enormous interest to me. If I could crack that I'd be very happy. And then there is restricted interests – which are so odd, I mean, you know, why are…I mean okay, some of them are just like normal little kids only more so, but, you know, when you get these kids that are fascinated by drainpipes, or lamp-posts,, you know, what the hell is that all about? It's just sort of…if you're not fascinated [*laughs*]…so, you think that there's…there's something there, that potentially, if you could understand it, and get to the bottom of what it's like to be human.

What attracts me to this extract is the way that this interviewee ties together the themes of the intrinsic interest of autism, its admirable qualities, and the privileged insight into the human it grants. But at the heart of this story, is the tracing of a now-memorialized intuition, that, through these different things, 'there's something there' – and that this must be something of 'enormous interest' to her, as a 'neurobiological person.' I am not at all suggesting that people talk about 'enigma' to strategically avoid hard questions, or uncomfortable truths. I am suggesting that creating space for the enigmatic is actually what makes it possible for a neurobiologically-oriented person to live with contradictions that would otherwise be hard to justify. It is also what allows her to begin to trace nonetheless firm neurobiological understandings across those differences. The enigma of autism, situated in memories of clinical encounters, and (no doubt) apocryphal stories of autistic difference, allows this neuroscientist to trace some coherent sense of autism, and of the neuroscience of autism, through time, past her own bewilderment, and across some radical human differences.

There's this thing we recognise when we see it and it's this thing called autism

Another striking and not-obviously-scientific register in which my interviewees talked about 'what autism is,' and how they knew it, was that of a *feeling* of distinction about autism. Several times, after people had talked (at some length) about the heterogeneity of autism, and about its trickiness as a disorder, and just, in general, about the lack of certainty surrounding its existence as a clinical entity in the first place – they would then confidently assert, nonetheless, that there was something distinct and knowable about autism all the same, even where this commitment could only be articulated as a feeling, or a just-knowing. This was first suggested to me by a young child psychiatrist (L08), who was also an fMRI researcher. This was actually quite an awkward and hurried encounter, arranged at short notice – and, with the conversation skating across unremarkable, mainstream, public-facing accounts of autism neuroscience, I never really felt as if I had broken through the professional veneer. However, when I asked him – because he was one of the relatively small numbers of interviewees who also maintained a clinical practice – about the difference between diagnosing autism for research (where specific cut-off points on two of the 'gold standard' scales, the ADOS and ADI-R[17], are typically used) and diagnosing it in the clinic, he said:

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[17] Respectively, the Autism Diagnostic Observation Schedule [ADOS], in which an assessor rates performance on a series of tasks with the person who is being diagnosed (Lord *et al.*,

> Yeah, well, in the clinic, as you say, there are often people who fall short on one or more of those [*the ADOS or the ADI-R*], and then you've got to use your clinical judgment to decide whether the level of impairment they have is sufficient to warrant the diagnosis despite falling short on one or more of those tools, or their, sort of, how they feel to you as a clinician, then the feel of somebody with Asperger's, despite falling short…I suppose the difference there from research is that, to actually get a paper recognised by the community, the research community, they look to see that people have been positive on the criteria. Whereas if you're doing it in order to work out what's best for that person, it's a bit different .

I was quite taken with the use of the word 'feel' in this context, especially from such an apparently straitlaced person, so I asked him to elaborate a bit. 'Yeah, feel is the wrong word,' he (perhaps predictably) said,

> because you don't actually use your hands to do it, because this is more about…[*Longish pause. I stupidly butt in: 'clinician skill?'*] Yeah, because with autism, it's a syndrome, so there's a collection of signs and symptoms. It's like a pattern almost. People will have a number of this very long list of signs and symptoms. And somebody who meets a lot of those, who has a lot of those symptoms, but not others, they'll have a certain way that they come over – so they'll for instance be using some of the language, they'll have some of the language features, for instance, of autism. Or some of the social features of autism. And it's that mix of features which makes somebody feel to a clinician whether they do or don't have a disorder, in some ways.

There are a number of interesting issues here. One, of course, is that the autism of the clinic is not necessarily the autism of the clinical trial –which is a well-recognised (albeit unresolved) issue within Randomised Control Trials (RCTs) of psychiatric diagnoses (Zimmerman *et al.*, 2002). But perhaps more interesting, in this case, is what marks the difference: for inclusion in the clinical trial, participants will have to pass a given cut-off on at least one, and preferably both, of the 'gold standard' quantitative scales. This is a requirement of publishing in a good journal – and is obviously governed by concerns about the homogeneity of participant populations across different studies. But for the clinic, where this kind of specificity is less of a concern, there is a different solution, and this is to cede some epistemological space to whether autism is actually 'felt' by the clinician in the course of the encounter. I

1989); and the Autism Diagnostic Interview – Revised [ADI-R], which is a structured interviewed conducted with the parents of the person being diagnosed (Lord *et al.*, 1994).

am interested, here, in the way that, for these interviewees, autism remains something qualitatively distinct and knowable, even when all available diagnostic tools fail to mark it out. I am also interested in the way that interrogating a feeling helps the researcher or the clinician to trace the neuroscience of autism across this tension.

Of course, these issues are not unique to autism. They track longstanding debates about the 'art' and 'science' of clinical diagnosis – and, in particular, discussions about 'evidence-based medicine', worries about the dissolution of the gap between clinic and lab, the emergence of a 'clinical science' to service this rupture, and so on (Gordon, 1988; Berg, 1995; Lambert, 2006). In one way, this is the intellectual and political context in which we should read, and try to interpret, the willingness of some clinicians and research to talk about their recognition of autism in terms of its 'feel.' But I am also keen not to simply re-create those longstanding discussions – which are well established in both the medical-sociological (Mol, 2008) and the medical-medical literatures (Malterud, 2001). What I wish to draw out here, instead, is an insistence on the qualitative distinction of autism as a disease-entity. This is an an insistence that allows my interviewees to trace autism across these questions of 'unchanging core' and 'symptom check-list.' And I want to draw attention, also, to the way that this tracing is done, and this is the part-deferral of 'what autism is' to some sort of *feeling* of autistic presence. Thinking through the 'writings, films and statements of those autistic individuals who seek to represent themselves,' Stuart Murray, has insisted upon both the multiplicity and indelibility of a specifically autistic 'presence,' which 'extends beyond the ways the conditions is labelled in medical and other institutional contexts' (2008: 33). And yet what my interviewees show is a quasi-medical commitment to something like that presence, and also a willingness to see it and to feel it, even when it went quite unmarked by the range of otherwise well-regarded clinical measures.

Consider, for example, and in this light, the following exchange that I had with a young neuroscientist – a postdoctoral researcher (R05) in a metropolitan psychology department that had a heavy research concentration on autism. He said to me, in the middle of a fairly frank discussion about autistic heterogeneity:

> there is something about each individual with an autism spectrum disorder that makes them part of the autistic spectrum. They do share certain difficulties, and certain areas, and they do share a cognitive profile of

> difficulties, that, even though they can be expressed in very diverse ways, there's still something that makes all of these individuals autistic, in a sense.

This, again, I read as insistence on qualitative distinction – an unchanging core, a 'something' that is remarkably indifferent to heterogeneity and complexity. I said: so it's not just an umbrella of convenience, then. And he replied,

> I think if you talk to pretty much, you know, generally the scientific community on autism, you will kind of - they will pretty much all tell you that you know whether somebody is autistic or not. So there's a certain kind of feel to the interaction, and you just….it takes a bit of time, once you've met a certain number of people with autism, you just kind of develop a radar for it.

Again, the word used here is 'feel' – this, if needs be, is quite a valid 'way that you know' about autism, within the 'scientific community.' Later on, I asked the same interviewee about a hypothetical 'brain scan' for autism, and whether he would be inclined to rely on the brain scan for diagnosis, or on the feeling of an interaction. He paused for a bit. 'That's a good question,' he said eventually:

> Probably the interaction – because autism in the end is defined by a collection of behavioural manifestations. So, if I put you in the scanner, and your brain looks like an autistic brain, and then you behave in a completely non-autistic way, I wouldn't call you autistic. Whereas if your brain wasn't autistic, and you behaved in a very autistic way, I'd probably think that you were autistic. So if the two are in conflict then you always kind of go with the behavioural manifestations, and those are the ones you pick up in the interaction with somebody. So I'd probably go with behaviour over brains, or biomarker.

Here, I am struck first by the re-appearance of a view that 'autism in the end is defined by a collection of behavioural manifestations,' when, only a minute or so before, he had insisted to me that 'there is something about each individual with an autism spectrum disorder.' But then, second, he takes the feeling of an interaction, i.e. the very sense that tells him there must be some 'something' there, as his guide to the presence of this collection. And he does so over and above the possibility of the brain scan. It is a bit difficult to parse all the different elements in play here, and I am also wary of over-interpreting one young researcher's off-the-top-of-his-head account. But let me at least draw attention to the way that this tension, between a collection of behaviours and a discrete 'thing, is again traced together by a reliance on, and a deferral to, the quasi-affective labour of feeling and sensing the quality of an autistic interaction. This sense of feeling, here, grants him a simultaneous commitment both to the 'collections of behaviours' *and* to the 'something' that 'makes someone part of the autistic spectrum.'

This is also how I read the following extract, which comes from an interview that I conduced with a more senior researcher (SS07), well-known for her input to debates around the 'heterogeneity' of autism, and what this means for the unity and coherence of its core symptoms. I think that when I went to interview this person, I had expected her to expound more on this theme – but even though she did talk through this topic, I came to realise that she was still keen to stress that autism was something that nonetheless remained – in her words – 'true to itself.' One of the ways she had come to recognise this fact, she pointed out, was by recognising autism in other cultures: 'people with autism that I've met around the world are very recognisably alike in some important ways,' she said:

> So, when I went to Japan, I understood the autistic culture much more than the neurotypical culture – and could recognise much better where the questions that the person with autism was asking me came from. And the colleague I met, who I think had Asperger's syndrome – I understood more about maybe what his view of the world was, and what his expectation of my behaviour would be, and what he wanted from me, than what the colleagues who'd invited me…[with] who[m] I wasn't sure if I was maybe doing the wrong thing or, you know, stepping on toes […] And then there's the element of seeing how neurotypical kids start off very idiosyncratic – very funny and very much themselves. And then they get sort of…it's not really peer pressure, they just become part of the herd. And children become less interesting, in the sense that they become more predictable – you know, you can say what most children will be interested in by a certain age [*inaudible*] so there's a kind of a blinkers put on through socialisation.

In this extract, the interviewee very ably describes autism as something that can be known precisely because of its qualitatively distinctive, in-itself qualities. The specific quality of autism is not marked by a 'feeling,' here, so much as a kind of visual and inter-personal 'recognition' of it – in areas (such as other cultures) where you might expect it to be arranged differently, if it was just a matter of diagnostic convenience. And this is not at all a question of whether or how autism is diagnosed in other cultures – but rather of whether the individual who had learned to see it, to feel it, to recognise it, to interact with it…whether or not she can see, feel, recognise and interact with it there too. As another quite senior professor (SS04) summed up this view:

> We know that autism is heterogeneous in terms of aetiology, So all the individuals who have what we call autism won't have it for the same reason; the cause won't be the same always – [although] in some it might be a pathway that overlaps or is common to some presentations. And we know that it's heterogeneous, in regard to [the fact that] different people who have autism can look very different from each other, and individuals change over the life course a lot. So there's all this heterogeneity. But we also think there's this thing we recognise when we see it, and it's this thing called autism.

This is precisely what I have been trying to draw attention to, here – that we know all about the heterogeneity, and the complexity, and the different causal pathways. But despite all of that, there is something we know when we see it – and this something is autism. This is an almost embodied, affective idea of autism as something that, for neuroscientists, manages to transcend both the laboratory space of biology, and the clinical space of diagnosis; in these accounts, autism is simply known and affirmed, when it is seen, or felt, or recognised.

This commitment of feeling and recognition is, I argue, another of the most important registers that neuroscientists draw upon in order to trace autism through its tricky and crossed appearances. Here, an appearance in the indelibility of biological and embodied presence; there, in the more quantitative and pattern-based commitment of the contemporary neurosciences. What I am trying to draw attention to, here, is the way that autism neuroscientists talk simultaneously about biological truths and umbrellas of convenience. I am also trying to focus on the way that autism is traced through a research programme as both an unchanging reality *and* a convenient way of linking up some genetic and environmental nodes. My argument has been, then, is that we may wish to see the capacity and willingness of neuroscientific researchers to talk about autism as an enigmatic object of fascination, and also as a felt or a recognised 'thing,' as one of the most fundamental elements of that tracing.

Conclusion

In this chapter, I began by saying that even the most basic facts about 'what autism is' are open to contest. And so I approached neuroscientific accounts of 'what autism is' as empirical objects in their own rights. I asked: what do neuroscientists, who are working on a still-in-progress neurobiological account of autism, actually say when you ask them about what autism is – and what (if anything) does this tell us about the way that those kinds of (neurobiologized) accounts are put together? When I did that, I encountered a range of different positions that I thought could be brought under two broad headings: one that took autism to be something like a 'biological truth' with an 'unchanging core,' and one that focused more on the 'heterogeneity' of its appearances, that saw it as a 'symptom checklist,' and also as an 'umbrella of convenience.'

Thinking broadly about processes of biomedicalization and neurobiologization, we could, in one sense, see autism as an exemplar of these trends. Here, after all, is a relatively new diagnosis, one heavily bound up with emerging technologies and with the marketization of health. It is also a diagnosis in which we could clearly locate a shift in the medical gaze, away from categories of discipline and cure, and towards the maintenance of health, the management of chronic illness, and the endless sifting for as-yes-unknowable genetic and neo-natal risk factors (Rose, 2007a). But in this chapter, I also tried to look in depth at just one part of the complexity that lies beneath this story, and this is a complexity marked by the inability of neuroscientists to even agree that autism is much of a meaningful biomedical category in the first place, and also by their willingness to find themselves sometimes a little dumbstruck by the complexity of it. Most of all, I have focused on their ability to trace their practice across an often tricky two-way commitment to autistic heterogeneity and autistic distinction. As I argued above, none of this is intended to particularly run against the convincing framework put forward by Adele Clarke and her colleagues, which I think is well-able to absorb this account. But it is still to say, all the same, as far as the new brain sciences are concerned at least, and particularly as they run into developmental diagnoses, that the path to biomedicalization might not always be very smooth; that it even may sometimes run *so* unsmooth, as to no longer leave us in great confidence about the destination.

I identified two registers in which neuroscientists talked about autism, and which, I argued, helped them to trace the disorder through this tricky and varied terrain. These were: (1) 'fascination' with autism as an 'enigma' to be solved, and (2) 'recognition' of autism as a 'feeling' in an interaction. Drawing on these two categories, I argued, helped neuroscientists to hold autism together, across the researchers' dual commitments to registers of diagnostic convenience and biological truth; indeed, it might well be argued that it is just these kinds of esoteric categories of practice, epistemology and affect, within both research and clinical spaces, that may help us to more precisely explain what animates such a tricky and sometimes contradictory development as 'biomedicalization.'

In a related discussion of the 'geneticization' of schizophrenia, Adam Hedgecoe has pointed out that researchers who work in this area actually employ a subtle and multi-varied discourse of causation, and one that is at least discursively generous to processes of nurture, rather than being crudely reductionistic – which Hedgecoe characterizes as a 'reasonable' and 'non-extremist' rhetoric of 'enlightened geneticization' (although Hedgecoe remains suspicious that this may be as much a strategic as it is an ecumenical positions) (2001: 882, 903). I do not believe that most of interviewees were terribly interested in my experiencing them as enlightened. But I *do* believe that the equally sensitive, open-minded and non-reductionistic discussions of potential 'neurobiologization' that I encountered, in the space of neurobiological autism research, may well help scholars to think in some more detail about the specific inflections, complexities and vagaries of this process. My suggestion is that that these delicate and unexpected registers, like feeling and fascination, that help neuroscientists to carefully trace organic phenomena through, pattern-changes, parametric vectors, and pattern-classifications, should probably play a more prominent role in discussions of neuropsychiatric and neuropsychological disease more generally.

All of this matters too. Although I have presented this, for clarity, as a discussion taking place within clinics and laboratories, in fact many people and groups have a stake in the 'biology' of autism, and in the specificity of the relationship between autism and the brain in particular. On one level, there is the politics of parent activism: ever since the psychogenic account of autism located the disorder in a kind of maternal coldness, autism research has been beset with a familial and gendered neuro-politics, and also an intricate politics of expertise – which have together formed a series of decades-long contests, in which the mantle of 'science' has been (successfully) claimed by formal amateurs, and also in which laboratory-based biological knowledge has proceeded in alliance with parents and their advocatory organisations (Feinstein, 2010: 116). Unquestionably, the kinds of oscillating accounts that I relate in this chapter, and the uneasy movements between ‘biological truth’ and something more disparate that I have described, are at least part-structured by precisely this politics.

On the other hand, there is another politics at stake here, and this, as briefly discussed in the previous chapter, is the politics of neurodiversity – which draws on the rhetoric and strategy of older identarian and liberatory movements, to stake a claim for autism as a difference to be respected, and not a disorder to be cured. This claim – newer, and less mainstream than the familial politics described above – is even more heavily invested in a more-or-less solid identification of autism with some ‘innocent’ neurological substrate (Silverman, 2011: 163; Ortega 2009: 434). While I have no urge to advocate for either of these (often competing) kinds of claims, I do want to be point out that as long as the brain itself can be positioned as innocent (e.g. in the famous ‘my brain made me do it’ claim. See Mackintosh, 2011) then neurobiological accounts of disorders like autism will remain politically potent, and will ultimately have to be complicit in something. How effectively, and to what degrees, autism gets traced though both an organic and a pattern-based view, and also how successful and convincing such a delicately traced neuroscience can continue to be – this, ultimately, matters to someone.

3. The trouble with brain-imaging – dynamics of hope and disappointment in a tracing neuroscience

Introduction

One of my key aims for the thesis is to show that an interested sociological literature has missed the complexity of putting together firm neurobiological accounts of psychological and psychiatric diagnoses. In the introduction, I said that I wanted to describe some of the uncertainty and ambiguity that I had found in this space – and that I would show how doing a neuroscience of something like autism is often a practice of trying to hold together, or work across, some of these ambiguities and complications. In the last chapter, I showed how one basic uncertainty in autism neuroscience is held together, and this was the uncertainty over definitions of autism as such, i.e. discussions about whether autism constituted a 'biological truth' or a 'diagnostic convenience.' In this chapter, I will consider some of the ambiguities and complications in how autism neuroscientists actually relate to neuroscience itself.

My goal for the chapter is to show that, within my interviews, there were some remarkably divergent and contradictory ways of relating to neuroscience[18]. Interviewees differed markedly in discussions of how hopeful they were about neuroscience as a set of practices, about how useful they found it, and how reliable they considered it – a degree of complexity not well-described in a commentating literature focused on senses of self-confidence and disciplinary strength in neuroscience (Andreasen 2001; Iacaboni, 2008; Ramachandran, 2012). To show this, I will first describe how, for some interviewees, the methods and rubrics of the new brain sciences are a mark of disciplinary and epistemological strength, the sign of a maturing neuroscience of mental disorder, and a cause for clinical and therapeutic hope. Here, I will particularly discuss the excitement of using the tools of the new brain sciences to go beyond behaviour and to find a biomarker for autism – as well as the hope of uncovering autism as an essentially organic phenomenon, and something amenable to molecular intervention. Second, however, I will show that there is also a strong current of disappointment running though these interviews,

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[18] Although, when I talk about problems with 'neuroscience' in the chapter, this will mostly mean brain-imaging. As I discussed in the methods sections, what gets to count as – or stand in for – 'neuroscience' is a complex and dynamic process. See Abi-Rached (2008) for a discussion of the 'fields' of neuroscience – but bear in mind that most of my interviewees were cognitive neuroscientists, and brain-imagers.

and a potent sense of anxiety about the methods, assumptions, and technologies that are inherent to the new brain sciences. In particular, I will show that quite a few of my interviewees talked about their own practice within a register of unease about the artificial nature of the brain sciences; they consistently expressed a discontent with its lack of objectivity and its relatively small contribution to psychology, and several held an anxiety about the presence of 'blobs,' practices of the air-brushing of data, and so on.

The key argument of the chapter is that just as the neurobiological account of autism is traced through exclusive-looking registers of biological truth and diagnostic convenience, so do my interviewees trace their object through a strange and dynamic relationship to neuroscience itself. And this is a relationship that is as well-versed in the language of disillusion and disappointment, as it is in the strait-laced semantics of hope. Aligning this suggestion with the broader themes of the thesis, however, I will argue that the co-presence of these contradictory accounts does not simply reveal an unexpected complexity in how neuroscientists actually think about neuroscience. I have already described the complex holding-together of very different ways of talking about autism as a 'tracing' – my metaphor for describing the way in which these neuroscientists actively work very different things through one another, but without giving up on a distinct entity or phenomenon 'on the other side.' In what follows, I will also draw on the imagery of 'trace' and 'tracing' to interpret the presence of these simultaneously hopeful and disillusioned attitudes to neuroscience. My argument, which I will elaborate through a reading of Karen Barad's 'agential realism' (2007, 2011), is that a scientific practice characterised by twin-commitments to the (sometimes frustrating) entanglement of its object, but also to a (potentially promising) singularity of that object, might well have a complex and equivocal relationship to rhetorics of disappointment and hope.

Scientific relationships to hope are not new topics within discussions of the life sciences. Most prominently, the 'sociology of expectations' (Brown and Michael, 2003) has directed attention to the way that emerging scientific ventures are often maintained by, or oriented around, discursive structures of hope, optimism, and positive expectation (van Lente and Rip, 1998; Borup *et al.*,2006; see Slaby and Choudhury, 2011, for a version of this argument that applies particularly to neuroscience). In what follows, I will more precisely position my broader claim (i.e. that my interviewees trace the neurobiology of autism through crossed discourses of expectation and disappointment) against this literature. In particular, I will argue that the 'expectations' literature lends an important new perspective to the study of the new brain sciences, but that my data show how we may need to take fuller account of the complex way that scientific objects may also be traced through an intellectual and attitudinal culture of *low* expectations – one that sometimes sits awkwardly against the promissory rhetoric of 'futureing.'

The chapter proceeds through six sections: in the first two, I will discuss accounts of hope, positive expectation, and disciplinary strength in neuroscience, focusing particular on hopes of finding a brain-based biomarker for autism, and of using this as the basis of intervention. I will also relate these accounts to previous disunions of scientific expectation. In the following two sections, however, I complicate this view, showing that autism neuroscience is also traced through a sense of disappointment and deflation, and here I will focus especially on interviewee's consistently-related references to epistemological problems within neuro-imaging. In the fifth section, I turn to Karen Barad, to argue that hope and disappointment might not be such surprising co-habitees within a tracing neuroscience, one that is as keen on 'agential cuts' as it is aware of ceaseless 'entanglement' (2007:333-334). In the final section, I draw all of this together to argue that a recognition of neuroscientists' critical relationship to their *own* practices might also call for a significant shift in broader sociological attention to the new brain sciences, and especially as that attention marks a call for more 'critique' (Choudhury *et al*, 2009; Campbell 2010; Kirmayer 2011).

The dream is to intervene

Throughout my research, I found that when my interviewees talked about their orientation to neuroscience particularly, their talk was often shot-through with rich discourses of hope, possibility and expectation. This was not a great surprise: the study of mental disorder has often been quite formally structured by a sense of clinical or therapeutic hope for the future (Moreira and Palladino, 2005). For more complex neurodevelopmental problems, this hope has been recently become embedded in, and articulated though, the search for brain-based biomarkers particularly – and also through the emergence of novel neuroscientific technologies that might mark these out (Raff, 2009). In the first pages of the first issue of *Nature* published this decade, for example, the editors self-consciously framed the 2000s as ‘a decade for psychiatric disorders,’ a sense of optimism and expectation that was quite precisely rooted in the idea that 'new techniques – genome-wide association studies, imaging and the optical manipulation of neural circuits – are ushering in an era in which the neural circuitry underlying cognitive dysfunctions...will be delineated' (Nature, 2010: 9). The authors went on, quoting the US National Institutes of Mental Health (NIMH) head, Thomas Insel:

> 'whether for schizophrenia, depression, autism or any other psychiatric disorder, it is clear…that understanding of these conditions is entering a scientific phase more penetratingly insightful than has hitherto been possible' (ibid).

My interest, here, is not in how accurate this claim is (although the ensuing decade certainly proved more awkward for neuropsychiatry than these authors anticipated. See Hyman, 2009; Kapur *et al.*, 2012). Instead, I am only focused on the degree to which these kinds of hopes, common enough in such a public-facing literature, were also often mirrored by the frontline researchers that I spoke to. ‘What intrigued me in the early days about MEG,’ said one brain-imager (R01) that I interviewed,

> is that, first of all, it is a beautiful combination of quantum physics, which is the underlying principle of the scanner, and the application to not only biological, but human, and even psychiatric problems, or neurological problems…it was sort of immediately a very sort of appealing way of having the dynamics of the human brain measured with a tool which is capable of capturing these dynamics.

This view, that technologies like MEG would open up the human brain, and give new insight into psychiatric and neurological problems, was clearly not uncommon: 'all the neurology-type people are looking for the biomarker, you know,' one professor of psychology (SS01) told me, '…and I think they have implicitly in their heads this notion that we will find something which will then, *[makes a whooshing noise*], it’ll part like the Red Sea.' Or as a senior third-sector employee (3S02) put it:

> there was a very strong sense about 5, 6 years ago…that the technologies to create the breakthroughs in conditions like autism were coming through – the neuroimaging technologies, the genetic analysis technologies, you know, and the sort of bringing to bear, if you like, of those technologies, you know, the sort of access to brain material and the kind of imaging that you could do with brain material, and indeed the chemical procedures that you could do with brain material. And that’s proved to be true. Things have moved forward enormously in the last five years – to the point where forms of intervention that are based on biology are now feasible.

This theme of 'technologies coming through' that will give 'access' to 'brain material' for 'intervention' perfectly expresses the basic register of hope though which quite a few of my interviewees narrated their own relationship to neuroscience.

For researchers and research-funders themselves (as opposed to service users or their families) what this technological hope ultimately expresses is the prospect that the field will significantly advance in some way. In the case of psychological and psychiatric research on autism, this goal often manifests as an expectation of reducing the field's reliance on behavioural measures for diagnosis. This came up quite forcefully during a conversation that I had with one researcher (L02) – herself very much involved in innovative work to find a quantitative-organic biomarker for autism, but whose background (perhaps tellingly) was much more embedded in the statistics of MRI analysis than it was in psychology. She told me about her experience of joining her current research project on autism, and being trained to use the ADOS. 'I was amazed at how many details these people [the trainers] pick up on,' she said,

> like, you know, you speak about instrumental movements and so on, goal-directed actions, and I just couldn't see it. And I could only do it with a lot more training – I'm talking months here.

Her amazement at the skill required by behavioural analysis is not only a compliment to clinical skill, I think – it was also expressive of a more fundamental surprise, i.e. that such skill is required at all. For this researcher, there had to be better, more predictable, way to go about this. And the best hope for advance lay with the new brain sciences: 'If you look at the behavioural studies,' she said,

> there are not too many differences on the behavioural level, when you look at adults. But there are also a few brain studies now coming out that show, actually, in terms of their anatomy, people with Asperger's [Syndrome] are different from people with high-functioning autism… if I was a behavioural researcher, I would feel that that [behavioural research] has maybe come to an end, because if we are now speaking about, actually, Asperger's or HFA [high functioning autism] is the same behaviourally, what are we going to research on – what comes next?'

Her basic hope is that developments in brain-imaging technology will reveal a difference in brain anatomy (between autistic and 'typically-developing' people) wider than the difference in behaviour – delineating and marking autism at a much finer level than is currently possible for even the most skilled clinician. 'Biomarkers could not only reveal causes of the condition,' as Pat Walsh and her colleagues note in a tactful review, 'but could also be clinically useful in complementing or improving the behavioural diagnosis of autism and in enabling earlier detection of the condition' (2011: 603).

A second and related technological hope is that, with the disorder locked down to an identified anatomical pathway, a clear entry-point for new kinds of diagnosis and treatment is opened up. As Singh and Rose have pointed out, this sense of opportunity is characteristic of contemporary psychiatric-biomarker research in general, but is particularly acute within the realm of the neurodevelopmental disorders (2009: 202). Hopes of diagnosis and treatment, in turn, are premised on another expectation, i.e. that if clinicians could intervene on the neurological substrate even before behavioural symptoms appeared, this would likely prove more effective, and more efficient in the long term: 'people with autism have got biological differences in brain development,' said one senior professor (SS10),

> you know, so that that's…and that's related to some of the things they do. The thing that we're work…and we've just identified what those differences are [using brain-imaging]. And we're in the middle of saying 'can you use those differences to diagnose people with autism rapidly, and/or in a cost-effective way?'

This was a common theme: 'the dream is to intervene prior to the onset of symptom,' one Principal Investigator [PI] (SS09) of a major imaging project said to me, 'you know, to try and divert the developmental pathway before the full core symptoms of autism become kind of embedded in the system.' Discussions of the desire to wring early diagnosis and treatment from neuroscience were never unsubtle, in my interviews, nor were they always present. But they were there all the same – and frequently so. Perhaps summing up this view, one senior professor (SS07) put it to me like this:

> I don't think there are many people in autism who would say that they don't want to understand other people, even if they choose not to engage with other people at the level, so I would expect that [one day] we would be able to intervene psychologically, neurologically.

The idea of treatment, especially, is controversial in autism – and particularly so the idea of intervening neurologically at a very early stage (Barnbaum, 2008; Barnes and McCabe, 2011). I didn't really encounter anyone who was entirely unsympathetic to the view that there are good reasons to be wary of these – but I still encountered, quite frequently, the hope that people with autism would be able to be diagnosed earlier and treated, in the future, specifically by acting on the brain. As Laura Schreibman points out – directly after acknowledging the 'controversies *within* controversies' that structure this debate – 'we still have no cure for autism. Yet there is reason to be hopeful' (2005: 133). I encountered many neuroscientists who still carried this hope.

Now we have all these wonderful tools

Quite a few scholars of science and technology have lately turned their attention to thinking about the role of hope and expectation in gathering together large-scale, diverse technoscientific projects – such as the search for a brain-based biomarker of autism – and have begun to identify some of the ways that these projects actually get justified and assembled in the present, through the expression of some promise or prospect for the future (van Lente and Rip, 1998; Brown and Michael, 2003). In particular, scholars within this tradition have focused on the role of the specifically *promissory* 'expectations' that get attached to scientific and technical projects, and around which resources and actors can begin to assemble themselves: 'technological futures are forceful,' van Lente has pointed out: 'once defined as promise, action is required' (2000: 59). By expectations, then, is meant 'wishful enactments of a desired future…hyperbolic expectations of future promises and potential' (Borup *et al.*, 2006: 286). Thus the emphasis is mostly (although not entirely) on a collective desire to imagine something basically *good* for the future, through the assembly and propulsion of some scientific and technological practice. On the basis of this promise, it becomes reasonable, and even imperative, to actually enact that project or practice in the present. It is in this sense that, for these authors, expectations are thought to play a generative role in scientific contexts: they 'guide activities, provide structure and legitimation, attract interest and foster investment' (ibid.: 285-286). Indeed, as Brown and Michael have argued, through the articulation and enactment of varieties of expectation, the epistemic and practical distance between the past and the future is discursively (if not materially) elided: 'the future is mobilized in real time,' they point out (2003: 4). Across this narrowed gap, elements of a research programme can be rapidly drawn together.

Unquestionably, this ‘sociology of expectations,’ although usually focused on more public discussions (Kitzinger, 2008), at least partly explains what’s going on in my interviews – insofar as these expressions of hope can also be read as one element of actually putting a project together. In this sense, loose promises of neurological diagnosis and therapy in the future become the ground on which large-scale projects are enacted in the present. Brown and Michael have also noted an inverse correlation between closeness to the actual scientific practice, and the level of expressed hope (2003: 12-13). And while I found these expressions at all levels among the scientists in my interviews, from PhD students to senior professors, some of the more compelling and thought-through articulations came from the (slightly more distanced) leaders of large-scale projects – who were clearly not articulating their sense of hope for the first time, and for whom a convincing image of expectation likely played a more directly instrumental role.

For example, when I tried to tackle the PI (SS10) of another large project about why, exactly, someone like him, a prominent neuropsychiatrist with diverse interests, would actually pursue something as awkward-seeming as a neuroscience of autism, he imagined precisely the kind of promising-future scenario that the biomarker discourse is organised around. 'Say you go in to accident and emergency department with a cardiac arrest,' he said:

> Now, option A: you describe to me your symptoms. Crushing chest pain, burning sensation going up into your neck, pain coming down your arm, right? Feeling sweaty. Not feeling chipper. And I say to you, ‘oh, really? Sounds like you might have something going on in your chest.’ But you would expect me to do an ECG [Electrocardiogram] to measure the function of your heart, right? Or if you went in there thinking ‘I’ve got diabetes,’ you’d expect the doctor to measure your blood-sugar, right? If you went in there with epilepsy, you’d be expecting him to measure your brain-waves. Well why should you not be doing the same thing if you go in with a biologically-based neurodevelopmental disorder? I want to be measuring whether you’ve got an abnormality in the organ in question […] if you think there’s an abnormality in an organ that’s causing a behavioural difference or behavioural abnormality, you’ve got to measure what’s going on in the organ.

We can see here, in fairly bald terms, the basic hope of diagnosis and treatment that is invested in brain-based biomarker research, and around which that programme has become organised – i.e. the hope that neuroscience will one day make autism as instantly and definitively diagnosable as a heart attack. But what is also interesting about this imagined scenario is that it plots both backwards and forwards in time, to argue (and from memory, rather forcefully) that the basic promise of a neuroscience of autism is to provide access – conceptual, and methodological – to the organ that researchers had really *always* been investigating, but to which their methods, up to now, had simply been inadequate. This, I think, is at least one part of what a discourse of expectation can do for the autism researcher particularly – which is to make sense of an awkward past and present in the light of some visionary future (I will have much more to say about the relationship between science, memory, and especially psychology, in Chapter Five). Essentially the same view was expressed even more bluntly by another of the PIs (SS09) just quoted, who also sat at the apex of a fairly large programme of research. 'I think neuroscience always believes that psychology was always a sub-part of neuroscience,' he said,

> but in the 1970s and 1980s within psychology there was a very, very strong push to, you know, not be misled by data from neuroscience. And I think it's, you know, partly a theoretical thing, partly a methods thing as well – because we didn't really have the methods, other than looking at patients with a very messy brain haemorrhage which wasn't very, you know…or doing animal studies. Now we have all these wonderful tools for functional imaging of the brain which we didn't have in those days.

Another (SS02) told me how, today,

> a lot of psychologists have re-directed the focus of their work onto looking at not just the cognitive basis of some kind of process like memory, or attention, or in my case, social cognition – but also the brain basis.

Again, we can see how one of the most significant promises of the new brain sciences – the use of new, 'wonderful tools' to get at 'the organ in question' and, thereof, the 'brain basis' of disorder – underpins many of the basic hopes that run through many autism neuroscientists' reflections on their own practice. Previous work in the 'sociology of expectations' literature tells us that these are not empty hopes, and that their expression actually helps to assemble the various elements of the biomarker research in question. I found these hopes at all levels within my interviewees – but expressed with particular clarity and force by the project-leader scientist, for whom the work of project-assemblage is clearly very direct. This is a good fit for the existing 'expectations' literature, which has analysed the role of these kinds of imagined futures in the contemporary sciences, and also the role that the articulation of such futures plays in the space of present action. It is perhaps no surprise to find the presence of these expectations within autism neuroscience especially, a notably awkward area of biomarker researcher, one known for its troubled past – and thus an area of research where expectation and future fulfilment may have particularly prominent roles to play. But what was also interesting about my interviews with these scientists is that hope, optimism and expectation were only one part of the story – that, at the heart of these conversations about the relationship between neuroscience and the autism spectrum, there was also a strong current of unease, and disappointment, and even some anxiety, about the developing programme of research. This moves us away from the 'expectations' literature. It also begins to tell us something about autism neuroscience, and about the ambiguous terrain in which it is conducted.

A very indirect measure

As it happens, I had rather expected to find interviewees to be cautious, as well as hopeful, when they talked about what could actually be achieved by a contemporary autism neuroscience. Indeed, despite prevailing popular and media sentiments about 'the rise of neuro-everything' (Vidal, cited in Rapp, 2011: 7), the urging of restraint, and of explanatory parsimony, is a recognisable feature of the public discourse of these disciplines (Logothetis, 2008; Vul *et al.*, 2009). But I had still not anticipated the sheer volume of negative sentiment about neuroscience, especially imaging neuroscience, and what it could or could not tell you about autism, that I heard so frequently from autism neuroscientists in the course of this project. Indeed, and in spite of my own self-consciously bland and uncritical presentation, interviewees from cognitive neuroscience consistently, and often with some vehemence, drew my attention to, for example, the problem of false positives, the distance between what their methods measured and what they purported to measure, the degree to which neuroimaging simply replicates what is already known through other means – and even the basic inadequacy of brain-imaging to phenomena like autism in the first place.

I am not claiming that any of these issues are shocking, or unknown. But what I will still try to show, in what follows, is that there is more at stake among these interviewees than an appropriate caution. I argue that the consistency and depth of these cautionary anecdotes, which I encountered among autism neuroscientists who were often positively expectant too, must complicate discussions about the role that 'expectations' play in structuring scientific research. Above, I noted that, in my data, and in line with the literature on scientific expectations, I encountered individual scientists who were given to talking about their practice precisely within hopeful registers of improved clinical and diagnostic intervention for people with autism. In this section, I want to situate this claim a bit more precisely, by re-considering it in light of some alternative accounts of the future of autism neuroscience. These are those accounts in which sentiments of hope and promise had at least temporarily receded, and in which the dominant tropes were things like frustration, resignation, worry, and critique.

Consider, for example, the following account, which is about fMRI neuroimaging of autism in particular. It comes from a young autism researcher (R08), whose intellectual and methodological hinterland was actually more in a hard-nosed cognitive neuroscience than it was autism research or psychology as such; 'you've got to be careful with neuroimaging and the questions you ask,' she said,

> because the problem with neuroimaging [is that] you'll always get a result – you'll always get some blobs, you know? […] I always say, I used to laugh to people and say 'oh my God, this is an art, not a science' [*laughs*] because…you've just got to be so careful. And I think there's a real truth to neuroimaging. I believe in it. But it's one of those things that require replication – and the truth will out, and if you've done 40 studies on social cognition and 38 of them are showing the superior temporal sulcus, then I think you can hold your hand up and say, 'well this area is involved in social cognition,' which is really important, but there's a hell of a lot of other blobs, and that's not a very nuanced finding either [*laughs*], it's a bit crude, so I think to get…I think it's got a long way to go, and people have got to be really careful.

Here, I particularly want to note the tension between this neuroscientist's commitment to the basic truthfulness of the image, and, nonetheless, her acknowledgement of how heavily mediated the process of production is, and also how much artifice is potentially involved in the interpretation. And although she finds some resolution in urging care, and also in deferring to replication, the nervous laughter here, and also the anxious doubling back of the account even when it seems some basic resolution had been achieved ('it's a bit crude'), suggests, to me, the presence of a deeper and a more on-going concern.

In her ethnography of MRI imaging – a related but significantly different neuroimaging technology to fMRI[19] – Kelly Joyce draws attention to some of the 'rhetorical practices [that] produce a construction of MRI in which the image and the physical body are seen as interchangeable' (2005: 438). In particular, and especially in clinical settings, Joyce argues that popular discourses about transparency, revelation and truth often obscure the manifest variability and unreliability of brain-imaging – even in discussions between clinicians and technicians who are quite aware of the limitations of the method (ibid.: 458). 'Wow,' said one physician to Joyce, when talking about his relationship to MRI, 'it's as if you sliced a person in half and looked at them' (ibid.: 437). Granted that it is a more epistemologically compromised practice, but many of my interviewees, usually using fMRI, expressed almost the complete opposite view. Indeed, rhetorical practices among my interviewees *repeatedly* constructed brain-imaging as something that was artificial, unreliable or even manipulative – 'it bears no relationship to reality' said the highly regarded leader of one laboratory (L03) to me.

In particular, technical problems with the generation, processing and handling of brain-imaging data were repeatedly fore-fronted by interviewees. The following quote comes from a psychiatric neuroimager (L09) – who had worked on quite a few autism projects, but who also, and perhaps even more so than the person quoted above, was intellectually embedded in the hard science of MRI and fMRI analysis. Lamenting the generally weak understanding of the physics of these technologies among psychiatrists and psychiatric researchers, he drew particular attention to the phenomenon of resting-state data[20] (Raichle *et al.*, 2001). 'The resting-state data came about,' he said,

> because people started thinking about so-called deactivations, and noticing that these deactivations were appearing in virtually every data-set. And people ignored them. People literally airbrushed them out of their results. They just didn't want to know.

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[19] See n.7 above. Despite its antonymic similarity; fMRI clearly raises a whole host of interesting technological and epistemological issues that don't arise in MRI (Roepstorff, 2004; Poldrack, 2012).

[20] Also known as the 'default mode,' this is data collected while the subject is supposed to be 'at rest' in the scanner, and not performing any specific function – and also in which, it later transpired, the brain was surprisingly active (Callard and Marguiles, 2011).

I was struck, at the time, by how scathing this interviewee was about people's use of the method ('they just didn't want to know'), and also how irredeemably problematic he found the method in general ('virtually every data-set'). This is not a story about the need for proper scientific caution; nor is it a story about the basic scientific pragmatism and scepticism that over-ride a nonetheless tempting narrative about the objectivity of brain-imaging. 'The thing about science in general,' the same person said later in our conversation, 'is that what counts is money and real estate…

> So, people in offices give you power, give you influence. As does grant money. And the two things tend to go together. Again, if you're just sitting in an office writing things down on a piece of paper, that might be great research, but doesn't necessarily bring in much income. What brings in income is doing big studies that employ lots of people, then those people become dependent on your goodwill, and so then you have influence on them. And so obviously that's the way it works. The huge increase in scanning, of course, people are thinking that would be a way to get power and influence by, you know, bringing in research money and so on and so on. Well, that's what happened.

Although this situation of technology, and the desire for technology, within the political economy of contemporary science, and the academic politics of the university, is probably not so rare – it is striking to have this view narrated through the large-scale advent of brain-scanning, particularly. Moreover, the generally deflationary approach to brain-imaging that he expressed was not at all unusual. 'Brain imaging is based on a lot of assumptions,' sighed a young postdoc (R06), 'you know you must be measuring something in the brain…but it's correlates of that thing.' Or as another researcher (R02) expressed the same view: 'fMRI is a very strong [technology]…but it is a very indirect measure.'

I am not pitching autism neuroscience as a contradiction or a counter-example to the 'sociology of expectations' literature – which acknowledges both that 'expectations' are not always positive, and also that even positive imagined futures will generally co-exist with some sense of failure, or simply frustration. As Brown and Michael have pointed out: as far as the desired future is concerned, more often than not, 'practical and material considerations fail to play along' (2003: 7). Or as Borup and his colleagues put it: 'disappointment seems to be built into the way expectations operate in science and technology' (2006: 290)[21]. Perhaps most pertinently, as noted in passing above, Brown and Michael have also proposed a more-or-less direct correlation between how close a person is to the science, and how high will be their admitted degree of uncertainty in it: discussing their interviews with various actors around xenotransplantation, they point out that '[respondents'] close proximity to the production of "facts" about xenotransplantation means they are more likely to experience uncertainties both about the current state of knowledge and its potential future therapeutic value' (2003: 12-13). Thus, there is ample space within the 'expectations' model to theorise the particular role of negative expectations in the way that scientific projects get assembled and re-assembled.

And yet still it seems to me that the roles of deflation, anxiety and uncertainty are not emphasised enough within this corpus – that still, by and large, when we are talking about the sociological import of 'futures' and 'expectations' in scientific project-making, we are talking about actors orienting themselves to something they imagine to be basically good or optimal. As Brown and his colleagues describe it, the future in question still tends to be one in which:

> gene therapy and nanotechnology will cure disease, cars will drive themselves, pigs hearts will be used for organ transplants, computers will become an even more ubiquitous part of life, the Internet and the Cybercafe will become the venue of choice for our relationships, and so on' (2000: 4).

I have shown, in the first empirical portion of this chapter, how such a view may well provide an insight into some of the ways that autism neuroscience, as a specific 'project,' comes to be both assembled and held together, particularly at the level of institutional or programmatic leaders, i.e. that it is assembled around an expectation that complex problems in mental and social life will turn out to be basic malfunctions in known organs, that we eventually will be able to use brain-imaging technology to separate autistic kids from typically-developing kids, that psychological and psychiatric research on autism will receive fulfilment and justification, and so on. These all line up quite well with the analysis of positive-futureing as set out by Brown and his colleagues (2000) above. What we also see in the close-up space of neurobiological autism research, however, is a significantly

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[21] See also the discussion of the HIV prophylactic pill, 'PrEP,' in Rosengarten and Michael (2009) for an example of the way that expectations can form and re-form around the changeable and emergent nature of scientific objects

more complex and dispersed terrain of expectation – and one that works through some notably deflationary, uneasy and even quite disappointed views of its own basic project. It is not enough to describe these accounts as under-currents of knowledgeable scepticism within everyday research. This sense of disappointed uncertainty was too present, and too much a feature of my interviews with autism neuroscientists. What I want to argue, instead, is that the on-going generation and sustenance of autism research is in fact much more thickly entangled in a dynamic and ambivalent relationship between languages of uncertainty and discourses of hope (Silverman, 2011: 159-160).

The trouble with brain-imaging

Here, again, is a post-doctoral autism researcher (R06). When I spoke to her, this researcher was contributing to several major brain-imaging studies of autism (although, notably, more with EEG than fMRI measures) – but having come to brain-imaging from biology, she was also keen to express her early disappointments in this field. 'When you know how the brain works,' she said, 'as a biologist,'

> so you know what makes brain activity, which is connection between neurons, and it matters with which part of the brain you're connected, and how fast you get there, and how much information you converge…um, the only thing you get from brain imaging is 'this part of the brain is activated at a particular time.' It tells you very little about the neural mechanism, and how things get connected to each other.

Here, the interview shifts from a basic concern with the distance between brain activity and some measurable vascular response, to a more specific and profound worry about the relationship between the kind of data generated by brain-imaging measures, and, in general, 'how the brain works.' I want to draw attention, in particular, here, to the ways that some important normative divisions are being constructed – between brain-imaging and biology, on the one hand, and even between brain-imaging and brain *science*, on the other. A senior molecular biologist of autism (SS05) said something very similar, but she embedded her qualms, not so much in the way that connection was being elided, but in the degree of fineness achievable from brain-imaging:

> in autism, along with a lot of other conditions, like schizophrenia and even the neurodegenerative conditions, you really need to understand what's going on with gene expression in the brain… The trouble with brain-imaging is that it

> only gets you down to a certain level of fineness in its detection. So you can't tell what's going on at the cellular level, and at the molecular level – which is what you really need to understand if you're going to see what the genes are doing, and what it might be possible to do to improve symptoms that some people with autism have.

On one level, we could read, here, a wet-lab biologist's anxiety about the oft-remarked 'seductive' nature of the brain image, and the degree to which the brain is quite widely thought to have an intuitive appeal through its visual relationship to some given notion of organic truth; as the neuroethicist Martha Farah has pointed out, people do tend to 'view brain scans as more accurate and objective than in fact they are' (2005; *cf.* Weisberg *et al.*, 2008). In his *Picturing Personhood*, Joseph Dumit has similarly argued that 'there appears to be something *intuitively right* about a brain-imaging machine being able to show us the difference between schizophrenic brains, depressed brains, and normal ones' (2004: 6-7; Dumit's emphasis). He has gone on to suggest that that we might read, in this sense of rightness, even the novel construction of an 'objective self' – i.e. an emergent way of thinking about human subjects that is increasingly made up of 'our taken-for-granted notions, theories and tendencies regarding human bodies, brain, and kinds considered as objective, referential, extrinsic, and objects of science and medicine' (ibid.). 'This tremendous emphasis on imaging,' said the interviewee just quoted, '[…] has led people to think that everything's virtual these days when, actually, it only gets you a certain way, that virtual reality.'

It is interesting that the focus of her unease is not an over-interpreting public discourse; her worry is about the limitation of a brain-imaging study of something like autism in the first place. This sense of limit, which was one of the most consistent sources of unease and disappointment expressed about brain-imaging within my interviews, came out in a few different ways. For some, it was about thinking small: 'in some forms of research,' said a young psychiatrist (L08),

> I suppose you might come up with a finding which sort of clearly changes the game. And in brain-imaging in autism, it's rarely that sort of finding. So, the findings usually sort of move things on in very small steps.

For others, it was about recognising ineffable complexity: 'I don't think it's ever going to be as simple as, "there is this point in the brain that is dysfunctional and this is causing autism,"' said a PhD student (P01), 'I don't think that's ever going to happen. I don't think that's true.' For one of the senior investigators already quoted

(SS09), the problem was lack of specificity: 'quite a good pub game,' he said, 'is name a region of the brain that hasn't been associated with autism, by somebody or some paper. It's virtually impossible.' Of course, these are not suggestions that the neuroscience of autism is intrinsically bad or misguided. But there is nonetheless a subtle but consistent sense, here, in which the neuroscience of autism is described as limited small-scale, dispersed, and (so far, at least) not very specific. Whatever hopes had been attached to their research, these scientists also expressed some quite consistently *low* expectations for the neuroscience of autism. None of them thought that this meant neuroimaging research shouldn't be done (and several went on to talk about the move to 'connectivity,' or some other new paradigm. See Wickelgren, 2005; Anderson *et al.*, 2010), but I was nonetheless struck by the way in which the neuroscience of autism was consistently self-constructed through an idiom of uncertainty, one that emphasised the biases, the difficulties, and the partial truths.

It is important to note that these claims are not just aberrations or counter-examples of a broader structure of hope, nor are they the predictable *post*-hoc sentiments of people whose research hasn't worked out. And it is also important to emphasise that low expectations do not correlate either with disappointed careers in my sample, nor are they particularly found among the junior and the put-upon: all those just quoted are 'successful' scientists by any reasonable measure. My point is not that I have found scientists who are unhappy or drifting, or who find themselves inadequate; nor is it the case that I have interviewed comfortable field-leaders, whose long-established sinecures let them give free range to their doubts. Locating myself within research on the neurobiology of autism, particularly, my point is that analyses of 'promissory hope' may need to be tempered by a greater attention to more modest visions– that among this set of interviews with researchers who work on autism, and who principally conduct their research through neuroscience, the work of actually putting this neuroscientific account together gets traced as much through a sense of unease about how a neurobiological autism might come about, and what it would look like if it did, as it is structured by a sense of hope for this practice in general, and for the therapeutic and diagnostic hopes that it may realise.

A final set of examples: perhaps most damningly for a practice that lives or dies on its sense of efficacy, there were also suggestions that brain-imaging will only ever go over ground already well-trodden by other experimental psychologies. 'I don't necessarily see brain-based work as an explanation as such,' said a lecturer in cognitive psychology (L05):

> So, I think people…it's...to me, it kind of adds a layer of description. So, for example, this is a very simple example, but say we're taking about face-processing and I say that children behaviourally have difficulties processing faces. And you can do tests to show this. And then, at the neural level, they show less activity in the fusiform face area for faces. To me, that kind of is just another level of description. It doesn't explain anything.

Reading this again, I think this lecturer – again, someone who has been involved in quite a few brain-imaging studies – is quite deliberately trying to enact a firm division between cognition and the brain; she is positioning them as different (even competing) areas in which to seek the most richly explanatory substrate of a given mental state, and she is suggesting that neuroscience doesn't really 'add value' to what we already know. But aside from disciplinary or institutional positioning – and that at least part-explains what is going on here – I think that this view expresses another element of this general neuroscientific self-critique, which is that autism neuroscientists have been scanning brains for about two decades now, and yet it's not clear that the field has dramatically moved forward in that period. 'I think looking at the brain is useful in some respects,' said a young postdoc (R11) echoing this view 'but, um, I mean I am always saying that I think a lot of sort of neuroscientific work, especially in terms of fMRI or stuff like that, is a process of re-describing what we know already.' Or as another lecturer (L06) put it:

> I did see a talk here recently on - it was called 'the neuroimaging of ADHD' and that was what it was. And of course functional neuroimaging by itself is meaningless. Because it is just lighting up pictures.

What these researchers are articulating, when they say things like 'it's meaningless' or 'it doesn't explain anything' is a basic anxiety that there has been a disciplinary over-investment in the brain – and that, in fact, attaching categories like autism to localised neurological signatures might not add a great deal to the field. This is where I think we reach something close to the opposite of a promissory vision. Indeed, there was even a suggestion within my sample that the opportunity cost of neurological re-descriptions, by stymieing other kinds of investigation, were holding the field back. The same lecturer just quoted also described, with no small passion, how neuroimaging had:

> really overshadowed experimental psychology, i.e. the examination of the psychological mechanisms underpinning behaviour. But the fact that the technology excited people so much. And there is a whole swathe of research published in the last 10 to 12 to 15 years, particularly the earlier stuff that simply is, 'oh, that lights up when you show them that' and, you know, not very much else… there's a sense in which unless it's got some neural signature, this research, it isn't of any validity. That has to be wrong. It's scientifically wrong.

The emphasis on arguing that this approach is not simply wrong, but 'scientifically' wrong, is telling – it suggests that that, not only is brain-imaging flawed in the various ways that we described already, but that its overwhelming application to phenomena like autism might actually be misguided. And this is not out of an attachment to any dualism; it is a more complicated view that the science of mentation is not exhausted by the localisation of increased neural activity. This is precisely the kind of nuanced and critical self-construal that begins to require the addition of a sociology of low expectations.

Again, I am not suggesting that there is anything very revelatory or shocking about this claim – that people expressed these views to me without great prompting is fair evidence, in fact, of their mostly uncontroversial nature. But what I *am* trying to argue is that the consistency of these kinds of claims, in which this specific group of people, whose professional identity is wholly or partly invested in some practice of doing brain-imaging studies of autism, but who nonetheless frequently position this practice as either partial, or flawed, or misleading, or invalid, or maybe just inappropriate to studying things like autism in the first place – that the preponderance of these accounts finally adds up to something noteworthy. This thick patina of low expectation, suspicion, anxiety and critique, which seems somehow inseparable from so many autism researchers' accounts of their own basic neuroimaging practice, has to be firmly positioned – and theorised – alongside the more well-known sense of hope and promise.

Entangling expectations

But if the generative role of positive expectation seems clear enough (i.e. hopes and promises make it more likely that actors and resources will gather around a scientific project or object), then the role of *low* expectations among this group of autism neuroscientists scientists seems less obvious – and recall my earlier insistence that these scientists continue to work, even while maintaining these low expectations, and without at all conceding that a deflationary rhetoric requires them to stop gathering-up a true neurobiological account of autism. As I noted earlier – variously firm and tentative neurobiological biomarkers continue to be both researched and proposed, even if, as yet, none of them have entirely caught on (Roberts *et al.*, 2010; Ecker *et al.*, 2010; Spencer *et al.*, 2011). This is what I have called 'tracing autism.' 'Tracing' is my word to describe the difficult connecting, marking and diagramming work of the neuroscientists I spoke to. But it is also a word that does not (as the scientists I was interviewing did not) collapse the agency and identity of 'the brain' or of 'autism' into fundamentally human-centric sociology of science; it hangs onto an idea of the objective thing, *being traced*, all the same.

In the introduction to the thesis, I claimed some affinity for this usage with the 'agential realism' of Karen Barad – a more thoroughly-developed theoretical apparatus for thinking about the independence of non-human agencies, even while recognising the fundamental entanglements and ambiguities of worldly phenomena. Of course, 'agential realism' is just one among a group of related terms that try to do similar work. In fact, Donna Haraway (1997: 268-269) usefully provides a list, including modest witnesses, boundary objects, situated knowledges, and misplaced concrescences –to which we could probably also add 'vibrant matters' (Bennett, 2010), 'arche-fossils' (Meillassoux, 2008), 'quasi-objects' (Serres, 2007) and no doubt a whole host of others. While all of these carve out their own theoretical space, what they have in common is a desire to more-or-less re-provincialize human interest and practice, within the spaces of both ontology and agency. They share the recognition of a world of non-human objects and agencies that may well be caught up in (and partly generative of) human affairs, but that also may be sometimes entirely indifferent – and, indeed, actively indifferent – to human systems of language, culture, meaning, symbol, and so on.

While my account of 'tracing autism' is in considerable alignment with such efforts, I draw on Barad's rubric, particularly, for a number of reasons. First, in general, I am attracted to Barad's 'agential realism,' because it is deeply embedded in a refusal to separate the practice of science from the practice of studying science (i.e. from the outside): 'the tradition in science studies,' Barad points out, 'is to position oneself at some remove, to reflect on the nature of scientific practice as a spectator' (2007: 247). But if we take the physicist Niels Bohr as indicative – for whom, Barad claims, 'epistemological, ontological, and ethical considerations were part and parcel of his practice of science' – then we can begin to think about the ways that answers to the traditional questions of the social studies of science might also be intrinsic to the scientific practices in question (ibid.). This move, in which an 'understanding of the entangled co-emergence of "social" and "natural" (and other important co-constituted) factors' might best come from 'engaging in practices we call "science studies" together with practices we call "science"' is quite a radical one (2011: 446; indeed, see Pinch, 2011, for a sense of the incredulity with which such a suggestion is greeted by a more traditionalist Science and Technology Studies). But it has been vital for helping me to see the intricacies with which neuroscience *itself* thinks about neuroscience – and the way that neuroscientists work through the larger epistemological and ontological fields which their practice is ceaselessly implicated. I have learned to describe the neurobiological pursuit of autism as a practice of 'tracing autism,' only by attending closely to the way that neuroscientists talk about their own practice (albeit this is not quite the same as Barad's advocacy of 'holding the instruments of science in one's own hands...and placing the implements at one's lips to draw in the rich and penetrating aromas of scientific practice' [2007: 247]).

More important for present purposes, though, has been Barad's specific interest in tilting 'mattered' accounts towards a concern with 'intra-action' and 'phenomena' (2007: 33). What marks out 'agential realism,' particularly, is Barad's focuses on a specific, constitutive relationship between the mess and ambiguity of entanglement, but also the confounding possibility of distinction or singularity – with the latter qualities coming *after* entanglement, and not before (which is how Barad distinguishes 'intra-action' from 'interaction'; the more common 'interaction' assumes that distinct agencies come before their interaction with one another). What is essentially at stake in the deployment of intra-action is that, for Barad, an agential realism

> does not take separateness to be an inherent feature of how the world is. But neither does it denigrate separateness as mere illusion…relations do not follow relata, but the other way around' (2007: 136).

Thus reversing (but not annihilating) the relationship between separateness and entanglement, Barad argues for a metaphysics based on 'phenomena' – a term that designates both 'the ontological inseparability/entanglement of intra-acting agencies' and the 'primary ontological units' of the world (ibid.: 139-141). Barad's argument that the inseparability of agency does not mitigate against 'determinate boundaries and properties of "entities" within phenomena' is crucial for this account (ibid.: 148). Distinction-making 'agential cuts' (as opposed to 'Cartesian cuts,' which take the distinction between subject and object to be inherent) in which distinctive 'other' entities are sundered from the great mass of entangled agencies, are, in Barad's account of phenomena, quite capable of setting things apart from one another (ibid.: 333-334; *cf.* Bruno Latour on 'plasma,' and on the variety of kinds of 'explication' and 'composition' that can come out of it [2012: 3]). And it is, in the end, precisely through this sense of a cut 'within' phenomena that Barad rescues objectivity from entanglement – in which she sets herself apart from the 'positioning of materiality as either a given or a mere effect of human agency' (ibid.:183). In other words, the ceaseless intertwinement of human and non-human, biological and social, does not leave us with a hopeless morass of entangled identity and sameness. As Blackman points out in a related analysis of voice-hearing and telepathy, dynamics of affective and psychic entanglement are not in opposition to the emergence of objects (2007: 171-172); movements and intertwinements may be more accurately characterised as the stuff from (and within) which real objects, entities and agencies are cut.

I am a long way, now, from neuroscientists talking about whether they are still hopeful about the prospects of brain-imaging, or more disappointed by it. But I have set out some of the key aspects of Barad's work for two important reasons: one is that it forms an important part of the general hinterland of my account of tracing autism – a practice that makes a similar commitment to the inseparability of a neurobiological autism from the scientists who draw it out, but that *also* makes a claim for the reality, agency and independence of a subsequent 'cut' all the same. I draw on Barad's framework to focus on the way that the ambiguous entanglements that are so prevalent in my account of neuroscience do not at all preclude the cutting-away of later neurobiological entities.

But I am also raising this here, particularly, because the intricacies of Barad's framework give some good reasons why we might not actually see a contradiction in the shifting dynamics of hope and disappointment within these interviews – at least as these map onto a similarly mobile relationship between entanglement and distinction, or between the act of tracing and the acts of things traced. My argument is that within a complex and ambiguous 'tracing' neuroscience, which nonetheless maintains the sense of a very *un*ambiguous neurobiologized autism somewhere within itself, long-running dynamics of promise and disappointment may well be intertwined with, and even co-productive of, on-going scientific labour. Autism neuroscience is characterised by a sense of sometimes-frustrating entanglement. But it also remains committed to, and hopeful of, something 'beyond' that entanglement all the same. It is in this sense that I want to argue that a tracing neuroscience of autism is always going to be at least partly a marriage of hope and discontent: these wavering views on the future of autism neuroscience, I suggest, closely echo the dynamics of entangled complexity and cut-away singularity that are inherent in just this kind of intra-active pursuit. And so the shifting co-presence of promise and disappointment, within these interviews, begins to seem quite understandable. My suggestion is that these dynamics of hope and disappointment should be heard as ways of talking about, and dealing with, the work of tracing autism – i.e. a work of connecting, and diagramming and labouring; of seeing the inseparability of your own work from the thing worked on; but still remaining committed to a future 'cut' all the same. This shows, finally, how a sense of disappointment might actually be built into an account of generative expectations in analyses of technoscientific projects. It shows the positive role that the sensation and expression of these more dynamic senses of the future might play in the generation and sustenance of scientific projects.

Critical Neuroscientists

This runs against the grain of some recently prominent ways of thinking about the sociology of the new brain sciences. In Chapter One, I talked in particular about the move to a 'critical neuroscience,' an emerging body of work which rests on the claim that, in pushing forward their various hopes and expectations, neuroscientists are insufficiently critical of the political, economic and ideological contexts in which they labour – and that neuroscience must therefore reform itself to not only take heed of the role of these biases, but actually to work on them, and even to improve the social conditions that give rise to them (Choudhury *et al.*, 2009; Choudhury and Slaby [eds.], 2012; Campbell, 2010; Kamenova, 2010; Ebensperger *et al.*, 2011).

One of the main drivers of this literature is a desire to question (what the authors see as) a broader cultural urge towards neuroscientific explanations. These authors point to the problematic bases both of this urge and of the brain science it wills into existence – and they imagine, in their stead, a different, critically-aware neuroscience, one that questions its own 'givens' and that recognises its own history and context. This are in pursuit of a neuroscience, in other words, in which 'historical, anthropological, philosophical, and sociological analysis can feed back and provide creative potential for experimental research in the laboratory' (Slaby and Choudhury, 2012.: 29-30). Particularly important is what Slaby and Choudhury, in their 'Proposal for a critical neuroscience,' advocate as a 'political' rethinking of neuroscience – i.e. a focus on 'the increasing commercialization of academia,' a concern with the 'cultural tendency favouring voluntarist conceptions of the self,' and also an awareness of the 'correspondence between economic imperatives and normative schemes, and so on' (2012: 31). At the heart of this claim is a desire to see both neuroscience, and the things that neuroscientists work on, as fundamentally enveloped in a social context:

> the brain and nervous system are nested in the body and environment from the outset…their functions can *only* be understood in terms of the social and cultural environment' (ibid.: 33. My emphasis).

These authors argue that to the extent that such 'social factors' are indeed allowed into neuroscience, this is only to the extent that, as Campbell points out, they remain 'reductive and abstract, rather than concrete and substantive (2010: 91). And the concern, then, is that these kinds of things will either be elided or irredeemably reduced by a neurobiological imperialism: 'while psychological distress no doubt has manifestations at the level of the brain,' Choudhury and her colleagues point out elsewhere:

> the biological claims free the person from the social and cultural complexities surrounding her…future advancements in neuroscience will ensure the displacement of several psychiatric practices including psychodynamic, social and cultural psychiatry— by biological approaches (Choudhury *et* al., 2009: 71).

While undoubtedly there are public spaces of neuroscientific discourse that could indeed use some more self-awareness, the accounts that I have discussed here, on the entangled expectations of my interviewees – and of the tracing neuroscience that these expectations bespeak – suggest that neuroscientists are, in fact, surprisingly critical of their own practices. It also suggests that they frequently express this critique, and their wider sense of deflation, precisely by recognising the implication of their science with some wider social context.

For example, in a discussion of whether or not brain-imaging studies were actually very reliable, a lecturer (L07) argued that one of the main problems with neuroscientific autism research is that

> a lot of research doesn't work with those [more 'impaired'] people because they're just so difficult – certainly an adult – to work with, because they don't understand, they're frightened, they can't co-operate.

Or as another interviewee (SS08) put it even more strongly:

> The world is not as tightly controlled as in an RCT trial […] In RCT's, you chuck out all the people you don't want – so, you know, chaotic families, parents who don't speak English, parents who are a bit bolshie, kids who are a bit bolshie, and you've usually got particular criteria for IQ and language…so all the difficult kids and difficult families aren't involved.

In these extracts, modest expectations for autism research, and a self-critical appraisal of autism neuroscience, are precisely embedded in the intertwinement of neuroscientific research with its social context. In another memorable anecdote, an interview from a third-sector autism funder told me about a conference that her organisation had co-organised, and which brought together scientists involved in different kinds of cutting-edge neurobiological research on autism. But there was a twist: 'we arranged as a final session,' she said, 'for a member of our staff to present who has a son with autism:

> And she just described sort of, you know, how it had been, and what she'd done, and what she'd tried, and for the first time, she talked about – he has a number of compulsive behaviours and things – and she talked about what these fixations and compulsive behaviours were in practical terms, and the implications of them. And it was transformative for a number of researchers in the room. There were researchers in tears – she wasn't; but they were. Because I think this was the first time for some of them, particularly those who were working on mouse models and things like that, repetitive behaviours in mice are not the same thing as repetitive behaviours and compulsions in humans. And she just gave some practical examples […] and a lot of people went away and really re-thought, you know, what they were actually in the business of doing – and I think started to see for the first time that they were actually in the business of trying to reach therapies that improved the outcomes for people with autism.

Here, the question of therapy, the question of neurobiology, the question of what people like my interviewees 'were actually in the business of doing,' are all deeply entangled in the shock of one person's confrontation with the daily life of another. And notice how tearfulness, and the confrontation with the sometimes harsh realities of social life, are not narrated, here, as something inimical to science. I hear this not only as an expression of the kinds of low expectations that I have been trying to call attention to throughout the chapter ('repetitive behaviours in mice are not the same thing…') but also exactly the sort of critical sensibility that Choudhury and Slaby and their colleagues are seeking – *viz.* a recognition of entanglement and context, a rejection of any kind of 'naïve' objectivity, and the setting-in of a more modest set of clinical and therapeutic hopes.

Moreover, by tracing the neurobiology of autism through senses of both hope and disappointment, my interviewees recognise that there are no easy boundaries to be drawn between the findings of the lab, and the lives of the lab's human participants. In this sense, focusing on the complex interplay of expectation and disappointment gives us a glimpse of how neuroscientific discourse is not only much more complex and sophisticated than its critics allow it – it is actually a much more entangled view of the relationship between the biological and the social, or the human and the non-human, than the critics *themselves* actually hold. It shows us not only how tracing neuroscience is a more and ambiguous practice than much sociology recognises – it shows a much deeper and richer form of entanglement than most sociological accounts even desire. Hope and disappointment flow together, here, not only because an 'objective' neuroscience is always embedded in some 'social' context – but because the mutual entanglement of these two, and the complexity of the relays between them, requires some very dynamic and nimble footwork. This, finally, is how I think we should understand the vivid co-presences of hope and disappointment in these accounts.

Conclusion

In this chapter, I have tried to do three things: first, working through my basic commitment to setting out the ambiguity and complexity of the new brain sciences, and also the novelty and seriousness with which neuroscientists work through these ambiguities, I focused on the ambivalent registers within which interviewees talked about neuroscience itself. I showed how autism neuroscientists talked through their practice within a tricky dynamic of hope and disappointment – expressing both a desire for (and an expectation of) some future neurobiological marker of autism, as well as a sense of disillusion and deflation with the reality of the tools that they had to hand. Second, I used this account to build on my description of a 'tracing' autism. I again described the way that tracing is bound up with a difficult and active practice of following, or marking; but I also argued again that it is founded on an expectation that some brain-based autism biomarker that might still be 'cut' from this entanglement. Associating this double-dynamic with the sense of hope and disappointment already described, I embedded my account more firmly in the dynamics described by Karen's Barad's (2007) 'agential realism.' Finally, I showed how thinking about autism neuroscience through these registers of complexity, ambiguity and trace also helps to re-think the relationship between neuroscience and some well-known paradigms in science and technology studies. The paradigms discussed were the 'sociology of expectations'(which, I argued, needs to take greater account of the role of disappointment, and of low expectations), and 'critical neuroscience' (where I argued that scholars not only miss the complexity of neuroscientific practice, but that they actually want neuroscience to function in a way that is less complex, perhaps even less 'critical,' than it actually is).

What has started to emerge here, of course, is one of the more obvious subterranean themes of my over-riding suggestion that the complexity and ambiguity of the new brain sciences has not always been well-appreciated by an interested sociology. This is my location of the thesis within a particular way of thinking sociologically about the natural sciences that has consistently refused to take the 'social' and the 'natural' to be different kinds of thing, and therefore that refuses to let one explain or over-ride the other – remaining attuned, instead, to the constant co-production, entanglement, mingling, and co-location of these categories (Haraway, 1985; Latour, 1993). In Chapter One, I suggested a rough division within social-science accounts of the new brain sciences. I described a literature that grants social interaction priority over the operation of machines, bodies, and non-human agencies, and that goes on to create a hierarchy of explanation on this basis (Martin, 200, 2004; Ortega and Vidal, 2007). But I also described literature inclined to see a more symmetrical relationship between these categories, one that sees the new brain sciences as an area both structured by and generative of this symmetry, and one that therefore maintains a trickier (but no less rigorous) relationship to methods of both explanation and description (Wilson, 2004; Rose and Abi-Rached, In Press).

My suggestion that 'tracing neuroscience' complicates more traditional STS accounts of both 'expectations' and 'critique' might well be read as an index of this division – in that both of these literatures are primarily interested in the human and 'social' phenomena that underpin scientific action. This kind of complication forms a significant under-current to the thesis. In aligning my account of tracing neuroscience with, in this chapter, Barad (2007), but also, in upcoming chapters, Wilson (2004), Whitehead (1979), Latour (1987) and Haraway again (1997), I am of course stringing together a nexus of affiliation and disaffiliation. I am also positioning the thesis within a web of scholarship that is marked by an attention to the seriousness of non-human material, a commitment to seeing it as ontologically and agentially symmetrical to human categories, and a descriptive attention to the delicacy and care with which actors of all kinds can work with, on and through these symmetries. I will return to this discussion in some more depth in the overall conclusion to the thesis.

4. And then it hit me in the stomach – emotion, scientific subjectivity, and the affective labour of autism neuroscience

Introduction

This chapter is about the relationship between autism research, emotion, and feeling. In particular, it is about the way that neuroscientists who work on autism often talk about their research through notably emotional narratives, and about the way that they sometimes articulate their science through the bodies and feelings with which those emotions are registered. Emotion has long been in question within autism research (Baron-Cohen, 1991; Williams, 1996), but my focus is not on the emotional lives of people diagnosed with autism. I am interested, instead, in the little-discussed flows of emotion and feeling through (and among) the bodies of autism neuroscientists themselves. At the heart of the chapter is an argument that as much as autism neuroscience might be thought of as an identifiably 'intellectual' or 'technical' endeavour, the neurobiological account of autism is also traced through some scientists' unambiguously affective and emotional labour.

This was actually not a planned interested: in general, I set out to do the interviews for this project with a self-conscious commitment to (what I thought of as) 'conceptual' discussions about the way that autism neuroscience was done. For the interviewer, though, and especially for one not well-endowed with either experience or confidence, there is no clear entry point to talking conceptually about how a solid neurological account of autism might come about – nor is there an obvious intermediate discourse that would link a sociological attention to this kind of question with the interests and concerns of (mostly) neuroscientific interviewees. Quite early on in the process of interviewing, and purely as a device to get the conversation going, I began to ask my interviewees about their initial interests and motivations for doing neurobiological autism research. In particular, I began to ask interviewees to tell me the story – if there *was* a story– of their entry into autism research, the things that grabbed their attention within it, and the questions, opportunities or concerns that pushed them forward. In reply, I had expected a well-worn litany of books read, lectures attended, mentors cultivated, and intellectual interests developed. And sometimes that's what I got. But sometimes, too, I got stories that were very different, and these were stories whose major themes were not guiding theories, or powerful explanatory paradigms, or key figures – but were instead about the quality and depth of the feelings experienced by the individual scientist in the course of her research. There were stories about feelings of upset, sadness, and fear; but also stories of pride, desire, and even love.

In her account of the emergence of the autism spectrum and of the waxing understandings that have appended it, Chloe Silverman urges attention to the use of love, specifically, as an 'analytical tool' for the social studies of science: talking about love, says Silverman, 'shifts the focus from psychiatrists, epidemiologists, and geneticists to parents, counsellors, diagnosticians, and lawyers' (2011: 3). If, for psychologists, love has simply been something that they studied in autism:

> beyond the laboratories…love continues to function in normative claims about the practice of research. Parents and their allies say that emotional knowledge enables them to observe and attend to their children in the right way, guides them in medical decisions, and helps them make the right choices for the person they love (ibid.).

Thus, for Silverman, if love has sometimes been seen 'as a liability or a barrier to reliable knowledge,' there is room, now, to start thinking about love as 'the source of specific, focused and committed knowledge' (ibid.: pp.3-4). In this chapter, I want to advance this suggestion in two ways. First, I want to build on Silverman's specific account of love, to think about the broader role of emotion, and of affect more generally, in the putting-together of serious, concrete knowledge. Second, though, I want to say that paying attention to emotions, and to the role that emotions play in putting together and sustaining knowledge of something like autism, does not necessarily move us beyond the laboratory. My basic argument is that focusing on the role of emotions in making knowledgeable claims tells us something important about scientific work; here, and among autism researchers especially, I argue that an unimpeachably scientific, laboratory-based work of looking for, and thinking about, the neurobiology of autism is often an emotional and an affective labour too. By the 'affective labour' of science, here I am not referring to a specific 'economization' of the emotions (Blackman and Venn, 2010; Gill and Pratt, 2008) – but to a more elementary recognition of the troubled boundaries between scientific work, emotional work, and body-work.

Of course, recognition of emotion's presence in scientific spaces has a long history in the science and technology studies (STS) literature. Indeed, Michael Lynch (1986), who might claim both the first sociology of neuroscience and the first laboratory ethnography, long ago noted the degree to which scientific talk is *not* 'coolly objective, detached, unemotive, scrupulous and "stiff"' (1986: 169). Or as Bruno Latour put it: ‘there really is love in technologies’ (2006: 282). But if the place of affect in the laboratory is not a surprise, there still remains a question – most famously posed by Max Weber (1919) – about the relationships between the passionate attachment that might motivate a scientific interest, the actual performance of scientific work, and the status of the objects and truth-claims that emerge from that work. Recently, and drawing in particular on long-standing attentions to the body and affect within feminist science studies (Haraway, 1988; Hayles, 1999), scholars have started to work through the entangled nature of this relationship. Elizabeth Wilson, for example, in her work on Turing’s calculating machine, has turned her attention to the way that ‘feeling and thinking might coassemble’ in the unfolding of a modern technoscience (2010: 24). Natasha Myers, focusing on the relationship between dance, body-movement and molecular biology, has shown some of the ways that scientists use their bodies to ‘figure out’ the subjects of their research – dancing and body-work, she argues, can become ‘effective media for articulating the forms, forces and energetics of molecular worlds’ (2012: 153-156).

In the chapter that follows, I want to ally myself with these accounts, in order to think more precisely about the role of emotional labour in autism neuroscience. The major purpose of the chapter is to show that in addition to being traced through different accounts of ‘what autism is,’ and through different ways of relating to the ‘promises’ and ‘disappointments’ of contemporary neuroscience (as I described in the previous two chapters) the neurobiological account of autism is also traced through interviewees’ experiences of being both ‘interested’ in autism intellectually,’ and also being ‘hit’ by autism ‘in the stomach.’ Interviewees frequently talked about their research in an emotional or embodied register, even as they located that work in specifically cognitive or intellectual domains. As in previous chapters, my interest is in the mingling and holding-together of these registers, and in the tracing of a neurobiological account of autism through them.

In the thesis so far, I have said that creating neurobiological accounts of complex diagnoses is a more ambiguous and intricate practice than is yet realised in much of the social science literature. I said that ‘tracing’ would be my metaphor for talking about the ways in which autism neuroscientists seem to live with, and work through, this ambiguity – while not abandoning their commitment to some objective neurological account of autism all the same. In this chapter, I will show how the process of 'tracing autism' also works through lines of connection between an emotional or affective labour, and an explicitly intellectual pursuit. To situate this account more precisely, I will draw on the process philosophy of AN Whitehead (1935, 1964, 1973). For Whitehead, thinking about the way that things come to exist in the world (such as a well-described neurodevelopmental disorder called autism) inevitably directs our attention to the inseparable and on-going process of subject-object relations. And these relations that are, for him (and I will say much more about this below), essentially constitutive of all individual things. Troubling the boundary between the perceiving work of individual subjects and the qualities of objects 'cut' from those subjects, Whitehead's well-known objection to the 'bifurcation of nature' (i.e. the long-held distinction between subjectively-perceived qualities that belong to objects, and subjectively-perceived qualities that are independent of those objects) has a general affinity with what I have called 'tracing' (Whitehead, 1964). I draw attention to it in this chapter, particularly, because one of the most interesting aspects of Whitehead's philosophy is that even as he remains committed to thinking seriously about the way things come into the world, and how they are held together in it, he also offers an analysis of this process as a specifically *emotional* phenomenon. Using Whitehead helps me to think about the way that attending to emotion is also a way of thinking about how things come to exist – I will draw on his philosophy, below, to argue that the notably affective character of autism neuroscience may well be congruent with thinking the way that a neurobiological autism is actually achieved.

The chapter proceeds through four sections: in the first two, I set out and work through some of the more affectively saturated stories from my interviews; in the second two, I draw on the work of Elizabeth Wilson (2010) and Natasha Myers (2012), as well as Whitehead, to begin to theorise the precise role of emotion in these accounts. Drawing on these, the key argument of the chapter is that registers of thinking and feeling are *held together* here, precisely because tracing the neuroscience of autism across the two plays a concrete role in generating and sustaining a convincing neurological account of autism.

I remember one woman. It was heart-breaking.

Let me leap straight into the empirical basis of this chapter, though, and begin with the following extract, which comes from an interview that I did with a very senior professor of cognitive neuropsychiatry (SS07). In fact, this was an early interview, conducted when I had yet to solidify my approach. These extracts, appearing unbidden in the middle of a conversation about this professor's early career, first taught me to listen for the specifically affective commitments that often circulated scientists' accounts of their own intellectual work. We were talking about her research pre-PhD, and she began to tell me about her first real job in the field, which involved travelling to schools around the UK doing tests on children with diagnoses of autism. 'I was travelling around on my own,' she began,

> and I remember the first time I walked into an autism school – which I think was [*names a well-known school*], one of the first autism schools there was. This of course was in the late eighties. And...it's hard to convey because actually autism schools aren't like that anymore. But...the sound when you go through the door – the kind of particular sounds that low-functioning children with autism make in place of language that aren't like the squeals of ordinary kids. To just walk into the playground and see all the children busy, all this movement but none of it co-ordinated, children not moving together - you know, one child in a corner rocking, one child examining the light through their fingers, another child running in circles, with this extraordinary squeal that other children can't produce. It's just so different. You walk through a door, and on the other side of the door is a world that's so utterly different from the high street in [that part of London]. So I found it completely captivating, and...terribly, terribly upsetting.

'That break between the ordinary world and the world of the school,' I asked, 'can you remember...on what level did it hit you? Was it kind of an emotional, kind of...?'

> It's very visceral, yeah. Very visceral. And although I thought I knew a lot about autism, because I'd *read* a lot about autism, I'd *heard* a lot about autism – actually I was utterly unprepared for it. Nothing I'd read conveyed the level of lack of language, intellectual impairment (although of course that may be secondary to other things but, you know, presenting intellectual impairment). So, here was this thing that I was really passionate about and interested in intellectually. And then it hit me in the stomach.

It is perhaps an elementary point, but it is still important to note that this account, as a description of entering the field and actually doing science, is in a markedly different register than those understood by many formative sociologies and histories of science. Merton, for example, famously described the 'ethos of science' as a structure made up of 'universalism, common ownership, disinterestedness and organized scepticism' (1979: 270). For Merton, the 'sentiments embodied in the ethos of science' would best 'be characterized by such terms as intellectual honesty, integrity, organized scepticism, disinterestedness [and] impersonality' (ibid.: 259). Of course, what is notably lacking in Merton's description is what is actually primary in the above self-account – i.e. the memory of a specifically emotional and affective undergirding to this (nonetheless) scientific ethos. More to the point, though, this is not just an aspect of scientific practice left unaccounted-for by the Mertonian scheme – in fact, it is a description very clearly anomalous to Merton's ethos. Note, for example, the degree to which the account is made up of images like 'upset,' 'viscera,' 'passion,' even being 'hit…in the stomach' – and contrast with Merton's description of the 'disinterested zeal' of modern science, in which, for example, a 'puritan spur,' allowed 'the exaltation of the faculty of reason' to emerge as 'a curbing device for the passions' (ibid.: 228, 238). It is true that Merton's sociology of science is no longer very influential (and also that, however one-dimensional this view may look now, it cannot be separated from the memory of a very *interested* scientific zeal during the Second World War). But while I do not want to spend a great deal of time contrasting my data with Merton's ethos, it is important to note that similar themes of impersonality, disinterest and distance still structure many accounts of what science is like – even where it is confessed that such tropes are only fit for public consumption. Consider psychology, for example, which, as Theodore Porter has pointed out, following Mitchell Ash,

> has been more self-consciously scientific than the natural sciences precisely because of its institutional weakness and intellectual disunity. Inflexible methods of quantification compensated for the lack of a secure community…[statistical tests] were part of a regime of replication and impersonality, necessary if the study of psychical phenomena was to win even a modest degree of scientific credibility (1996: 211).

But contrast that view of the disinterested and disembodied self-consciousness of psychology with the following description – which came from a different and much more junior researcher (R08), who had actually been talking in a fairly routine way about the prevention of 'bias' in research, but who also then began to relate the ways that you inevitably 'bring yourself' to the research process. 'I remember one woman,' she said, 'it was heart-breaking.'

> Her child was very high-functioning, won awards for this, that, and the other. It was her only child – she said, 'oh, he was my blessing.' She didn't think she'd have children – she was, like, forty-plus. But she said 'oh, it's so sad, I'm there with my husband eating breakfast and just wish that...Daniel or whatever...would just sit and eat breakfast with us. But he's just not interested. He just gets up and walks away.' And she was really grieving, because this relationship with this blessed child that she'd longed for just hadn't materialised, but he was incredibly articulate, incredibly bright, very nice boy - but that kind of emotional, just not engaged. I thought, 'it's so interesting' because she's, you'd think she had the easier deal [*i.e. than parents of 'low-functioning' children with autism*], but she was the one who I think was emotionally finding it a lot harder than some of these other parents who you think 'well, I don't know how...your lives are so disrupted,' you know, so that was a really interesting lesson. I don't know how it informs my...but I think it does make me sensitive, and one thing it does make me think about is that, kind of issues that people will probably say are to do with political correctness, I'm very careful about the language I use, like you know, 'individuals with autism,' and people say 'autistic,' you're just, you know, very careful with the language. And being respectful, and things like that. And think, you know, these aren't, you know, your subjects - well, they are but, you know, you don't, that's not the way you...relate to them.

Here is the psychologist nether as disinterested puritan, nor as impersonal seeker of scientific credibility. Here, instead, is the psychologist as an emotionally invested, slightly heartbroken young woman – a self-consciously sensitive scientist, one who is keen to stress her hearing of a mother's grief, who fumbles a little with the politics of language, and who worries about whether or not she relates well to her 'subjects,' a term that she is even reluctant to use. What was particularly striking about this account was that the researcher described the laboratory not only as a space laden with emotion, sensitivity and heart-break – but that she related her scientific work as much more emotionally structured space than her life outside. 'I think I'm quite an empathic person,' she said at another point in the interview: 'I don't want to take this sort of stuff home with me.' As a view of what it is to do science, and of what it is to get drawn into the scientific life, I am struck not so much by how emotionally heavy this account is, but by how willing its author is to pass over any implied or normative injunction towards distance and impersonality. Here is an account of the scientific life, and of scientific labour, embedded, quite precisely, in a deeply personal and emotional commitment to other people.

Here are two related accounts but with a slightly different valence, in which disinterest is not just elided, but in which the moral imperative of scientific work is specifically addressed and emotively affirmed. The first comes from a physics-trained psychiatric neuroimager (L09), working in a major UK imaging centre. He had started out by referring to his own relatively difficult childhood in provincial England, and talking about the way that his life and career had developed subsequently, as well as his desire to make some kind of 'contribution.' In the middle of a conversation about one of these contributions, his involvement in research ethics, he said to me:

> I was joking the other day with a friend of mine who used to work in the motor trade. We were talking about service managers. And I'd just met some service managers who were friends of friends of ours, at a dinner party – and I was surprised at the lack of conscience that they had. They had no conscience. You know, their family and their friends were very important to them. But essentially if it didn't influence them, or didn't have an influence on them directly, they had very little interest in it. And it just seemed to match with the kinds of work that they had to do – you know, they were very keen on meeting business targets, and that's the priority rather than…people. And the way people feel. […] You know, that's the way some people are and that's fine. But some people are the other way around, and they care perhaps a little bit more about the way people feel.

Of course, when he's talking about the 'some people' who are 'the other way round,' he means himself and his colleagues – i.e. bright, highly-educated people, working to alleviate the problems of people with neurodevelopmental disorders, and doing so in a fairly unglamorous office, for relatively small salaries. I was particularly interested in the way he narrated his relationship to scientific work as a way of attending to 'the way people feel.' There is a clear sense, here, of the affective commitment of his science, and also of his self-construal as a researcher – and, indeed, as a physicist – precisely through registers of feeling and care. There is also an interesting feeling of almost moralistic pride running through this account – which may well not be misplaced, but which also, again, disrupts any assumptions about the disinterested and impersonal nature of scientific work. The same researcher told me a story about going with a group of friends to a comedy club some years previously – where he was picked out of the crowd for mockery by the comedian. But when the comedian asked him what he did for a living, my interviewee was pleased to be able to give the joke-defying response, 'cancer research' (which he has also worked on, early in his career as a medical imager). 'It's just about getting some sleep at night,' he said to me, 'so, I suppose if I can go to sleep at night thinking I've made some sort of contribution [...] then I'm happy.'

This idea, of worrying about being happy in scientific work, and in locating that happiness in a commitment to care for other people, was echoed by another postdoctoral researcher. She initially maintained a distance from any kind of passionate or emotive account, telling me that she had gotten into autism 'by chance,' and that her early interests were in mathematics and neuropsychology particularly. But then she also went on to talk about how important it was for her to work directly with parents of children with autism, and with their teachers, and she described the qualities of those interactions, and their centrality to her work, in a way that I thought was very obviously heartfelt, and that belied her earlier account of a simple intellectual interest. So I asked her about the difference between the two ways of accounting for her early interest in autism. 'For myself, personally,' she said,

> if I was doing more of a pure science that wasn't...didn't have an end-point that was kind of applicable – I'd sort of find that quite difficult. I think I need to, um...either be a clinician myself [*laughs*] or do applied research.

When I asked her why she couldn't just be happy producing data, she said:

> Um...[*long pause*]. I suppose it's just that satisfaction...that whole knowing that what you're doing has a purpose. Um...and that it's not just a job. You're dealing with people's lives and, you know, a report that you write about a child does go on and have an impact. So, um, it's...yeah [...] Well, it's...the fact that, you know, you're listening to parents and hearing their stories and you're, you know, hearing their suffering and what they've had to put up with and the fights and battles. And, I suppose...the fact that they give a lot to you in taking part in research. So they are, um...it's very different from a clinician's role. So they give a lot of time to you. And so I suppose it's just wanting to give something back to them.

She then talked more generally about talks that she gives to parents and clinicians, and how she contributes to the field more generally, before laughing self-consciously and saying only half ironically: 'I hope I have a purpose in life.' What I find striking, in these last two accounts, is the sheer refusal of disinterest, and the sense that scientific work is privileged in the way it attends to other people's feelings, and especially other people's suffering – something that, both of these interviewees emphasised, made them feel happy. What I am trying to account for in this chapter is the way that these researchers trace an objective and scientific neurobiology of autism precisely through these kinds of affective registers. I am interested, in other words, in the way that an 'intellectual interest' in the neuroscience of autism runs parallel to accounts of stomachs, feelings, suffering, and heartbreak.

I want to do this. I want to do this.

These kinds of affective, emotional, and embodied categories have not always played a central role in narrations of scientific work and scientific lives. In their well-known genealogy of objectivity, for example, Daston and Galison point to the emergence of a new and rather particular kind of scientific figure in the nineteenth-century: whereas the 'enlightenment savant' had been an active and critical sifter/synthesizer of data, the scientific subject of the nineteenth century, by contrast, 'strove for a self-denying passivity' (2007: 203). Far from the almost 'otherworldly' figure of previous eras, the goal now was to 'practice self-discipline, self-restraint, self-abnegation, self-annihilation, and a multitude of other techniques of self-imposed selflessness' (ibid.). In his account of the scientific life, Steven Shapin makes a similar argument, identifying a turning point in the last couple of centuries, in which the passionate, feeling individual was slowly effaced by narratives centred on slow, processual, collective endeavour. For Shapin, the key turning point was a shift in emphasis to 'method' – and the abstraction, thereof, of the lone genius (2010: 33). Thus, he argues, did we move from a passionate Benjamin Franklin in the 1770s, to a 'stress on mundane methodological discipline' a century later – and, thereof, from speculation to technique; from metaphysics to facthood (ibid.). Scientific work had moved from being the knowledge achieved by an individual, feeling body to being the faceless abstraction of a collective process. Strikingly, Daston and Galison record that whereas eighteenth-century hagiographers of Isaac Newton knew him as the font of 'by far the greatest and most ingenious discovery in the history of human inventiveness,' for their Victorian counterparts he was something very different, *viz.* the epitome of 'self-control in speculation, and…great-souled patience in the pursuit of truth' (2007: 216). In our century, says Shapin, this has developed into a claim for the 'moral equivalence' of scientific practice – or a drive, particularly in the wake of the technological horrors of the Second World War, to stress the moral ordinariness of most scientific labours, and their consequent abstraction from the vices, virtue, and feelings of individuals (2010: 47).

Of course, contemporary scientific practices need to be carefully distinguished from the establishment of these historical norms. Not least, the dispersed laboratory of a modern technoscience (and my own interview sample bears witness to this) is filled with all kinds of bodies that were perhaps not well anticipated by the methodical and disinterested gentlemen of the eighteenth-century – including, as Donna Haraway reminds us, women, working-class people, people from ethnic minority communities, and other inhabitants of 'nonstandard' corporality and affect (1997: 269)[22]. Certainly, norms of disembodied self-abnegation still have some valence in the present. One of my interviewees told me about a problem she had when she used the word 'thrilling' to describe her own attitude in a journal special issue, and how objectionable a reviewer found this usage: 'I really don't think this is the way to write scientific papers,' was the pithy comment that came back. And yet her attempt to use the word in the first place, as well as her unruffled amusement at the reviewer's rather antediluvian attitude, bespeaks the changes: whatever public norms have emerged and receded in the last couple of centuries, it is unlikely that contemporary scientific labour tracks their ebb and flow with great fidelity. As Shapin reminds us, there is a danger of mistaking a receding public script of scientific propriety with the actual emergence and entanglement of technoscientific work: the fact is, says Shapin, 'the closer you get to the heart of technoscience, and the closer you get to scenes in which technoscientific futures are made, the greater is the acknowledged role of the personal, the familiar, and even the charismatic' (2010: 5).

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[22] See also Hilary Rose (1994) on disruptions to the peculiarly masculine discipline of the eighteenth-century scientific institution. This is a topic that I will return to in more detail Chapter Six

I want to ally myself with this shift away from the analysis of norms of disembodiment and method, and towards thinking more about the role of the individual and the familiar 'at the heart' of technoscientific 'scenes' – categories that I read in the distinctively embodied and emotional stories of scientific labour that I related above. I am precisely trying to think about the way that a contemporary scientific project, even one as drily statistical and methodological as the search for a firm neurobiology of autism, is also traced through the feelings, and the body, of the unapologetically individual and familiar autism neuroscientist. As well as accounts of emotional laboratory work that I described above, the role of emotions and the body were also evident in interviewees' recollections of the formation of their scientific subjectivities, and in their affectively-registered *desire* for the scientific life – both of these often memorialised as strikingly revelatory and emotional moments. One interviewee (L04), remembering her decision to become a psychologist, recalled how,

> when I was thinking about university, I'd really had a very - I was intending to do English literature, actually, and then just one day, literally, I woke up and thought 'no, I want to change that to psychology,' having no really formal idea about psychology education [...] looking back on it, I still think that that was how it was. It was literally a wake up in the morning and say "I'm not going to do English literature. I can still read the books I want to read. I'm going to do psychology"

Moreover, she was not alone in embedding the formation of her scientific subjectivity in a moment that was entirely personal, passionate, and embodied – the senior professor already quoted above, for example, mentioned an early encounter with Clara Claiborne Park's (1982 [1967]) pioneering autism memoir as something which 'made me very excited' (SS07) Another (R12) told me that she had just felt 'some form of intrinsic desire to enable people to […] live their lives in a more comfortable environment,' and yet another spoke of how cognitive explanations for autism 'really captured my imagination' (L05). One PhD student (P01) even related how, when still at school

> I was quite into literature and drama, and quite creative, I suppose – but I couldn't like really figure out ever, how am I going to get a proper job out of this [*laughs*] and then we were doing Dr Jekyll and Mr Hyde at school, and one of my teachers recommended that I go and read some Freud because this ties in, so then I went to the library and I looked up Freud. And I remember

> – I just specifically remember – being, like, people research how other people think and feel. And I never knew this before […] this was a massive revelation to me, and I was like, 'I want to do this, I want to do this.'

Max Weber (1919) describes this as the scientist's feeling of 'strange intoxication' – which, along with a 'passionate devotion,' Weber was actually quite well aware of, except that Weber was very keen to rigorously separate interest from practice in the course of the 'scientific vocation.' For Weber, a passionate zeal was something quite distinct from the labour of science. But what I am trying to show, in this chapter, is that interviewees worked across those boundaries in a much more complex manner than Weber's rubric would allow: what I heard from these interviewees, over and over again, were accounts of scientific subjectivity, and of scientific labour, that bespoke a much more entangled relationship between intellectual and affective practices. In these interviews, doing the neuroscience of autism emerges as the product of an intoxicating revelation. And this revelation is borne out in the emotional and affective commitments of a very particular kind of scientific labourer – here, someone who feels, desires, and imagines; someone who is easily thrilled but also slightly upset; someone who hears other people's suffering; someone who is morally good, and also someone who needs to feel happy about that goodness.

Thinking and feeling

In the last decade or so, 'emotion' and 'affect' have emerged as major points of inquiry in social-scientific and literary theorising, to the extent that some have even diagnosed an 'affective turn' in these literatures (Clough and Halley, 2007; Gregg and Siegworth, 2011). It is not an aim of this chapter, however, to seek affinities with this development – and I am not especially concerned here with parsing, for instance, the analytic specificity of the emotional or the affective *per se* (the distinction between the two, which I do not follow, belongs to Massumi, 2002). What I am interested in is the broadly affective, emotional, and embodied way that some autism neuroscientists talk about scientific work and their scientific lives; and, in particular, about what the actual role is of tracing a neurobiological account of autism *through* the feeling body of the individual scientist. When I talk about the place of 'emotion' within autism neuroscience, I do so while recognising the broad nature of this term; and while I have no great objection to the careful and parsimonious theorising that surrounds the concept in the literature, such an approach does not especially fit the empathic and 'hospitable' method demanded by the jostling, circulatory nature of my data (Wilson 2010: xi)[23]. Indeed, if this chapter is to claim any affinities with the broad literature on feeling, it will be via a related area with perhaps more stable roots, and this is the longstanding material-feminist attention to the significance of the body – a corpus which, for some time now, albeit without necessarily setting out from the study of the emotions, has been looking for ways to think and live with the always-hybrid nature of corporeal human life (see e.g. Haraway, 1991; Grosz, 1994; even Butler, 1993, to some degree. See Alaimo and Hekman, 2008, or Hird and Roberts, 2011, for overviews).

Within the texts on emotion that are broadly affiliated to this tradition, some have recently begun to focus on the role of bodies and emotions within scientific and technological spaces especially. In a discussion of the life of Alan Turing in her *Affect and Artificial Intelligence* (2010), for example, Elizabeth Wilson explores the role of affect not only within Turing's own life and practice, but within artificial intelligence and robotics more generally. At the heart of Wilson's discussion is the attribution of affective registers not only to the human scientists, but also to the non-humans in these spaces: machines, robots and programmes appear, here, not just as figures or elements of emotional exchange, but as much more complicated and active relays, both figured and figuring within a broader affective entanglement. Wilson is keen to stress the degree to which machines and machine-properties (her focus is the automatic calculating machine of early artificial intelligence research) are not exemplars, nor are they *products* of a cold, knowing intelligence: machines, as well their creators, are bound up with 'a fusion of intellect and muscle and beauty and nerves' (2010: 8). Within the laboratory, Wilson argues that the relationship of thinking to feeling is one of 'introjection' (i.e. an integration, or a bringing-inside), which significantly re-figures how we think about the interiority of both the rational scientist and the calculating machine – as well as the creative and generative back and forth between the two. In Wilson's account, this more generous definition and attribution of affect opens a door for thinking about the role of emotional salience in technological and scientific labour: if Alan Turing's work and life are animated by *both* 'affective and intellectual concerns,' then 'it is his errant curiosity, his capacity for enjoyment and surprise,

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[23] For more carefully analytic discussion of affect itself see, aside from those already cited, Hemmings (2005), Thrift (2007), or even Deleuze and Guattari (1988). For a critical take on this 'turn,' see Papoulias and Callard (2010).

and his childish engagement with computational machinery that underwrite the importance of [his canonical] 1950 paper' (ibid.: 35). Wilson's account provides a picture of an affectively-committed scientific and intellectual labour, in which the scientist is not simply beset by feelings for her objects, but is instead one node within a much broader circulation of 'affective commerce' (ibid.: 16; *cf.* Ahmed, 2004, on the generative role of 'affective economies'). And the scientist's ability to recognise and negotiate the complexity of this emotional web is intimately entangled with her scientific practice. In Wilson's account, Turing appears as a brilliant scientist not least because 'the traffic between [his] internal states and the internal states of others is a key methodological concern in his work' (2010: 16-18).

In a related account, Natasha Myers has drawn on her experience with the 'Dance your PhD' phenomenon, and on her ethnographic work among microbiologists, to show some of the ways that scientists use their bodies, and body-movement, to 'figure out' the subjects of their research (2012: 153). Dancing and body-work, Myers argues, can become 'effective media for articulating the forms, forces and energetics of molecular worlds' (ibid.:: 153-156). Myers, like Wilson, calls attention to the 'affective entanglements' of scientific research – or the way that the sometimes-self-described cognitive or intellectual aspects of scientific labour, as well as the objects of that effort, are not easily separable from the feelings, and the movements, and the bodily vibrations, of the scientists in the laboratory. Using the body 'can generate both new forms of knowing, and the things known,' Myers points out; it can also 'make explicit the kinaesthetic and affective dimensions of what are normally recognized as thought experiments' (ibid.: 162, 171). While Myers is particularly focused on dance and on the cognitive-affective work of bodies-in-motion, she also calls for a larger attention to 'researchers' capacities to *move with and be moved by* the phenomena that they attempt to draw into view' (ibid.: 177, emphasis in original). Bodies are '*excitable tissues*,' she argues – they have 'the capacity to collect up and relay nuanced molecular affects' (ibid., emphasis also in original). One of the primary metaphors that Myers draws on in her account of scientists' body-work is 'rendering' – a term whose multiple valences carry the sense of a representation, of the work of producing that representation, of a cut, and also of a communication. There are strong affinities between what Myers has described as 'rendering' among the molecular biologists in her study, and the work that I have called 'tracing' among the autism neuroscientists in my interviews. In both cases, what the term tries to nuance is the awkward relationship between producing something, and the independence of the thing produced; 'what holds all of th[e] uses of the term [rendering] together,' Myers argues, 'is that each refers not just to the object that is rendered, but also to the subject, the one who renders, and the activity of rendering' (ibid.: 172). Dancing your PhD, like feeling your robot – and also, I am trying to show, very much like tracing the neurobiological account of autism – is an act of rendering, in that it articulates the entanglement between the body of the researcher and the thing researched, but it does so precisely in the service of animating and collaborating (even dancing) with the object in question.

With Myers and Wilson, then, we begin to see not only how only how the intellectual work of the laboratory might be a very emotional and embodied experience – but, more specifically, we can see the productive and even generative roles of affect in scientific work. In Wilson's account (quoting Fortun, 2008), 'cognition and affect "feed off each other…and set possibilities in motion"' (2010: 22). For Myers, meanwhile, 'scientists…conduct *body experiments* to work through hypotheses about how molecules interact' (2012: 156). There are important differences between these two accounts – but, for now at least, I want to draw on what unites them, and this is a suggestion that high quality intellectual work, within scientific and technological spaces, is often deeply embedded in the ability or willingness of scientists to give themselves to a kind of affective relationality; the capacity for thought, here, is thickly entangled in the capacity for feeling. For Wilson, cognition and affect form a kind of symbiosis; for Myers, feeling gives form to thought; in both cases, though, difficult scientific projects, like artificial intelligence and molecular biology, proceed by being traced through categories of both thinking and feeling. This is precisely what I think is in play among the autism neuroscientists that I have already quoted: affect makes the 'constricted space' of both brain and diagnosis more expansive (Wilson, 2010: 38); it helps to 'render' new possibilities for relationships between the two (Myers, 2012: 172). My suggestion is that within a 'cognitive' task that is as difficult and awkward as finding autism in the brain, the connective, expansive, and generative possibilities of affective relations begin to appear in a new light.

Let me return to the senior scientist (SS07) I quoted at the beginning of the first section. Having told the story of how her visit to an autism school had been a deeply 'visceral' experience that 'hit [her] in the stomach,' this professor then began talking about one of the children in particular that she had encountered there.

> So there was a boy in the class I was helping with, who didn't speak at all – very beautiful-looking (it's a cliché, but it's true - a lot of children with autism are); always had his hood up against sound; easily distressed; and spent a lot of time drawing in the air with his finger. And, just occasionally, if you sang to him, he would finish off what you were singing - with words, with sung words. But never ever spoke any words (of speech or communication). Just think: if you're interested in this, I mean, how fascinating. How completely fascinating. And how completely tragic. I remember, from my undergraduate experience, walking back out of the door and into [the] high street. And on one occasion seeing some mum, not exactly slapping their kid, but pulling their kid along and giving them a hard time about whatever it was – whining about something or other. And thinking that...it was so extraordinary that that ordinary child could communicate - you know, that this two- or three-year old could whine to their mum about this, compared to these kids inside this school who, if they'd been able to do anything like whining, we'd all have been cheering and clapping. So, I don't know, it's hard to describe. Of course it's a very long time ago now. But I remember it clearly. I remember it very clearly. And it was partly probably intensified by the fact that I was travelling to these places on my own, and so I would go into a centre for a couple of days, and go to a different place for a couple of days. The staff and children were very helpful and very likeable. It was still a very kind of, umm, a very upsetting experience.

Looking again at these two extracts, it is striking how central bodies and feelings are to the two narratives. The entirety of the first story is carried along by the squeals and cries of one child, by the rocking back and forth of another, by the light filtered through the fingers of a third, and, finally, by the indefinable, visceral, upsetting feeling in the stomach of the researcher herself. It is also noteworthy that the interviewee interprets my question about emotions as a question about the body – and she affirms the specifically affective nature of her experience on that level: 'it's very visceral, yeah.' In the second story, again her account of becoming an autism researcher is a story about bodies and feelings – her memory of the experience emerges from a narrative of singing voices, splayed fingers, slapped hands, beautiful faces, and the researcher's own feelings of confusion, fascination, and upset.

But I also want to draw attention to the triumvirate that appears at the end of her first story – where autism is something that the researcher is 'passionate about,' that she is 'interested in,' but that then, finally, 'hits' her in 'the stomach.' There is very much a sense, here, and I encountered this in other interviews too, of these three working in relay – that, once this (then) youthful researcher goes out into the tangible exterior world of special schools and disordered children, her intellectual interest in autism enters into exchange with, and becomes thinkable through, affects, emotions and bodies. Wilson (2004: 41) has argued elsewhere that the gut, specifically, is the boundary that 'allows the outside world to pass right through us' (2004: 44). She quotes Gershon to the effect that 'the open tube that begins at the mouth ends at the anus. Paradoxical as it may seem, the gut is a tunnel that permits the exterior to run right through us' (ibid.). I cannot help, now, but think about this story as a memory of the solid, exterior world of autism, with all its complexities, and fascinations, and possibilities, beginning to run through the body of the young researcher – and she since, it's worth adding, having gone on to major prominence in British psychology and neuroscience. Her early intellectual encounter with autism, as she recalls it, is not a simple placement of some diagnostic or biomedical object before the thinking, willing subject. It is, instead, a more much complicated form of embodied understanding and articulation, where the researcher's intellectual interest in autism begins to almost literally pass through the stomach, a relation, I think – and the clarity and force of her memory sustains this impression – *of* which she was quite aware, and *to* which she was remarkably receptive. Equally, in the second account, discussion of the science cannot go unaccompanied by an acknowledgement of a very visceral sadness – and vice versa. 'How completely fascinating,' she says, on the one hand, and 'how completely tragic,' on the other. In fact, immediately following this story, which ended with the telling phrase 'a very upsetting experience,' she said, fairly quickly,

> Upsetting and fascinating in equal measure. And I suppose when I decided to do the PhD I decided the fascination outweighed the distress. It wasn't motivated by some hifalutin idea that I was going to help.'

I am not especially interested, here – and I don't think there's an answer to this anyway – in whether her intellectual fascination or her feeling of upset were the proper instigators of the work that she went on to do. I am arguing in this chapter that it is the *mingling* of these two notions, or experiences – distress and fascination; upset and interest – their circling around one another, and their doing so in the midst of such a heavily embodied account, that give the clue as to what is genuinely at stake in the conduct of an autism neuroscience, *viz.* the use of the body, and of emotion, to sustain, generate, and animate an intellectual concern with the brain-basis of these children's idiosyncrasies. In the service of this argument, I am trying to draw particular attention to the complex way in which, between the first story and the second, this researcher's intellectual concerns have acquired some quite identifiably affective commitments. And I want to tentatively take issue with her own conclusion that one of these ultimately outweighed the other – that the PhD only became possible to the extent that the upset was spent, or that 'hifalutin' ideas of empathy and aid had been disbursed to clinicians and paraprofessionals. Even the essentially unbidden relation of this story to me bespeaks, I think, a much more complicated, and much more longitudinal, relationship between this researcher's capacity to think autism, and her willingness to feel it. I am arguing, ultimately, that it is her ability to trace her science through the two experiences, to memorialise them and articulate them together, that enables her to continue to push through the very complex work of autism neuroscience.

Objects of love

But why trace a neurobiological account of autism through emotional commitments especially? Can we infer any concrete relationship between, first, a scientific practice in which the affective commitments are so manifest, and, second, the object to which that practice ultimately addresses itself, *viz.* in this case, the neurobiological account of autism? We already have an idea, drawing on Wilson (2004, 2010), and on Myers (2012), that there is sometimes an identifiable relationship – one characterised, for example, by generation, sustenance and articulation – between the feeling and the embodiment of scientists, on the one hand, and the ability of those scientists to think or articulate a complex scientific or technological problem, on the other. In what remains of this chapter, I want to build on these suggestions; in particular, I want to move towards considering a constitutive relationship between the presence of these scientists' emotions, the affective nature of their scientific work, and the possibility of a neurobiological account of autism. I am particularly keen to make this move because the recollections and feelings that I have recounted clearly have some quite specific end in mind. I already quoted one interviewee saying, 'here was this thing that I was really passionate about,' and another saying 'if I was doing more of a pure science that wasn't...didn't have an end-point that was kind of applicable – I'd sort of find that quite difficult.' Below, we'll find another interviewee who, in the midst of a blatantly emotional account says, almost as an aside, 'I want to know…what it is, fundamentally.' But what 'it'? What 'end-point'? What 'thing'? In beginning to answer these questions, I want to put some more flesh on the key suggestion of this chapter, and this resonating with my larger description of a 'tracing' neuroscience – i.e. that autism neuroscientists maintain a strikingly emotional connection to neuroscientific research on autism *because* emotional exchanges and relays are heavily implicated in the specific coming-together of a concrete and convincing neurobiological account of autism.

In fact, there is already a well-known discussion of the role played by emotion in the coming-together of things, one that is also concerned with the troubled boundary between entangling and cutting, and this is AN Whitehead's 'process philosophy' (1935, 1964, 1979). Whitehead's complex and somewhat confusingly rendered system is difficult to set out in such a confined space as this. But very basically: Whitehead's 'process philosophy' is centred on what he calls 'prehensions' (1979: 19), which are essentially moments of substantive attraction or connection between one entity and another (1979: 19). To put it at its simplest: for Whitehead, such substantive connections, or 'prehensions,' between entities, are basically constitutive of all objects, and of all elements of all objects, in this universe. But when Whitehead talks about the connections between 'entities' he is including, for example, both a person regarding a chair, but also, within the timber frame of the chair, the regard that the various chemical components of the wood have for one another. A prehension is thus 'any grasping or sensing of one entity by another, or response of one entity to another: whether this takes the form of a stone falling to earth, or my looking at an object in front of me' (Shaviro, 2009: 3). But this grasping is described by Whitehead as a *feeling* – and it is particularly 'feeling,' Whitehead goes on to suggest, that describes 'the basic generic operation of passing from the objectivity of the data to the subjectivity of the actual entity in question' (1979: 49). The crux of it for us is this: the moment of substantive and constitutive connection – which, for Whitehead, in its on-going, processual character, is basically constitutive of all things – is a specifically *emotional* and affective moment, and always an instance of feeling. Whitehead describes the quality of these interactions in terms of 'the *affective* tone determining the effectiveness of that prehension in that occasion of experience' (1935: 227, my emphasis). And thus any 'occasion,' for Whitehead (i.e. a successful instance of this movement, in which some concrete thing is achieved), 'enjoys its decisive moment of absolute self-attainment as emotional unity…*the creativity of the world is the throbbing emotion of the past hurling itself into a new transcendental fact*' (ibid., my emphasis). If experience is the pre-eminent ontological fact, then 'the basis of experience is emotional' (ibid.: 226).

What Whitehead is trying to breach, with this claim, is the fundamental division between subjects and objects that has bedevilled our capacity to conceive of, and talk meaningfully about, the concrescence of objects for some centuries: this is his famous objection to the 'bifurcation of nature,' in which, according to Whitehead, a rigid separation was enacted between (in his example) the redness or the warmth of the sun, and its chemical or molecular structure. Instead, Whitehead wants to say that it is not the case that one of these categories is proper to the sun, and one proper to the subjects, minds or bodies experiencing it – but that, in fact, what the sun *is* is the series of positive and negative prehensions between things that have the positions of subjects and objects at any given moment (and here, in positioning subjects and objects in temporary relationships to one another, he draws no distinction between electrons, particle waves, neurons, people, warmth, and so on) (1964: 30; 1976: 41). More to the point, for my purposes, these relays are *emotional* in character; they have the quality of feeling. These acts of perceptions and prehension, Whitehead argues, can 'be conceived as the transference of throbs of emotional energy' (1979: 116). What we begin to see with Whitehead, then – at least the sliver of his philosophy that I lean upon here – is a disruption of the boundary between the feeling of something, and that thing's constitution. As Stengers points out, feeling, in the Whiteheadian scheme, begins to have the power of a vector: 'the point,' she says, of acceding to feeling, 'is thus to take literally the common-sense statement "this thing is present in my experience insofar as it is elsewhere," and to construct its concept' (2011: 294). Emotional, affective experiences begin to have the power of knowledge, here – and real, concrete knowledge about what a thing actually is. To think with Whitehead, as Stengers evocatively puts it, is to begin 'transforming scars into data' (2011: 310).

Here, I think, is where we begin to approach a more precise account of the relationship between my interviewees' deeply emotional stories of autism research, and their desire to nonetheless understand and describe the objective (in Whitehead's language: 'actual') neurobiological basis of autism. In the Whiteheadian scheme, insofar as an actual neurobiology of autism might come into existence, it is as a product of the very concrete emotional apprehensions of one another by its various elements – neurons, electrons, cognitive theories, scientific bureaucracies, and not least, individual scientists themselves. In this sense, I think, it becomes quite understandable that scientists working at the very coalface of neurobiological autism research, straining to understand this relationship, struggling to bring it into view, would begin to talk about their practice in such a deeply affected register. In talking through their attempt to apprehend the delicate connection between neuroscience and autism, and in reflecting on the fragility, still, of their understanding, would we actually *expect* them to say things like 'it was heart-breaking,' or 'I found it completely captivating, and...terribly, terribly upsetting,' or 'it's not just a job: you're dealing with people's lives,' or 'I *love* the kids and I love the families,' or 'it made me very excited,' or 'I want to do this, I want to do this.' Would we not predict, of an autism scientist, a heavy investment in feeling, and the presence of a notably emotional discourse? And would we not then anticipate, in her talk, precisely the kinds of descriptions of doing autism neuroscience that we have gathered here?

I am working hard to avoid over-doing this analysis: there is no comparator here, and so I make no general claim about the specificity of emotional discourse to autism neuroscientists (and, patently, it is not so specific). Nor do I argue that good autism neuroscience has to be, or is always, a deeply emotional experience (again, clearly, this is not the case). But I am marking a reluctance, all the same, to see coincidence between the delicate and complex process of a 'tracing' neuroscience, its awkward relationship (already much described here) to the separation of its own perceptions of the neurobiology of autism from that neurobiology itself, and the strikingly emotional and affective way in which many of these scientists talked about their own work. To solidify this claim, I will move onto my final example, in which both feelings about, and the constitution of, the brain-basis of autism are very much in question. It loops back to where we began this account: with feelings of love.

'It was from a theoretical stance, really, that I was interested in autism,' began a young researcher (L05),

> But then I started working with two children with autism - one who was four, and one who was five. And both had very little speech. And I did applied behavioural analysis [ABA] with those kids. I did therapy once a week for, like, I think four or five years [...] so I was quite involved with the families and the kids, and their schooling. And I saw huge improvements, in kids who didn't speak at four to five, and didn't really communicate very well at all, to then being eight or nine and [who then] could converse - not in a grammatically correct way necessarily, but [they] could make themselves understood. And also there were stark differences in these kids, which is quite, I think, characteristic of autism in general - just the variability, in that one child in particular, was so...he was just...he just wanted... [*here she struggled audibly*] he wanted to sp... ... [*very quickly*] he wanted to have friends. He wanted to interact with other kids. He just didn't know how to.

Already, here, there is the now-familiar juxtaposition between the more prosaically scientific account, and something that is altogether more self-consciously humane and emotionally committed: in the space of just a few sentences, we are discussing her 'theoretical stance' on autism and the way that some clinical differences are likely to be 'characteristic of autism in general,' but we are also discussing an unambiguously poignant and even upsetting image – which is the position of the child she was helping, who had developed to a stage where he really wanted friends, but not in a direction that left him with any clue about how to acquire or keep them. This researcher does not name her upset as some of the previous did – but I strongly remember this interview being a bit uncomfortable, and, to be honest, unexpectedly so, in that I seemed to be asking more troublesome questions than I had designed. As I have tried to render in the extract, there are also a lot of gaps and silences on the tape when I listen back to it now, especially in places where you probably *would* name the emotion in question, and there are even a couple of places where this very clever, accomplished and articulate young scientist is almost talking quietly to herself. 'Was [this child] kind of articulating a desire for friendship?' I asked.

> Yeah, yeah, oh, yeah-yeah-yeah. Whereas the other child wasn't particularly interested in other kids. And his language didn't take, interestingly, as much as the other child's did. Because he just didn't interact with those other kids. They were both in mainstream schools. Um...but anyway that really got...I

> wanted to know why...I wanted to know why there was such variability in the autism spectrum, um, in terms of their outcomes. And...what we could do about it, I guess; what kind of factors might actually determine their outcomes. So might it be intrinsic child-related factors, like level of language to start off with, or IQ, or personality, temperament, or...general personality – or whether there were more kind of environmental or extrinsic factors, like how much intervention they've had, or the type of schooling they've got, or the type of family structure they've got. So I kind of got interested in that, and what we could do about it. I guess what we can do to try and...ensure that children with autism reach their potential, basically.

'Yeah,' I said, 'because that's a bit heart-breaking – the kid who wants to have friends but doesn't know how.'

> Oh, I know. I mean that's what it was – I mean, the school he was in was lovely. So, that's just luck in some respects – he could not have been in a school that was lovely. And of course this was in primary school. When you get into secondary school, it's much more...it's hard for everybody, not necessarily just people with autism, but everyone.

She then steered away again from these kinds of topics (schools being lovely; kids having a hard time) and spoke in some detail, and again in that much more obviously 'scientific' vein, about differences in autism. 'Those are such different things to articulate,' I said, drawing attention to this – 'so, that early interest in 'theory of mind,' as a kind of theoretical idea to explore. And, then, from that to this almost quite visceral kind of ABA, very close interaction with these kids and these very emotional kind of difficult things. I mean – I guess I'm wondering how did you transition from one to the other, or how did you negotiate the tension between those two things?' 'In some respects, there wasn't necessarily a tension,' she replied.

> My research was on theory of mind - but also on other cognitive skills of kids with autism. And what I realised from my observations of working with the kids but also during my research and the results of my research, was that these kids did have problems in theory of mind, which limits the sorts of interactions they can have and understand. But they also have additional weaknesses and strengths as well. And that also places them with advantages, but also disadvantages as well. And so that was the tension, I think – [it's] how are we going to explain how these children can negotiate their social

> interactions, but not just their social interactions, but kind of their everyday lives...um, and, theory of mind wasn't up to explaining those difficulties.

From memory, I think I heard this as an unwillingness to acknowledge the obvious polarities in her account – in fact, in one sense her answer is quite unrelated to the question, but nonetheless takes the discussion onto ground that is entirely safe for her. And yet it was still apparent to me that the autism she was working with was involved in a much more complex system of apprehension and exchange – one that lacked a clear or obvious 'bifurcation' between competing cognitive theories, neurodevelopmental disorders, anxious children, and curious, empathic researchers. At the time, I remember being frustrated at her refusal to confront this. What I did not hear in them, however, and which definitely strikes me now, is her focus on, and insistent return to, the cognitive and neurological specificity of autism itself – *not* as an evasion, but in fact as an *elaboration* of this relationship. As she says, and I think quite correctly, relations between different ways of constructing autism, '*that* was the tension.' She is very clear – although, again, I am deaf to it in the exchange – that she is not interested in emoting for its own sake, or to convince anyone of her empathic and humanitarian credentials, but to 'know why.' In other words, she is trying to talk about autism specifically – but not as a way to get around emotion, or to ignore it. In fact, in the Whiteheadian sense, being able to thus talk about autism might even be the whole purpose of her openness to feeling in the first place.

In the interview, nonetheless thinking we had gone off topic, I re-phrased the question in a (possibly) more palatable way for a biomedical researcher, and asked if a therapeutic desire had always driven her work.

> Yeah, I think so, I'm an educational psychologist by background, so I guess...yeah...[*long pause*]. I guess I want...[*another longish pause*], I'm quite, um...[*almost talking to herself*] it's not empathy...I guess empathy, with...I really get on with my families, so I've just written 120 Christmas cards to my...all my kids [*she laughs uproariously on 'kids'*], all my autistic kids in [my home city] and here – just, you know, I do this every year. So, I get on really well...I love....[*she catches herself here, but then repeats quite definitively*] I <u>love</u> the kids and I love the families, and I think I feel that kind of rapport with them, so there's the sense that I kind of want them to do...I want them to do as best as they possibly can – because I can see that they have potential, and so, I think that's what drives a lot of my work.

As I noted at the beginning, the emergence of love is particularly striking here. I remember being a bit taken aback at the time – and I think she was too, given how much of an audible effort it was to even articulate its presence. In some quite prosaic ways, feelings of love should not be surprising here. My interviewees, many of them (like this one) relatively young women, many at quite an early stage in their careers, could not afford to be innocent of the emotional and pragmatic complexities of familial love (Donald, 2012). But there is more to be said about the specific role of love here. Just as it was in the previous account, the move here is, again, very obviously circulatory, as once more the narrative moves from theoretical stances, to love, and back out again: just like the hurried claims to empathic disbursement that I described above, this psychologist's very next words were:

> But at the same time, I want to know why - what it is, fundamentally, about kids with autism that is different to typical kids, so how do they perceive the world, and view the world that might be different to us...and how...and what we might do to ameliorate any differences.

Again, I have said that accounts of autism neuroscience are often shot-through with thick reports of sadness, upset, love, anxiety and even pride. I have said that scientists who are methodologically open to affect as these reports suggest, are not just emotional for its own sake but that, in fact, their capacity for feeling, and for working through (and with) emotion, is heavily implicated in their attempt to work across an un-'bifurcated' nature. This move is once again remarkably explicit here – we see that the actual practice of autism research is heavily invested in the simultaneous passing-by of thinking and feeling: that passionate attachments ('I love the kids') are not separated from intellectual interests ('that's what drives a lot of my work'). But also, again, this researcher's basic concern, as she is very keen to stress, is the 'why' – what, exactly, is this thing underlying all these problems? I am now disinclined to think about this as I did at the time, which was to assume that my interviewee focused on the 'why' in order not to talk too much about an embarrassing surfeit of affect, i.e. that she 'love[s] the kids…and the families.' My feeling now is that the 'why' of autism might just be precisely what this whole system of affective labour is directed at in the first place; I am beginning to suspect, in other words, that the specific, dry, and technical issues about the objective make-up of autism that skate endlessly across the top of these accounts are *not* simply a way to avoid talking about love; there are there, in fact, precisely to explain it.

Conclusion

I began this chapter with a commitment to thinking about the strikingly emotional and affective registers that some of my interviewees drew on when they talked about their experiences in autism research. I embedded this observation within a science and technical studies literature that, on the one hand, has noted the historical effacement of the personal and the familiar in the turn to a collective scientific 'method,' and, on the other, has been keen to describe the preponderance, nonetheless, of individuality, personality, feeling, and emotion, at the heart of technoscientific spaces. I allied myself with two more recent descriptions that went further in locating the precise role of feeling and bodies in scientific and technological spaces – Elizabeth Wilson's (2010) discussion of the generative relays between thinking and feeling in Alan Turing's career (and in the fields of artificial intelligence and robotics more generally) and Natasha Myers's (2012) description of the embodied articulations and renderings that help molecular biologists to understand, interact with, and account for, the molecules that they bring into view. Taking these as an inspiration, I then pushed my account towards some more concrete description of the role of emotion in autism neuroscience – in particular, drawing on AN Whitehead to posit a generative relationship between thinking, understanding and feeling within autism neuroscience. Using Whitehead, I argued that 'feeling' the neurobiology of autism, and maintaining an explicitly 'emotional' connection to the work of both understanding and describing it, plays a concrete role in potentially generating and sustaining a convincing neurological account of autism.

While the role of the emotional labour of researchers has a well-established place within the critical-psychological and psychosocial literatures (Cromby, 2007; Greco and Stenner [eds.], 2008), it is still not widely acknowledged – at least not by researchers themselves – within the self-consciously 'harder' spaces of cognitive neuropsychology and biological psychiatry. Both of the latter are publicly invested (and, in the contemporary academy, probably with good reason) in un-problematized discourses of quantification, distance, method, simple objectivity, and so on. It is not at all my purpose to try and remove the 'veil' of these logics, or to argue that they are 'really' still very obviously emotional, or that they are working through much more 'subjective' practices than they would care to admit. I have also worked to avoid a gendered claim – and this will become more pertinent in the subsequent two chapters – that psychology is somehow, still, a very 'fluffy' science (much like sociology, needless to say), evidenced by the irrepressibly emotive subjectivity of its practitioners. My purpose, instead, has been to describe the quiet, complex, but still generative alliance between these logics of distance and impersonality, *and* the nonetheless very-obvious on-going presence of feeling and emotion in these spaces. The aim of my description has been to show that tracing the neurobiology of autism across categories of both thinking and feeling, of trying to understand it 'out there,' by feeling it 'in here,' shows not only the porousness of the boundary between quantificatory logics of distance and the more personal labour of feeling, but it also shows how the two might be traced together in attempts to understand the brain-basis of such a tricky and ill-defined diagnostic entity as the autism spectrum.

On this basis, I want to conclude with a normative suggestion, implicit throughout the chapter, which is that researchers' experiences of flows of love, desire, fear, sadness, and so on, can be a positive quality in neuroscience – and indeed a good thing for both the quality and reach of neuroscientific research. This also means that there should be room to talk more frankly, and more positively, about the roles of emotions and feelings – of stomachs and loves – within the spaces of the contemporary brain sciences. My interviewees talked quite openly about their affective commitments. And yet, although the function of these commitments seemed undeniable, no interviewee really talked about the positive place of feeling in her neurobiological practice. In fact, there was a tendency for interviewees to step away from these accounts, and to circle back to a distanced discourse of 'understanding.' But I want to argue that there should be room to talk more explicitly about the concrete roles of emotion and feeling in bringing the objects of a scientific neuropsychology into view; and also that there should be scope to think about creating spaces for reflection on, and even refinement of, that emotional labour. This is not to say that I find the reluctance of my interviewees to be explicit about this wholly unreasonable. For many, acknowledging the role of feeling inevitably raises questions about the degree to which psychology might, in such circumstances, continue to call itself a 'science,' and, this of course, a status only lately conceded it. In fact, as my conversations developed, it became clear that the relationship between psychology and science, and the role of neuroscience in mediating this relationship, was a specific area of concern for a lot of my interviewees. This is precisely the topic of the next chapter.

5. Falling in the middle: psychology and autism between 'science' and 'the social'

Introduction

In reflective mood during the prologue to his *Making up the Mind* (2007), Chris Frith, the distinguished British social neuroscientist, discusses his undergraduate decision to switch from the study of physics to an entirely new disciplinary offering: psychology. Frith describes, with evident regret, how the switch immediately placed him lower down the scientific pecking order: 'I have continued to study psychology ever since,' he assures the reader, 'but I have never forgotten about my place in the hierarchy' (2007: 1). As the book progresses, though, it becomes apparent that this is not the whole truth. Indeed, precisely illustrating why this hierarchy is *no longer* so meaningful is the real purpose of Frith's reminiscence. 'Much has changed in psychology over the last 30 years,' he says:

> We have borrowed many skills and concepts from other disciplines. We study the brain as well as behaviour. We use computers extensively to analyse our data and to provide metaphors for how the mind works. My university badge doesn't say "psychologist" but "cognitive neuroscientist"' (2007: 2).

The section of the book in which this recollection appears is called 'real scientists don't study the mind.' As the argument of the prologue becomes more apparent, though, it emerges that Frith's intention with the title is either ironic or historical – because his claim is precisely the opposite. 'Big science' he tell us later, in a passage pasted underneath a picture of an MRI scanner, has 'com[e] to the aid of soft psychology…we no longer need to worry about these soft, subjective accounts of mental life. We can make hard, objective measurements of brain activity instead' (2007: 12, 15).

This excerpt – this small, slightly self-regarding slice of one well-known neuroscientist's imagination of his own past – would need its own thesis to be fully unpacked. I quote it here, though, only for the reason that it marks a claim that is sometimes characteristic of the meeting-point between psychology and neuroscience – and this is a claim to the (now) unambiguous status of a *science*, from within psychology itself. Throughout the thesis, I have discussed some little-understood ambiguities at the heart of neurobiological research on complex psychiatric and psychological diagnoses. I have told this story principally through an account of neuroscientists delicately – but doggedly – 'tracing' the neurobiology of autism through all kinds of definitional, epistemic and affective complexity. In doing so, I have emphasised the novelty, the dexterity, and the care, with which autism neuroscientists are actually able to recognise and work through these entanglements. I have shown them drawing different sorts of knowledges and commitments together; I have described them as working with contradiction, and as refusing to be cowed by ambiguity – and yet I have said that they are always, still, carefully working towards a coherent and convincing neurobiological account of autism. In this chapter, that story runs up against something of a limit. Because amid all this complexity, and sensitivity, and care, there was often in my interviews a very explicit and jarring characterization of psychology, under neuroscience, as now, unambiguously, a straightforwardly 'hard' and objective scientific practice. These invocations of science were not only descriptive or incidental. As I will show below, they were quite specifically scient*istic*, in that the image of contemporary neuropsychology as an idealised, reductive 'science' was also the basis of a knowledge-politics – one that made sharp distinctions between the hard/scientific good and the soft/subjective bad. This politics emerged from an insistence that psychology had only recently emerged from decades of mystification – that psychology was now, thanks to very recent technological and conceptual developments, premised on an uncomplicated materialism and addressed to a measurable organ; that within this organ lay the seat of a whole series of valid and specific psychopathologies; and that, soon enough, many of these psychopathologies would be robustly characterised by quantifiable, image-based biomarkers. There was a sense, in other words, in which the work of locating autism in the brain was related by interviewees as the practice of a hardening science – one that would banish the 'soft' and the 'subjective' from the space of psychological knowledge-making.

This was odd. I have rather laboured my claim that, within a broadly conceived neuroscientific discourse, autism neuroscientists resists straightforward accounts: within this space, autism is ceaselessly entangled in not only medicine, psychology, and the brain, but also, as Stuart Murray has pointed out, in a much 'wide[r] fabric of narrative, representation and characterization' (2012: xiii). And I was taught this basic truth about autism mostly by psychologists/neuroscientists themselves – who, as discussed particularly in Chapter Three, often maintained a deeply sceptical and deflationary attitude to neuroscience. And so, without putting too fine a point on it, I struggled to understand my interviewees' nonetheless frequent enrolment of their own practice into a simplistic, and frankly inaccurate, caricature of some peculiarly 'scientific' neuropsychology. How might such an enrolment relate to the broader picture of neuroscientific ambiguity and care that I have been describing throughout the thesis? Where do these claims sit within a longer history of vacillation between psychology and 'science'? And what is the significance of the neuroscience of *autism*, now, in the mediation of that relationship?

These are large concerns for a single chapter. In what follows, though, my strategy will be to approach them from one direction – and this is to focus on the way that my interviewees talked about psychology's coming-into-science as a specifically *temporal* or *historical* phenomenon. As I show below, when these invocations of science came up, they were often introduced (as Chris Frith introduces them) either as moments in the interviewee's own career or lifetime, or, otherwise, via reference to the discipline's collective memory of its own shameful and mystificatory past. Thinking about the temporal nature of these claims allows me to draw in an interpretive frame from Georges Canguilhem, via Dominique Lecourt (1975) and Nikolas Rose (1996a), and this is the observation that told histories of psychology have a tendency towards 'recurrence' – i.e. that they are often a way of arranging the past into justifications of the contemporary, and thus, also, that these histories are formed, and reformed, in order to demarcate and regulate the boundaries of psychology's present (as well as its future). In what follows, I will draw on this frame to interpret claims to 'science' as ways in which my interviewees were, indeed, beginning to think, articulate, and negotiate the wider boundaries of a contemporary psychology.

Drawing on this frame also creates resonance with my broader account. While I have said that this rush to claim the (unreconstructed) status of 'science' might run counter to the careful work of tracing already described in the thesis, I nonetheless want to work the data in this chapter through a slightly different portrait of a tracing neuroscience, one not yet fully discussed here, and this is their relationship to a kind of disciplinary and epistemic boundary-working (Gieryn, 1983). As described in earlier chapters, to 'trace autism up' is also to think it and pull it across borders: these are often the borders of things like 'the intellect' and 'the body,' or like biological truth' and 'diagnostic convenience.' But a tracing autism also sometimes asks the researcher to work across the borders of very different disciplinary areas or 'epistemic cultures' (Knorr Cetina, 1999) – and this is not always easy. So I want to ask whether we might not begin to hear these incongruous and suddenly-declared commitments to some crude ‘science’ as a way of actually thinking about, and working through, particular kinds of disciplinary entanglement. My gambit is that when an interviewee says to me that, for example, she was relieved to learn, as an undergraduate, that psychology was a ‘proper’ subject, that was ‘scientific,’ and that it was not about ‘Freud’ and ‘faff’ – that there may be scope for interpreting this as a way of articulating, working through, and dealing with, some quite recent disciplinary entanglements. And these, in fact, having lately begun to trouble the boundary of just this ‘proper’ subject, and also the sanctity of its claim to ‘science.’

An entangled boundary that I have in mind is one frequently traversed by a traced neuroscience of autism, and one that also troubles the specificity of its claim to a 'hard' science' modelled on physics, and this is the boundary between a quantitative and reductive science of psychopathology, and the on-going incursion, into psychology’s milieu, of a too-often unruly and unfathomable *social* world. There are two reasons that I am minded to hear the presence of ‘the social,’ particularly, in these accounts. First, although the long-running story of psychology-as-a-science reaches something of a climax with the institutionalisation of cognitive neuroscience at the start of the twenty-first century, there has nonetheless been, at the beginning of that century, something of an unexpected coda. The brain-imaging era has indeed produced a renewed scientific confidence (Kandel, 2007). And yet, particularly in the case of mental disorder, the great flowering of biomarkers, diagnoses, and drugs – which were to quickly come from access to a realm ‘beneath’ ordinary mentation – has never really come to pass (Hyman, 2008). This disappointment is strongly marked by the re-emergence, within psychology, of an etiologically- and diagnostically-salient *social* world. This is an influence which is remarkably difficult to account for in a laboratory science, but which still wraps itself around neuropsychological accounts of distress, via the familial and cultural salience of epigenetic effects, gene-environment interactions, and so on. Second, I said in Chapter One that autism is a disorder (diagnostically) predicated on particular ways of thinking about social interaction and communication, and also that autism has always been particularly hard to disaggregate from the complex movements of the societies running through it. One of the most noteworthy things about autism as a clinical entity, then, is its etiological and diagnostic candour about the nature/culture crossings that run through so many contemporary psychologies and neurosciences. I also wish to explore, below the possibility that working the border of psychology and science might reflect the marginal work of tracing *autism*, particularly – as studies of brain meet claims from culture, representation, and sociality.

The key argument of the chapter is that sudden claims to 'science,' in these interviews, might be read as articulations of the awkward boundary-work that is required of a tracing neuroscience. In particular, we can hear these claims as ways of thinking about a psychology conducted at the sharp edges of 'neuroscience' and 'the social' – a border-point that is salient within a contemporary neuroscience of psychopathology generally, but also in studies of autism in particular. To make this case, the chapter proceeds through four sections: in the first, I set out and discuss the data on claims to science, noting both their historical orientation, and also their specifically scientistic overtones; in the second, I situate this account within the history of psychology, and I introduce the notion of 'recurrent' history as the historicized disciplining of troubled boundaries; in the third section, I discuss the boundary that particularly interests me here, and I focus on the emerging clinical and research relationships between neuroscience, psychology and 'the social'; in the final section, I discuss this entanglement as something that is especially at stake in the neuroscience of autism, noting both autism's historical relationship to interpretations of social interaction, and also some more recent and quite specific claims to an autistic culture.

There is a lot going on, here, for one chapter. But I want the reader to remember the basic argument that I am trying to make, which is that the troubled boundaries of a complex, tracing neuroscience are not always negotiated with ease and grace; that sometimes there are quick and crude attempts either to re-articulate, re-erect, or even just find oneself within, specific markers of distinction. We see this particularly in the case of the neuroscience of autism. But I will also suggest, in the conclusion, that there may be elements of this argument that could be applied to the emergence of psychology as a 'cognitive neuroscience' more generally. My tentative suggestion will be that that this century's psychology is to be found neither in the harsh sterility of the laboratory, nor in the discursive misery of the seminar-room. Instead, it will emerge from a growing recognition that, for psychology, the delicate hygiene of a laboratory practice, may *only* be meaningful, and indeed practicable, when it is entangled in the delicate webs of some more 'social' interaction.

Freud and Faff

In these interviews, positive invocations of psychology as a science, as well as dismissals of approaches that were deemed *not* to be scientific, often arose rather suddenly – arriving without a great deal of foreshadowing, and usually without obvious reference to whatever else was going on during the interview. These invocations tended to circle around a small set of common images – e.g. referring to a strong intellectual preference for physiology and the brain, explicitly differentiating the speaker from an older era of psychological flakiness (often represented by Sigmund Freud), referencing a desire for quantification, or sometimes even leaning on an image of base human animality. Consider the following extract, which comes from an interview with a young lecturer (L03), which I already quoted in the chapter on critical approaches to neuroscience. At one point in the interview, this interviewee told me about how she had become interested in both autism and neuroscience as a young scholar, but had been rather unsure about psychology. 'Psychology, she said,

> especially when you think of it at a school level, being sort of Freud and faff, and not really scientific, [whereas] what I wanted to do was, you know, do something scientific about the brain. So then I was really pleased when I discovered that, actually, you could do experimental psychology, that it was a proper subject that you could study and things. I guess I'd always been interested in these kinds of questions about social cognition, um, but it took a while for me to get round to finding a way that I thought those questions could be addressed scientifically, rather than in a sort of arm-waving kind of sense.

I am interested in how much of her earlier nuance goes missing here: ways of thinking about mental phenomena are split between 'faff' and approaches that are 'really scientific'. Freud, meanwhile, whatever the pretensions or possibilities of his early desire for a 'scientific psychology,' is dismissed out of hand as an unscientific 'arm-waver' (although a *rapprochement* with Freud is, of course, the focus of much neuropsychological attention elsewhere – see Hustvedt, 2010, or Papoulias and Callard, 2012, for discussions). The non-obvious 'arm-waving' is an interesting image: it suggests someone performing a piece of rhetoric, an attempt to convince and to build argument, the location of an argument within the body of an individual, *personal* knowledge – all those images of an eighteenth-century proto-science that I discussed in the previous chapter. And what's interesting, for my purposes, is this interviewee's evident sense of relief at the discovery of an experimental psychology that was, by contrast, a 'proper' subject – by which she meant one that was 'really scientific.' This is what I mean by the invocation of a scientistic imaginary in these conversations. The use of a word like 'proper' is a clue: this is a memory that is precisely trying to mediate what counts as legitimate practice in the present; it shows the art of memory as both self-conscious recurrence, and present-centred programme (Rose, 1996a: 43).

Consider, in the same vein, the following discussion, which comes from a more senior scientist (SS01), trained in the pre-brain-imaging era, but for whom Freud still plays the same role, drifting through her memories like the ghost of psychology's shameful, half-remembered, past. 'I had actually not wanted to do [psychology],' she said to me, remembering the advice to switch from an undergraduate tutor

> because the only psychology I was aware of was Freud and Jung and that, which I'd tried reading and found irritating. But then I realised that it was actually a much more sort of scientifically-based [at her undergraduate institution] – it was experimental psychology, it was linking in with physiology that I found…I was quite interested in…I suppose I've always thought, 'are there physiological differences that explain why people are different?'

The recourse to a specifically physiological approach as the only one acceptable to her marks something important here – as does the rhetorical deployment of 'physiology' to mark the division between practices that are either (a) Freudian and irritating, or (b) experimental and scientific. Later on in the conversation, I asked her: 'why is it important that it would be a physiological psychology – or one that's in touch with physiology?

> I don't know, um, the answer to that. It's just…I think it's just my, perhaps a somewhat reductionist bent I have, um….

'Right,' I said, struggling to properly articulate my question: 'or what irritated you about Freud – let me put it that way.'

> Oh, I just found it all untestable and vague and um…I just felt there was no way of testing it. I mean, I was right from the start, I was very interested in

> the idea that if you've got a theory it should be testable. And I thought he, you could make up…at the time, this was when I was about 18, I'd just given up religion. And Freud struck me as part of the same sort of, you know, you believe what the great man says, but you don't sort of have any way of really testing it.

Here, we encounter a particularly rich account of the claim to psychology as a science. What marks it out for me, among many such, is a specific embedding of this scientist's own narrative in references to the physiological body, expressions of a specific tendency towards reductionism, and, right at the end, a re-making of the same division she erected at the beginning of her story, here marked as a split between approaches that are either 'testable' or 'religious.' It is telling that she positions her own entry into psychology at the precise moment in her life in which she had 'given up religion.' The coincidence of the chronology is, again, neither here nor there – what I hear, specifically, is personal memory being put to a very specific use, and this is to embed the interviewee's own intellectual career within a concretely and unapologetically scientific tradition.

Here is another account of psychology, given in the same sort of physiologically-prescriptive vein. In fact, this interviewee (SS03) had started our interview, without any prompting from me at all (this was quite early in the process), with a quick and blunt claim about science, saying of his undergraduate degree in psychology:

> It was very much a branch of the biological sciences and presented as that […]. It used to be in the Arts faculty and then you did a degree in philosophy and psychology, and then for a few years they moved into science, which happened at the time I was there – where you did physics, chemistry, biology, mostly biology – and then moved to graduate specialising (it was then a 4-year degree, which was terrific) and, uh, my degree is really natural science […]. Most of the students hated it. You must remember, in those days, you know in my final-year cohort there were 14 students. These were the good old days of when university was a kind of elitist experience [*laughs*]. And I think I was the only one who said 'yes, I think this is the way psychology degrees should be.' And I still think that.

What intrigues me about this contribution is the self-conscious effort not only to present himself as a biological scientist, but also his desire to let *me* know that he approved of this designation, and that, in fact, he had approved of it even at a time when such approval was neither popular nor profitable. Later, this same interviewee embedded his own self-image within a similar account of physiology to that just discussed: 'I think we are embodied organisms,' he said,

> …and while I am not a complete biological reductionist by any means, you cannot talk meaningfully about the way people act in their environment without understanding the fact that we are biological organisms. It's an absolutely necessary part of understanding that.

We should note that there is an important qualifier here ('not…by any means'), and I will be faithful to it when I re-introduce this interview in the second-next section. But let me just note here that not only was he keen to stress that the object of psychology was, specifically, a 'biological organism,' but he went on to add a scatological, and even animalistic, coda to this reasonable-seeming description:

> Without wishing to be gratuitously rude or whatever, but, you know, we have to piss and shit and sleep and have sex and stuff like that. And there's a point for any of us in which those kinds of things take over. And, uh, our life is kind of about managing – Freud in a sense was right: we have to manage the social niceties, and the fact that at some point I feel, you know, during this day, I feel like I'm going to have to take myself off to the loo and take a shit.

I am slightly struck upon re-reading, here, by the presence of a more nuanced and sympathetic reference to Freud than we have had previously. At the same time, and precisely in the midst of this relatively wide-ranging and sympathetic view, he insists on a deeply scatological and self-consciously biological image of human animality – which is not only about shitting in general, but quite deliberately (I think) breaks a 'social' taboo by emphasising the priority of his own biological need, at some point, to go and take a shit. This is not to position animality as a necessarily narrow or reductive position (Wolfe [ed.], 2003) – but it is precisely via a reference to a sense of biological animality, and a memory of his own early reconciliation with it, that he justifies a deeply natural-science approach to psychology: 'those kinds of things take over.'

And these are only some of the more vivid accounts. In fact, these kinds of memories and claims came up again and again, as did other memories, often second-hand, of a different era in psychology's formation, as well as fears of unscientific approaches pursued in other places. 'The psychology department [at my undergraduate university] is a department in the natural sciences,' said yet another researcher (R04) of her first encounter with psychology

> that also influenced [my] approach very much, and I also like the biological

> view of psychology and the natural sciences view of psychology very much that they have there.

Another (SS02), in response to my frankly-expressed puzzlement at the persistence of this emphasis within my interviews (this was well into the process, when I was starting to assemble an image of 'ambiguous,' 'uncertain' neuroscience) explained to me that psychology had become 'more biologically focused, based in the brain. So a lot of psychology departments are now called cognitive neuroscience.' Yet another (R03) told me how (and again, a particular image of Freud looms large in the background here),

> um, amazingly I think, in this country, often, if you get a diagnosis [of autism], you will be referred to the Tavistock [*an historically famous centre of British psychoanalysis*]…which amazes me. And I've spoken to friends who've gone there and of their experience, and I'm absolutely horrified by the things that they're told and the things that are being done, so….but again, that's sort of my scientific bias coming in I guess here.

Or consider also this account from a clinician (SS04), who is equally keen to root himself within a scientific and biological tradition – but who does so against a mirror-image of time, rather than place:

> I'm not sure whether…because this would be true for other disorders, where you know 20, 30 years ago there wasn't the sort of brain, neurosciences-type of research, and now there sort of is and that must relate to a whole bunch of clinical conditions […] I've always had a relatively medical model [and been] happy to embrace sort of biology.

And there were still other ways of talking about this too, that I have not really considered here – a postdoc (L02), for example, qualified her own intellectual investment in psychological phenomena as a quantitative interest only: 'I like physics too,' she said: 'Everything that deals with hard data or numbers, experiment, things that you can measure, that appeals to me.'

Let me close this description with the following account, which comes from one of the small number of PhD students (P01) whom I interviewed, and who, in fact, after an undergraduate degree in psychology, was working towards a PhD in neuroscience specifically. She was talking to me about her experience of studying A-level psychology, which sounded like a surprisingly ecumenical experience: in the exam, you had to consider a fictional patient's psychological problem from the point of view of a clinician, and then take whatever approach you wanted – including the psychoanalytic. 'So when I did this,' said the now-PhD student

> and I started to look at all these different approaches, the biological one always gave me answers that I just trusted a bit more. It always just fulfilled me a little bit more when I thought about what's going on in the body. [...] But bizarrely, I don't know how I got so...fixated on this idea that I needed to know about the brain. But for some reason I did, and so then I [*laughs*] I like pestered the people in the biology department [at her undergraduate university] to let me do some of the courses on, you know, receptors and neurons and stuff like that, and so they did. Although it wasn't actually on offer.

This memory might actually capture an important moment in the history of British psychology – one in which even psychoanalysis is still part of the discipline's visible intellectual hinterland, although it seems, somehow, intrinsically unsatisfying. But it is equally a moment in which a young would-be researcher is drawn inexorably towards a specifically biological approach, for reasons that she can't quite articulate (and this interviewee's loose grounding of her relationship to biology over psychoanalysis in a sense of 'trust,' especially, is interesting in this context). It is certainly remarkable that, so recently, her (prestigious) psychology degree programme did not include a neuroscientific component, or much of a brain-based focus ('it does now,' she assured me later). But my interest, in what follows, is in her clearly-articulated sense that to *be* a psychologist, was to seek out brain-explanations all the same: despite an institutional and pedagogical lag, the 'science' of psychology had become – to this young interviewee at least – second nature. 'When I really can't understand something,' she said to me later on, 'then I often, like, try to take it down to: okay, what would the neurons be doing?' What I wish to think through, in what follows, is the ground of the seeming naturalness, or inevitability, of such an approach to a young scientist. Why is it that, in 2012, amidst a cohort of autism neuroscientists so well-versed in the strangely-traced entanglements of their own pursuit, would a nascent career be *so strongly* narrated through a commitment to these unabashedly organic and scientistic tropes? Why would it be located within such an explicit – but also slightly unargued – rejection of any major historical alternatives?

‘Everyone has his classics’

In a 2010 article for *Scientific American*, and situating his analysis within a discussion of autism particularly, NIMH Director, Thomas Insel, lauded the emergence of approaches from the new brain sciences within the general study of psychological and psychiatric distress – which Insel took as an invitation to declare that, now, finally, in 2010, psychology and psychiatry had become fully and properly scientific. If, in the past, psychological and psychiatric analysis addressed itself to some purely mental and therefore speculative function – then ‘today,’ wrote Insel, ‘scientific approaches based on modern biology, neuroscience and genomics are replacing nearly a century of purely psychological theories’ (2010: 44). Claiming a re-definition of psychiatric and psychological distress as a series of problems in ‘neural circuitry,’ Insel argued that,

> from the scientific standpoint, it is difficult to find a precedent in medicine for what is beginning to happen in psychiatry. The intellectual basis of this field is shifting from one discipline, based on subjective “mental” phenomena, to another, neuroscience. Indeed, today’s developing *science-based* understanding of mental illness very likely will revolutionize prevention and treatment and bring real and lasting relief to millions of people worldwide’ (2010: 51, my emphasis).

If we go with Insel, it might be said that the accounts in the previous section simply reflect the march of history – that my interviewees associate themselves with an unproblematized science only because they are now, unproblematically, scientists. This is the explanation that would perhaps appeal most strongly to many of my interviewees. It has all the attractions of parsimony about it. But it doesn’t work. As I have laboured to make clear throughout the thesis, the ‘neuroscience’ that I encountered during this project was almost always a complex, contested and multifarious intellectual activity – occasionally silly and arrogant, certainly, and sometimes crudely reductive. But more often than not, I found a neuroscience that was not only happy to trace neurobiological accounts across all kinds of intellectual, affective and definitional border-lands, but a neuroscience that seemed to be, in fact, quite specifically, mining these margins.

Perhaps more to the point – and quite related to the discussion in Chapter Three – the relationship between psychology and neuroscience is perhaps less stable than commentators such as Insel might suggest. First we could note, for instance, that whatever their burgeoning facticity within (some) popular and journalistic imaginations, the neurosciences are still quite self-consciously hybridized from other disciplines (Abi-Rached, 2008; Abi-Rached and Rose, 2010). And while some of the elements of this hybrid (such as physics and medical imaging) carry a lot of scientific and clinical capital into the new endeavour, other parts (like psychology and psychiatry) are less well-endowed. This leaves studies of psychopathology a bit vulnerable: while the admittance of especially psychology into a neuroscience practice has removed much of the whiff of scientific marginality that has tended to trail behind the psychological sciences like a dust-cloud – recent internal discussions on the efficacy of brain-imaging for the study of mental phenomena have also wondered whether that confidence has not, in fact, been a temporary development (Neurocritic, 2012; Bor, 2012; Diener, 2010). Most of my interviewees were quite well aware of the 'contingency' of their neuroscience, and of the deeply 'situated' nature of the connection between psychological phenomena and neuroscientific knowledge – whatever their colleagues (it was always colleagues) said to journalists or to press officers.

Thus, I cannot explain these invocations of science using Insel. Indeed, we might take Insel's own ability and desire to make such a claim as part of the phenomenon under discussion. But then what does explain these claims? It is difficult to read these lines from Insel without remembering George Canguilhem's acid remark, made more than half a century ago, that 'if one terms classical psychology that psychology which one is proposing to refute, we must say that in psychology everyone has his classics' (1980: 44). Canguilhem had his own axe to grind with psychology of course. But his observation draws our attention to a quality of psychological talk, and psychological memory, that I will lean on quite a bit in what follows, and this is the suggestion, rooted in Canguilhem's historical epistemology, that histories of psychology tend to work within a double move. In one sense, the history of psychology might be written as a straightforwardly teleological story of, for example, the establishment of particular investigatory practices, the emergence of a laboratory-space and a set of techniques and interventions, the formalisation of methods, the emergence of testing and statistics, the growth of professionalization, the emergence of the major paradigms of the twentieth century, and so on. This kind of account tends to begin with Aristotle, passes over Locke, and then runs straight to Wilhelm Wundt and to Henry James, skips lightly across the psychoanalytic detour – before landing more-or-less squarely on the quantitative biological science that we know today (in whatever form that psychological science takes. Boring, 1950, remains the canonical account. See also Koch and Leary, 1985, or Malone, 2009). In a second sense, though, these histories are set up and told quite specifically to arrange the past into a reasonable account of, and justification for, the speaker's claim on the present. More to the point, perhaps, for our purposes, the dominant trope of such recurrent histories is often the (eventual) settlement of psychology as a *science*. 'The behavioural sciences have left the armchair and entered the laboratory,' says Wertheimer at the close of his *Brief Introduction to Psychology*,

> Reliance on wise, experienced minds, equipped with oratory and quill pen and paper, has given way to reliance on impersonal scientists with their precise measurements, their cold numbers, and their electronic computer (1987: 156).

Writing for a textbook in 2003, Fuchs and Milar suggested that psychology finally 'appeared to be less self-consciously concerned with the status of psychology as a science and more concerned with the kind of science psychology was to be' (2003), which is both a neat acknowledgement of this long-standing anxiety and the role it has played in psychological historiography – but also still an expression of it too ('*now* we are scientists'). What this double-function of related histories of psychology directs our attention to, then, is the degree to which claims to the historical presence or absence of science in psychology might be ways of negotiating similar boundaries in the present. And this opens the door to hearing personal and collective memories of psychology-as-a-science as a language for talking about the contemporary border between, precisely, psychology and science.

Certainly, the circular and on-going nature of claims to science in psychology, and the variability of the bases of such claims, seems to make such an argument attractive. Theodore Porter, for example, has drawn attention to the emergence of statistics in psychology around the time of the First World War and he has shown how, just as neuroscience and genomics would function a century later,

> up-to-date statistics became a mark of self-consciously scientific experimental psychology…researchers were urged to follow statistical rules as a matter of scientific probity, and to feel guilt if, for example, they reformulated the hypothesis after the data came in' (1996: 209-212).

But as Porter demonstrates, this resting of psychology's scientific respectability on the 'rigor and certainty' provided by statistics had less to do with the obvious objective of statistics (then comprehensively split between the approaches of Pearson and Fisher), and rather more to do with a desire to locate, on whatever basis, some strongly scientific basis for this growing discipline (ibid; *cf.* Rose on numbers as mediators of the boundary between objectivity and politics [1999: 198]). Indeed, the simultaneous reliance on, and black-boxing of, a calculatory logic, specifically as a way to ground the scientific credibility of psychology, runs very much parallel to the more organic and biological claims that we were more concerned with above. I well recall my own naïve surprise, during an early foray into a clinical brain-imaging site, upon learning that an fMRI brain-image was really only a way of illustrating a spread-sheet – that behind each thick, rotating, coloured brain-image, lay nothing but a two-by-array of individual numerical calculations and directions. Indeed, Anne Beaulieu has drawn attention to the degree to which the process of 'biologisation' that many scholars have described is also a process of 'digitalisation' – a rendering of soft organic edges into hard, numerical figures (2000: 12, 63).

The emergence of an explicitly experimental behaviourism is another way to think this circular, past-repudiating scientism, again memorably described by Canguilhem as 'the principle of the biological psychology of behaviour' based on 'the definition of man himself as a tool' (1980: 47; *cf.* Mackenzie, 1977). In the second half-of the twentieth-century, Trudy Dehue (2001) has made a similar claim for the emergence of the randomised control trial within psychological research, noting, within an institutionalized and governmentalized scientific/experimental psychology, 'the aspiration of ruling by technique rather than tradition, of replacing the individuality of both the governors and the governed by impersonality' (2001: 296). A century before both of these developments, we could also look at the emergence of a claim-able science of psychology by eliding the personal characteristics of experimental participants – Danziger shows how, for example, in the *American Journal of Psychology*, the percentage of studies naming the subjects dropped from fifty-four to twenty-four in less than half a century (1994: 74). As Mitchell Ash has pointed out, these on-going, recurrent claims to science '*now*' should tell us something important about psychology, and about the way that it occupies a peculiar situation in 'the status system of the sciences,' being

> a collection of quarrelling specialities and schools, pulled to and fro between methodological demands presumed to have been derived from the "exact" physical and biological sciences and a subject matter extending uneasily into the social and human sciences' (1992: 193).

Rooting myself precisely in this observation, I am trying to show that there is nothing new about the claim that psychology is 'now' scientific, and that this repetition, and the different forms in which it is made, draws attention to the way that psychological research has thought and worked this border (between 'the biological' and 'the social and human') over the centuries. I am arguing, therefore, that this neuroscientific-organic claim within my data, yet another claim to an ultimate scientific basis for psychology, may be read as the latest movement of a wheel – one that, at each recursive turn, is ever surprised at the crudity, and the subjectivity, of its own very recent past.

Let me stress that my point, here, is not to disrupt or to 'critique' the claims of a scientific psychology. Danziger has suggested that to talk about 'a field like scientific psychology' is to talk about 'a domain of constructions,' and he has suggested that 'the key to understanding its historical development,' would therefore 'seem to lie in those constructive activities that produced it' (1994: 2). But it seems to me that another interesting way to think about the claims of a scientific – and scientistic – psychology might emerge from thinking about what such claims are actually trying to do, positively. What I want to focus on, here, is why my participants actually make these claims in the first place. Rose (1996a) has argued that we cannot disaggregate the way that psychologists want to represent their history, from what is possible or desirable within a psychological rubric at any given moment – in other words, that there are important and functional relations between the kinds of psychological knowledge that hold sway, the scientific histories with which those knowledges seek affinity, and the sorts of things and people that those knowledges help to bring into existence. This is what Rose, following Lecourt (1975) and Canguilhem calls the recurrent nature of the history of psychology – an ever-spiralling move in which the past invariably helps to 'demarcate that regime of truth which is contemporary for a discipline' (1996a: 42-3). The history of psychology is therefore neither artifice nor lie – it is rather a methodology, one that enables its practitioners 'to police the present, but also to shape the future' (ibid).

My concern in what follows will be with the same endlessly recursive move, between history and the present – between science 'now,' and enchantment 'then.'

Using this frame, I want to focus on the way that, within my sample, science-talk is also border-talk. And if these claims to science are not so much ways to 'police' the present, specifically, they are at least ways to think about the disciplinary consequences of tracing a neurobiological account of autism across so many different boundary-points. I want to argue that one boundary-point that these claims are specifically concerned with is the re-emergence of that ontological and epistemic grab-bag of which a scientific psychopathology has been long trying to rid itself (a few interludes aside) – and this is the etiological and diagnostic entanglement, within studies of psychopathology, of human social life.

The Witch's Brew

As a way into this, let me return to one of the interviewees I quoted above – it's the charismatic and youngish professor (SS03) who was keen to emphasise the pissing/shitting/eating animality of human experience, and of the necessity of understanding this experience biologically. Within the course of the same monologue, though, there is, albeit quite briefly, another vision of psychology in play:

> Psychology is about the behaviour of the individual, and the individual in relation [*here he catches himself a bit*] – so, I mean that covers everything that I do, but I don't say that everything can be reduced to brain, even though I do brain stuff. But, uh, psychology is at very interesting cross-roads between systemic, societal, emergent explanations of why somebody does things.

This is interesting because my interview with him does not at all read like an interview with a man at the cross-roads of anything. And yet here emerges a rather different vision of psychology – a discipline whose objects are not especially given to a biological approach, but which also have 'systemic,' 'societal' and 'emergent' properties that need to be understood. Re-reading my interview with him now, in fact, he seems a bit back-and forth on this question. A bit later in our conversation, he used the analogy of a broken computer to differentiate between the ways that a sociologist and a psychologist would individually approach a problem. 'I mean, the computer is a physical thing,' he said,

> which has particular properties which are determined by its structure. And an understanding of that structure in a sense determines all the kinds of things right up from, you know, trying to understand the impact of the internet on society or something. The internet is partly the way it is because of the social structures

> that make it possible. But it's also partly the way it is because of the strengths and weaknesses and characteristics of what it is made of.

Again, this might be read, as I originally read it, as a biologically reductive claim about what psychology should be. But here, again, there is a sense of something else in play, something that slightly troubles the account of the computer as an object with 'particular properties…determined by its structure' – and this is the need to expand the definition of what qualifies as a biologically determinist account of some thing, such that this account might now include 'the social structures that make it possible.' But the tricky part – and here is the 'cross-roads' he mentioned above – is including these elements such that a psychological account would be no less biological, and certainly no less (even very reductively defined) *scientific*. Perhaps this mention of 'society' was also a concession for my benefit. But I came across similar expressions quite a few times. In one case, a psychiatrist (L09) told me that the sheer amount of discussion and debate around the categories within DSM-V made many of his colleagues still 'fee[l] like it's in the realm of social psychiatry.' A social neuroscientist (L03) discussing some of the particular difficulties of her own special interest said that that 'in social cognition, the more you try to control the [experimental] situation, the less you end up studying social cognition because social cognition has to be very, very flexible.' Elsewhere, a young psychologist (R11) spoke about her dual training in psychology and sociology, saying:

> I was very interested in the brain, and I guess its functioning in isolation from social groups. [But] actually it's even now quite difficult for me to think about there being a stark boundary between psychology and sociology. Because I think now a lot of what we know about human, about, um, psychological processing, is strongly influenced by being in a group or not being a group. And so, it's not very easy to draw a sharp boundary between them.

This relatively recent revelation ('I think now') of the difficulties of drawing a 'sharp boundary' between the object of a 'psychology,' and the proper concern of a 'sociology,' is precisely what I am trying to finally get to here. What is common to all of the contributions that I just cited – and this, whether for good (as in social cognition, a burgeoning field premised on it), or for ill (in psychiatry, a medical specialism still grappling for organic respectability) – is a re-emerging sense that, whatever the claims that 'big science' has made on 'soft psychology,' a lot of research on the neurobiological underpinnings of psychopathology is still not so easily disentangled from the complex machinations of the social world.

Above, I suggested that we might read a partial history of nineteenth- and twentieth-century psychologies as a series of changes and developments united by a particular claim, and this made for each of these developments in turn, that *now* psychology had become scientific – and that it had become so in great contrast to the mystifications of the era just passed.[24] Although there is much more that might be said about the intricacies of these histories, there is nonetheless a sense in which we can trace a thread of claims to 'science' from the creation of Wundt's laboratory, to the rise of personality testing, to the emergence of behaviourism, to the recruitment of psychology to the new brain sciences.

What unites these claims, moreover, is that, whether based on the ability to locate psychological phenomena in the laboratory, or the capacity to quantify these phenomena, or the tools to use them for averaging and prediction – the pull that they exert on psychological classifications and diagnoses tends *away* from the social context in which the individual finds herself. As Danziger points out, throughout the twentieth century, much institutionalised, mainstream psychological research had gradually found itself in agreement with the late-capitalist notion of an 'independent individual for whose encapsulated qualities all social relations are external' (1994: 296) What we begin to see in the extracts just quoted, however, is that just as genetic and brain-imaging technologies have emerged that made it possible to get some measure of the organic substrates of these now well-described, externalised, and individualized phenomena – it has become apparent, even within biologically reductive areas of psychology and psychiatry, that looking at a diagnosis, or a brain, or even a lone synaptic connection, independently of the context in which it has emerged, may not actually be adequate for locating or describing many categories of psychopathology. In other words: at the beginning of the twenty-first century, and even within self-consciously biological and reductive approaches in psychology and psychiatry, it seems now increasingly difficult *not* to bring the social back in.

This recognition is embroiled in a number of important recent developments, and I can only roughly gesture at them here. For example, critical in the post-

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[24] I call this history partial because of course my account (here, and following) ignores important developments that either countered or ran tangential to this story. Not the least of these is the mid-century dominance of psychoanalysis in many spaces, which has had, and indeed still has, its own, rather singular, investments in psychology, science, and history. See e.g. Woody and Philips (1995), Lakoff (2005) or Stepansky (2009) for accounts. The complex histories of 'social psychology' and 'social psychiatry' would also require a different kind of analysis – see Danziger (1992), Rose (2007b) or Staub (2011).

genomic era, have been the dissolution of the 'gene-for' paradigm, the growing concern with gene-environment interactions, and, in particular, the emergence of an epigenetic understanding of psychiatric and psychological problems – which has involved researchers 'coming to accept that DNA sequences alone do not comprise the master plan of organic existence' (Rose, 2007a: 47; *cf.* Rutter, 2005). Epigenetics describes heritable changes in gene expression caused by something other than DNA – frequently including 'environmental' factors such as parental grooming of an infant (Weaver *et al.*, 2004). Without getting into significant detail, I want to draw attention to the sense in which such an understanding must recruit a complex and unpredictable environment to discussions of psychiatric and psychological pathogenesis – and must do so to the extent that, quite suddenly, the social is at stake *also* within self-consciously scientific discussions of mental distress. As Niewöhner has shown,

> epigenetics forces biologists to think about genomes in context…context here is not understood within a reductionist mode of thinking that reduces other levels of analysis to feeding into the baseline of DNA sequence. Rather the approach is systemic focusing on the multiple interactions between different levels of analysis' (2011: 285).

Steven Hyman, among others, has written about one of the most famous examples of this process – the effects of 'childhood adversity' on the way that genes are expressed in adult behaviour. This is one example of how, as Hymans put it, a person's social context can 'get under the skin' (2009). Looking at a recent study that suggested a relationship between childhood grooming and later responses to stress in rat pups, Hyman argued that

> the frontier [in psychopathology] lies in understanding the mechanisms by which environmental factors (whether experiential, metabolic, microbiological or pharmacologic) interact with the genome to influence brain development and to produce diverse forms of neural plasticity over the lifetime…the *experience* of rats is transduced into long-lived *molecular adaptations* that influence adult behaviour (2009: 241, my emphasis).

Of course there were other moments in twentieth-century psychology and psychiatry to which this would not come as news. But my interest, here, is in the inescapability of such understandings, now, within a vision of psychology and psychiatry that, if it might not be fairly called 'biologically reductive,' has at least aligned itself with a quantitative, physics-based 'big science' of brain scans and biomarkers. A similar conceptual move can be seen in gene-environment-interaction research, such as the now-famous studies of Avshalom Caspi, Terrie Moffitt and their colleagues (Caspi *et al.*, 2002; Caspi and Moffitt, 2006), which have similarly spun the social context quite directly into the basic molecular structure of psychiatric distress. Caspi and Moffitt have gone on to argue quite forcefully and widely for the role of gene-environment interactions in psychiatry. 'Many scientists have despaired of the search for a straightforward association between genotype and diagnosis,' they argued in 2006 – pointing out that this has led to a breakdown in the straightforward assumption that 'genes cause disorder' (Caspi and Moffit, 2006: 583). Now, however, 'the gene-environment interaction approach assumes that *environmental pathogens* cause disorder and that genes influence susceptibility to pathogens' (ibid., my emphasis). Thus, there has been a self-conscious move towards thinking social entanglement, not only within the remnants of, for instance psychoanalysis and social psychiatry, but also within specifically neuroscientific and genetic research on psychopathology – now facing a 'witches' brew,' of biology and environment, as Arribas-Ayllon and his colleagues put it (2012; this is, needless to say, an interestingly gendered descriptor). But I also want to stress that what makes up this brew is not some thin, impoverished 'ecological' view of immediate environmental inputs. The 'social under the skin' is precisely 'the social' as most sociologists would understand the term, i.e. a series of complex structuring webs of social interaction and culture. For example, in a wide-ranging review of gene-environment-interaction research on emotional and behavioural problems, Michael Rutter and Judy Silberg look to 'the operation of racial discrimination, availability of guns, local authority housing policies, availability of family planning, and schooling, to mention just a few examples' (2002: 478).

There is, of course, much more that could be said here. But even this quick account might illuminate what is going on in the empirical material with which I began: I want to argue that these still relatively recent understandings are deeply present, in all sorts of ways, in my discussions with biologically-focused, big-science-minded neuropsychologists and neuropsychiatrists. Consider this interview extract, which I quote at some length, because I think it well-illustrates the kind of shifting complexity with which these scientists are grappling. It comes from someone (3S02) who had been involved in funding a project to see how far back in infancy an autism diagnosis might be pushed. The answer, as it turned out, was: not so early.

‘One of the things that’s coming out of the [research project]’ she said ‘this is kind of a relatively new finding, the first thing they found…

> they’re tracking these children over time, was that they expected them to fall into one of two groups, because they’re doing similar work with children who are at risk of autism, so they had some notion of what their control group looks like – and they thought that their baby siblings would either look like the control children, or they would look as though they they’re going to head down the autistic route. But what they found, as a group, they sat somewhere in the middle. They were neither like the control children, nor did they display early signs of autism, but they did develop differently to other children, to control-group children, to typically-developing children. And then what they’re finding now is that some people sort of start to head off towards the autistic side, and then veer back again. Um, and they obviously carry, in many cases, the same genetic risk as the sibling that has already got a diagnosis of autism – so some sort of protective factors are coming into play, we don’t know what they are, but it’s actually – the original notion was that you would be able to diagnose autism much earlier, and you’d be able to start intervening, and so on. It’s now starting to look as though, actually, three [years old] is it.

In other words, and at the very cutting-edge of autism-diagnosis research in 2012, this laboratory has run into the limits of what it can deduce from biological information alone: kids with the same genetic load don’t just take different paths; they even veer off these paths, and then sometimes come back again. And no one really knows why. As another lecturer (L04) put it – it’s ‘a dynamic process, and that dynamic process is really under-considered in developmental research generally, and particularly when you think about developmental disorders.’ What this clearly indicates is the degree to which, within the last few years, what Hyman (2009) aptly calls ‘the frontier’ of neuropsychological and neuropsychiatric research has shifted, from being the province of a basically organic and biological science, to a practice that must now, and without great training, begin to take account of shifting movements between biology, and things like experience, individual biography, family relationships, social context, and so on. As Rayna Rapp puts it, ‘an appreciation of complexity and nondeterminism,’ within developmental psychology, ‘has replaced an older enthusiasm for the deterministic one-way rules’ (2010: 66)[25].

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[25] Here we may also wish to acknowledge the role of a critical psychology in arguing for just such a nondeterminism over many decades previous – see e.g. Burman (1994).

I offer this brief account, here, because I want to argue that this re-emergence of society, culture and context, even into a self-consciously biological consideration of psychopathology, is among the most salient emerging layers that make up the broad field in which my interviewees were operating. My suggestion is that this emergence, still relatively recent, forms a crucial part of the epistemological ground on which my neuroscientific interviewees were standing. Note that I am not at all arguing that there is anything 'unscientific' about an attempt to think the relationships between, for example, brain development and family context. Nor do I claim that scientific rigour, reduction and technique are defeated by a recognition of the generative role of social relationships within the formation and experience of mental distress. The literature discussed above does, in fact, a good job of drawing rigorously scientific data from the psychogenic swirl of the social, the familial, and the contextual. But my interviewees – neuroscientists, brain-imagers, and biologists – are also part of a latter development, that nicely described by Chris Frith as the defeat, within psychology, of the softly subjective by the bigly scientific. And it is this move, Frith claims, that makes a *science* of psychology. My argument is that, as it becomes increasingly apparent that this may be a more complex procedure than first suspected, so do these renewed claims to 'science' begin to look rather shaky. Within the lifetime of my interviewees' intellectual careers, a rigorously scientific approach to the kinds of things they were researching had gone quite concretely into gene-sequencing and brain-imaging laboratories, and into the isolated body of the individuals concerned – but it had then, and surely in quite an unexpected development, found itself having to once again account for the environment, and context, and social life. Suddenly, entirely respectable neurobiological review papers, from major authorities on developmental disorder, were talking about racial discrimination, and local authority housing policy.

It is as a relation to *this* moment that I am locating these rather lurching insistences on 'science.' If it is true, as suggested above, that claims to a mystical past within psychology have often been attempts to mark what might count as truth in the discipline's present – then I also want to claim that these invocations of 'science' within my interviews, and the very clear and unambiguous demarcations drawn between my interviewees' own work and some enchanted pre-modern other, may signify an attempt to do some very similar boundary-work. As the neurobiological underpinnings of psychopathology are increasingly located across disciplinary and epistemic borders – fMRI studies on the one hand; analyses of racism in housing policy, on the other – so does it become more necessary for practitioners to think about where, exactly, their own work is situated. And this is not because they have a crude or reductive view of what gets to count as 'science' – but because they are tracing the neurobiology of autism across the border between the 'big science' commitments of an imaging neuroscience, and the awkward, hard-to-read sociality of so many psychological and psychiatric diagnoses. The insistence on 'science,' and not 'Freud' or 'faff,' within these interviews is a way of talking about, and picking a way through, these subtly-shifting borderlands; they provide a language for my interviewees to re-think, and re-negotiate the limits between psychological practices that may be safely called 'scientific,' and some swirling, uncertain sense of social context.

Falling in the middle

My suspicion is that any project that interviewed roughly the same numbers of psychological researchers as this would find, whatever the topic, some element of the same conundrum – i.e. the presence of both a rich and critical awareness of the mutability of epistemological boundaries, but also a slightly lurching affinity with some more simply-construed 'natural science.' And yet I want to suggest, nonetheless, that the presence of autism is unlikely to be entirely incidental here. Throughout the course of this project, I have been deeply interested in the on-going recruitment of 'the social' into accounts of autism: through case descriptions (Sacks, 1985), diagnostic manuals (APA, 2000), familial accounts (Claiborne Park, 1982 [1967]) and autistic autobiographies (Grandin, 2005), it often seems impossible to talk about autism, as either a diagnosis or an experience, without *also* talking about the specific understandings, meanings, and sensations that mediate some person's social environment. In this final section, I want to expand the discussion that I began in the previous, to propose that there is something distinctive about the socially-salient qualities and experiences of autism.

The following interview extract comes from a lecturer (L05), with a broad interest in neurodevelopmental disorders. 'Autism's not just about brain development *per se*,' she said,

> Obviously the environment affects brain development, and we need to figure out what environmental factors might actually impinge on children's development, in a positive way. And one of those might actually be social

> interaction in the classroom, or outside the classroom. And there are other things, like family…family structure. There's lots of studies in typical kids showing that…um, having a sibling improves theory of mind skills. Because you just interact with the sibling, and you talk more, and you talk about other minds, kind of thing. So does it do the same for kids with autism? Or: what kind of…those are important things to answer.

Here, we begin to get a sense of the tricky back-and-forth between a brain-based account of autism, and the on-going impact of the social environment – both on what that autism eventually looks or feels like, and on whether or not it is even diagnosable. Later, she said:

> The possible focus on biology is that it's deterministic, and I think we have to be careful about that – because it's not deterministic […] there's not one path, you know, if you've got autism, you don't necessarily go, in fact we have no idea which path you'll necessarily go down. It's not just the case…there are ways in which the environment can modify one's autism. I just don't think we understand what those conditions are, at the moment.

What I am trying to draw attention to, here, is precisely this quality of things not being understood about autism, specifically, because of its complex entanglement (both etiologically and diagnostically) with social interaction and the environment. As much as the emerging science of epigenetics and gene-environment interactions are troubling psychological researchers in general, it does seem that autism forms a particularly knotty problem.

It is true, of course, that something very similar might be said about many psychological and psychiatric diagnoses – and particularly about the subset of those diagnoses explicitly recognised as 'developmental.' (We might also recognise that 'the social' is very such at stake in, for example, schizophrenia. See Littlewood and Lipsedge, 1982; Corcoran *et al.*, 1995). I will show below, the particular accounts of 'autistic culture' also mark out something distinctive). But autism has a long tradition of discursive and clinical references to the presence, nature and meaning of 'the social,' as well as to a kind of autistic 'culture' or 'planet' which may have its own account of these qualities. My suggestion is that if claims to 'science' in psychology may be read as attempts to negotiate that discipline's troubled epistemic boundaries, and in particular the incursion of some aspects of 'the social' into the generation and formation some kinds of psychopathology, then studies of autism may be particularly potent spaces for this negotiation.

I intend this in two senses: in one sense, we can say that more than any other diagnosis, the social has always been what is actually in question in autism – that it is a disorder, more than anything else, of social interaction. Leo Kanner's (1968 [1943]) early descriptions are the obvious guide here: describing his very first patient, for example, Kanner notes that 'it was observed at an early time that [Donald] was happiest when left alone, almost never cried to go with his mother, did not seem to notice his father's homecomings, and was indifferent to visiting relatives' (1968 [1943]: 218). Kanner's second case Frederick W., was described as having 'always been self-sufficient…

> I could leave him alone and he'd entertain himself very happily, walking around, singing…when we had guests he just wouldn't pay any attention. He looked curiously at small children, and then would go off all alone. He acted as if people weren't there at all, even with his own grandparents' (ibid.: 222-223).

Read through the prism of 2012, Kanner's shrewd, self-aware, and affecting article (he describes how one little boy, having been given up by his parents to a foster home, uttered his first words – "good night" – to a staff nurse) reads like a pantheon of autism's ghosts: the busy father, the college graduate mother, the strange eating habits, the gifts for memory, the regressions, the false dawns, the silences – so it goes on. But above all, what unites these children, what convinces Kanner that he is dealing with a phenomenon that 'differs…markedly and uniquely from anything reported so far,' was, precisely, and solely, the preponderance of a specifically *social* alterity: 'the outstanding, "pathognomonic," fundamental disorder,' Kanner concludes,

> is the children's *inability to relate themselves* in the ordinary way to people and situations from the beginning of life…there is, from the start, an *extreme autistic aloneness* that, whenever possible, disregards, ignores, shuts out anything that comes to the child from outside' (ibid.: 242, emphases in original).

Amid the diagnostic and etiological fluctuations of the decades that followed, this cardinal feature never left autism. DSM-IV gives the primary symptom as a 'qualitative impairment in social interaction' (APA, 2000: 75). Meanwhile, the proposed changes to DSM-V collapsing the communicative and interactive deficits into one category, makes autism, if anything, even more of a socially-defined condition than it had been previously (APA, 2011). For a disorder principally famous for its diagnostic and phenotypic heterogeneity, this consistency is really remarkable: albeit, 'several other developmental disabilities typically accompany autism's social dysfunction,' wrote Ami Klin, in 2002, '…*the core social disorder defines the condition*' (Klin *et al.*, 2002: 895. My emphasis). So, to think about autism has always been to think about the social. Put otherwise, autism research has long been a something of a boundary-point for the frontiers of psychology and context, of individual biology and nurturing environment. It is, perhaps, no coincidence that one of the most popularly successful psychoanalytic accounts of a disorder, Bruno Bettelheim's *The Empty Fortress* (1967), was a book about autism.

But this relationship can also be inflected in a different way – and here we need to acknowledge that autism has also been a testing-ground for exploring what gets to count as social in the first place. This reflects the degree to which, within autism research, we find a notably cosmopolitan discussion of sociality-in-the-first-place. Here, I am thinking, for example, of longstanding claims to an autistic culture. And I am not only referring to well-known political attempts to stake this claim, such as Judy Singer's (1999) smart deployment of the word 'neurodiversity', or Amanda Baggs' (2007) complex, youtube-d elaboration of the subtly-sensed and tactile grammar of her social world. I am also referring to less obviously self-aware accounts that manage to unfold the same sense of a distinct autistic cultural presence. I am thinking, for example, of Oliver Sacks' well-known encounter with Temple Grandin, *An anthropologist on Mars* (1995) – the striking title of which (and this the best thing about it) comes from Grandin's description of herself trying to make her way in an 'alien' culture. That same sense of autism-as-cultural-difference is echoed in Chloe Silverman's description of social-scientific writings about autism as comprising 'fieldwork on another planet' (2008: 325), and in Ian Hacking's identification of the specific difficulty faced by parents raising an autistic child, *viz.* that 'your child is an alien' (2006: 3). In the opening chapter of her memoir, the autistic primatologist, Dawn Prince-Hughes, vividly describes herself as inhabiting a 'culture of one' – a culture that, she makes clear, is much more attuned to the thrills, pains and rituals of gorilla life, than it is to the social niceties of human zookeepers and small-talk (2005: 11-15). Or as the autistic author, Lucy Blackman, has similarly put it:

> it may be that the social deficits which are the cornerstone of an autism spectrum diagnosis tell us far more about the person who made them markers for such a diagnosis than about then child whom he observes. I realise that social life and affections are essential for being human, but I still wonder whether the "me" factor is properly understood. That is, the whole

> testing procedure is somehow actually constructed on whether the tester observed the person to socialise in a way the tester understood to be socialisation (2005: 149).

What these kind of descriptions and claims begin to show is that not only has autism long been a disorder of the social – but, as interpreters of both a diagnosis and a lived experience, autistic people have consistently challenged those who would do research with them to *re-think* both what gets called 'social,' and where it can be sought. So autism has not only been *about* the social – it has been much more intricately entangled in what gets to count *as* social in the first place (Davidson, 2008; Grinker, 2010; Ochs and Solomon, 2010). Here, I think, the particular difficulties and border-sensitivities of the psychological researchers in my sample become a bit clearer.

This sense of autism's particularity for psychological research, and the degree to which that particularity might contribute to the story that I am trying to tell here, was brought home to me by another interviewee's (R12) use of an analogy with ADHD, and the single-gene disorder, Phenylketonuria [PKU]. 'So, PKU,' she said,

> which I just mentioned, this genetic disorder, it tends to be thought of as a biological disorder, because it has a single biological mutation, and everybody who has the disorder has that single mutation. Whereas ADHD is generally thought of as a behavioural disorder, because everybody admits that this is just a group of people who have been gathered together, who all have sort of some attention problems and some hyperactivity problems, but there's basically nothing that draws those kids together at the cognitive or biological levels…Autism tends to be the one that falls in the middle

It is precisely this sense of autism research 'fall[ing] in the middle' that I am trying to draw out in this discussion. Even if autism is among the current paradigmatic organic brain disorders of social interaction, still no one is quite sure what counts as social interaction – which is important because, actually, that interaction is going to loop back to affect the organic brain. I want to say that it is within precisely this kind of *milieu*, and especially within this sense of 'falling in the middle', that my interviewees experience a desire to more carefully (and even assertively) negotiate the boundary between scientific confidence and social uncertainty. It is not only the case that these otherwise sensitive and ambiguous neuropsychologists want to be taken seriously as hard, reductive, objective scientists. It is also that the disciplinary and epistemic border-crossings of a carefully-traced neurobiology of autism require some particular attention to these limits. Thus, the invocations that I discussed in the first section are not so much simple, self-interested claims to the status of science; they are expressions of the ways that neurobiological research, and, in particular, neurobiological research on autism, require some attention to the on-going labour of social and scientific bordering.

I want to close this section with one final extract from my interviews. It comes from an interviewee (R03) who was more expert on the complexity and valence of these things than any other – because she was both a researcher on autism, and the mother of an autistic child. Where this extract picks the conversation up, we had been talking about psychoanalysis, and its infamous 'parent-blaming' history in autism. I brought up a mouse study that I had been recently reading (Mines *et al.*, 2010) – which, it seemed to me, and in a way that I did not quite understand, was starting to somehow bring the role of the parent back in. 'The parent-child is a very sexy subject at the moment,' she said

> and you know it tries to sort of steer a clear course, but it's difficult. It's a tricky one. It's a tricky one. […] It's a fine line, I think, but it's interesting that it's coming back to that.

I asked her how she felt about this return, as both a researcher and a parent.
'Emm…' she said – and, after a long pause, laughed:

> I have issues with it, I do have slight issues. And, you know, it's interesting – I've sat in meetings where we've discussed genes and, you know, where I've looked around the table and, you know, people have been saying 'well, you know, and clearly it's not surprising that parents would have an input because the parents themselves are probably slightly autistic, or at least broader autism phenotype kind of thing.' Um, and I've kind of sat there at the table thinking 'actually, you know, I think I'm probably more socially adept than most people around this table' [*both laugh*]. But, you know, I won't say anything. So, I find myself being in compromising positions, every now and then – surprisingly infrequently actually, given the position I have. But, em, I do find that quite…quite difficult, um…And because I fundamentally, going to the original question, I do fundamentally believe that it is a neurological thing. And, you know, even just the way that it happened with my son, there was clearly this kind of window, there was this point at which something is happening developmentally. Because he was apparently fine, and then something happened. And I don't believe it's because he had a vaccine, or anything like that – it was something, you know, there was a point in development at which

> something, you know, he went one way, and most children go another way, and, you know, I feel that's very clear.

Caught both personally and professionally between 'the parent-child' relationship and 'something happened,' this scientist and mother exemplifies the delicate crossings that characterise this space, as well as the rhetorical, practical and affective labour required to position oneself among these crossings. Working between medical objects and neurological change, on the one hand, and children's lives, and parental love, on the other, she knows, with particular acuity, the 'compromising position' inhabited by so many neurobiological researchers on autism. My argument in this chapter is that what look like a series of crude and simplistic statements about science, might perhaps be better heard as some early ways to think and talk about precisely these kinds of compromises. They might be heard as a way of moving between the on-going, generative loops of familial and social life, on the one hand, and the 'hard' science of individualized brain biology, on the other. This complexity, for the autism neuroscientist, is characterised, first, by the 'witch's brew' of genetic and environmental inheritance within which all human action has to be interpreted. But it is also well represented by the longstanding and richly 'social' nature of autism, which not only names a disorder of interaction and communication, but also marks a demand for particular forms of sociality to be recognised and valorised in the first place. This last interviewee points out that emerging forms of ('sexy') research are actually re-articulating, in new forms, many of the hard social and scientific border-crossings that have entangled autism research for so many decades – and they are doing so precisely at the moment when many of these researchers though they might be able, finally, to move on.

Conclusion

I began this chapter by saying that within both the personal memories and institutional self-descriptions of my interviewees, there were a lot of oddly simplistic claims about the relationship of psychology to science – odd because they came from the same cohort who, throughout many hours of conversation, had shown themselves to be (and, indeed, taught me how to be) sharp thinkers about both psychology *and* science, and particularly so when these two get conflated within a convergent 'cognitive neuroscience.' This made me resistant to taking these claims about science at face value – and I found a platform for thinking about them in the image of a 'recurrent' history. This suggested that the best way to understand these kinds of accounts might be neither to take them simply as-was, nor to 'deconstruct' them – but instead to interpret them as ways of thinking about boundaries, and especially about the kinds of epistemic and disciplinary boundary-work implicit in a tracing neuroscience of autism.

Taking this view, it seemed to me that one coherent account that we might make of these insistences – upon biology, physiology, natural science, quantification, neurons, and so on – was one that they bespoke a strong concern about an ambiguously traced autism being taken, nonetheless, as a properly scientific pursuit. I argued that we might begin to see the salience and the purpose of such claims within a circumstance that has historically troubled psychology's claim to the status of science, *viz.* the inseparability of its objects from some social context. And I suggested that the complex entanglements of any meaningful neurobiological account of autism – caught between psychology, neuroscience, social interaction, and family life – make it a particularly potent area, for throwing into question the borders between 'psychology,' 'science' and 'the social.' Again: this not to say that 'the social' cannot be scientific, nor that that a properly scientific psychology cannot work itself through the contingencies of culture, environment, politics, and so on. It is to say that, for some scientists, like my interviewees, who ceaselessly trace research objects across all of these areas – that there may be some moments of tense, and even unsubtle, negotiation, about just where, exactly, autism neuroscience is situated in relation to all three. It is precisely the beginning of these negotiations that I hear in some of my interviewees' insistence on a slightly crude and reductive image of psychology as (now) a biological science.

The truth, of course, is that neither psychology nor psychiatry has ever been unambiguously associated with the natural sciences. Or at least, to the extent that either can now legitimately make such a claim, this has only been a recent development (Rose, 1985, 1996a; Luhrmann, 2001). The strong temptation, then, is to write another account of these kinds of claims, one that paints them as another attempt by psychologists or psychiatrists to ward off the social and cultural flakiness of their research objects. We might, in this vein, propose the opening of another unfortunate chapter in these disciplines' long-running formation of a scientific imaginary. But if this thesis has been about anything, it has been about the impossibility of getting any meaningful analytic purchase on this area if we enact the kind of rigid cut that such an argument would demand. So I want to conclude by emphasising that this is not a story about externality to science. Instead, it is a story about the shifting borders and taxonomies of what gets called 'scientific' within this century's study of psychopathology; it is a story about the ontological complexity of the world that is being named and organised under the rubric of such a study; and it is a story about some unstable intellectual and disciplinary spaces that seem to demand more fluid and open relations (in practice, if not always articulated as such) to epistemology, disciplinarity, science, and the social.

One of the main things I learned from being around autism neuroscientists is the delicacy with which an unequivocally scientific and 'given' object might *still* be traceable within an unstable and contingent world of social interaction, communication, politics, and so on. What I am trying to gesture at here, then, is the fact that what these scientistic insistences finally add up to is not a veil that covers psychology's inadequacy, nor is it an instance of psychology's blindness to its own shortcomings. What they may actually amount to, in the end, is a window onto a particular moment in the history of psychology – and this is a moment in which a specifically *scientific* account of psychology's object has, for reasons that are complex and multi-faceted, but without much warning in any case, become strangely allied to a candid methodological and conceptual entanglement with the social. I cannot stress enough: it is not at all my point to 'deconstruct' the scientific claims of a contemporary psychology, or to say that, whatever its pretensions to the contrary, psychology is *still* not a real science. The strange thing is that the opposite is probably true: the simultaneity of a contemporary psychology's claim to science *and* its investment in the social are not problems *for*, but in fact elements *of*, one another. This is an observation that will also be relevant in the next chapter.

6. *There's a unicorn – working through difference and distinction in the neuroscience of autism*

Introduction

Throughout the previous four chapters, I have described and analysed some of the tensions and differences that structure a neurobiological account of autism. These differences have been largely located in the spaces of practice and epistemology in neuroscience – finding expression in the strange and shifting simultaneity of, for example, emotional labour and laboratory work, of biological truth and diagnostic pragmatism, and so on. But I repeatedly took the view that the consistency of such tensions and differences, within neuroscientific autism research, does not bear any kind of negative relationship to the reality of autism, or to the efficacy of neuroscience, as such. I argued, indeed, quite the opposite – which is to say that that these complex and overlapping entanglements of science, neurobiology, and experience are all deeply entangled with 'what autism is,' and also signs of a normal neuroscience of complex developmental disorder. In other words, when I pointed to the presence of tensions and differences within autism neuroscience, it has been on the basis that the stable and singular objects of the life sciences are surely relational entities anyway, that relationality does not have to stand in an antagonistic relationship to distinction, and that these relations may well begin as relations of difference. As Karen Barad has it: 'the primary ontological unit is not independent objects with independently determinate boundaries, but rather…*phenomena* [which] are the *ontological* inseparability of agentially interacting components' (2007: 33, emphasis in original). As discussed in Chapter Three, this view is at the heart of Barad's 'agential realism,' which insists that relations, and sometimes difficult entanglements, surely come in *advance* of the active potential of individual scientific objects. Following Barad, I have worked through this thesis on the basis that the presence of delicate webs of entanglement and difference might be more an argument for identifying coherence and stability within both autism and neuroscience, rather than presenting an entry-point for critique.

More specifically, my suggestion has been that the preponderance of differences, multiplicities, and ambiguities that I have been describing, up to now, are less signs of the increasing *instability* of autism as a neuroscientific object, but may actually provide some insight into the ways that a more-or-less coherent autism is carefully *traced together* by the various objects and agencies that make up the new brain sciences. In this final chapter, I want to put some more flesh on this claim: at the heart of the chapter is an attempt to think through two more descriptions of autism and neuroscience that came up again and again during my interviews. The first of these is a constant reference to autism as quite a dispersed and disaggregated phenomenon. In particular, here, I will focus on interviewees' claims that autism can only be understood from multiple 'dimensions,' or via very different 'levels of understanding' (such as genes, cognition, behaviours, brain anatomy, somatosensory thresholds, time, the environment, life-experience, and so on), and also that these different levels do not connect with each other in an obvious or straightforward way. The second description was made up of a related series of metaphors for autism neuroscience which interviewees often drew upon when describing their scientific practice to me. These were metaphors that positioned neuroscientific research on autism as a practice of 'shuffling,' 'connecting,' 'assembling,' 'tangling,' or – this I took as a guiding image for the thesis as a whole – 'tracing.' Situating these clams more precisely, my suggestion now is that these metaphors of 'tracing' and 'connecting' are ways of thinking through an emerging functional relationship between the multiple levels on which the objects of neuroscience are manifested, the combinatory logics of the new brain sciences as they work *across* these levels, and the 'contingent stability' of something like the autism spectrum in between (Haraway, 1997: 267).

In alliance with Karen Barad's claim for interaction and difference as conditions of coherence and stability within technoscience, this argument is also indebted to the ways of thinking about scientific *fact* that scholars including Donna Haraway and Bruno Latour have lately pursued. For Latour, if we are accustomed to thinking of scientific facts – like the neurological basis of autism spectrum disorders – as things that are 'indisputable, obstinate, simply there,' then we need to instead move to thinking of 'matters of fact' as more 'matters of concern,' or that which needs to be '*liked,* appreciated, tasted, experimented upon, mounted, prepared, put to the test' (2008: 39, emphasis in original). For Latour, this shift to 'concern' is not at all a denial of the indisputability and obstinacy of fact – as he made clear in a much-cited *mea culpa* (2004) – but a way of describing how facts come to be facts. 'The question,' he points out, 'was never to get *away* from facts but *closer* to them, not fighting empiricism but, on the contrary, renewing empiricism' (2004: 231).

Shifting to 'concern' and to 'states of affairs,' as a way of learning how to believe in facts again, is the essence of Latour's 'second empiricism' (ibid.: 232).

Donna Haraway, perhaps rightly refusing to share Latour's guilt, is still happy to talk about 'matters of fact' – but, for Haraway, the fact is only ever instantiated as 'a crucial point of contingent stability for possible sociotechnical orders, attested by collective, networked, situated practices of witnessing' (1997: 267). For Latour's 'second empiricism,' Haraway offers commitment to a 'contingent stability,' and also to a 'figural realism,' both of which hold together geometrical and rhetorical logics within the formation of scientific facts – thus unfolding a contingent-yet-stable framework of practice, in which the relational 'constructedness' of technoscience's hybrids is 'not in opposition to their reality,' rather being 'the condition of their reality,' and even 'fast becoming the sign of reality as such' (1997: 267, 11, 120). In what follows, I too am trying to think about the emerging facticity of the neurobiological account of autism. In particular, I am trying to think about the relationship of this facticity to neuroscientific logics of collectivity, network, relation, mutual appreciation, and so on. As the chapter progresses, I will particularly lean on Latour and Haraway in my insistence that finding an autism that exists only at different levels which do not easily relate to one another, and then thinking with a neuroscientific practice that works to trace a scientific object *across* these levels, is not to diminish either autism or neuroscience.

Holding all of this together is quite a straightforward three-part narrative of autism's 'contingent stability' in neuroscientific research. The first part looks again at the on-going difficulty of making a singular autism cohere. Here, I will focus in particular on my interviewees' discussions of 'cause,' and on the difficulty of locating any causal account of autism that stretches coherently across the 'multiple levels of understanding.' In the second section, however, I will show how interviewees, while concerned about this feature of autism, nonetheless insisted precisely on the capacity of neuroscience to work nimbly across different scales and levels. Here, I will begin my argument that the 'differences' we see in autism research are mirrored by a self-consciously variegated and connective neuroscientific research practice. Drawing on Donna Haraway's (1997) account of the 'modest witness,' I will relate this consideration to the question of 'who,' exactly, is at work in this space. In the final section, I will specify the kind of mechanism by which neuroscientists participate in helping to draw scientific objects and agencies through the different levels of their appearance – and this is where I will seek a more solid hold of metaphors of 'tracing,' 'shuffling,' and 'connecting.' Relying in particular on Bruno Latour's (1987) account of 'immutable mobiles,' I will expand the argument I have made in the thesis so far, to show how 'tracing,' specifically, might be a way of thinking about contemporary neuroscientific research more generally – a practice that is perhaps particularly visible within early analysis of a complex and shifting 'developmental' phenomenon like autism.

Sort of a multi-level thing

The empirical genesis of this chapter, and of my general recognition that to think autism is to think with an object that appears quite differently in various sites[26], is in my early attempts to locate what my interviewees took to be the *cause or causes* of autism. Authoritative accounts of the cause of autism, especially those written for a lay public, are often quite opaque, and will generally say that causes are currently not known, but that they are likely to lie in some combination of biological and environmental factors (see e.g. the 'Autism Fact Sheet' produced by the US National Institute of Neurological Disorders and Stroke [NINDS], 2012, or the 'Overview' on Autism and Asperger Syndrome produced by NHS Choices, 2012, in the UK). This noncommittal account is perhaps not surprising, given the serious cultural side-effects of some previous discussions that were at least interpreted as more firmly causal (such as those ventured by Bettelheim, 1967, or Wakefield *et al.,* 1998 – the latter since retracted). With this in mind, I was interested in how autism neuroscientists would talk about cause in an interview – and, in particular, how they would negotiate the awkward histories of the environment and the body within popular histories of autism causality.

One of the most interesting things about these discussions, however, was the degree to which my attempts to talk about 'cause,' were not answered with discussions of the ways that cause was not known, or with arguments either for or

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[26] There is some affinity, here, with Annemarie Mol's account of how multiply-real medical objects are 'enacted' in practice (2003: ix, 33). I do not draw extensively on Mol, here, because my interest is not really in the 'whether' or the 'how' of multiple and real medical objects. As I go on to show, my concern is rather with the way that neuroscientists talk through the connections between diagnostic entities that are understood and apprehended on different levels, and a research practice that is nonetheless (at least formally) in pursuit of some kind of identifiable singularity. Although there is a temptation to generalise, focusing on neuroscientists' shifting dynamics of contingency and stability limns these accounts more precisely than a broader claim for the multiplicity of medical reality (ibid.: 5).

against biological and environmental cause. Instead, discussions tended to circle around the insistence that cause would only ever be known by picking autism out across the 'multiple levels' on which the disorder existed, or by working through the different 'levels of understanding' within which it had to be appreciated. The idea was that there would likely never be a single cause found for autism, but that it might be shown how a big-enough subset of numerous, independent, contributing factors, across the levels of genes, environment, behaviour, anatomy and time (amongst others), would sometimes congeal into autism, in certain individuals, and for different reasons. 'I don't think it's going to happen that we will find a single cause of autism,' said a PhD student (P02),

> and I think research should….it shouldn't necessarily give up on trying to find a cause, at least a single cause, I mean finding, sort of, numerous different contributing factors is definitely useful, and I think it's definitely going to be more the case that there's lots of different contributing factors, which kind of come together, and that sort of manifests as autistic spectrum, or something like that

It's interesting, here, that the student attempts to be generous to specific causal accounts of autism – and she lends vague support to the idea that fellow researchers 'shouldn't necessarily give up on trying.' But even in the midst of trying to speak up for the search for 'a single cause,' she immediately begins talking about 'numerous different contributing factors, which kind of come together' – which is something quite different. And even this loose idea of things 'coming together' was, for others, a bit suspect. A lecturer (L01) said to me, in the midst of a similar conversation: 'I mean it's terribly easy to think about this neat causal chain. But I don't think it's like that. And I think it's different in various cases.' Also resisting the idea that things might come together in any regular or predictable way, a senior researcher (SS09) said:

> [A final causal explanation of autism is] probably going to be a sort of a multi-level thing - so genetically you'll be able to describe the different gene variants that can contribute to it, but to me the final causal pathway if you want to use that sort of language is most likely to be a neurodevelopmental story […] we already know there are a lot of factors that can lead to this causal change, so genetic, environmental and so forth, so it's not going to be a simple causal story – you will have multiple different possible factors going into one final common pathway which will be a neuro-developmental story,

> and then that'll have multiple widespread consequences subsequently, that's my guess.

What I am initially struck by, here, is the researcher's reluctance even 'to use that sort of language.' But perhaps even more striking is the reference to autism as a 'multi-level thing,' which is centred around the different multiplicities of genetic variants, neurodevelopmental pathways, and variable consequences in the manifestation of symptoms. This is exactly the sort of account that oriented me to the sheer multiplicity of autism – which is not only to say that it is visible on different levels (which is trivially true for likely any biological disorder), but also that there is little sense of autism's path-dependence *across* these levels. In other words, the absence of a single cause starts to give a sense of how difficult it is to imagine how or where a singular 'autism' is drawn together at all. It's not only that autism is both 'genetic' *and* 'neurodevelopmental,' but that a particular subset of genetic factors for one person's autism, may have nothing to do with the neurodevelopmental unfolding of autism for another (because the neurodevelopmental path is linked to a different subset of genetic factors). And these may *both* be independent of the symptoms that are diagnosable as autism in a third (because, of course, those symptoms might be traced to a whole other subset of 'the autism genes,' and also to a quite different neurodevelopmental pathway). It is precisely this kind of analysis that began to draw my attention not only to the well-known variability or 'heterogeneity' of autism – but, more significantly, to the difficulty of working with any coherent sense of autism that might, even as a concept or a model, be held together across this all-pervasive sense of difference.

Consider this very similar account, but moving away from cause this time, and which came from a postdoc (R09) with a strong background more in neuroscience, and neuroscientific methods, than in autism research as such. 'If you try and differentiate often between people with autism on a univariate measure,' he said,

> so I just take, like, somatosensory thresholds, right, on one test, then, you know, it's difficult to see what's going on there. […] the best way probably to think about autism is not…you're not going to find a brain locus for it, it's not going to be an 'aha! Right, there's this big problem here, and that's it. And we're just going to sort that out.' It's: 'something happens in early development that causes multiple [*tape cuts out for a few seconds*] …ples, and we're dealing with the effects of that, and it might go in different ways with

> different people and we're not sure why yet, essentially'. But at least all of those differences are different enough and consistently like one another enough, even though you might have to go into n-dimensional space to see it, that you can distinguish it.

His critical point is that using just one measure, autism remains indistinct – that it is difficult to find 'an autism' using any single measure, such as an EEG measure of the somatosensory system (autism is often associated with strange relationships to touch, and to sensation in general. See Baranek *et al.,* 2006; Grandin, 2006). But measuring different aspects of the disorder, in different people, across multiple levels ('n-dimensional space'), might give you – and this, perhaps, the most that can be hoped for – 'differences [that] are different enough and consistently like one another enough,' such that something that looks like autism might be broadly isolated, at least as a kind of statistical aggregate suspended above these multiple levels. Similar to the previous account, autism has to be thought of as 'a multi-level thing,' but this multiplicity does not at all imply any single or necessary path between the various levels. This is an argument that autism is not simply 'heterogeneous,' but is something which can only be identified by starting from a top-down perspective – identifying more and more statistical commonality between the various sets of difference, until something that looks enough like autism can, even if only in terms of some quantitatively-significant relation, be said to hang together.

It is very possible that what I have identified, here, is not a way of thinking about cause that is particular to autism, but is the sign of a more general and multidimensional 'probabilistic' thought-style emerging in neuroscientific approaches to psychiatric and psychological diagnosis (Singh and Rose, 2009). But autism does nonetheless have a particular relationship to the way that cause can be modelled across some of the different layers that interest neuroscientists (in particular, the genetic, cognitive, and behavioural layers). It is the difficulty of this relationship that make the transition between the various 'levels of understanding' particularly interesting in autism research. For example, several of my interviewees referred me to John Morton's (2004) 'causal modelling' approach to developmental disorder. As they described his account, Morton distinguishes 'A'-shaped disorders from 'V'-shaped disorders, depending on where you see unity across cases. 'A'-shaped disorders have widely differentiated genetic inputs, but behaviour remains mostly stable (ADHD was one example provided by an interviewee). 'V'-shaped disorders show the inverse pattern – a discrete and well-described genetic cause, but with wide disparity in behavioural symptoms (PKU was give as an example). What's interesting about autism, though, is that it shows an 'X'-shape – which indicates disparity at *both* the genetic and behavioural levels, but with some consistency at the level of cognition (the centre of the 'X'). This goes some way to showing the degree of multiplicity that, for these interviewees, characterises autism in particular, and also the lack of an obvious winnowing, or a single path, between the behavioural and biological levels. As one interviewee (R12) put it, to explain the upshot of this view:

> Let's say you have a bank of genes that are the autism genes. But it's not just 'if you have that gene, you have autism.' And it's not even 'if you have this particular combination of genes, you have autism.' You've just got to have enough of a mix of them, with enough of them affected in a big enough way, for it to produce symptoms […] Cognition is difficult because you can't see it; it's this loop between biology and behaviour. But you can't actually get at it […] [tests for cognition] are always behavioural tests. If you do something that you obviously think of as biological, like brain-imaging – [it's] to get at the cognitive level. There's no way of actually getting at the cognitive level, apart from through biology or behaviour. It's an invisible thing. It's a concept, in that sense. So it has to have some form of reality at a biological level, in some form, at some stage. But we just don't know at what level that's at.

Two things are worth drawing attention to here. One is, again, the difficulty of connecting the biological and the behavioural: even if the behavioural level was consistent and well-described in autism – and it is not – this tells you very little about what's happening at the genetic level. Again, as noted above, very different combinations of genes, with no commonality between them, may be predictive of 'autism,' depending on the equally nebulous effect of the environment that the person grows up in (Persico and Bourgeron, 2006). Second, though, is this sense of invisibility of the cognitive picture of autism – 'it's a concept, in that sense' – which may be the only level at which, according to this set of interviewees, the definition of autism is at all robust, but which still has to be inferred though large, disconnected and differentiated pools of biology and behaviour. This goes a long way towards describing what is particularly hard about autism's multiple appearances across the different levels that these researchers are interested in. It captures the difficulty of finding any path for autism between these levels – such that some now even wonder if autism is *only* a phenomenon of 'multiple levels.'

Here, I have limited myself to a discussion of cause particularly. But the idea that autism was a phenomenon of different levels of understanding, and that these did not always or obviously tie-up together into a coherent disorder, was a feature of several other parts of my interviews. I saw this way of thinking, for instance, in discussions of translating between laboratory and clinic: as one person (R08) said, 'there's this disconnect, because the cognition's got nothing to do with the way clinicians look at it and how they're diagnosing it, but that's how *we're* looking at it'). I also saw it when people talked about about the potential for treatment: 'I'm never going to know,' the interviewee quoted above went on (R12),

> never in my lifetime am I going to know how all of the different possible factors that influence a person, or are likely to affect a course of treatment or a behavioural plan, or whatever it is, for that person. Because people are just too complex. There are just too many factors. And, let alone working out how they're going to affect an individual, we're probably not even going to know what all those factors are.

Others talked about the difficulty of maintaining a single autism across the lifespan of an individual, or within a cohort of research participants: 'probably a lot of the guys I've got are probably more on the Aspies[27] level,' said one researcher (R09). And then, after a pause: '….which doesn't exist anymore according to the DSM.'[28]

What I have tried to emphasise, in this section, is the multiplicity of autism: and the emerging idea that, unlike ADHD, for instance (and I don't endorse this distinction; I am just interested in the fact that it is made), to understand autism is to understand something which exists on different levels. These levels do not really winnow at any causally or biologically identifiable point, nor do they *translate* into each other in an easy way. The question that emerges, then, is whether autism actually coheres at all. There are a few different answers to this. One is to say that autism simply doesn't hang together – and, as I have already noted, we can make both sociological (Nadesan, 2005) and neuropsychological (Happé and Ronald, 2008) arguments to that effect. Another answer, and I touched on this above, would be to say that this difficulty in autism might simply be a sign of a broader psychiatric thought-style, in which disease-entities are defined precisely on their probabilistic relationship to one another *across* levels. A third argument would be related to the

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[27] A familiar term for Asperger's syndrome, and one especially used by self-advocates who have been diagnosed with Asperger's.

[28] See n.5 above

one that I attributed to several interviewees in Chapter Two – and this a more pragmatic claim to the effect that autism is only a 'diagnostic category' or a 'symptom checklist,' and anything else is, for now anyway, empty speculation.

In what follows, I want to venture a slightly different way of thinking about this multiplicity, albeit one which may well be congruent with one or more of the above – and this is to suggest that working towards autism's coherence is a potentially useful way of characterising what autism neuroscience *does*; and also that being traced together, within a sociotechnical order that includes the new brain sciences, and the environment, and cognition, and a host of other things too, is one way of thinking about what or how autism actually *is*. I am going to proceed, as I have throughout the thesis, on the basis that autism is not a singular diagnostic entity simply awaiting better description, but nor is it a thrown-together medical category in need of disaggregation either. Situating this claim more precisely, I argue now that we might read, in these accounts of multiplicity and different levels, an emerging sense of autism's 'contingent stability' – or a concession to the degree to which autism is indomitably 'present' in these spaces (Murray, 2008: 16), but in which that presence is nonetheless 'figured' (Haraway, 1997: 10) by a complex and entangled neuropsychological research practice. To unpack this claim, I want to focus on the last bit first – and this is the strange and knotted nature of the contemporary brain sciences.

A mix of everything

It is apparent, in the above, that the different 'levels' on which autism has to be understood are embedded in differences within knowledge, practice and epistemology – for instance, the difference between genes, cognition and behaviour; between neuroanatomy and brain-imaging; between laboratory research and treatment plans, and so on. It is tempting, at this stage, to simply say that the autism of one is hardly the autism of any other – and that there should simply be more care applied in the distinctions. This is an argument with some merit. But there is another current in the empirical material, which I have been insisting upon throughout the thesis – and this is a suggestion that what is interesting, here, is not the preponderance of difference between levels of understanding, but actually the way that these levels are (perhaps/sometimes/slowly) drawn or held together by the different researchers that I spoke to. This possibility was, in fact, initially suggested to me by an interviewee who had experience in research management (3S02), and who had worked at a senior level within a major UK autism funder. She said: 'I think one of the interesting things about autism research,'

> as distinct from some other fields of research is the degree of commonality of view across – I mean, they'll tell you the balance is wrong. You know, the psychologists will tell you we're spending far too much time doing genetics and brains scans, they'll tell you, you know, that will only take you so far. And on the other hand, you've got the neuroscientists saying, 'well the psychologists haven't produced anything that helped people with autism for 25 years' [*laughs*] […]. But I think generally [the scientists'] view of what autism is, how it develops, what the issues in autism are, how it translates into social difficulties, you know, the things you might do to alleviate that situation, I think there's a lot of common ground between the different scientific disciplines.

What I want to do, in this section, is precisely not to focus on the degree of difference, but to go with this – perhaps – less critically-exciting account of the persistence of 'the commonality of views across' and of the establishment of 'common ground between the different… disciplines.' For this interviewee, the importance of establishing the common ground was clear: 'I think acting as facilitators, brokers, harriers,' she said later on in our conversation – laughing a bit at my description of the purpose of her organisation as 'connecting things' – 'I think that's an effective role that a small charity can play.' I have just described some of the ways that autism seems to *resist* a singular definition and a singular accounting. What I want to do in this section is to focus on some of the ways that neuroscientific autism research is surrounded by narratives and practices that bespeak precisely the kind of facilitating, brokering, and *connecting* logic that this interviewee has proposed. My argument is that what is interesting, here, is not the difference between the levels. Rather what is interesting is the fact of connection between them, or at least attempts at connection, and also the perseverance, and the small successes, of those neuroscientists who connect.

Consider, for example, the following quote, which comes from a senior researcher (SS04) who maintained an identity in both the clinic and the laboratory. 'There's so much science that's relevant to understanding a complex brain condition like autism,' he said,

> a developmental condition as well – both in regard to brain development, and also to development throughout the lifespan […] But I'm the sort of person

> who wants to know a little bit about all the sort of levels of understanding. So I try and keep up at least at a basic level with what the sort of genetics story is, and that's interesting because for the first time in quite a long time it's changed in the last few years, and opened up potentially. And then thinking about sort of brain development, and some of those issues around how the social mind sort of develops and gets put together is a really interesting story, potentially, and I've been thinking about how that fits onto the emergence of autism

Here, what's interesting is not only that there are 'all the sort of levels of understanding,' but that the different levels are all still relevant for him. 'Autism research' cannot be a singular thing; it is, at the same time, a 'genetics story,' something to do with 'sort of brain development' and also a question of 'how the social mind sort of develops.' But the key thing is that he 'wants to know a little bit about all' of these. What I am trying to get across, here, is a sense of a neuroscientific research practice that wants to attend to, precisely, 'all the sorts of levels,' and which is interested in 'the commonality of views across' – and which is, therefore not entirely averse to diagnostic objects that drift through lots of different kinds of manifestations and understandings. As an interviewee (L01) put it:

> Behaviour always has brain correlates – there's no such thing as free-floating behaviour. Which doesn't mean to say I want to go down a reductionist route. If you can understand what those correlates are, then it is at least an intermediate step towards…possibly physical-based interventions and, further down the line when you get to first causes or aetiology, possibly to prevention […] I see a causal tangle. So I'm not going from aetiology to neurobiology to behaviour in any neat way. Because they all feed backwards and forward

Note, in particular, the relationship between the way that this person thinks about the multiplicity and variety of autism ('I see a causal tangle'), to how they conceive of their research practice as precisely, that which can 'feed backwards and forward – albeit not in 'any neat way' – in order to work itself, slowly, and differentially, *through* the many layers of this tangling. References to this kind of tricky, back-and-forth motion came up again and again: 'as far as I see it,' said another interviewee (L02), 'the behaviour needs to inform how we approach looking at anatomy, that's what I do.' Another (R05) said: 'if you describe or identify certain cognitive phenomena, you kind of conceptualise them in a certain way that needs to always make sense at a neurobiological level as well.' Another person (L04) spoke about her delight, as a PhD student in neuroscience, upon discovering 'these multiple strands of evidence that would show you how your model was apparently correct or where it fell down.' And later, talking about the likelihood of a big breakthrough in autism biomarker research, she said

> I would probably tend towards thinking [there's not going to be] such a big breakthrough if it's not set up as a sort of massive long-term study, or set of studies, with a lot of different experts from different areas, feeding in kind of the state of the art… I tend to think, 'oh, another study that looks at the genotype,' for example, Well, they come up with different kinds of candidate genes, and why would anyone think that is going to find the true candidate in isolation from other things.

What I am exactly trying to draw attention to, here, is the unwillingness among my neuroscientific interviewees to pursue a research practice that stands 'in isolation from other things,' an unwillingness that enables them to appreciate the 'tangle' of all of autism's 'levels of understanding.' This requires not only an ecumenical awareness of other views, but precisely the active pursuit of 'commonality,' by moving 'backwards and forwards' across the different epistemological and disciplinary layers: neuroanatomical, behavioural, cognitive, genetic, environmental, experiential, and so on. I found this kind of self-positioning to be a cardinal feature of many of my interviewees' descriptions of what was involved in thinking neuroscientifically. It formed a technique for locating yourself, and your own research practice, such that it was actually possible to attend to the strange multiplicity of a (nonetheless) singular diagnostic entity. This gave me an early picture of a research practice in which organising logics of contingency and stability might actually inhabit the same research space.

In many ways, moving across disciplinary and epistemological levels is precisely what the new brain sciences were set up to do. As Steven Rose points out:

> what were once disparate fields – anatomy, physiology, molecular biology, genetics and behaviour – are now all embraced within "neurobiology." But the ambitions of these transformed sciences have reached still further, into the historically disputed terrain between biology, psychology and philosophy: hence the all-embracing phrase: "the neurosciences." The plural is important' (2004:-3-4).

Indeed, and has also been noted elsewhere, one of the most significant outcomes of the 'decade of the brain' was the number of scientists – molecular biologists, computer scientists – not specifically trained in neurobiology, who found it both possible and advantageous to begin describing themselves as 'neuroscientists,' particularly (Jones and Mendell 1999). In a related sense, what I am trying to draw attention to, in this section, is precisely the degree to which *living with* connections between different areas, even where no dominant narrative connects them within the practice of each individual researcher, might be a core feature of the new brain sciences. As one person (R07) put it to me when I asked what had led her to her current research: 'It's just that I was very interested in psychology,

> and I just wanted to find a way to get to working with people with cognitive impairments, and I was very interested in autism, so, uh, via this route I knew that I could work and do research on autism. And I really like the fact that cognitive science is a range of sciences actually. There's a computer aspect, there's a social aspect - it's a mix of everything [...] I like this, um, diverse, um, this approach of mixing diverse ways together to investigate an aspect - especially as there's so much going on, and it's not just biological, there's a lot around the issue of why some children develop autism.

What I like about this account is her telling of the discovery – or, perhaps, her chiding of my assumption – that cognitive neuroscience might be something that ran against her more wide-ranging, human interests (which we had just discussed). But not only is her research implicated in 'a range of sciences,' these include such different domains as computer science, biology, social research, and so on. To do neuroscience, for her, was already to have some kind of awareness of the different things that needed to be joined up. It was the same for a neuroscience PhD student (P01) who spoke to me about her interest in the role of 'social priming' in autism, and the different ways that she might actually think about investigating it. She began by talking about the difficulties of looking for this phenomenon at the behavioural level – and argued that it might be easier to think about it at the neurological level, for example by thinking about connectivity:

> if you find that the connectivity is weaker, you can start asking questions like 'well, why is that? Is it because there are less connections, like physical connections, axons, between these areas of the brain and these areas of the brain? Or is it because those axons are narrower, less myelinated, something like that?' I just think that that [neuroscientific approach] gives you, you know, a lot of ways of exploring the problem. And then hopefully – so you should then really go back to development and try and find out, well why is that? Is it something that's genetic, that's associated with just these people who are born this way, or is it due to experience? But when you get to the experience

> question, then I think obviously you need behavioural experiments as well, because you really need to take into account the experience that this person has had, and that's a behaviour, so you need to use appropriate behavioural experiments for it.

I jumped in to say that I thought it was interesting how you could go from behavioural experiments to brain-imaging, and then back out the other side, as it were – to something experiential or developmental. 'Yeah,' she said

> because if you ignore that and only look at the brain, then, then you kind of have nothing, because you just have…it's not nothing I suppose. [*Laughs*].

Thus, the very specific interest of neuroscience, for her, was that it gave her 'lots of ways' for thinking about a whole range of problems: do people with autism have difficulties with social priming because they have fewer axons? Then you don't just need to scan brains, but you also need to think about genetics and experience too. And if brain-scanning is 'not nothing' in the neurosciences – nor is it the only level at which questions can be approached. As another person (R02) put it, quite bluntly:

> The basic, sort of, the bottom line, perhaps the most crucial insight [of the new brain sciences] is that our brains work at many different scales. So there are genetic scales. For instance, our neurons are the cells in the body that have the most prominent genetic expression of all cells in the body. So we have many scales there – anatomic scales, scales of genetics and gene expression. There is then this scale of the dynamic response – so, for instance, just in terms of time, expressing a gene before it is fully operational can take hours, or even a day or two. But obviously when we are talking to each other here, things are on a much more rapid timescale. So we have different technologies which basically probe into the different windows, the different areas, the different scales the brain is working at.

It is precisely a kind of organising logic of *connecting-difference* that I want to draw attention to, here. This includes working through the standard layering of gene/cognition/behaviour – but it also implies a differentiated sensitivity to other scales, such as time and anatomy. Through interview after interview, neuroscientists consistently reported to me that to *do* neuroscience was to think at different scales – and also that the *work* of neuroscience was making sense of this scalar tangle, but precisely not by hoping that the tangle didn't exist, or by trying to pull it apart in any easy way. Another person (L07) said: 'whenever anybody tries to set up some sort of dichotomy in brain sciences – whether it's nature versus nurture or anything else – initially it seems like a sensible question – rapidly people come and say its neither one nor the other, it's a combination.' The resistance of dichotomies in favour of a combinatory logic was a defining feature of my interviews with neuroscientists. But what I am trying to suggest is that presence of this logic has forced me to think more carefully not only about the inherent 'multi-disciplinarity' of the new brain sciences – but also about the kinds of complex, tangled, multi-layered and even incoherent research objects that such a science must be capable of both thinking about, and working on.

What is also implied in this account, though, is a particular kind of researcher who is both sensitive to, and collaborative with, these kinds of objects. I am reminded, throughout these accounts, of Donna Haraway's 'modest witness' – precisely the complex and active scientific figure who is able to work through forms of 'contingent stability' (1997: 23, 267). In Haraway's description the 'modest witness' of the life sciences is hardly a self-effacing, Baconian 'ventriloquist for the object world…endowed with the remarkable power to establish the facts' (1997: 24). Being particularly concerned with new sociotechnical orderings, and the strange sorts of object-stabilities that are worked across them, Haraway challenges us to re-cast both what counts as modesty and what counts as witnessing: 'I would like to queer the elaborately constructed and defended confidence of this civic man of reason,' she says, 'in order to enable a more corporeal, inflected, and optically dense, if less elegant, kind of modest witness to matters of fact to emerge in the worlds of technoscience' (ibid.). This proposal has clear associations with the kinds of difficult multiplicities in which my interviewees located themselves.

More to the point, perhaps, Haraway is attentive to the complex and ambivalent gendering of these practices, and to their particular association with scientists who are embedded in what she calls 'nonstandard positions' (ibid.: 269). If Haraway is correct that 'the exclusion of women and labouring men was instrumental to managing a critical boundary between watching and witnessing,' then we must pay some attention to who, precisely, is at work in this space (ibid.: 33). Here I want to draw attention, again, to the representation of women in my interview sample: as I noted in the introduction, and excluding the people I interviewed from third-sector organisations, I interviewed a total of eleven men and twenty-three women – a ration of more than two-to-one between women and men. It is not my purpose, here, to engage the broader scholarship on the exclusion of women's scientific labour from laboratory spaces (Fox Keller, 1977; H. Rose, 1994;

Donald, 2012). Nor can I do justice to the complicated and mostly US-based statistics on the place of women in psychology and neuroscience.[29] But while the long-running politics of these developments are far too tricky for such a short discussion as this, I still take seriously, and precisely amid a discussion of a complex scientific practice of entanglement and combination, Hilary Rose's observation that 'feminist biologists, in contesting the boundaries of nature and culture laid down by sociobiology, understood in a direct and practical way that as women we, our bodies and ourselves, are part of both nature and of culture' (1994: 2).

I favour Donna Haraway's conceptual apparatus, here, because it draws attention to a possible relationship between the openness of these combinatory logics, the frankness of the more coroporeal, more contingent, and thus more *modest* witnessing practices of (some parts of) the new brain sciences, and the presence of so many 'nonstandard' bodies working in this space. This is precisely what is missed in the more self-consciously disembodied actor-network accounts (discussed below). As an interviewee (R07) quoted earlier put it:

> going in to study cognitive science where there is a lot of computing, for example, can be a bit, uh...it was hard for me, because I'm not really interested in that. I knew that - and especially because I'm really interested in rehabilitation - I knew that if I could master these computing skills, I could help develop programmes that could help children with autism to communicate better. […] I need to master this aspect of science as well, even if it's been painful. It was a conscious choice that I've made.

While fully acknowledging the complexity and bipolarity of the politics that inhabits claims like these, and while trying not to mark 'nonstandard' accounts with a longstanding responsibility for 'care,' I am still mindful of the 'feminist reconstruction of rationality' that Hilary Rose has argued for, 'in which senses of responsibility and caring are restored within work and within knowledge' (1994: 49-50). But they are restored specifically so as *not* to move these categories over the line of reason. They aim, instead, to instead to trouble that line through an understanding of the entangled relationship between the social and the natural (and all the other levels) that draws both on a feminist epistemology and on the daily experience of

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[29] In psychology, far more women than men have received doctorates for some decades. See e.g. Willyard (2011). In neuroscience as such, the picture is more mixed, with about half of all doctorates awarded to women, although it is likely too early to say much about the picture for neuroscience in general. See Nature Neuroscience [Editorial] (2006).

women scientists. I put this here as a marker and reminder for the discussion that follows.

Let me close this section with one final extract – it comes from an interview with a postdoctoral neuroscientist (R06) who I described earlier as being quite downbeat in her account of what neuroscience was or what it could be. But what's interesting is that, immediately after that discussion, she began to talk herself into a slightly more positive view of what brain-imaging could actually do, and in particular how looking at multiple levels was making for a better neuroscience. 'Now,' she said, 'in brain imaging methods,

> and statistical methods, the way to look at this information from brain imaging has evolved as well. So while now one can infer for example when two areas talk to each other by looking at whether their activity is correlated. Again, it's hard to know which one started. Because, for example, fMRI is a very slow method, so it's still quite hard to tell what's the time…the unfolding, in time, of activity. So you see two areas that seem to be…whose activities seem to be correlated because they go up and down together, but you don't know which one started. Um, of course you can fill in some gaps with anatomical data – so now there are some ways to measure traffic to determine what is connected to what in the brain, and to measure the thickness of this tract. So you know how…you have an estimate of how fast information will go, for example, from one side to another. So this is called connectivity. So then you can put together these data from functional fMRI, where you see the areas activated with what you know from the connectivity in the areas, and then you may infer something.

What draws me to this extract, in particular, is that the researcher begins by describing limitations. But then gradually, and almost in spite of herself, she starts to give a really interesting, accretive account of how different components and levels ('anatomical data'/'the thickness of this tract'/'how fast information will go') can actually be strung across the statistical and anatomical gaps in brain-imaging data. Even if 'you don't know which [brain-area] started,' you can use anatomical measures to 'fill in some gaps,' get a hold of some measures that focus on 'traffic,' then 'put these together' with your fMRI data – and suddenly things start to look a bit more solid and coherent.

What I have tried to stress, in this section, is precisely this ability of modest-witnessing neuroscientists to move between, for instance, anatomy and behaviour, to think about both myelination and psychology, to work with computer science and family life, and to connect the expression of a gene to the conduct of a conversation. I have tried to show how the seemingly intractable ‘incoherence’ of an autism, strung as it is across different epistemological, disciplinary and corporeal levels, might also be read as the ability of the new brain sciences – and this precisely through the application of such a relational and modestly-witnessed scientific labour – to work with, on, and through things that manifest on these kinds of different scales. This is what I think begins to explain the ‘contingent stability’ of autism. In using this term, I am calling attention to both the spectrum’s implication in a particular sort of neuroscientific ordering, but also the fact that to be implicated in such an ordering might sometimes be a condition of *being present* in the first place. This is the quality, throughout the thesis, that I have called 'tracing.'

Trace it up

My interest, here, is in trying to show how autism is delicately strung through very different levels of understanding. More specifically, I am interested in the way that neurobiological research on autism is often done within a disciplinary logic that is attentive to, and appreciative of, ‘all the levels’ within which a research subject might be manifested. And it is so irrespective of the presence of any clear path *through* these levels. In this final section, I want to explore the degree to which these findings might give some insight into how a neurobiological account of autism is in the process of being carefully and skilfully put together.

One of my arguments throughout the thesis has been that cognitive neuroscience is not only the disparate practice that attends quite well to differentiated clinical entities. I have proposed, instead, that neuroscience is a practice in which the elements of some of these entities might be quite carefully *traced together*. To situate this claim more concretely, let me return at more length to an interview that I first mentioned in the introduction to the thesis. It comes from a conversation that I had with a senior lecturer (L07) in developmental neuropsychiatry. In common with a lot of the people I spoke to, he was particularly interested in finding the brain basis of some of the core symptoms of autism. In the early part of the interview, he responded to my question about what had gotten him interested in autism in the first place – the question that elicited many of the affectively-committed accounts that I discussed in Chapter Four – with a seemingly straightforward answer. He said that autism was a developmental disorder with pervasive symptoms, and it seemed reasonable to guess that these symptoms might be amenable to his methods, which included both EEG and fMRI. Okay, I said, but given all that we knew about the variability and even the ineffability of autism, isn't the interesting about autism the degree to which it is strangely *un*-amenable to these kinds of methods? 'If the symptoms are manifest in terms of relationships with others,' he said,

> differences in perceptual functioning, differences in motor functioning, differences potentially in responses to social and concrete stimuli – then, I have access to measures that measure those things. If I follow a line of thought which would say that whatever is going on in autism can be understood by a model of perturbed biological functioning, and if it seems at least halfway plausible that atypical biological functioning may be reflected in things like functional MRI, blood flow using SPECT [Single-photon emission computed tomography – another brain-imaging method], or EEG recording, [or] Event Related Potential recording, then I have access to those. If, for instance, it seemed to me – because obviously, as you say, one has to have one's own personal history and pathway – if it had seemed to me likely that the pathology there is associated at either, on the one hand, a purely societal level or, on the other, at a purely genetic or molecular level – then I don't personally have access or expertise in those areas of research.

Here, much like some of the discussion I set out above, he seems to place his (mostly brain-imaging) methods at a distinctive mid-point between the molecular (below) and the societal (above). In this account, his methods are those that can measure the functioning of organs *in between* – on the basis that there's good reason to believe that what is going on at the molecular and societal levels will be manifest in some kind of 'atypical biological functioning' at the level of the individual brain. For me, though, this instantly raised the question about how a line of coherence was actually maintained *between* those levels – or how someone doing a SPECT study on high-functioning autistic adults knew he was dealing with the same thing as his colleagues diagnosing severely autistic children in a family clinic. Could a diagnostic entity really maintain a sense of coherence amid such unknowns? 'That's a big question,' he said.

> Um, I guess the answer is yes, I would think that something certainly could. And it could probably travel in both directions. To take the one that I think is less relevant to my own work first: if, for some reason, at a societal level, people treat you differently, so they treat everyone with red hair differently,

> then there's research now that demonstrates – say, you know, they abuse and ignore someone with red hair – then that does exist as brain-biology changes. There's research on, you know, different field altogether, but people who've been subjected to various kinds of abuse, from early childhood onwards. So – it's not quite societal, but yeah, if you're a victimised group, then that can affect biology. […] And that biology can be manifest in terms of behaviour; it can be manifest in terms of hormonal function in the brain, and in terms of the functioning of particular regions within the brain, and there are studies that have shown that.

Here, he begins to describe an interesting loop from some kind of phenotypic abnormality (red hair) to society and the environment (different treatment of people with red hair), to anatomy and the body (brain changes on the basis of this treatment), and back out to behaviour (a whole host of clinical symptoms that are produced by those brain changes). What I am trying to draw attention to, here, is the relation between this neuropsychiatrist's ability to draw, participate in, and – to use Donna Haraway's term – *figure* such loops, and also the holding-together of autism as a more-or-less stable diagnostic category across different fields of research and experience. It is noteworthy that this account is not only an acknowledgement or appreciation of 'all the levels,' but is more the description of a research practice in which those levels are explicitly drawn and held together. And this is done irrespective of direct or obvious linkage between them. My argument is that it is precisely through this kind of strung-out, networked neuroscientific practice, which is able to collectively figure such close collaborative loops (and here, although my interview focus is on scientists, I include the collaborative agency of other bodies, machines, experiences, rhetorics, and so on) that lines can be traced through very different aspects of a variegated disorder like autism. Later, this interviewee put it even more concretely:

> Again, moving outside autism temporarily, there are clear isolated and probably rather rare, pure genetic lesions that have been defined in the laboratory that disturb language functioning. Then even if the rest of your brain, if you like, works fairly well, people behave differently to you. Your experience growing up is different because you have this molecular biological deficit that inhibits language development. And that will then start to interact – and it will lead to, or at least, you could easily develop this [*inaudible*] which would lead to biological differences in the way the brain develops, cognitive differences, interpersonal differences, and potentially societal effects, So that you can, without doubt, trace

> it up – now, not necessarily very easily. But you can.

I already gestured at this final suggestion in the introduction, but let me repeat it here nonetheless: his suggestion is that even if you take the 'purest' biological phenomenon you can think of – so some genetic phenomenon that produces a distinct, predictable brain lesion, in an area unambiguously associated with some feature of language, and one that has very well-described effects – even that will inevitably change as the affected person goes out into the world and interacts with other people. And that change should be measurable at the level of the brain. But what you do, then, of course, from any one level, is you 'trace it up.' And also you act on the basis of that tracing. He went on to describe, for example, attempts to reduce the 'stigma' of mental disorder as a way of intervening in mental health, because you could trace those social effects to the appearance of the disorder in the brain, and even potentially analyse the social effects using brain measures only. 'Tracing it up' is thus a very useful description, and one that I have drawn on as a dominant image in the thesis as a whole. It shows how this careful neuropsychiatrist works and moves between different levels, neither through guesswork nor construction, nor by simply following a natural path of discovery, His move is one of careful accretion and collaboration with different levels of knowledge, practice and experience.

The very same description was used by the PhD student (P01) who I quoted above on the potential relationship between social priming and myelination. Later on in that conversation, she said:

> I feel like I'm contradicting myself a lot…em….um… because I was previously saying 'oh, the brain's really important, I'm really interested in the brain' but I think that you need it all. You need behavioural experiments and you need to know what's going on in the brain, and you need to know, well it's an advantage to know about genes, about stuff that's inherited, compared to stuff that you've learnt. It depend what your question is though – like, what do you want to know? Like, do you want to know why individuals with autism don't exhibit this social modulation of imitation, if that were your question: why? Then I do think that you need to trace it back through development, and you need to take into account inherited biological stuff and also experience, because [otherwise] you're not going to get at the question of why.

Again, what I want to draw attention to is not only the desire to 'have it all,' but the degree to which this 'having' is associated with a careful, deliberate and active *tracing* practice. This is a practice in which clear lines of connection are established between development and social modulation, on the one hand, and genetic inheritance and brain biology on the other. For this researcher, to know the 'why' of autism is to help trace the lines between these levels. It also means collaborating with the other agencies that are at work within them (like 'development' and 'inherited biological stuff'). Here, tracing becomes precisely the goal of the rational-responsible neuroscientist in search of some ambiguously-constituted scientific object. Her sense that in describing this process she is somehow contradicting herself captures very well, for me, the way in which this kind of uncertain neuroscientific labour is not always intuitively obvious.

As described already, I have been particularly drawn to the image of 'tracing' because it carries the double-meaning of both a 'following' and a 'marking' – thereby capturing both the degree to which the obstinate fact might well be followed, but also describing the inseparability of its obstinacy from some kind of relational activity. Putting it quite literally – the neuropsychiatrist, sitting in her office in Cambridge, can *follow* the influence of the environment on autistic brain development, or the relationship of particular kinds of behaviour to point-changes in the brain; but then she also *marks* her own brain-level measurements of autism with these same agencies. This is the sense in which I have described autism as something that can be traced through a neuroscientific research practice. It is also the basis of my intuition that a tracing neuroscience, and a traced object, disrupt any sense of a solid line between, for example discovering and constructing – between being described and being made.

I am mindful, here, of the affinities of my account with what Bruno Latour has described as 'immutable mobiles' (1987 236-237). These are transportable resources and agencies (in this case, we might say: journal articles, electrophysiology, and red-haired kids) that are solid and well-regarded enough to maintain their shape. But they can also be arranged by resourceful 'centres of calculation' into the kinds of networks – Donna Haraway might say, 'sociotechnical orders' (1997: 267) – that rather often have stable scientific objects at their ends. Accounts that follow Latour's rubric often position themselves at quite a late point in the recruitment and stability of these mobiles (among Latour's own cardinal examples are the maps of Ferdinand de Lesseps, and the astronomy of Tyco Brahe), and this can sometimes create the impression of a self-conscious (and even a bit self-satisfied) ‘actor-network theory’ [ANT] – in which everything is already achieved, and (perhaps counter-intuitively for those of us basically sympathetic to a ‘second empiricism’ for the social sciences), in which strung-out, sociotechnical orders of achievement can look even a bit *too* stable. More to the point, such a *post-hoc* descriptive practice can begin to look like a science of the already-obvious, or a history of the winner – and I am not convinced that such a move is well-suited to the shifting plates of the contemporary life sciences. ‘An object,’ in the manner of an actor network theory account, at least as John Law has it,

> remains an object while everything stays in place and the relations between it and its neighbouring entities hold steady…the job of ANT is to explore the strategies which generate – and are in turn generated by – its object-ness, the syntaxes or the discourses which hold it in place’ (Law, 2002: 93).

But the neurobiology of autism, like many objects of the contemporary life sciences, and not at all like the movement of the stars, is not yet so stable, nor are the generative strategies and agencies so well worked out. Everything is not yet in place – although, as I tried to describe in this chapter, the elements may well be in the process of being traced together by various skilful and sympathetic agencies. Thus, while I draw on some of Latour’s very useful descriptive apparatus for talking about scientific facts below, I also want to re-state my commitment to Haraway’s elaboration of facthood. It is the case for *my* data, as it is in Haraway’s discussion, that scientific witnessing is often not at all associated with victory, finality or achievement (and here, Haraway’s impatience with the more militaristic rhetoric of some self-described ANT accounts is evident), but instead with forms of relay, exchange, multiplicity, complexity and entanglement – exactly as the image of 'tracing' has attempted to describe (Haraway, 1997: 268). Thus, I want to draw on the image of Latour’s ‘immutable mobiles’ in what follows, but my emphasis is on a still-in-process tracing and relaying, which requires a bit more uncertainty than such accounts sometimes admit.

Here, for example, is another account of the process that I am interested in. The contributor (R02) doesn’t talk about tracing, but he uses an allied metaphor of ‘shuffling things together.’ What is powerful about this account is that the researcher begins talking about one – currently popular – model of autism, but then quickly begins to recruit other agencies into a potentially stable account of just how such a model might be convincingly traced together, beginning with his own speciality (which is MEG brain-imaging):

> So, for instance, one model [of autism] is that there is inherent under-connectivity. So autism is seen, according to this model, as something where locally brain areas interact, but they're lacking the big picture if you like. So they're lacking the ability to connect over large distances. You can then take the MEG result and see how this fits into the model. So it is, if you like, it is a bit of an iterative process. You have correlations first from other task, from other experiments, perhaps even from other populations of subjects you have theoretical models, and you can now start shuffling things together […] Take this example – say the [cognitive] theory is holistic, local processing in autism. You could be inclined to attempt to match that one to [another] one by saying, well holistic refers to global synchronicity, and local or piecemeal processing refers to local processing. But we already know we can't be that simple. But the idea is, here, again, is to make as far as possible some prediction, even if they are conceptual in nature, from the theoretical models, and see how this can or cannot match.

Here, we can see how the 'hard' neuroscientist – this interviewee was a physicist by training, and he spent some time impressing upon me his ideas about the relationship between EEG and 'thought,' and his admiration for the work of John Searle – thinks about making different cognitive models speak to his data. He tests potential orders for precisely Latour's categories of 'mutability' ('well holistic refers to global synchronicity'), 'stability' ('see how this can or cannot match') and 'combinability' ('you can now start shuffling things together'). It is not at all clear how – or if – such an account would finally hold together, or who or what would need to be involved. But this early, speculative discussion gives us, I think, quite a strong sense of how it might happen. More to the point, it lets us see how an autism of all the 'levels of understanding' might be more closely drawn together, and thus it give us a sense of how shifts might occur in the 'contingent stability' of such an object.

Here is another example (part of which I already quoted above), in which again, what is precisely in question is whether there are enough mobile resources out there – cognitive models, brain measures, autistic subjects – so that an autism-bearing network might be gradually strung together. This is a process that the interviewee (R09) refers to as 'connecting the dots.'

> If you try and differentiate often between people with autism on a univariate measure, so I just take like somatosensory thresholds, right, on one test, then, you know, it's difficult to see what's going on there. But if you take the results

> of multiple tests, so that you have sort of like an n-dimensional space, you have nine points, you have n dimensions, you can imagine there's…I can never think in nine dimensions, which is probably good. I just take about it in three, so there's a cloud of dots in space, right? And the other dimensions come later. But it's some higher dimensional space and, you know, if you plot, all these things on these different tests, then there is some structure in people that have ASD.

This description of his attempt to 'plot…a cloud of dots in space' is, again, the practice that I refer to as tracing, and again we can see the cardinal elements of the kinds of successful scientific practices that Bruno Latour refers to as the recruitment of 'immutable mobiles' in 'centres of calculation' (1987: 236-237). Here we have mutability ('somatosensory thresholds, right, on one test'), stability ('if you plot…there is some structure') and combinability ('there's a cloud of spots in space, right?'). 'What I compare [autism] to,' said a more senior scientist (SS03),

> what I compare it to is like a tapestry like, there's uh, La Dame à la Licorne ['The Lady and the Unicorn'] at the Musée de Cluny in Paris – a very colourful tapestry, a medieval tapestry. And you cover it with black plastic, and what you do is you punch some holes in it, and shine a light through, and sometimes you'll see blue, and sometimes, you'll see gold, and sometimes you'll see a little bit of pattern. And all scientists are all kind of saying, 'no, autism is blue, no autism is red, no autism…,' you know. And we haven't punched enough holes even to see a decent bit of it – even to say, well actually there's a unicorn.

The unicorn – an image, of course, that already brings into play the mutability of the mythic and the real – is imagined, here, as a kind of tentative, potential 'thing' at the centre of all of these practices. But it is only something that becomes visible, meaningful, and identifiably 'there,' when all of the different elements – blue, gold, 'a little bit of pattern' – are carefully strung together into a whole. The point is that the unicorn is not only something waiting to be discovered (if only these researchers could somehow find a method to tear off 'the black plastic'). The unicorn is instead both made-up-of, and revealed by, the combination of on-going accounts of 'blue,' 'red' and 'gold' autisms. And the tracing-together of all these different elements precisely draws into question the helpfulness of distinguishing between the tracing of the elements, and the unicorn itself. The claim that, one day, we might realise that 'there's a unicorn,' is not a vision of ultimate or final insight. It is rather the tentative hope of an accretive and combinatory, tracing neuroscience..

The tapestried unicorn is as good an image as any on which to end this discussion: entwined within it are the tangled tropes of artifice and discovery that have run through this account, as well as images and memories of the kinds of uncertain, following-and-marking labour that I have been trying to describe for the neuroscience of autism throughout the thesis. Forming an ambiguous amalgamation of the really-real and the not-quite-real, and joining-together the work of revealing and the labour of making, tracing the unicorn, as this interviewee makes clear, is no small thing. But it is an image that we might draw upon and think with, finally, in order to see not only a neuroscience that is attentive to 'all the different levels'; and also not only an autism that is strangely and problematically dotted across different ways of thinking, knowing, and experiencing. It is an image that helps us to think about what is really at stake in the 'contingent stability' of autism, viz. an on-going, processual, tracing practice; one that is inherent to the relational and combinatory logic of the new brain sciences; which works by slowly recruiting different agencies, and drawing them together; and which tentatively fumbles, finally, to make some kind of coherent order out of them. This graduated, ambiguous and modest image, which bespeaks not only care, but potential success too – this is precisely how I think we should think about the strange multiplicity of the neurobiological account of autism.

Conclusion

I began this chapter with a desire to concretise some of my more general commitments to the neuroscience of autism, which have run throughout each of the chapters of this thesis. These commitments came from an insistence that it should be possible to think more-or-less sociologically about the tensions, ruptures and ambivalences that run through neuroscientific research on autism, but not to use these differences to destabilise (even in theory) the emerging and deeply-complex neurological account of where, or what, or if, autism is either 'of' or 'in' the brain. My way-in partly came from Karen Barad's 'agential realism' which has provided one of the fullest accounts of how sociologists of science might 'not take separateness to be an inherent feature of how the world is,' even while we refuse to 'denigrate separateness as a mere illusion' (2007: 37). If we can agree with Barad that 'relata' come *after* relationships, and not the other way round (ibid.) – then we can begin to see how it might be possible to pay attention to signs of tension, difference and ambiguity, as I have done here, while still maintaining a basic commitment to the solidity, agency and efficacy of all the entities under discussion.

Perhaps more to the point, I wrote this chapter precisely because *not* seeing the neurobiological account of autism as inherently unstable or misguided has been one of the most important commitments that emerged from this project. If the neuroscience of autism gives us some insight into the most important differences, difficulties and ambiguities at the heart of the new brain sciences, then I see this as hopeful smoke from a factory or a workshop, and not at all the discovery of a hidden burial ground. The point of the chapter has been to locate this commitment more precisely – and also to talk in a more empirical way about why I thought it worth making in the first place. The essence of my argument has been that even if autism is a disorder that seems to be strangely adrift between very different sets of scientific practices and assumptions; and even if it is not always clear that neuroscientists can indeed move as easily between these levels of discussion and practice as they might like; still might we begin to see their sense of quick and shifting creativity, and the emerging strength and solidity that it produces, as reasons to think more carefully about the specific elements of both the multiplicity and the movement in question.

As I suggested in the introduction, there has still been too little *positive* discussion in the sociology of the new brain sciences (and indeed in studies of science and technology more generally) about the careful ways that novel diagnostic entities are actually held together by the practices and epistemologies of the new brain sciences. Thinking 'positively,' in this sense, doesn't mean becoming a vacuous cheerleader for the new brain sciences. But it does require the sociologist to commit to a particular kind of criticism, and this is a criticism that basically wants the object of its own discussion to do well. Graham Harman has observed that

> An old maxim states that there are two kinds of critics: those who want us to succeed, and those who want us to fail. Debate is always tedious when conducted with persons of the latter kind. Wherever we turn, they are popping balloons and spilling oil on the floor; we find ourselves confronted not only with arguments, but with unmistakeable aggressions of voice and physical posture. Yet such gestures of supremacy yield no treasures even for the victors, and somehow always seem to solidify the *status quo*. It is analogous to 'critiquing' long distance buses by puncturing their tyres, assuring that no one leaves town and nothing is risked (2009: 119).

This is perhaps a bit more polarised than I would like – but it still stands, fairly accurately, for one of the central ambitions of this chapter, which has been an attempt to shift debate precisely away from tedious fault-finding, even while looking through structures of difference and ambiguity.

In making these kinds of claims, of course, I am quite aware of the difficult politics that I am stepping through. If, as I conceded in Chapter Three, there are indeed reasons to worry about some of the tendencies emerging in the new brain sciences (although this may owe more to a popular than a scholarly literature; see Johnson and Littlefield, 2011), these questions become even more acute in the face of a multiply-contested diagnostic category like autism. Far away from these conversations, of course, autism remains mired in important contests about the relationships between neurology and identity, the passage between difference and legitimacy, and the definition of what gets to count as either 'social' or 'neurological' in the first place. The politics of these questions are, of course, deeply intertwined with the kinds of laboratory-working that I have described. To talk about the neuroscience of autism in terms of a simple-looking 'creativity,' in the midst of these kinds of arguments, risks looking as if I have chosen to avoid some of the hard social and political questions that quite profoundly impinge on the scientific practice in question.

But part of what I have aimed to show, in this chapter, and throughout the thesis, is precisely the inseparability of socio-political and neuro-scientific gestures in autism research. I have tried to show how describing one is to always also to begin a conversation about the other. And I hope to have shown, too, the kind of neuropolitics to which I am, indeed, committed. This is a commitment to the on-going making of a neuroscientific research practice, and also to a set of practitioners, that does not always try to settle debates, to reduce levels, or to announce victories; it is a practice, on the contrary, that builds complex accounts of the human world, and puts together interventions for that world, through the use of carefully-wrought, and deeply cosmopolitan, alliances, coalitions, and even consensuses. More to the point, it does soon the basis of a scientific rationality and practice that is as corporeal, affective, and ecumenical, as it is careful, and methodical and tactical. This is the neuroscience of autism that I am committed to. It is precisely the practice that I have tried to describe in this chapter.

Conclusion – key themes for a tracing neuroscience of autism

Introduction

Throughout this thesis, I have added different layers to a portrait of the new brain sciences that is not, I think, yet well-recognised within the sociological literature. In each chapter, I have shown an aspect of neuroscience that is coloured not by confidence, or intellectual imperialism, but instead by ambiguity, uncertainty, and a particular kind of epistemic modesty. But in each case, my point has not been that these moments are inimical to neuroscience in general, or that they reveal an ugly truth beneath the bland exterior. My point has been that, in the space of autism research at least, the epistemic and ontological force of the new brain sciences lies precisely within the care and novelty through which these uncertainties are recognised and worked-through. What I want to do, in concluding, and rather than summing up a definitive series of findings, is to return to some of the main themes that undergird this suggestion. In particular, I want to situate five key themes more precisely, in light of the discussion in the substantive chapters. These are: complexity and ambiguity in the space of neuroscience; locating the trace; neuropsychological naturecultures; the specificity of sociological space; and figuring autistic presence.

Complexity and ambiguity in the space of neuroscience

I began the thesis by showing that there is still much concern within the sociology of the new brain sciences about the reductionist intentions of neuropsychological and neuropsychiatric research. Social scientists continue to worry that some complex, differentiated and entangled aspects of the human social world are at great risk of being reduced to the bare material of the brain (Martin, 2000, 2004; Ortega and Vidal, 2007; Choudhury *et al*., 2009). One of my main claims throughout this thesis, however, is that when the interested social scientist talks to neuroscientists who are engaged in the 'cerebralization' or 'neurobiologization' of some complex psychological or psychiatric diagnosis – such as autism – they are unlikely to encounter a confident language of monolithic neuroreduction. What they will probably hear, instead, is a much more complex, ambiguous and uncertain discourse. They will encounter a way of talking about neuroscience that is characterised by an ability to live with, and work through, forms of entanglement and complexity – a way of talking that could not at all be described as a desire to simply reduce one thing to another. During the empirical phase of this project, I carefully sought out neuroscientists who were trying to establish the brain basis of a complex, heterogeneous, socially-inflected, biologically-recalcitrant category of disorder. I had mentally prepared myself for the possibility of running into a monolith – i.e. a discourse that would ride a bit roughshod over the complexity of autism, and that would elide the difficulties inherent in associating autism with a neural substrate. What I found, instead, was a story about the neuroscience of autism that was, on the surface, filled with vague definitions, practical anxieties, frankly-expressed emotions and desires, attention to social influences, and so on. And throughout all of this, still, I found neither crudity nor desperation. I found novel strategies for working to put all of these things together, and to carefully pursue a neuroscience of autism all the same.

Emily Martin has described the neurosciences as a 'vortex' – indeed, one of 'the most dangerous kind' (2000: 574). A vortex is a mass of particles swirling around a central axis. In Martin's metaphor, I think, a remorseless popular-intellectual strategy of neuro-reduction has become that axis. These reductive tendencies coalesce, in Martin's description, into something, central, powerful, and unambiguous – a circulatory force into whose ambit the more subtle and piecemeal practices of an interpretive social science are inexorably swirled and sucked. This is not a worry to be dismissed lightly. But Martin, like many other critics of the new brain sciences, is focused on a particular 'neuroscience.' Hers is the neuroscience of journalism, and popular books; of famous, heavily-cited scientific articles, and of semi-popular philosophy; it is neuroscience as a kind of current or zeitgeist, an ever-present, always-available, loosely-applied science of everything; and it is neuroscience as the expression of an institutionalised desire for hard, organic answers to complex, social problems. This neuroscience is, no doubt, a real phenomenon, and one deserving of critical attention. But I have argued for more attention to, and engagement with, another neuroscience. This is the neuroscience that one finds amid the complex entanglement of neuroscientists' reflections on their *own* work, and especially when that work is as awkward, tricky, and hard-to-pin-down, as looking for some kind of neural correlate of the autism spectrum

My key finding has been that the things neuroscientists say, when the social scientist talks to them in the middle of such a project, are embedded in notably ambiguous and uncertain registers. They are wrapped around discourses that are shot-through with the epistemological and ontological potency of difference, complexity, and contradiction. When I asked a group of autism neuroscientists what autism *is*, for example, they said variously that it was a 'biological truth,' but also that it was a diagnostic 'umbrella of convenience' – and anyway, there was probably something out there called autism, at least insofar as they could feel it when they talked to someone who had it, and they knew it when they saw it. When I asked them to talk about their relationship to the basic practices of neuroscience, they sometimes said that neuroscience was indeed a powerful framework for psychological and psychiatric research, and that neuroimaging, in particular, finally gave psychologists and psychiatrists access to the 'organ' that interested them. But they also said that neuroscientific measures were really partial and simplistic, that neuroimaging was actually quite biologically disappointing – that it always gave positive results no matter what you did, and that its growth might have had as much to do with careerist self-interest as it had intellectual discovery. When I talk about the on-going presence of ambiguity and uncertainty at the heart of some neuroscientists' accounts of their own practice, and when I call for more sociological attention to – and positive engagement in – the 'neuroscience' that prevails at this level, this is the kind of thing I mean. I am trying to draw attention to the capacity of many neuroscientists to talk about their work in a way that moves delicately and carefully through ambiguous and contradictory commitments. This is a way of talking about neuroscience that is sometimes self-conscious about these contradictions, and sometimes glosses over them, but that is, nonetheless, often filled with accounts of labours and strategies that work to hold the whole thing together.

Locating the trace

To think a bit more precisely about the kind of neuroscience that actually comes out of such a commitment, I drew on the image of 'trace.' The term was initially used by one my interviewees to describe the way in which, faced with so many different biological and social factors circulating in the neurodevelopment of a person with autism, he was still trying to gather all of these together and, in some sense – as he put it – 'trace it up.' What struck me about this word, 'trace,' was that it rejected a sharp distinction between, on the one hand, the scientist's novel generation of something, and, on the other hand, her labour of following or seeking evidence of some already-existing thing. A 'trace' can also describe a traversal, or an active process of working across different areas. At the same time, it can name an object to be followed in that traversal – something potentially out there, even if always out of sight. There is a risk of putting too much conceptual weight on this metaphor – but it has helped me to stop thinking there was anything worrisome or problematic about all of this difference, uncertainty and ambiguity. 'Trace' allowed me, instead, to focus on all of the different and contradictory things that my interviewees were talking about, and the obviously-creative work of their gathering-up and juxtaposing these different things. But 'trace' also made room for my interviewees' tacit conviction that there might be a neurobiology of autism, out there, all the same. It allowed me to maintain the sense of a brain-based autism quite independent of all of this careful, creative labour.

But 'trace' has been an awkward ally for the thesis too. Certainly, there is a risk of over-interpretation. I have talked about tracing autism to get some purchase on an important unifying element within my interviews: the way in which these neuroscientists' talk did not usually concede a sharp divide between the sense of ambiguous entanglement that they were working through, and the potential for a singular account of the neurobiological substrate of autism. But 'tracing autism' is not an attempt to describe a coherent system, nor does it offer any kind of theory of how neuroscience works. It is, instead, a kind of mobile metaphor – working to locate commonality within the different kinds of complexity and ambiguity that run through the neuroscience of autism. I said in Chapter Two, for example, that interviewees used an image of 'feeling' to *trace* the definition of autism across registers of both biological-facticity and diagnostic-convenience. Describing this work as 'tracing' allowed me to suggest that my interviewees were not naïve about their own work of holding uncertain things together. But it also allowed me to maintain some sense of an autism that had an existence quite beyond all of this uncertainty. In Chapter Four, I said that interviewees *traced* the laboratory labour of autism neuroscience though both an intellectual interest in autism, and an embodied feeling for it. Here, 'tracing' allowed me to maintain the sense of a neuroscientific practice that did not distinguish strongly between intellectual and affective labours. But it also clung onto the image of a neuroscience that might, nonetheless, objectively isolate the neurobiological substrate of a neurodevelopmental disorder called autism. Although it has different inflections, 'tracing' captures something important that is common to all the chapters: on the one side, there is a sense of ambiguity within these interviews, and of gathering-things-together, and of entanglement, and even of some confusion or anxiety; on the other side, there is the sense of a potentially successful neuroscientific practice, i.e. a good, positive, concrete, objective description of the brain basis of autism spectrum disorders.

The second reason that 'trace' is an awkward ally, of course, is that it is almost *too* polyvalent. 'Trace' has a long and distinguished history in other areas, including, as Wilson (1998) points out, in both psychological and deconstructive literatures. I should be clear that, even where there is some resonance, my usage has no direct affiliation with this history. I do nonetheless wish to seek a specific lineage for my own use of this word, and this is in the suite of terms that scholars have used to think across similar gaps. In Chapter Three, I affiliated my notion of trace with Karen Barad's 'agential realism,' which is similarly keen to recognise the 'intra-active' priority of entanglement, while also creating space for the independent existence of 'agentially cut' non-human things (2007: 33, 175). In Chapter Four, I associated the notion of trace with AN Whitehead's account of subject-object relations, which not only troubles the distinction between being subjectively-apprehended and being independently-existing – but even makes the two conditional upon one another (1979: 49). In Chapter Six, I proposed a relationship between the act of of tracing and the generation of what Bruno Latour calls 'immutable mobiles,' which is his name for an identity between carefully-strung-together networks of things, and things themselves (1987: 236-237).

My use of tracing is less metaphysically ambitious than any of these. It is, at heart, a metaphor for describing some similar intuitions, and practices based on those intuitions, drawn from different parts of my interviews. I want to claim, though, that precisely the value of my account of 'tracing autism' is that it is *not* metaphysics. Nor is it even philosophy of science. It comes instead from unglamorous empirical sociology. Even within that, it comes from some fairly old-fashioned interpretation of the accounts of key actors within a circumscribed social field: as both a word and the description of a process, 'tracing' is lifted entirely from neuroscientists *own* accounts of their daily assumptions and practices. But there is vindication, in that, for social theorists who insist upon the conceptual richness and complexity of so much work in the life sciences – especially the new brain sciences

– and who have tried to direct critical attention to thinking *with* these practices, at least as they are narrated and articulated by practitioners themselves (Wilson, 2004; Rose and Abi-Rached, In Press). But there is also vindication (whether or not they desire it) for neuroscientists themselves. My borrowing of the image of 'trace' from their own descriptions shows that ways of working and thinking within neuroscience are not only congruent with much cutting-edge social theory. My suggestion, now, is that neuroscientific labour, as a rich and complex method for thinking both with and about entangled worldly phenomena, needs to be understood as a deeply sophisticated apprehension of, and collaboration with, the hybridised, 'material-semiotic' entanglements of contemporary naturecultures (Haraway, 1988).

Neuropsychological naturecultures

At the heart of a 'tracing neuroscience,' in other words, is an ability to work through the entanglement of methods, ideas, materials and deductions that may be called 'social' (or 'cultural'), and initiatives, feelings, processes and conclusions that would more commonly be described as 'neurological' (or 'scientific'). When I began this study, I was very interested in neuro-reductionism, and I thought that the neuroscience of such a socially-inflected disorder as autism would make a good place in which to think about it. In fact, a significant part of my interest was in finding neuroscience's limit. I was looking for a space in which a reductive scient*istic* neuroscience would run into the end of its own ambition, where it would find something too complex, too heterogeneous, too irredeemably *social* for its own ends. The story that resulted, of course, was a great deal more complicated than I had supposed. And one of the most significant aspects of that complexity was my inability to locate a rigid dividing line between neurological and social facts within these conversations.

There has been much attention within recent studies of science to the problematic of firm nature-culture dualisms – not least in the work of Donna Haraway, who uses the term 'naturecultures' to designate moments that 'greatly increase the density of all kinds of …traffic on the bridge between what counts as nature and culture' (1997: 56). For Haraway, the fact of this crossing directs attention to the proliferation of spaces

> in which all the actors become who they are *in the dance of relating*, not from scratch, not ex nihilo, but full of the patterns of their sometimes-joined, sometimes-separate heritages both before and lateral to *this* encounter' (2007: 25. Emphases in original. *cf.* Latour, 1993).

As Lisa Blackman points out, considerable work in critical psychiatry and psychology has also argued for the breaking-down of a nature/culture dualism, in which

> nature and culture [are] discrete entities which interact in particular kinds of ways…The term natureculture is a figuration which points to the dialectical interchange between nature and culture such that they cannot be disentangled, viewing nature as thoroughly cultural and vice versa' (2005: 186-186).

Having first gained ground in critical psychology, Blackman points out, this has led to a troubling of the psychological distinction between the fixed attributes of the interior of the individual, and some mutable 'outside.'

My suggestion is that we may also see a naturecultural moment, or space, within a tracing neuroscience of autism – a kind of 'neuropsychological natureculture' in which scientific work proceeds without a firm distinction between these poles. I do not mean this in the sense that – of course – science is 'always-already' culturally entangled, whether it likes it or not. What I am trying to gesture at, here, is the way that a neuroscience might specifically proceed, and might bring its objects into view, via an in-practice attention to exactly the kinds of bridges and figurations that Haraway and Blackman refer to. In Chapter Two, for example, I showed how autism neuroscientists are sensitive to the scientific and political differences between thinking autism as a natural fact, and thinking of it as a diagnostic category that serves an insurance and medical bureaucracy. But what was interesting is that my interviewees did *not* sharply separate these natural and cultural categories, preferring inside to mingle the two. I showed how these neuroscientists indexed the definition of 'what autism is' through categories of 'enigma' and 'sensation' – creating a bridge between the natural and cultural definitions of autism by figuring autism itself as a kind of mystery, and concrete knowledge of autism as an almost occult sensibility. In Chapter Three, I showed how interviewees did not enact a clear separation between the scientific promise of natural facts generated by brain-scanning, and the cultural presence of a collective anxiety about this practice. What was interesting, here, was that the latter concern did not function as a normative apparatus designating good neuroscience from bad. Instead, the generation and sustenance of a neuroscience that held onto senses of *both* promise and disappointment maintained a traffic between the scientific certainty of the former, and the inevitable cultural 'outside' indexed by the latter.

It is uncontroversial, now, among researchers, to designate autism either a diagnostic or natural category – but one that, in any event, is very widely dispersed genetically and neurologically, and also that, in its current description, has inevitably had some of its natural edges brushed off for clinical convenience. My claim for a 'neuropsychological natureculture' is a bit tangential to this, however. I am arguing that a 'tracing' neuroscience of autism does not always make a great distinction between natural and cultural facts – that it is, in pursuit of the neurobiology of complex developmental diagnoses, quite directly *productive* of what Blackman calls 'dialectical interchange' between natural and cultural forms (2005: 186). In other words: neuropsychological research on autism does not at all run into a limit when it collaborates around such a richly- and complexly-figured diagnostic entity as the autism spectrum. Instead, the naturecultural elements of this research are quite well-placed to work with, and on, the autism spectrum – and, indeed, to thereof produce convincing scientific accounts of autism's neurobiology.

I insist on the term 'natureculture,' here, because I want to reconfigure the conversation between neuropsychology and sociology. Doubtless, and as the former impedes more and more upon the traditional concerns of the latter (see my discussion in Chapter Five), the way in which sociology actually figures neuropsychology is going to be a pressing question for sociologists in this century. And sociologists will not do very well in this figuring, I argue, by rushing to defend the decaying boundaries of a once-powerful critical discipline – to insist, in front of a dwindling audience, that neuroscience must be forever and all times 'in' society or culture (Choudhury *et al.*, 2009). Thinking with a neuropsychological natureculture, however, proposes a different move. In the conclusion to her *Neural Geographies*, Wilson asks if we might not respond to neurocognitive determinism

> not with a repudiation of the neurocognitive but with a neurocognitive overdeterminism – that is, with a cognition and a neurology that operate in excess of the limits of presence, location, and stasis'(1998: 201).

Wilson's proposal, here, is not to respond to a perceived neurobiological reduction with some reciprocally reductive gesture. Instead, she suggests risking the attribution of even *more* complexity, both to neurobiology and critique – and thus finding more scope for the two to work through, and also to reconfigure, one another's sacred objects. What is the likely response of the neuroscientist to such a gesture? Writing precisely two decades ago, in their original proposal for a 'social neuroscience,' Cacioppo and Berntson were insistent on an over-determined relationship between 'neurochemical events' and 'social processes' through the vagaries of normal human life – an implicit proposal for collaboration that has since gone largely unrequited (Cacioppo and Berntson, 1992: 1020). As many have pointed out, such collaboration carries considerable risk. Not the least of these is the political risk of dissolving analysis into an undifferentiated and uncritical sense of 'mutability' or 'flexibility' (Martin, 2004; Malabou, 2008). Nonetheless, it seem to me that to the extent that a critical social science will have any significant purchase on the contemporary life sciences in this century, and particularly on the new brain sciences, it will come precisely from accepting a (risky) invitation to over-determination, and to hybrid investigation. I have tried to show, in this account, how much of the ground is already in place in some parts of the new brain sciences.

Specifying the space

With all of that said, the way that I set out about building this thesis, of course, has a number of drawbacks, and I flagged some of the more significant of these in my discussion of method. Most prominently, the reader needs to maintain a distinction between what I say about 'neuroscience' from conversations with one small group of neuroscientists (all of them working on a single disorder, and all labouring in a particular part of the world), and what might otherwise be said about 'neuroscience' by looking for the emergence of a neuro-reductive discourse within public policy, or by working through popular books and newspaper articles that use 'neuroscience' to reconfigure some contentious or political phenomenon. Yet another neuroscience might be produced by focusing on the genealogy of neuroscientific concepts and assumptions, by focusing on some of the wilder and looser public pronouncements of leading neuroscientists, or by carefully observing the daily, tacit assumptions of a neuroscientific laboratory practice. All of these will isolate a different 'neuroscience,' and anyone who wishes to talk about neuroscience in general will need to find some way to gather up the various elements of such a wide and dispersed field.

My focus has only been on one such element – the kind of thing that a neuroscientist will say to the social scientist who asks them about their on-going, difficult work of 'doing' a neurobiology of an awkward and recalcitrant psychological or psychiatric object, such as the autism spectrum. There is an open question, of course, about why there is such a difference between the ambiguous 'neuroscience' of my conversations, and the more certain neuroscience of the public sphere. I can only very briefly speculate, but it seems plausible that the lower value of doubt and ambiguity in public conversation reflects the increasing entanglement of scientific careers, and of scientific funding, with the instrumentalized expectations of the contemporary academy, and with the promissory discourses designed to meet those expectations (Smith, 2010; Couldry and McRobbie, 2010; see also Chapter Three for a discussion). A more rounded discussion of varieties of 'neuroscience' would need to consider, too, the entanglements of a brain science with varieties of capital and political imperative (and these, needless to say, no more happily purified from the laboratory than the workings of 'society' or 'culture'). What this means is that I can make no claim for mine as the 'real' neuroscience. My argument, instead, is that this kind of ambiguous talk, and the tracing neuroscience that works through it, needs to be worked into the broader sociological discussion of both the impacts and procedures of the new brain sciences.

The second significant limitation of the thesis is in my gathering-together of a 'neuroscience' in the first place, and in my erasure of the important distinctions between interviewees' various trainings, locations and affiliations. There is an important account, still to be made, about the competing disciplinary genealogies of both 'neuroscience' and 'neuroscientists.' Such an analysis would bring together the diagnostic procedure and the biological desire of contemporary psychiatry, the broad conceptual framework of a mainstream cognitive psychology, the organic assumptions and therapeutic focus of a medical neurology, the body-imaging capacities of a nuclear physics, and the quantitative logics of a biostatistics. One interesting circumstances for anyone currently working on the new brain sciences is that this 'black box' is rather open – but also, perhaps, in the process of being shut. My worry is that if neuroscience is in a 'disciplining' moment, then by contributing to the erasure of these differences, I aid the production of that disciplining closure. But I took the chance that riding a bit roughshod over these differences was a price worth paying for the ability to work through and analyse the talk of a group of 'neuroscientists.' Whatever its genealogical and epistemic frailty, there really is a thing called neuroscience – and there really are people whose intellectual labour, and the methods and assumptions that underpin that labour, emerges from their ability to use this name.

Moreover, if the very recent coalescence of methods, objects, institutes, and journals that together form a 'neuroscience' is only too obvious – still this sense of a 'diverse unity' does not fundamentally mark neuroscience out from its predecessors, not least psychology itself (Rose, 1985: 226). Rather than worrying too much about the disciplining of neuroscience, what has been more interesting, to me, is an exploration of what is actually at stake in the emergence of this field. What I have offered, in place of a genealogy, or a critical history, is a willingness to hear how a group of people working within this emerging and coalescing space actually talk about their own zone of investigation. This helped me to get a sense of the registers used in this zone, the assumptions that structured it, the difficulties and problems it identified for itself – and so on. My contribution to the understanding of this emerging zone is that it contains, within it, many kinds of ambiguity and uncertainty, and that its practitioners talk about their work in a much more uncertain and careful way than might have been predicted.

Figuring autistic presence

Finally: in one sense the biggest overall drawback of the thesis is another reluctant erasure that I undertook, and this is the erasure of a concrete sense of autistic presence from my account. When I began this thesis, it was with the intention of exploring the interface between neuroscience and autism. I wanted to think about what, precisely, was at stake in the figuring of the latter by the former. And although my primary interest was always in the formation of particular kinds of neuroscientific knowledges, and in the assumptions, practices, and questions that made these up, I was keen that autism not be positioned as a 'case' for thinking about neuroscience. To think more symmetrically about the relationship between the two, I had intended to bring autism much more centrally into the story I was to tell – and to work the narrative from neuroscience through a series of interviews with people diagnosed with autism, in order to get a fuller sense of how they, in turn, might figure the neurobiological account of autism. Quite early on in in my interviews with neuroscientists, however, it became obvious that the accounts that circulated among neuroscientists themselves, of how they were thinking about, and figuring autism within their own practice, was *already* deeply contested, entangled, multi-faceted, contradictory, and so on. And it became apparent that there was a particular story about neuroscience here, one not yet told, and one that would be well-positioned to draw out a surprisingly deep-set sense of ambiguity and contest within neurobiological research. For the purposes of the thesis, I committed myself to this story – but at the cost of effacing the autistic part of the narrative.

Of course, in one sense, it's not an either/or: no doubt, much of the uncertainty described in the thesis is quite directly a product of my interviewees *own* interactions with autistic people. We saw this quite directly in Chapter Four, where reflections on love and distress in a laboratory neuroscience came precisely from interviewees' memories of their interactions with, and their feelings towards, people with autism. And, although I cannot know this for sure, it seems a not unreasonable intuition that the diagnostic and etiological uncertainty that I discussed in Chapter Two, or the sense of uncertainty and anxiety about neuroscientific methods that I identified in Chapter Three, might both be traced to my interviewees' many clinical and research encounters with autistic people. Although, again, it did not form part of my research horizon, my interviewees were certainly embedded in a broader autistic narrative – one in which, for example, scientific papers and theories are re-positioned within blogs, memoirs, and conferences of autistic people and their allies. So even if I had wanted to isolate autistic experience from my story, it would have been impossible. One of the defining features of autism, Stuart Murray points out, is its possession of a presence that 'cannot, and will not, ultimately be removed' (2008: 16). Certainly, the loops and relays of a figuring autistic presence long precede my conversations.

Still, I retain a sense of unease: for all my interviewees' sensitivity to, and entanglement in, autistic discourses; and for all the inescapability of those entangled discourses even for those who were not so concerned, I do not feign ignorance of the long history, within psychology and psychiatry particularly, of erasing the subjects of knowledge from the space of scientific knowledge-making. By placing autism at one remove from the centre of my story, I worry about my own implication in reproducing the erasure of autistic people from this history. But I also want to say that, as coherent and enclosed as the thesis may be in its own right, I leave it well-positioned to be reconfigured within a larger, and a much more longitudinal and multi-stranded project, about the multiple interfaces of autism and neuroscience. Such a project would not only focus on the working-through of a contemporary neuroscience of autism from within the spaces of that science. It would attempt to draw out the intricacies of how people with autism interact with, contribute to, and sometimes quite directly tilt and reconfigure, the spaces of neuropsychological knowledge-making. It would think about how these knowledges are apprehended, understood and talked-back – but also how they are lived, re-made, and re-apprehended, through the daily work of autistic living.

But my story, finally, has been a story about neuroscientific research on autism. I focused on this because, from a very early stage in my research, my data-set began to show something important. It showed that amid all the bluster of a popular neuroscience of everything; amid all the troubling 'translational' claims that an institutional neuropsychology feels itself required to make; amid all the inanity of an emerging political and policy-pointed neuro discourse; amid all the anxieties and condemnation of an attendant chorus of critique – amid all of these, my data were beginning to show how talking to neuroscientists of autism, themselves, positioned these neuroscientists as much more careful, much more ambiguous, much more modest and much more self-reflective, than any of these discourses yet fully realizes. Through these accounts, I was able to detect the presence of what I came to call a 'tracing' neuroscience. This, among many other qualities, is a neuroscience that works through different spaces and epistemologies. It is a neuroscience that does not require the erasure of this work for the identification of discrete, neuropathological objects. It is a neuroscience that maintains a tricky and sophisticated relationship to the potency of both nature and culture. And it is a neuroscience that is remarkably open to, and ready to ally with, the interests and work of an interpretive sociology. This is the note that I want to close on: without a significant re-consideration of what the new brain science have to offer, and without a significant shift in how a sociology thinks about these science, a real opportunity for hybridity, and collaboration, is going to be missed. My account of a careful, modest, thoughtful, sophisticated neuroscience of autism has been an attempt to re-figure the potential for this relationship. How that works out, though – indeed, *if* it works – will be a significant question for some years to come.

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