

**SOCIAL ORDER AND DISORDER IN AUTISM**

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## **Abstract**

This thesis investigates autism as it has been understood in the cognitive and social (neuro)sciences, within the United Kingdom, since 1985. Of specific interest is how these sciences discover, construct, and experiment upon individuals who are understood as socially abnormal. Theoretically, the thesis is positioned between Foucauldian History and Philosophy of Science, Medical Sociology, and Science and Technology Studies. Empirically, two key sources of information are relied upon. Firstly, there is an extensive critical reading of the published literature from 1985 to the present. Secondly, twenty qualitative research interviews were conducted with academic researchers, based within the UK, and with core interests in psychology in general and autism in particular. It is firstly argued that the cognitive sciences rely upon a particular, historically novel, construction of the social in order to articulate autism as social disorder. It is then argued that, although autism is frequently reported as heterogeneous and illusory within the laboratory, social disorder appears self-evident when the autistic individual is required to interact with both the researcher and broader society. Following these findings it is argued that the researcher does not observe autism but, rather, that they achieve it. Finally it is argued that the language of autism is itself capable of ushering in novel conceptualisations of social conduct that may apply to all individuals and not just those diagnosed with the condition. Following these empirical findings it is argued that autism is best understood as the consequence of particular socio-historical conditions. It is asked if these socio-historical conditions may include a novel knowledge-power nexus arising in the mid-twentieth century, named here a socio-emotive politics, of which autism is just one consequence.

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## Introduction

This thesis is concerned with two contemporary phenomena. Firstly, the emergence, rise, and nature of autism as a diagnostic classification. Secondly the emergence, rise, and nature of laboratory sciences which lay claim to an objective knowledge of ‘the social’; a claim most recently embodied in social/cognitive neuroscience. As will be argued throughout this thesis, these phenomena can be viewed as entangled and mutually reinforcing in nature.

### A contemporary Subject

The condition of autism was first hypothesised to be a discrete nosological entity by American-based psychiatrist Leo Kanner in 1943. The history of the word autism itself is slightly longer, the term (origin: *autos*, self) being used by Eugene Bleuler in 1912 to refer to a mode of thinking evident “...in dreams, pretend play and reveries, and in the fantasies and delusions of the schizophrenic” (Feinstein 2010: 6). While the idea of ‘autistic fantasy’ has been largely abandoned (Evans 2013: 4), Bleuler’s original notion of a retreat towards the self and away from interpersonal relations has remained reasonably stable and is evident in the title of Kanner’s first paper: *Autistic disturbances of affective contact* (Kanner 1943). In more recent decades, symptomology has been described in the form of an ‘autism triad’, featuring “impaired social interaction, impaired verbal and non-verbal communication and the presence of repetitive and restricted patterns of behaviour” (White 2013: 114). Within the 5<sup>th</sup> Edition of the Diagnostic and Statistical Manual of Mental Disorders (DSM-5; American Psychiatric Association 2013) this triad has been condensed into a dyad of socio-communicative impairments and restricted interests and repetitive behaviours. While autism is widely understood as a ‘neurodevelopmental’ condition (Norbury & Sparks 2012), there is neither cause nor cure known for the condition.

While autism’s formal history may start in 1943, it is well recognised that there was an epidemic of autism diagnoses in the 1990s (Eyal et al. 2010: 2) and it is this contemporary era that is of foremost concern to this thesis. Prevalence rates for autism increased from around four cases per 10,000 in

1978 (Wing & Potter 2002: 151) to an estimated one percent of the United Kingdom's school children in 2009 (Baron-Cohen et al. 2009: 500). One remarkable study, conducted in South Korea under the wing of an American research team, concluded that prevalence rates in 7 to 12 year old children was 1:38, and 1:27 in boys (Kim et al. 2011).

With this increase in autism prevalence there has been concomitant increase in financial and academic investment. Between 2007 and 2011 there were over 100 'competitive funding awards' given to projects concerned with autism in the UK, the geographic region of primary concern within this thesis. These funding awards amounted to just under £21million, around 80 percent of which came from government sources (Pellicano et al. 2013: 20). This significant UK investment is, however, dwarfed by that made in the USA, where \$357million (£234million) was spent in 2010 alone (Pellicano et al. 2013: 20). Within the UK, funding was split between a number of disciplines, ranging from molecular genetics to education. Fifty-five percent of all funding awards (£11.6million) were allocated to projects on 'biology, brain and cognition'. The most significant areas of funding within this category concerned questions of "cortical development, social cognition and animal models of autism" (Pellicano et al. 2013: 22).

Again since the 1980s, the nature of the population classified as being autistic has changed significantly. A fledgling cognitive research programme being established in the 1970s consistently used samples with mean I.Q.s in the 60s, and autism was believed to be a condition which almost exclusively affected those with intellectual disability (Baron-Cohen et al. 2005: 628). By comparison, it is now argued that "intellectual disability is not part of the broader autism phenotype...[and] the association between extreme autistic traits and intellectual disability is only modest" (Hoekstra et al. 2009: 534). Similarly, it is argued that:

"...the apparent association between mental retardation and autistic syndromes is not because they usually have common causes, but rather because the presence of both features greatly increases the probability of clinical ascertainment." (Skuse 2007: 387)

Within recent decades, then, the relationship between intellectual disability and autism has been questioned in a novel manner. Diagnostically, High Functioning Autism and Asperger's Syndrome have excluded intellectual disability by definition.

Further, over the last thirty years autism has become an all pervasive cultural experience. 'Autistic fiction' has become a recognised genre (Carlson & Kittay 2009: 320) and:

"Autism narrative is a boom industry. Autobiographies. Biographies. Stage plays. Movies. Documentaries. Novels. Stories for children. Improving manga for adults. Space fantasy comics for more-or-less grown-ups, now called graphic novels. There's the exhilarating 2007 HBO special, *Autism: The Musical...* And above all, there is a vast amount of story-telling in the Internet." (Hacking 2010: 632)

As Stuart Murray has stated, this genre of autism fiction reflects the fact that, within our contemporary society, autism is:

"...the condition of fascination of the moment, occupying a number of cultural locations that reflect a spectrum of wonder and nervousness - the allure of potentially unquantifiable human difference and the nightmare of not somehow being 'fully' human." (Murray 2008: 5)

Somehow this condition of autism which was named just eighty years ago and that just thirty years ago lay entirely outside of public imagination (if production of, and response to, films and texts concerned with autism is at all indicative (Murray 2008: 120)) and which referred to a tiny number of individuals with intellectual disabilities, has become central to what it means to be human. As one scholar has noted, we "...are participating in a living experiment in concept formation of a sort that does not come more than once in a dozen lifetimes" (Hacking 2009a: 506).

#### *A note on terminology: Autism*

Over the time period covered in this thesis (broadly, 1985-2013) autism has undergone several name changes. Within DSM-IV - text revisions, for

example, the official title was ‘Autistic Disorder’ (American Psychiatric Association 2000). ‘Autistic Disorder’ was also demarcated from similar developmental conditions, notably Asperger’s Syndrome (described as clinically identical to Autistic Disorder, but with normal age of language onset) and Pervasive Developmental Disorder – Not Otherwise Specified (PDD-NOS), or ‘atypical autism’ (American Psychiatric Association 2000: 134). During the writing of this thesis, those responsible for the publication of DSM-5 had both Asperger’s Syndrome and PDD-NOS removed from the diagnostic manual and the categorical Autistic Disorder transformed into the (pseudo-)dimensional ‘Autistic Spectrum Disorder’ (ASD). A significant number of researchers (e.g. Sucksmith et al. 2011) deviate even from DSM-5’s ASD, preferring Autistic Spectrum Condition (ASC). The usage of ASC nods towards the significant debates over neurodiversity that surround ASD and the claim that autism is a form of human difference, rather than human disorder (Kapp et al. 2013).

In an attempt at consistency, the word ‘autism’ is used throughout this thesis. While it is appreciated that terminology cannot be politically neutral, ‘autism’ is used for stylistic rather than academic reasons. The use of the term autism is not intended to imply either the inclusion or exclusion of individuals with, for example, Asperger’s Syndrome or a particular position on the difference/deficit debate.

Even given their intensity, debates over the naming of autism as a classification have been dwarfed when it has come to the naming of particular individuals. Contemporary papers referenced within this thesis refer to “individuals with autism” (e.g. Ropar & Mitchell 1999), “autistics” (e.g. Mottron et al. 2006), even “autists” (e.g. Camerer et al. 2005). ‘Autistic’ is preferred by some individuals as it recognises the centrality of autism to their identity, and the belief that autism cannot simply be ‘subtracted’ from their personality. This terminological debate garnered significant interest recently when Michelle Dawson, an autistic author of a paper published in *PLoS One* (Soulières et al. 2011), publically chastised that publication for editing “autistics” to “persons with autism” in final proofing; a decision that was

subsequently apologised for and reverted from<sup>1</sup>. Throughout this thesis, an attempt has been made to use terminology that is consistent with the data under discussion (e.g. in keeping with interviewees' wordings)<sup>2</sup>. It is therefore the case that 'individuals with autism' and 'autistics' are intended to be synonyms.

### *A contemporary subject*

The 1990s were christened the 'decade of the brain' (Beaulieu 2003: 561), largely in response to the emergence of various novel forms of neuroimaging technology which purported to allow the visualisation of live, unimpaired (human) brains for the first time. These novel technologies were immediately believed to hold great promise within medicine, but the experimental sciences, too, saw the possibility of innovation. Within the experimental psy-disciplines, diverse neuroscientific methodologies were put to use in attempts to understand various aspects of human behaviour. As noted above, the majority of autism research funding in the UK goes into projects concerned with 'biology, brain and cognition' (Pellicano et al. 2013) and it says a great deal about how fundamental this biological and neurological research has become to laboratory based psychology that these three streams – biology, brain, cognition – are taken to be a cohesive category. It is now the case that neuroscience has been incorporated into experimental psychology so fully that cognitive psychology and cognitive neuroscience can be argued to have a symbiotic relationship (Beaulieu 2002: 76).

Vidal argues that continuities in thinking concerning the brain extend far beyond the contemporary neurosciences and traces the belief that 'we are our brains' back to the birth of modernism in the 17<sup>th</sup> century (Vidal 2009: 7). Nonetheless, it has been widely argued that emerging biosciences, and neurosciences in particular, have had particularly profound effects upon contemporary understandings of the human subject. As Meloni has stated:

“Although human beings are certainly not more biological beings than they were thirty years ago, they tend to identify themselves, explain

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<sup>1</sup> <http://www.plosone.org/annotation/listThread.action?root=7361>

<sup>2</sup> This is the case both for 'autistic'/'individual with autism' and for other phrases used by within the field of autism study, e.g. 'normal'.

themselves, and see their image more and more through the lens of neurobiological, genetic, and molecular terms.” (Meloni 2011: 156-157)

It is thus claimed the neurobiological enterprises are affecting the ways in which contemporary subjects are constructed and construct themselves. Ortega, for example, states that the:

“...preference for cerebral explanations cannot be reduced to an aversion to psychoanalysis or psychologizing in general. Rather, it emerges as part of the spread of neuroscientific claims beyond the laboratory. Neuroscientific theories, practices, technologies and therapies are shaping the way we think about ourselves and related to others.” (Ortega 2009: 436)

This reshaping of the self, caused by a “folk neurology” (Vrecko 2006), Ortega names the ‘cerebral self’. Similar, if not identical, conceptualisations have been made by Vidal ('brainhood'; Vidal 2009), Rose ('neurochemical self'; Rose 2003) and others. The claim within this literature is that, when considering an ‘ontology of ourselves’ neuroscience is amongst the most important of contemporary forces.

If neuroscience in general has been an important contemporary scientific activity, then one particular trend with neuroscientific research seems of particular interest: the turn towards the *social* brain:

“The human brain, in its current configuration, emerged 50,000 years ago, perhaps a bit earlier. The social brain – a distinctive perspective on the human brain – emerged only 20 years ago.” (Young 2011: 640)

It is undeniably true that the psy-disciplines have not only been concerned with using neuroscientific technologies to examine those behaviours constructed as concerning individual conduct (intelligence, memory, and so forth) but also those concerning social behaviours and social cognitions. It is also true that such activities aimed at knowledges of social behaviour began very shortly after the onset of the neuroscientific revolution (e.g. Brothers 1990). These projects aiming at investigating social behaviour came to be articulated under

the banner of ‘social neuroscience’ (Matusall et al. 2011) and now a vast range of ‘neurohybrid’ disciplines circulate and apply themselves to the topic matters which were once reserved for sociology, economics, philosophy, and so forth. These emerging fields of study join existing work within experimental psychology that has been conducting laboratory investigations into ‘the social’ since the first decades of the twentieth century (Danziger 1992: 311). In this sense, the disciplines of social and cognitive neuroscience are only the latest attempts aiming at an objective, scientific knowledge of ‘the social’, albeit particularly high profile attempts. There may have been a crisis in social psychology during the 1980s (Parker 1989), but the neurosciences have once again made the laboratory study of social behaviour a boom area.

*A note on terminology: Social/cognitive (neuro)science*

To say that the experimental psy-disciplines have embraced the contemporary neurosciences is rather imprecise. The neurosciences are united by an interest in the brain, but the means to satiate that interest differ wildly. Some neuroscientific techniques, such as the Magnetic Resonance Image, require participants to lie alone in the dark surrounded by powerful magnets while an analysis is conducted on the amount of oxygen in the blood at a particular site at a particular moment (see, Logothetis 2008: 874). Other experiments using electroencephalography may be conducted with the participant sitting at a desk, staring at a computer screen and conversing with the experimenter. Some experiments examine brain function while others examine structure. Some experiments require little more than the participant to lie still or watch a computer screen, while others require the comprehension of instructions every bit as complicated as those employed within traditional cognitive experiments. These different methodologies come with very different epistemological assumptions (although, perhaps, similar ontological commitments) and are often used to answer very different questions. Some of the varying assumptions of the different neurosciences come under investigation within this project, but it is important to remember that neurosciences is a plural noun and that, when used in this thesis, the term is being done so in this general sense. Specific references to particular forms of neuroscientific investigation are therefore numerous throughout this project.

## The current project

### *Aims and rationale*

Surprisingly little work has concerned itself with socio-historical constructions of either autism or the social, although the research that has previously been conducted will be considered in chapter 2. That there is such a paucity of research into both autism and the social is particularly striking given that, for the reasons described above, scientific investigations into these phenomena have the potential to be two of the most potent sources of self-making within contemporary society. What is more, no research has been conducted which examines the relationship between constructions of the social and constructions of autism.

That the relationship between autism and the social has yet to come under investigation is noteworthy given that the two subjects are so intimately related. Autism, constructed as it is as a social disorder, is logically dependent upon descriptions of the social. Similarly, autism is of particular importance to experimental studies of the social. Striano and Reid's recent textbook, for example, is entitled *Social Cognition: Development, Neuroscience, and Autism* (Striano & Reid 2009). In experimental studies into the social, autism is frequently constructed as a naturally occurring experiment, an instance of human-minus-social, capable of illuminating upon the nature of social cognition and the social brain. Thus, understandings of contemporary productions of the social must surely include a consideration of autism.

It is the aim of this project to examine, for the first time, the intersection of constructions of the social and constructions of autism. This is a relationship which, as discussed above, is of potential importance for understanding both autism itself and broader questions concerning ontologies of the self. This thesis will thus seek to ask:

- How have and how do particular constructions of the social from within the experimental sciences influence constructions of autism?
- What is the nature of the social disorder hypothesised to be at the core of autism?

- How is the incorporation of autism into experimental studies of the social influencing those sciences' conceptualisations of what it means to be social?

These questions will be investigated via two primary methods. Firstly, an extensive socio-historical document analysis of the published literature since around 1985 will be completed. The incorporation of document analysis gives the current project an historical scope which would not have been possible without reference to written sources. Secondly, qualitative interviews will be conducted with a range of British (neuro)psychologists who investigate autism. In such a diverse and contested field, these interviews are intended to give insight into how a range of scientists, at different academic institutions and points in their career, construct autism and the social.

#### *Overview of chapters*

Chapter 2, *literature review*, lays out three areas of research which inform this project. First is a discussion of normalcy. This portion of the review takes the form of an extended analysis concerning the work of Georges Canguilhem. Specifically, it is asked how Canguilhem's thought has been modified and extended upon by his student, Michel Foucault, and those working within a broadly Foucauldian framework. A second portion of the literature review concerns constructions of the social from within the experimental human sciences, most notably social psychology and social neuroscience. These literatures show that 'the social' has been a far from stable category over the last hundred or so years. Finally, the existing social science literature into autism is considered. While no other pieces have concerned themselves with autism as it relates to questions of the social, valuable contributions are outlined which have begun to chart the emergence of autism from around 1960.

Chapter 3, *reflecting on method(ology)*, considers issues of reflexivity at three different levels. Firstly the nature of the project itself is questioned and it is asked how the samples and methods selected for, and omitted from, this project may affect the overall findings. This section also includes details of the literature and scientists sampled for this project, the interview schedule,

and the ethics procedures that were adhered to. Secondly, the role of the researcher and their particular history in relation to the objects under investigation is considered. Finally, there is reflection upon this project's status within Science Studies and upon the status of the knowledge claims put forward by that discipline given its repeated questioning of other disciplines' knowledge claims. It is concluded that to be understood, the findings of the current project must be situated within this complex relational framework.

Chapter 4, *constructing a social subject: autism and human sociality in the 1980s* is centred upon a five year period between 1985 and 1989 during which time the three dominant psychological models of autism (Executive Dysfunction; Metarepresentations; Weak Central Coherence) were articulated. The manner in which these theories construct the social disorder of autism is examined and related back to existing literature outlined in chapter 2. A further, fourth, model of autism (Emotion/Affect Theory) which has since fallen from favour is also presented, and the construction of the social inherent within that theory is contrasted with the construction of the social from those that have remained dominant.

Chapter 5, *a coherence of alien forms: order and disorder in autism* turns from the literature and towards data obtained through interview. Discourses of 'autistic heterogeneity', which contest that autism is multiplicitous in nature are examined. Given these narratives of heterogeneity, it is subsequently asked where autism's singular essence is to be found. It is shown that researchers report a qualitatively distinct experience of social disorder when encountering an individual with autism. The chapter concludes with a discussion concerning the nature of these experiences and what their presence shows about the nature of autism research.

Chapter 6, *autism, the social, and society* examines the apparent separation between the objective nature of 'the social', as examined within the laboratory, and the normative, value laden concept of 'society'. The relationship between autism, the social, and society is considered in relation to four key areas drawn upon by the research scientists interviewed for this project; DSM-5, diagnostic practice, laboratory practice, and neuroplasticity.

It is argued that, in all four instances, the social of the laboratory is understood in relation to societal frameworks.

Chapter 7, *ontologies of our social selves: normalisation and pathologisation in autism*, examines two recent research trends. Firstly, it is argued that there has been a move to ‘research up’ the autism spectrum. It is argued that research into the symptoms of autism as exhibited by sub- and non-clinical individuals, individuals with autism who are particularly high functioning, and autistic savants are all beginning to normalise autism and make its symptoms intelligible within the context of normal behaviour. Secondly, it is argued that resting state neuroscientific experiments allow individuals previously unsuitable for experimentation to come under scientific examination. The reduced task demands of resting state technologies allow individuals – for example those with severe intellectual disability or infants – who are not capable of following instruction to be experimented upon. It is argued, however, that a great deal must be ‘othered’ in order to assume that resting state technologies are able to capture social behaviour. The chapter concludes by analysing the potentially novel construction of the social found within resting state imaging studies, and how these constructions of autistic sociality may be related to the ‘normalised’ end of the spectrum considered at the beginning of the chapter.

Finally chapter 8, *conclusion: autism, history, and socio-emotive politics*, considers the main findings of the thesis. The first conclusion concerns the placement of the social in laboratory science and it is suggested that existing discussions have thus far failed to appreciate the complexity of this relationship. Secondly it is argued that autism can only be understood within a particular socio-historical framework it cannot meaningfully have said to have pre-existed. Thirdly, it is asked whether the emergence and rise of autism is indicative of a more significant movement in the understanding of the human condition, a socio-emotive politics. The thesis concludes by considering the limitations of the thesis and the possibility of moving beyond the current experience of autism as social disorder.

## Literature Review

“a core symptom in autism is social abnormality”

- Jolliffe & Baron-Cohen 1997: 527

### Introduction

The above quotation captures much of what the cognitive (neuro)sciences know about autism, and it is an interrogation of this knowledge which forms the basis of this PhD. Specifically, when the goal is to examine the historical ontology of autism, three sets of questions arise from claims of this sort:

- What does it mean to say that an individual, or a particular behaviour, is or is not ‘normal’? How has normalcy come to look as it does?
- What is meant here by ‘social’? How have the psy-disciplines come to articulate and classify particular behaviours as ‘social’ and others as ‘nonsocial’?

And finally:

- How have descriptions of (ab)normality and (non)sociality from within the psy-disciplines congealed<sup>3</sup> into the form of autism?

These are questions this thesis will seek to address.

This review will examine literatures which have asked questions concerning the construction of the normal, the social, and the classification of autism. Firstly, a discussion of the normal will be based around the early work of Georges Canguilhem and his tri-partite claim that the normal: i) took on a new appearance in the nineteenth century; ii) always arises from conceptualisations of the pathological, rather than vice versa and; iii) that biological norms are always value laden. Canguilhem’s work will be analysed in relation to that of Michel Foucault and those working within a Foucauldian

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<sup>3</sup> ‘Congeal’ is used here in a sense similar to that conveyed by Butler when it was stated that gender “is the repeated stylization of the body, a set of repeated acts within a highly rigid regulatory frame that congeal over time to produce the appearance of a substance, a natural sort of being” (Butler 1990: 44).

framework, with similarities and differences with Canguilhem's contributions being analysed. Secondly, the small literature which has examined the construction of the social within experimental psychology and social neuroscience will be discussed. As well as noting the contrasting descriptions of the social across the history of psychology, the conditions under which particular constructions of the social arose will be considered. Finally, social scientific analyses of autism will be presented. In particular, and in keeping with the aims of the current thesis, literatures which have examined the historical emergence of autism and those which have examined practices of knowledge creation in relation to autism will be discussed.

*Constructing the normal*

"Nothing is more commonplace than the distinction between fact and value. From the beginning of our language the word 'normal' has been dancing and prancing all over it."

- Hacking 1990: 163

When considering notions of normality and abnormality within the human sciences, a founding text of particular importance is George Canguilhem's *The Normal and the Pathological* (1991). Canguilhem's text has been of longstanding interest in France, yet has gained increased attention in the English language only more recently, from scholars (e.g. Rose 1998) initially drawn by Canguilhem's close association with Michel Foucault (see Miller (1993: 103) for an overview of this relationship). The portion of Canguilhem's text first published in 1943 as part of his doctoral thesis (Canguilhem 1991: 29) makes, for current concerns, three core claims: i) in the nineteenth century a novel conceptualisation of the relationship between normalcy and pathology emerged in which abnormal states were articulated as being quantitatively rather than qualitatively distinct from normal states; ii) normal states are necessarily defined in relation to pathological states and never vice versa; iii) biological norms are never value neutral but are themselves normative. This portion of the review will be framed around an articulation and discussion of these three central claims of Canguilhem.

*i) Is the pathological state merely quantitative modification of the normal state?*

Canguilhem begins his text by considering two historical models of disease. The first, what might be called a Pasteurian model, localises pathology in, for example, a visible bacterium. The second, a Hellenistic model, does not localise but totalises, affecting all of man and her equilibrium. While evidently differing substantially, these two representations of disease do, according to Canguilhem:

“... have one point in common: in disease, or better, in the experience of being sick, both envision a polemical situation: either a battle between the organism and a foreign substance, or an internal struggle between opposing forces. Disease differs from a state of health, the pathological from the normal, as one quality differs from another, either by the presence or absence of a definite principle, or by an alteration of the total organism.” (Canguilhem 1991: 41)

According to Canguilhem, these representations of disease were joined in the nineteenth century by a third, championed by Auguste Comte, FJV Broussais and others. Rather than viewing normalcy and pathology as qualitatively distinct states, there was:

“...the formation of a theory of the relations between the normal and the pathological, according to which the pathological phenomena found in living organisms are nothing more than quantitative variations, greater or lesser according to corresponding physiological phenomena. Semantically, the pathological is designated as a departing from the normal not so much by *a-* or *dys-* as by *hyper-* or *hypo-*.” (Canguilhem 1991: 42, italics in original)

Canguilhem would devote a significant portion of his text to contesting this contemporary conceptualisation of the relationship between the normal and the pathological (e.g. Canguilhem 1991: 86-89) and some of Canguilhem’s reasoning for this is of importance to ii), considered below. For the moment, however, the claim is more important as an historical, rather than a medico-philosophical, thesis. In this historical respect, Canguilhem’s thesis has

received strong empirical support (e.g. Hacking 1990: 162). Indeed, the shifting conceptualisation of normality detailed by Canguilhem seems to be just one (albeit perhaps the most important) of many changes in the construction of the human subject that occurred during the early nineteenth century.

The most important conceptual articulation of this change in the early nineteenth century has come from Canguilhem's student Michel Foucault. It is well known that Foucault saw the beginning of the nineteenth century as an absolutely crucial moment in human history, going as far as to say that the knowledges of things and their orders which formed around 1800 made it possible for "the figure of man to appear" (Foucault 2002: 422). That the turn of the nineteenth century should be so important for contemporary subject formation is perhaps understandable given that the Age of Revolution was transforming life on both sides of The Channel during this time (Hobsbawm 1962). One change noted by Foucault, and into which Canguilhem's new construction of the normal neatly fits, is the emergence of a new social configuration which brought into existence new objects, new forms of knowledge to monitor those objects, and new institutions to control them. Foucault referred to this new domain as that of 'biopolitics' (Foucault 1997b: 239-263).

To talk of biopolitics within contemporary arenas remains divisive, but that is not the goal here. It is surely part of Foucault's enduring appeal that analyses which concern strictly historical episodes seem so pertinent within contemporary society, an effect that was not unintended (Foucault 1994: 245). Nonetheless, critiques (Martin & Williams n.d.: 14-15; Raman & Tutton 2010) of contemporary articulations and redcriptions (e.g. Rabinow & Rose 2006; Rose 2007) of biopolitics do not generally concern the historical thesis set forth by Foucault and, by implication, Canguilhem. Neither do Foucault's own analyses of the effects of biopolitics upon socialism (Foucault 1997b: 259), fascism (Foucault 1997b: 261), or capitalism (Foucault 1978: 141). Of course, Foucault's history lessons must also be viewed warily (Miller 1993: 210; Madsen et al. 2013: 43) but, following other scholars, there is good reason to accept the contention that an intensification, if not a complete emergence, of

practices relating to the governance of populations occurred at some point around 1820 (Hacking 1983: 292), and that this led to new visions of the human in general, and the normal human in particular.

As detailed in *The Birth of the Clinic* (Foucault 2003) and extended upon in *Discipline and Punish* (Foucault 1991a), Foucault saw a particular construction of the body, and a political apparatus explicitly concerned with this construction, emerging in the centuries preceding the 1800s:

"...in the seventeenth and eighteenth centuries, we saw the emergence of techniques of power that were essentially centred on the body, on the individual body. They included all devices that were used to ensure the spatial distribution of individual bodies (their separation, their alignment, their serialization, and their surveillance) and the organization, around those individuals, of a whole field of visibility. They were also techniques that could be used to take control over bodies. Attempts were made to increase their productive force through exercise, drill, and so on." (Foucault 1997b: 241-242)

This notion of 'anatomo-politics' is perhaps best captured in Foucault's image of the panopticon which individualises prisoners and makes the body available to the constant gaze of the disciplinary apparatus (Foucault 1991a: 200; Armstrong 1985). In the 1800s, however, anatomo-politics was joined by a new gaze which saw health and illness residing in a different space outside of the body. The knowledges of this new spatial configuration were:

"...addressed to a multiplicity of men, not to the extent that they are nothing more than their individual bodies, but to the extent that they form, on the contrary, a global mass that is affected by overall characteristic of birth, death, production, illness, and so on. So after a first seizure of power over the body in an individualizing mode, we have a second seizure of power that is not individualizing but, if you like, massifying, that is directed not at man-as-body but at man-as-species. After the anatomo-politics of the human body established in the course of the eighteenth century, we have, at the end of that century, the emergence of something that is no longer an anatomo-politics of the

human body, but what I would call a "biopolitics" of the human race." (Foucault 1997b: 242-243)

The difference regarding this type of knowledge of the population to that which went before it is captured by Hacking when he notes that:

“Hearths and windows were for long not only the basis of taxation but also the most reliable estimator of the size of the population. One can tell the story of biopolitics as the transition from the counting of hearths to the counting of bodies.” (Hacking 1983: 280-281)

Foucault does not mean here that the attempt "to rationalize the problems posed to governmental practice by phenomena characteristic of a set of living beings forming a population: health, hygiene, birthrate, life expectancy, race..." and so forth (Foucault 2008: 317) halted the existence of anatomo-politics but rather that they came to “intersect along an orthogonal articulation” (Foucault 1997b: 253). Census statistics may, as described by Hacking, provide a relatively pure example of biopolitics but, by and large, phenomena will emerge at the intersections of anatomo- and bio-politics. That the novel conceptualisation of the normal which emerged during the early part of the nineteenth century was one such phenomenon occurring at the intersections, incorporating ideas of what it meant to have both a normal body and a normal population, seems particularly likely.

*ii) Health is life in the silence of the organs*

A second core tenet of Canguilhem’s view of normalcy is that the normal is always defined in relation to the pathological, and never vice versa. Canguilhem contends that articulations of normalcy always come in the wake of articulations of pathology for it is only when an individual reports a qualitatively distinct disease experience that medicine turns its attention towards the phenomena in question:

"Physiology is the science of the functions and ways of life, but it is life which suggests to the physiologist the ways to explore, for which he codifies the laws. Physiology cannot impose in life just those ways whose mechanism is intelligible to it. Diseases are new ways of

life. Without the diseases which incessantly renew the area to be explored, physiology would mark time on well-trod ground. But the foregoing idea can also be understood in another, slightly different sense. Disease reveals normal functions to us at the precise moment when it deprives us of their exercise. Disease is the source of the speculative attention which life attaches to life by means of man. If health is life in the silence of the organs, then, strictly speaking, there is no science of health. Health is organic innocence. It must be lost, like all innocence, so that knowledge may be possible. Physiology is like all science, which, as Aristotle says, proceeds from wonder. But truly vital wonder is the anguish caused by disease." (Canguilhem 1991: 100-101)

Medicine is not capable of imposing disease experiences upon subjects and, therefore, trails in the wake of those experiences. This enables Canguilhem to claim that:

"...medicine always exists *de jure*, if not *de facto*, because there are men who feel sick, not because there are doctors to tell men of their illnesses." (Canguilhem 1991: 93. italics in original)

In particular instances this model surely holds, but as a general rule subsequent scholarship has cast grave doubts upon it. Unsurprisingly, given that Canguilhem published his thesis in Paris, 1943, "a not-so-latent existentialism, albeit of a distinctive and idiosyncratic sort, shadows Canguilhem's conception of medicine" (Rabinow 2000: 18). In short, Canguilhem gives a primacy to the subject that the Foucauldian tradition will not accept, something which is evident when considering what Foucault himself means by 'experience'.

Foucault spent much of his career attempting to distance himself and his tradition, including Canguilhem specifically, from existentialist phenomenology (e.g. Foucault 1991b: 8) and it was quite possibly for this reason that Foucault abandoned the term 'experience' for the best part of a decade (Lemke 2011: 28) after it played such a crucial role in the lexicon of *History of Madness* (Khalifa 2006: xiv). Nonetheless, Foucault would come to

understand his analyses, examining radical discontinuities in thought and ways of seeing, as concerning experience:

“I have tried to define to some degree the possibility of the history of what could be called “experiences.” The experience of madness, the experience of disease, the experience of criminality, and the experience of sexuality...” (Foucault 2010: 5)

Foucault claimed that ‘historically singular experiences’ were constructed by the mingling and interrelations of three axes (Foucault 1984b: 2), the so-called tripartite nature of experience (Foucault 1994: 3), featuring “...forms of possible knowledge, normative frameworks of behavior, and potential modes of existence for possible subjects” (Foucault 2010: 254). Lemke (2011: 39) refers to these axes, in turn, as “the problem of truth, the problem of power, and the problem of individual conduct”, and Nikolas Rose has summarised this attitude towards experience in the following manner:

“Devices of ‘meaning production’ – grids of visualization, vocabularies, norms and systems of judgement – *produce* experience; they are not themselves *produced by* experience.” (Rose 1996a: 130, italics in original)

It can be seen just how readily this conceptualisation of experience differs from the existentialist-tinged Canguilhemian notion. If experiences are formed within a knowledge-power nexus (Foucault 1997d: 61) then Canguilhem’s claim that, *de jure*, normalcy proceeds from pathology appears to have been, at least potentially, undermined.

If the Foucauldian conceptualisation of experience undermines Canguilhem’s case *de jure*, there is strong empirical support to undermine it *de facto*. Historical investigations into, for example, the fugue (Hacking 2002), multiple personality disorder (Hacking 1995a), paedophilia (Balmer & Sandland 2012), post-traumatic stress syndrome (Young 1995), and the biosciences more generally (Rose 2007; Vrecko 2010) all convincingly trace processes of subjectification. With particular reference to the current project, it has also been noted (Hacking 2007: 309) that the diagnosis of autism emerged

following increased surveillance over the normal development of children in the 1920s and 1930s (Armstrong 1983: 27; Danziger 1990: 79) and not, it seems, in response to any particular childhood experience of pathology.

It should also be noted that there have been claims that the contemporary biosciences have significantly altered experience, and experiences of normalcy, in ways which could not have been foreseen by Canguilhem. Rose, for example, has suggested that biotechnological enterprise throughout the twentieth century has led to the molecularisation of biopolitics (Rose 2007: 44) and, thus, affected experiences of health and illness in entirely novel fashions. Further, the related concepts of biosociality (Rabinow 1999), biomedicalization (Clarke et al. 2003; Clarke et al. 2010), and biological citizenship (Novas & Gibbon 2008; Novas & Rose 2001; Rose 2001; Rose 2007; Rose & Novas 2005) have suggested quite radical forms of subjectification within the biomedical age, perhaps most notably the claim that subjects now experience themselves as being “at risk” of various pathological states (Rose 2009; Rose 2010; Shostak 2010). These claims have themselves been critiqued as overgeneralisations (Bröer & Heerings 2013; Pickersgill et al. 2011; Weiner 2006; Weiner 2009) but it does seem to be the case that some individuals, perhaps those with Huntington’s Disease (Novas & Rose 2001) or Phelan-McDermid Syndrome (Navon 2011), do experience themselves in quite radically difference ways as a response to novel knowledges, contra the Canguilhemian model that experience of pathology always precedes intervention and, thus, descriptions of normalcy.

### *iii) The normativity of biological norms*

The third of Canguilhem’s claims is that biological norms are not value neutral but are themselves value-based. Canguilhem illustrates this point with reference to one of the most fundamental of vital characteristics, the duration of life:

“...the techniques of collective hygiene which tend to prolong human life, or the habits of negligence which result in shortening it, depending on the value attached to life in a given society, are in the end a value judgment expressed in the abstract number which is the average human

life span. The average life span is not the biologically normal, but in a sense the socially normative, life span." (Canguilhem 1991: 161)

This analysis leads Canguilhem to the conclusion that "the sick man is not abnormal because of the absence of a norm but because of his incapacity to be normative" (Canguilhem 1991: 186). Whilst there are certainly many who contest the notion that the body is an ideologically inscribed space (e.g. Eagleton 1991: 10), it is at least an area of broad agreement amongst those following a Canguilhemian tradition. Foucault was, as is discussed above and is evident elsewhere (e.g. Foucault 2006b), obsessed with the body. In one of his most famed essays Foucault states that:

“We believe, in any event, that the body obeys the exclusive laws of physiology and that it escapes the influence of history, but this too is false. The body is molded by a great many distinct regimes; it is broken down by the rhythms of work, rest, and holidays; it is poisoned by food or values, through eating habits or moral laws; it constructs resistances.” (Foucault 1977: 153)

Thus, “the body is the inscribed surface of events” and it is the task of genealogical enquiry to “expose a body totally imprinted by history and the process of history’s destruction of the body” (Foucault 1977: 148).

Rather than consider, once more, the literature which expands upon processes of anatomo-politics, biopolitics, and subjectification and how these processes relate to the body - as sexual, criminal, pathological and so forth – it instead seems worthwhile to consider what is missing in these analyses for a project concerned not only with medicine but with the psy-disciplines. Ian Hacking has introduced the concept of memoro-politics (Hacking 1994) as a ‘third pole’ to Foucault’s anatomo-politics – biopolitics binary and thus seeks to address a gap in Foucault’s theorising by introducing ‘governance of the soul’ alongside governance of the body and governance of the population. Hacking and others (Danziger 2008; Rose 1985; Rose 1996b; Young 1995) have made two points with regards to memoro-politics. The first is that the latter half of the nineteenth century was a crucial moment for contemporary productions of the soul. One need only think of Freud, his suggestion that

“what has been forgotten is what forms our character, our personality, our soul” (Hacking 1994: 33), and the absolutely fundamental nature of this insight to contemporary constructions of, for example, post-traumatic stress disorder (Young 1994) and child abuse (Hacking 1991) to appreciate this fact.

A second point is that the majority of work conducted within the psy-disciplines is not concerned entirely with memoro-politics to the exclusion of anatamo-politics and biopolitics. Knowledges of both the body and of populations have been crucial to psychology and, as Young has noted, studies of memory were “born at the intersection of two streams of scientific inquiry: somatic and psychological” (Young 1995: 11). The concepts and subjects that emerge, at least partially, on the basis of the psy-disciplines take the form they do because of the clustering of intermediary relations (Foucault 1978: 139) influenced by memoro-, anatamo-, and biopolitics. It is to one of these constructions, the notion of the human as a *social subject*, which we now turn.

### *Constructing the social*

Critical reflection upon psychology’s engagement with ‘the social’ has thus far been highly restricted, both historically and sociologically. Historically, the exceptions to this paucity of research come in two forms. Firstly, there are two journal special editions which (broadly) concern themselves with the topic; the first an issue of *Journal of the History of the Behavioral Sciences* in 2000 (Lubek 2000), the second an issue of *Theory and Psychology* in 2006 (Stam 2006). Secondly, a book by John Greenwood entitled *The Disappearance of the Social in American Social Psychology* (Greenwood 2004a) has considered the topic at length.

While these literatures will be relied upon within this review, several aspects of this small literature are worthy of consideration. Firstly, the body of work is almost entirely self-referencing, garnering little attention from any authors other than those who contribute to the volumes themselves. Dispute between the parties is thus unsurprisingly at a minimum and contestation from alternative theoretical/methodological positions effectively non-existent.

Secondly, with the notable exception of Kurt Danziger, the texts do not engage at all with either Science and Technology Studies or the Foucauldian tradition of history and philosophy of science and instead put forward straightforwardly realist narratives. That so much of this scholarship comes from authors based in Canada and working in the years immediately following the publication of Hacking's *Rewriting the Soul* does make the absence of both STS and &HPS surprising, however it also makes explicable claims such as Greenwood's that there has been a "dearth of attempts to explicate those properties by which social phenomena are intuitively characterised as social" (Greenwood 1997: 2); this is simply not a tenable claim in an era of Actor-Network Theory.

Thirdly, behaviourism, psychoanalysis, and their impact upon the experimental sciences are broadly ignored, perhaps because of the literature's tight focus upon laboratory experimentation with human subjects. Indeed, when considering Danziger's *Constructing the Subject* (Danziger 1990), by far the most cited and influential text in the broader field, Hacking notes that "Danziger's book lists Freud once in the index, and we are referred to a sentence that begins 'Another psychologist...'. Now one thing that Freud was not, was another psychologist" (Hacking 1994: 40). Given the limited scope of the current project it is not realistic for a novel analysis (or indeed integration) of constructions of the social from within behaviourist or psychoanalytic thought to take place here. Nonetheless, such research – perhaps considering empathy as a psychoanalytic rather than a neuroscientific construct (Pigman 1995; see below) – would certainly be a worthwhile project.

Despite the above caveats, this small body of work on the construction of the social within experimental psychology is certainly of value. It will, however, be one of the goals of this thesis to examine its claims, within the context of autism, and in more detail than has thus far been the case.

#### *'Social' Psychology before 1930*

Prior to 1930 Greenwood claims that 'the social' was, within social psychology, conceived of in the following manner:

“...social (or “collective” or “group”) cognition, emotion, and behavior are forms of cognition, emotion, and behavior engaged by individual persons... *because and on condition that they represent other members of a social group as engaging these (or other) forms of cognition, emotion, and behavior in similar circumstances.*” (Greenwood 2004a: 18, italics in original)

Perhaps the most pertinent aspect of this construction is that ‘the social’ refers not to particular objects of cognition but to particular forms of cognition. It is not that interpersonal activities are inherently social while chopping wood is inherently individual, rather it is the form that either of these activities takes which determines whether it is social or not. To use one of Greenwood’s examples, an individual’s belief that abortion is wrong may be called a social belief if it is held due to that individual’s membership of a particular group (for instance, the Catholic Church). The same pro-life belief may be deemed ‘individual’ if:

“...it is held individually, for reasons or causes independent of whether any other Catholic (or any member of any social group) is represented as holding this belief – if, for example, it is held because the person has accepted rational arguments or evidence in favor of this belief.” (Greenwood 2004a: 21)

Greenwood claims that this is the conceptualisation of the social broadly shared by both Durkheim and Weber, positioning social psychology prior to 1930 quite closely to a sociology (Greenwood 2004a: 85-86) which had been determining its own conceptualisation of the social during the 19<sup>th</sup> century (Donzelot 1977; Donzelot 1988). Further, Greenwood largely endorses this articulation, despite the fact that work into the history of rationality (e.g. Foucault 1997e), not to mention forty years of STS, might make us wonder quite what exactly a ‘rational argument’ that is ‘individually held’ without the impact of society might look like.

Greenwood calls this era, dominant prior to 1930, the ‘lost world’ (Greenwood 2004a: 18; Greenwood 2004b: 19), not least because, as Kurt Danziger states, during a period between 1920 and 1970 a new form of

experimental social psychology became “programmatically and potentially normative” (Danziger 2000: 330). Research regimes operating after 1920, and Danziger notes three such regimes, each conceptualise the social in markedly different ways to those operating around World War One.

*‘Social’ Psychology from 1920 onwards*

During the 1920s Floyd Allport argued strongly against the so-called ‘group fallacy’ (Danziger 2000: 333), claiming instead that all social behaviour was reducible to individuals, and that there was no social reality outside of these individual agents. Allport still insisted upon the distinction between the social and the nonsocial however; the social “involved “social stimuli” and the individual’s response in the presence of them, social stimuli being simply other people” (Danziger 2000: 333). In Allport’s studies, for example:

“...subjects were graduate students who were given such tasks as cancelling all the vowels in newspaper materials, multiplying two-digit numbers and producing lists of associated words. Their output was measured when they worked at the same table with three or four others and when they worked alone.” (Danziger 1992: 315)

Experiments such as the above were believed to examine the ‘indirect’ effects of the social, investigating a scenario in which individuals ‘co-act’ but focus upon a stimulus rather than each other. Direct social experiments focused instead upon face-to-face interaction (Greenwood 2004b: 26). This depiction of the social is, both Danziger and Greenwood note, ‘the social in the shape of the crowd’ (Danziger 1992: 313) and takes as its basis the assumed difference between the rational individual and the madness of that same individual when surrounded by their peers; it is the difference between these two responses that constitutes the subject matter of social psychology.

It should be apparent that under Floyd Allport’s definition of the social, the phenomena constructed as social were quite different to those within the existing models of politics, sociology, and indeed previous incarnations of psychology, described above. Not only “was the environment that humans created for themselves desocialized – even tools are not social – but the social

objects that remain, that is, other people, were reduced to a concrete physical presence” (Danziger 2000: 33). What Danziger means by this second point is any influence over behaviour that membership of a particular, abstract group (being British, autistic, a member of the Labour party and so forth) had over behaviour no longer counted as social. In short, this conceptualisation of the social limited itself to "*exploring effects that were local, proximal, short term, and decomposable*" (Danziger 2000: 334, italics in original).

A number of reasons have been posited to explain Floyd Allport’s rejection of existing constructions of sociality. Firstly, Allport wished to distance social psychology from any notion of a ‘group mind’ or ‘group consciousness’ that had a reality outside of the actions of individual agents (Greenwood 2004a: 109; Greenwood 2004b: 23). Regardless of the rights or wrongs of any ‘group mind’ hypothesis, Greenwood is right when he notes that rejecting previous constructions of the social as a consequence of a dissatisfaction with group mind hypotheses:

“...was a non-sequitur, since there is no intrinsic connection between the original conception of the social dimensions of cognition, emotion and behavior and doctrines about the emergent properties of group minds.” (Greenwood 2004b: 23)

This non-sequitur was, however, an error made not only by those wishing to dismiss existing conceptions of the social, but also a great number of its proponents (Greenwood 2004b: 23-24).

Danziger notes that Allport’s rejection of group mind hypotheses served at least two purposes. Firstly, in individualising ‘the social’, Allport sought to demarcate the subject area of social psychology from that of sociology to which, as noted above, it previously lay close (see also, Samelson 2000: 500). Secondly Allport was “a man with a distinctly ideological mission; for in pushing the claims of psychology he saw himself as defending the truth of individualism against the dangerous illusions of collectivism” (Danziger 1992: 316).

Greenwood picks up on this ideological theme by considering Allport's approach to (social) behaviours which did not seem to be explicable with the framework that he advanced; such 'social' behaviours were taken to include silent prayer and motorists stopping at red lights which despite being localised within an individual, appear only to make sense when considered within the context of group membership (Greenwood 2004b: 24). Allport's response to such behaviours was rather straightforward; he declared them to be "not only statistically but also... psychologically abnormal" (Greenwood 2004b: 24). Several decades later, Floyd's brother Gordon would explicitly "associate theories of socially engaged psychological states and behavior with the forms of subservience required by totalitarian communist and fascist states" (Greenwood 2004b: 24), a move which seems to have dissuaded various psychologists from challenging the Allports' notion of the social (Greenwood 2004a: 158). The social and historical context of the Anglosphere provided the ideological backdrop against which psychological theory was to be judged and psychological subjects formed.

#### *'Social' Psychology from 1940*

Danziger claims that, during the great depression, the ideological opposition to notions of 'collective action' weakened sufficiently to allow for the possibility of experimental analysis of groups (Danziger 2000: 340). While there are other figures within the movement, Kurt Lewin is taken as the figurehead who revolutionised social psychology after World War Two, credited with "the courage to experiment on real-world problems and with the ingenuity to bring complex social situations into the laboratory" (Danziger 1992: 317). For Lewin, what was of interest was not the behaviour of the individual when alone compared to when they are in a crowd, but rather behaviour in different types of *groups*, for example experimentally manipulated 'authoritarian', 'democratic', and 'laissez-faire' groups (Danziger 1992: 317), and group phenomena such as 'group climate', 'group cohesion', and 'group decisions' (Danziger 2000: 340).

Working from within a Gestaltian tradition, Lewin contested that:

“...instances of individual behaviour had no intrinsic meaning. Their significance could be assessed only in terms of the situation in which they occurred [for example a democratic group]. Group phenomena were part of the situation. They could be observed directly, just as Gestalt patterns could be observed in visual fields.” (Danziger 2000: 341).

Lewin’s position gave social reality to groups and not individuals and, thus, the construction of the social differs notably to Allport’s:

"...the individualistic orientation had been replaced by a primary interest in the properties of groups. Methodological individualism ceased to be equated with ontological individualism. One observed individuals in groups not in order to assess the "influence" of others on a target individual but to illustrate group characteristics taken to be as real as the individuals composing the groups." (Danziger 2000: 320)

Vast portions of social psychology claimed to have been strongly influenced by Lewin, although Greenwood sees these links as largely illusory (Greenwood 2004b: 27). These groups broadly split into one of two schools; the first abandoned the laboratory and moved into observational research. The second group were impressed by Lewin's analysis of complex social phenomena within a laboratory setting but found that Lewin's Gestalt based phenomena - group cohesion and so forth - were not easily reconciled with a contemporary conceptualisation of the experiment becoming dominant:

"At the time of Lewin's death, at the beginning of 1947, the current of methodological precepts in American psychology was running strongly in another direction. Experiments were conceptualized in terms of the demonstration of functional relationships between specific stimulus elements, now known as independent variables, and specific response elements, known as dependent variables. For such demonstrations, the variables needed to be defined unambiguously and measured along a single scale, that is, treated as unidimensional. Complexity would be represented by the multiplication of variables and their essentially additive interaction." (Danziger 2000: 342)

Danziger's notation here fits into a broader observation; within experimental psychology, what it meant to conduct an experiment was fundamentally changing.

As Science Studies has long argued, there is little that is self-evident about the scientific method; it has looked different at times – most famously illustrated in *Leviathan and the Air-Pump* (Shapin & Schaffer 1985) – and looks different in different places (Dupré 1993; Knorr Cetina 1999). As with Danziger, above, MacMartin and Winston argue that, prior to 1930, the notions of independent and dependent variables were entirely absent from descriptions of what constitutes a psychology experiment (MacMartin & Winston 2000: 350). Similarly, correlational studies *were* deemed to be experimental. As might be expected, it has been noted (Lubek & Apfelbaum 2000: 409; Stam et al. 2000: 368) that the regime of truth which advocated a particularly austere definition of 'experiment' fought vigorously in its defence until, by the 1970s, the construction had become hegemonic.

As Danziger notes, when extended to the phenomena of interest to social psychology, the construction of an 'experiment' as a space where the effects of the independent variable upon the dependent variable is measured "implies a metaphysics of social situations and social conduct as composed of multitude of separate, unambiguously identifiable elements and their functional connections" (Danziger 1992: 322). Within such a framework, the writings of the Gestaltian Lewin become unintelligible (Danziger 2000: 343), as do experiments in the earlier constructions of the social:

“[The] methodological redefinition of the experiment in social psychology ensured that there was no longer any methodological space for the experimental investigation of the social dimensions of cognition, emotion and behavior in American social psychology.” (Greenwood 2004b: 29)

Thus, the 1960s saw the study of fixed or pre-existing groups almost entirely cease, being replaced by "hypothetical groups that had a purely statistical reality" (Danziger 2000: 344). These were groups formed for the purpose of a

particular experiment and who adhered to the methodological requirements of the new regime.

The requirement for participants to be randomly allocated to particular experimental groups only made sense, however, if the social was constructed in a very particular manner:

"The random assignment of individuals to experimental treatments would have the desired effect [of capturing social reality] only insofar as the relevant factors were carried by individuals in isolation and insofar as they differed on these factors *as individuals*... Only to the extent that one can think of society as an aggregate of essentially unconnected individuals does it make sense to control the effects of social background by statistical randomization." (Danziger 1992: 324, italics in original)

Within this model, 'social factors' are carried by individual agents and their social behaviour is governed by the aggregation of these factors. Individuals differ quantitatively (e.g. more or less of a given attitude) but not qualitatively (e.g. a unique autistic culture). This model is essentially similar to Allport's individualistic conception, but the social is shaped slightly less like a crowd and slightly more like a normal distribution.

Given the requirement of randomisation, social psychology was left with a choice here between history and statistics, and experimental social psychology chose statistics. Society, from this perspective, consists of statistical populations and not historical populations. Danziger retains enough optimism in psychology to believe that this model would only be accepted if it was shown to have some validity outside of the laboratory. Certainly some 'social' groups do not appear to cohere to this new model; we cannot randomise members of a family for example. However, two such environments existed which closely mimicked the statistical society posited by psychology and which were of great interest to those funding research: the army and the classroom.

*'Social' neuroscience*

With work in the neurosciences, which have accelerated rapidly within the last twenty years (Beaulieu 2003: 561) and which can be traced in their contemporary forms to the 1960s (Abi-Rached & Rose 2010: 20), on the one hand informing, and on the other supplanting, research within experimental cognitive and social psychology, the notion of a ‘social brain’ has begun to be incorporated into the lexicon of psychology (Rose & Abi-Rached 2013: 141). Allan Young has critically examined this notion of the ‘social brain’ within cognitive and social neuroscience and concludes that the ‘social’ in the social brain simultaneously means two things (Young 2011: 642; Young 2012a: 164; Young 2012b: 402). Firstly, the brain is ‘social’ because of what it *does*; it is concerned with interpersonal conduct. In this sense the social brain is continuous with existing constructions of the social from within social psychology, outlined above. Aside from the explicit turn to biology, there is a further change of focus within this body of literature: there is far less research into the social *per se* and far more into empathy, a ‘social emotion’ believed to be the ‘glue of the social world’ (Young 2012a: 170, 2012b: 414). Considerations of empathy broadly centre around mirror neurons (Young 2011: 642-644), first described in 1992 (di Pellegrino et al. 1992), and believed to be a class of neurons that respond to an action both when an individual performs that action and when an individual sees another individual performing the same action. While the nature of mirror neurons remains highly controversial (see, for instance, Heyes 2010), it has been proposed that it is through mirror neurons that individuals are able to assign goals, intentions and beliefs to other agents (Gallese & Goldman 1998). This centralisation of the ability to understand the mental states of other persons, to empathise and take their perspective is, for Young, one of the hallmarks of the social brain.

A second aspect of the social within the social brain literature, absent from psychological narratives for decades (Matusall et al. 2011: 10), is concerned not with what the brain does, but with how it was *formed*. Specifically, there is a claim that the brain has evolved in the manner that it has, not only because of physical environments (e.g. the cognitive capacity to hunt and light fires) during the so-called ‘era of evolutionary adaptedness’, but also because of the social environments in which it found itself. Specifically,

with regards to this social environment, Young traces an evolutionary ‘myth’<sup>4</sup> which has been constructed in order to explain the centrality of empathy to the social brain.

The myth of empathy is centred around what Young calls ‘the problem of one and many’ (Young 2012a: 168-169), namely how do societies form when they are comprised of individuals who are taken to be self-interest maximisers? It is worth tracing the tangled journey which leads to the solution: empathy. That journey begins with the problem of altruism which was so key to early debates around sociobiology (Brown 2002). This problem of altruism was apparently solved with concept of kin selection, the claim that altruistic behaviours can be self- (or rather gene-) interested if related kin can be spared whatever fate was to become them by an altruistic act. Kin selection, in turn, raises the issue of altruism in non-kin, a problem solved if social groups were stable enough to allow the possibility of reciprocity of altruistic acts. And yet, an assumption that those benefiting from altruistic behaviours will reciprocate those acts raises the spectre of free-riders who simply won’t reciprocate. The possibility of free-riders is solved by introducing a desire to punish those individuals who do not contribute to the social group’s wellbeing, although this leads back to the initial problem of altruism, as those doing the punishing are risking themselves for no immediate gain. This final problem is solved by the evolution of *schadenfreude* and a pleasure in giving punishment. Experiencing *schadenfreude* requires an understanding of others mental states, empathy, thus empathy sits at the heart of the social.

This elaborate back-story, concluding with the centrality of empathy, leads to a certain tension in contemporary discussions. Firstly, empathy is

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<sup>4</sup> When Young describes an evolutionary ‘myth’ the term is not taken to imply anything as straightforward as a story that has been ‘made up’. Rather, and drawing upon the historian of ancient Greece and Rome Paul Veyne (Veyne 1988), Young describes an evolutionary back-story that coheres as a whole despite being largely implicit, unarticulated, and “erected piecemeal” (Young 2012b: 11). By deploying this terminology alongside an evocation of Veyne, we are left in little doubt that what Young has provided is, in the true Foucauldian sense, one of the first genealogies of the social brain.

decidedly pro-social; human kindness to others and a sense of morality are innate facts of our biological existence, a ‘moralisation of biology’ that has recently been given detailed consideration by both Meloni (Meloni 2013) and Matusall (Matusall et al. 2011: 11; Matusall 2013). Simultaneously, however, empathy has evolved out of a need to punish *and* enjoy this punishment. The status of empathy, and the social, is thus conflicted within this narrative.

As Young has been careful to stress, not all researchers within fields examining the social brain adhere to or are aware of this proposed, complex genealogy. This string of partial histories is why Young stresses the notion of the ‘myth’ of empathy:

“The story is... implicit, in that it is part of the collective consciousness of social brain researchers, but there is (as yet) no occasion when it is retold in its entirety. Individual investigators are not necessarily familiar with, or subscribe to, all of the episodes. While individual episodes can be (and are) operationalized through empirical research, the entirety – extending perhaps seven million years – can only be inferred, and a researcher can justifiably claim that her work is confined to the topic that currently attracts her attention.” (Young 2012b: 409)

Thus, that Meloni analyses trends within primatology and notes far more of an emphasis upon pro-social behaviour than upon punishment (Meloni 2013: 89) does not contradict Young’s thesis; we need only look towards evolutionary psychology to find the alternative emphasis upon punishment and ‘cheater-detection’ (Buss 1995: 17; Buss et al. 1993: 544).

The key arguments put forward by those who have examined the ‘social’ of the social brain are, therefore, firstly that empathy has been centralised within the accounts of neuroscientists and, secondly, that there is a consideration of social history absent from the existing social psychological literature. This inclusion of this historical narrative is a significant change from previous conceptions of the social given the a-historical nature of the social within experimental social and cognitive psychology, detailed above. Nonetheless, the history relied upon here is an a-cultural biological history

rather than a history concerned with culture, institutions, and so forth. Within the discourses of the social brain, the social remains in the shape of a crowd, albeit crowds that traversed the savannahs of the Pliocene as well as the school playing fields.

With regard to the newly found focus upon empathy, it could be argued that this shift towards an examination of the ‘glue’ of the social world indicates that, for neuroscientists, the social is no longer at stake, that it is now the workings of the concept that are being questioned rather than the concept itself. With a little reflexivity, it might be noted that this also seems to be the case for Young who, though thoughtfully questioning the manner in which empathy has become central to concerns over interpersonal, never seeks to question how interpersonal itself came to be articulated as social. Indeed, Young states that the "brain is undoubtedly social" (Young 2012a: 164) suggesting that that concept is beyond critical interrogation. That claim, as this review has sought to show, is a contentious one and it is a goal of this project to examine the nature of the social itself.

It should finally be noted that centralising empathy within contemporary constructions of the social opens new avenues of research, new ways to test hypotheses. Given that autism is so often characterised in terms of a lack of empathy it is unsurprising that the condition, alongside similarly classified disorders of sociality such as psychopathy and alexithymia, has begun to take on importance within narratives of the social brain (Young 2012a: 165; Young 2012b: 405). Continuing a long standing tradition within the psy-disciplines, personified in Phineas Gage<sup>5</sup>, of examining ‘normal’ cognitive functioning in those cases where that function is perceived to be lacking, autism (or more specifically the high functioning child with Asperger’s) has come to stand at the centre of research into the social. Within the contemporary power-knowledge nexus of social and cognitive neuroscience autism offers a pure case of human-minus-social. The individual with autism has thus become an example of what Viney has called inherently useful humans, useful “not for what they do, simply for what they are” (Viney

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<sup>5</sup> See Kotowicz (2007) for an analysis of Gage’s case within the contemporary psy-disciplines.

2013). Given this obvious utility, how could autism not have risen to prominence?

*Constructing autism*

In a review article Chloe Silverman notes that:

“The idea of ASD—as a metaphor, a neurological disorder, a mental state, an increasingly prevalent diagnostic category, or a species of neurological difference—has an almost ready-made appeal for social scientists, concerned as they are with questions of human identity, difference, perception and subjectivity within a social and cultural context.” (Silverman 2008: 325)

As a field, Disability Studies has perhaps shown the most prolonged interest in autism (e.g. Brownlow et al. 2006; Chamak 2008), noting that the messages of autistic self-advocates chime with broader critiques of the medical model of disability stemming from within the social sciences<sup>6</sup> (e.g. Hughes & Paterson 1997; Oliver 1990; Shakespeare & Watson 2002). Anthropologists have shown similar emancipatory interests when conducting:

“...ethnographically informed social science research focusing on ASD as a personal, family, and community/social group experience as evidenced through the analyses of social interactions, narrative accounts, and participation and engagement in activities in the home and educational, clinical, and other institutional settings.” (Solomon 2010: 242)

This literature (e.g. Bagatell 2010; Grinker 2010; Kaufman 2010; Ochs & Solomon 2010; Prince 2010; Sirota 2010; Sterponi 2010; Solomon 2010a; Solomon 2010b) has sought to investigate questions of identity formation within the autistic community and, thus, frequently seeks to undermine claims from within the psy-disciplines that individuals with autism are ‘asocial’. Similar claims are to be found within the work of Tom Muskett (Muskett et al.

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<sup>6</sup> Likewise, critiques of the social model of disability (e.g. Dowse 2001) equally apply. Indeed, the issue of who is able to self-advocate and provide their voice is particularly relevant to a disorder such as autism in which communication difficulties are a central deficit (see chapter 3: 65-66).

2010) who has sought to continue the legacy of a conversation analysis approach to disability (e.g. Rapley 2004) by examining the complex interactional abilities of individuals usually believed to lack such skills.

These studies make an important contribution, elegantly showing, for example, the complex influence of group membership upon the identity formation of particular individuals with autism (Bagatell 2007). They do not, however, directly influence the present study, concerning themselves with neither scientists nor the scientific concept of autism. Surprisingly, such work remains reasonably sparse, although the number of contributions is increasing rapidly. The rest of this review will be broadly divided into two sections; the first concerning itself with the historical emergence of autism, the second with a sociology of (scientific) knowledge surrounding the concept.

#### *The historical emergence of autism*

As outlined above, the 19<sup>th</sup> century saw two forms of knowledge emerge which allowed for the creation of new types of object. Firstly, ‘biopolitics’ a form of knowledge concerned with populations rather than individual bodies, intensified to a quite spectacular degree sometime around 1820 (Hacking 1983: 292). Secondly, memoro-politics (knowledge and governance of the soul) emerged at the end of the nineteenth century as part of an ‘event’ extending between 1875 and 1925 (Rose 1985: 3). Unsurprisingly, scholars have viewed these developments in the 19<sup>th</sup> century as a crucial pre-history to the development of autism (e.g. Nadesan 2005: 37). There have been, however, three twentieth century events which have been repeatedly linked with the emergence of autism in its contemporary form.

The first event believed to have been of particular importance was the emergence of a surveillance over the mental health of the child, and extension over the surveillance of bodies, believed to have occurred in the 1920s and 1930s. Certainly:

“Early nineteenth century texts did recognise disorders of the mind in infancy and childhood, but throughout the nineteenth century they were accorded a somewhat ambiguous status. On the one hand, the child had

a kind of immunity from insanity, on account of its under-development, simplicity and freedom from stress. Where children did suffer insanity it was of simple form: infancy was not exposed to many of the predisposing and exciting causes which operated at other periods of life; fewer faculties had developed and therefore fewer faculties were likely to be assaulted by disease; the delicacy of the infant brain made it likely that morbid changes would lead to death.” (Rose 1985: 177)

This situation began to change towards the end of that century, with the child beginning to come under the medical gaze (Armstrong 1983: 13-14; Nadesan 2005: 45-46), and “the 1920s and 1930s in Britain witnessed a vast expansion of charitable and governmental services to cater for the psychological problems of children” (Evans 2013: 3; see also Armstrong 1983: 27; Nadesan 2005: 67; Rose 1985: 176). As Armstrong has stated, it was during this period that:

“As with physical development, psychological growth was construed as inherently problematic, precariously normal. The initial solution was for psychological well-being to be monitored and its abnormal forms identified... The nervous child, the delicate child, the enuretic child, the neuropathic child, the maladjusted child, the difficult child, the neurotic child and the *solitary child*, all emerged as a new way of seeing a potentially hazardous childhood.” (Armstrong 1995: 396, emphasis added)

Given that research “relentlessly focuses on the figure of the child when seeking to explore what autism is and what it might mean” (Murray 2008: 139), it seems likely that knowledges of childhood were a prerequisite for the emergence of autism, and it was during the 1920s and 1930s, immediately preceding Kanner and Asperger, that these knowledges of childhood, and the requisite structures to monitor that knowledge and those children, came into existence.

A second event attributed particular importance within the history of autism was the widespread process of deinstitutionalisation that occurred

around the 1960s. Within the United Kingdom this deinstitutionalisation followed the Mental Health Act of 1959 (Evans 2013: 11) although, as analyses by Eyal et al. (2010) demonstrate, this deinstitutionalisation occurred throughout the Anglosphere. Indeed, within their text *The Autism Matrix* Eyal et al. award deinstitutionalisation *the* central place in the history of autism, claiming that:

“The current rise in autism diagnoses, we argue, should be understood as an aftershock of the real earthquake, which was the deinstitutionalization of mental retardation that began in the late 1960s.” (Eyal et al. 2010: 3)

It is argued by both Eyal and others that while deinstitutionalisation was, in part, brought about because of the increased surveillance over childhood (described above), it was deinstitutionalisation itself that further encouraged surveillance “as it became paramount to demarcate the mental problems and needs of children and adults who had previously been confined but were now being integrated into the population” (Evans 2013: 11).

The claims of Eyal et al. are compelling and, at least initially, intuitive. Certainly it seems to be the case that particular forms of interpersonal (dis)ability are far more likely to be of interest when individuals engage with other individuals; something far more likely to happen (or at least, be of concern) outside of the Total Institution. Further, Eyal et al. offer some strong evidence in support of their claim. Firstly, states (such as Massachusetts, Minnesota, and Oregon) and indeed States (United States, United Kingdom, and Scandinavia) that have embarked on the most widespread programmes of deinstitutionalisation are those that have the highest prevalence rates for autism; the opposite is also true, with low levels of deinstitutionalisation coupled with low levels of autism (Ohio, West Virginia, Oklahoma; France; Evans (2013: 3) makes a similar claim).

Secondly, Eyal et al. claim that the changing nature of the individuals diagnosed with autism is also demonstrative of diagnostic substitution (Eyal et al. 2010: 80). In the past there was a correlation between social class and autism prevalence, with poorer persons far less frequently diagnosed. Indeed, as Eyal et al. note, all 11 of Kanner’s original subjects were white and from

middle-to-upper class backgrounds. Over time, Eyal et al. claim that this correlation between class and autism seems to have disappeared (Eyal et al. 2010: 80; although see King & Bearman 2011). This disappearance, claim Eyal et al., is indicative of the increased surveillance of childhood that has occurred post-deinstitutionalisation. In the 1940s wealthy parents were those most likely to reject a diagnosis of ‘mental retardation’ and procure a second diagnosis, which Kanner offered in the novel form of autism. Since the programmes of deinstitutionalisation, however, all children are under vastly increased surveillance (placed as they are in the family home and a comprehensive educational system) and therefore behaviours typical of autism – but, perhaps, irrelevant within the confines of the institution – are surveilled and diagnosed. This process explains, at least according to Eyal and colleagues, the ‘autism epidemic’ and the no-longer-significant correlation between autism and class.

Convincing as they may be, Eyal et al.’s arguments seem slightly speculative and are certainly open to critique. Firstly, correlation is not causation<sup>7</sup> and it may be possible to reach numerous conclusions with a similar level of face validity to the retroductive analysis of Eyal et al.. Perhaps, for example, ‘progressive’ liberal States are simply more likely to offer diagnosis of autism with the aim of supporting families *and* simultaneously resist the incarceration of their citizens? Eyal et al.’s analysis cannot discount such possibilities. Secondly, it might be speculated that two effects would be expected if deinstitutionalisation were the driver of the contemporary autistic concept. Firstly, and as Eyal et al. claim, we might expect associations with class to diminish and, as discussed above, there does indeed seem to be some (disputed) evidence for this. However we might also expect I.Q. to *decrease* in individuals diagnosed with autism following deinstitutionalisation as those previously considered to have (reasonably) profound disabilities were moved into the community. In the case of autism, however, I.Q. has *increased* in recent decades, to the point where it can now realistically be questioned if intellectual disability is associated with autism at all. The increased diagnosis of individuals with high functioning autism or Asperger’s Syndrome, who

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<sup>7</sup> At least, not since the 1930s.

often have jobs and families, seems to be a much harder phenomenon to explain without additional processes to deinstitutionalisation. As Nadesan has noted, while autism “is a disorder of the early twentieth century... high functioning variants...are fundamentally disorders of the late twentieth and early twenty-first centuries” (Nadesan 2005: 3). This is not, however, to dispute that processes of deinstitutionalisation have been anything other than crucial for the emergence of social disorders, as discussions of autism within an educational settings reveal those sites to be potent areas of concept formation (Fein 2011).

Finally, a third crucial moment in the history of autism is taken to be the move away from psychoanalytically inspired conceptualisations of autism, which dominated until the 1960s, and towards the constructions of the cognitive and biological sciences. For while Murray has identified the ‘ghost of Bettelheim’ within contemporary narratives of autism (Murray 2008: 190) and Nadesan points to the impact that psychoanalysis continues to hold with regards to lingering metaphors of fortresses, shells, and an inner-self seeking release (Nadesan 2005: 175), it is undoubtedly the case that autism is now understood as a neurodevelopmental disorder and within the realms of the biological and cognitive sciences. Eyal et al. (2010: 172) see this shift, too, as being instigated by deinstitutionalisation, claiming that parents became far more powerful figures after their children returned from various institutions in order to live at home<sup>8</sup> and were much more comfortable with the ontological assumptions of the biological and cognitive sciences than they were the ‘parent blaming’ of those influenced by psychoanalysis such as Kanner (Murray 2008: 171-173), the Tinbergens (Silverman 2010), and most infamously Bruno Bettelheim.

Evans has noted that the move from psychoanalysis to a bio-cognitive model involved a significant change in dominant methodology. As noted above (Danziger 2000: 344), psychology had been largely statisticalised by the middle of twentieth century and, thus, as autism research moved towards experimental psychology in the 1960s what was required was an examination

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<sup>8</sup> For the role of parents in the knowledge production of autism, see below.

of statistical populations. By comparison, psychoanalytic reports remained largely based upon case studies. This move away from case report and towards epidemiology and statistics, claims Evans, ushered in significant changes within in the field:

“The most significant development following the introduction of statistical and epidemiological methods in child psychiatry has been the expansion of behavioural, communicative and cognitive categories and the virtual disappearance of the concepts of child hallucination and fantasy. Autism was thereby disassociated from the key concept of descriptive psychopathology – hallucination.” (Evans 2013: 21)

Thus, just as for conceptualisations of the social, changes in methodology appear to have instigated significant changes in the constructions of autism, with the previously central phenomena of fantasy and hallucination becoming divorced from the concept. Nadesan further claims that these changing methods allowed the space for an examination of the modularisation of cognition, meaning that it became possible to think of specific cognitive deficits as causing autism outside of an interactional framework (Nadesan 2005: 120, and considered at length chapter four).

While research is limited, therefore, these three events – increased surveillance over childhood, deinstitutionalisation, and the shift towards the statistical sciences – have been evidenced as fundamental to the contemporary concept of autism.

#### *Autism and knowledge creation*

There is no doubt that advocacy groups have held and continue to hold a crucial role in the development and current understanding of autism. Alongside longstanding American (1965) and British (1962) national advocacy groups, groups today exist in Turkey, Canada, France, Israel, Japan, South Africa, Sweden (Eyal et al. 2010: 53), as well as 27 ‘developing countries’ (Feinstein 2010: 233-264). The United Kingdom’s Autism Accreditation Programme lists over 300 organisations in the country concerned with autism (The National Autistic Society 2007), and there are over 3,000 such groups in

the United States (Eyal et al. 2010: 1). These groups range from large, long standing umbrella organisations such as the UK's *National Autistic Society* and the USA's *Autism Speaks* to a multiplicity of small, local and niche organisations. Many of the individuals involved in these groups became involved following the autism epidemic of the 1990s and thus entered the field of lay advocacy at time when, following the concerted efforts of groups concerned with Acquired Immunodeficiency Syndrome (Epstein 1995; Epstein 1996), women's health (Anglin 1997; Blackstone 2004; Gibbon 2008; Klawiter 2004), and Muscular Dystrophy (Rabeharisoa 2003; Rabeharisoa & Callon 2002), lay involvement in scientific knowledge creation was a well established phenomenon (Stewart 1990).

Given the rich history of the advocacy movement within autism, it is perhaps unsurprising that parents have become frequently examined figures in the literature on autism, with the 'lived experience', coping strategies, and perspectives of parents considered at length (Brewin et al. 2008; Gray 2002; Gray 2003; Gray 2006; Hastings et al. 2005). More importantly for the current project, however, a small body of work has begun to consider the role of parents and advocacy groups in relation to knowledge creation and concept construction in autism.

As noted above, Eyal et al. claim that parents became increasingly important figures within autism research following the waves of deinstitutionalisation that occurred throughout the 1960s. While experimental psychologists such as Ivar Lovaas and Eric Shopler actively sought parental support and engagement (Eyal et al. 2010: 167-193), the generation of psychiatrists and psychoanalytically inclined psychologists before them did not, frequently being accused of 'parent blaming'. That such theories, unsurprisingly, caused outrage among parents is well documented; one of the first parent authored texts on autism was in direct opposition to psychoanalysis (Rimland 1964), and that author is on record as having lengthy and occasionally heated correspondence with Bettelheim (Silverman 2004: 118-121). Within the body of work to consider knowledge creation in autism it is convincingly argued that the shift to consider autism, not as an affective inter-

personal condition, but as a neurodevelopmental disorder was at least partially provoked by parental advocates. That psychoanalytic theories continue to cause consternation amongst parents is certainly true (see Feinstein (2010), for example). That scientists respond to this parental consternation by shaping their research in order to avoid such sensitive issues seems similarly likely. Thus, if only in determining the types of questions which can't be asked and the forms knowledge which cannot be considered (Nadesan 2005: 175), the ghost of Bettelheim continues to haunt the corridors of autism research centres.

Chloe Silverman (2008) draws upon the work of Epstein (1995) and others in detailing how both parent and self-advocacy groups embody a form of biosociality (Rabinow 1999) in relation to autism, claiming that:

“The act of speaking for people with ASD is legitimated by multiple affinities built on genetic association and physiological likeness, or by the idea of heritability and the affective claims of parenthood.” (ibid: 39-40)

Silverman outlines this ‘genetic kinship’, stating that parents use genes as a way to talk about “affection, love, community, and innate understanding” (Silverman 2008: 39). This finding relates to a broader thesis of Silverman’s that runs throughout her body of work (Silverman 2004; Silverman 2008a; Silverman 2010; Silverman 2012). Silverman’s claim is that within biomedicine “Love is an advantage and a technique rather than a liability” (Silverman 2012: 99) and that the role of love in knowledge creation has been largely undervalued. With specific reference to autism, Silverman claims that focusing upon love as an analytic tool achieves several goals:

“First, it illustrates the degree to which ways of representing autism depend on particular institutional and epistemological arrangements. Second, it shifts the focus from psychiatrists, epidemiologists, and geneticists to parents, counsellors, diagnosticians, and lawyers, as they try to make sense of and apply systematic, authoritative knowledge in their daily lives and work. Third, and most important, in describing changes in autism over time and how expert knowledge works in

practice, it highlights the centrality of love as a way of knowing about bodies, persons, and relationships in biomedicine.” (Silverman 2012: 3)

In this sense, Silverman’s work is placed very much within the cannon of (feminist) science studies which has sought to emphasise that knowledge creation within the sciences is dependent upon a far greater number of individuals and institutions than simply those working within the laboratory (Latour 1987: 27), that a far greater number of techniques are required to make science work ‘in practice’ than is usually allowed within the scientific method (Collins 1974: 177), and that, in particular, forms of knowledge production associated with women are consistently ‘othered’ and deemed unimportant or potentially destructive (Keller 1985: 175).

Silverman claims, too, that focusing upon love shifts attention away from scientists and towards parents, teachers, and lawyers. However, conducting scientific research is an inherently emotional activity (Pickersgill 2012b), and Fitzgerald has argued that emotion is absolutely central to the activities of autism scientists, in particular claiming that “an unimpeachably scientific, laboratory-based work of looking for, and thinking about, the neurobiology of autism is often an emotional and affective labour too” (Fitzgerald 2013: 133). Indeed, Fitzgerald claims of a particular interviewee that:

“...the synchronicity of this researcher’s capacity to *think* autism and her willingness to *feel* it [is of importance]. My suggestion is that it is precisely her ability to trace her science through these two experiences, to memorialise them and articulate them together, which enables her to continue to push through the very complex work of autism neuroscience.” (Fitzgerald 2013: 143, italics in original)

It seems to be the case, then, that research into autism is not only a condition concerned with socio-emotive states, autism as a construct looks the way it does because of the affective labour of parents, scientists, educators, and of course those with autism themselves.

## Conclusion

This literature review has sought to walk a line between description and prescription (Butler 1988: 530), describing bodies of knowledge which begin to make the emergence of autism an intelligible phenomena whilst simultaneously prescribing a framework under which the current project can proceed. Focusing first upon description, Nikolas Rose has said that the scientific discourses of psychology were made:

“...possible by the existence of certain ways of thinking about populations, statistics, evolution and heredity, by certain theories of nature, origin and treatment of mental pathology, and by certain conceptions of the role and objectives of good government and the laws of economic and social life.” (Rose 1985: 7)

This review has sought to show, in uniting three disparate literatures, that autism can be understood in the terms outlined by Rose. Firstly, it has been shown that knowledges about populations (alongside bodies and souls) that emerged during the 19<sup>th</sup> century were crucial in forming psychological subjects prone to certain forms of abnormality. Secondly, that the statisticalisation of the psychology experiment reinforced a certain conceptualisation of the social, a conceptualisation that centres empathy within human evolutionary history and thus claims that disorders of empathy are central to understanding the human condition. And, finally, literatures arguing that the governing of economic and social life have been central to the construct of autism have been presented, with claims that of policies of deinstitutionalisation, and the activities of advocacy groups hold significant sway over scientific proceedings in relation to autism. Given the entanglement of these phenomena, it is perhaps no wonder that researchers talk of matrix of possibilities (Hacking 1975: 15), intelligibility (Butler 1990: 17) or grids of visualisation (Rose 1996a: 130).

Prescriptively, the literatures that have been presented provide theoretical context and avenues of explanation for the current project. Three questions were suggested at the beginning of this review. Firstly:

- What does it mean to say that an individual, or a particular behaviour, is or is not ‘normal’? How has normalcy come to look as it does?

Here, a discussion of Canguilhem’s work was presented and it was argued, given the degree of historical variance, that an analysis of the relations between normal and pathological social behaviour would, in the case of autism, be illuminating. By considering Canguilhem’s work in relation to Foucauldian processes of subjectification (and the specific description of ‘experience’ which arises from this perspective), a particular approach to the above question has been demarcated, whereby experience of abnormality is deemed the *result* rather than the cause of socio-historical (including scientific) processes. This is the perspective that will be taken throughout this thesis.

A second question asked was:

- What is meant here by ‘social’? How have the psy-disciplines come to articulate and classify particular behaviours as ‘social’ and others as ‘nonsocial’?

An analysis of historical constructions of the social from within experimental psychology and social neuroscience has revealed considerable variability in constructions of the social. Given that autism is articulated as a social disorder it seems a reasonable conclusion to suggest that such constructions of the social may be an important factor in constructions of autism and, again, this thesis will seek to investigate this question.

Finally, it was asked:

- How have descriptions of (ab)normality and (non)sociality from within the psy-disciplines congealed into the form of autism?

The literature considering autism from social scientific perspectives has suggests numerous factors contributing to the emergence and rise of autism over the last seventy years. Nonetheless, constructions of normalcy and pathology and, perhaps most notably, constructions of the social are almost entirely missing from such analyses. There does, therefore, seem to be scope for significant novel findings within the current project.

## **Reflecting on method(ology)**

### Introduction

Pierre Bourdieu has claimed that in order for sociology “to produce and to reward reflexive scientific habituses, it must in effect institutionalize reflexivity in mechanisms of training, dialogue, and critical evaluation” (Wacquant 1992: 41). Those who have attended post-graduate methodology courses will confirm that, if Bourdieu remained with us, we would be able to return good news to the Collège de France. The types of reflexivity deployed by today’s PhD students are diverse (Stronach et al. 2007) and may not be entirely to Bourdieu’s liking, but the concept itself is so mainstream that it holds prominent positions in even the most mainstream of methods textbooks (Bryman 2008: 682; D. Silverman 2010: 123).

This is not to suggest that we should be blasé about reflexivity or diminish its importance, but it is to suggest that it means different things to academic traditions and that we need to be reflexive when considering reflexivity and consider that, following deconstructions of the concept, it has become deeply problematised (Lynch 2000). This methodology section will be based around an extended reflection on the project from three different, although overlapping, levels in relation to the current project, researcher, and academic discipline. Justice will not be done to these conversations on reflexivity, and the prospect of a definitive solution is non-existent, but it is an important step nonetheless.

### The project in hand

#### *2.1 Methods*

Two principal, explicit methods are used throughout the analytic chapters (4-7) of this thesis; semi-structured interviews and a critical analysis of published literature. Document analysis dominates the first of these analysis chapters, concerned as it is with historical constructions of autism, while interview data are to the fore in the latter three.

#### *Critical analysis of the literature*

Analysis of the published literature is certainly not novel within this field (e.g. Danziger 1990). Nonetheless, the decision to draw upon printed sources rather than rely solely upon interviews with the key figures that were available for contact was a considered decision, and one made for several reasons. Firstly, given the questions of interest to the current project, it was deemed important for there to be some historical, as well as sociological, scope. Aside from the particular difficulties inherent in using sociological methods and data to explore questions of history, an analysis of the literature also allowed a much wider range of materials to be of use than would have been the case if qualitative interviewing were the only method used.

During the 1980s experimental research into autism was still a nascent field, with relatively few practising researchers. What is more, and despite being a relatively recent period, the majority of researchers senior to the field during the 1980s have now ceased to be actively involved in research, leaving only a handful of participants with personal experience of the field available for interview. Therefore, the possibility for any form of rigorous interrogation of autism research in the 1980s, even as it continues to exist in the form of memories, seemed improbable without reliance upon written sources. Introducing a critical review to the project allowed for an increased sense of rigour and an exhaustive reading of the primary literature.

The rigour and potentially endless wealth of empirical material does not, of course, make literary analysis a method beyond reproach. Perhaps the most pertinent critique in relation to Science Studies is that offered by Latour, namely that such an analysis breaks the so-called first rule of method:

“We study science *in action* and not ready made science or technology; to do so, we either arrive before the facts and machines are blackboxed or we follow the controversies and reopen them.” (Latour 1987: 258, italics in original)

By studying only the published literature, it could be argued, we have missed the action. If science is to be characterized as the two-faced Janus (Latour 1987: 4) then we may have arrived when only the left facing ‘ready made science’ remains visible, the right face already ‘blackboxed’. Even accepting

that we see only a partial picture of the sciences within the published literature, we may tentatively still launch two counterclaims to suggest that, while the method certainly does not provide unproblematic access to the truth, it is still among the best options available for current purposes. Firstly, we may claim that it would be exceptionally hard, if not impossible, to view autism research from the 1980s *in action*. As Latour has said in relation to the construction of new objects, “genealogy and archaeology of this sedimented past is always possible in theory but becomes more and more difficult as time goes by” (Latour 1987: 92) and this would seem to be the case in the current instance. A second counter would be that very little in autism is truly ‘blackboxed’ and beyond dispute. It may well be the case that any disputed claims surrounding models of autism and sociality are indeed more open to examination through the literature than in other areas of research.

#### *Critical analysis of the literature: Topics and Sampling*

Critical reading did not begin with any predetermined methods, topics or historical periods of particular interest. Contemporary text books (e.g. Volkmar et al. 2005) conclude that three psychological theories of autism - *Executive Dysfunction*, *Weak Central Coherence*, and *Theory of Mind* (and their associated offshoots) – dominate the field today. Further research seemed to show that these three theories all emerged during the late 1980s and, thus, an interrogation of that time period was deemed important. While reading the literature it became apparent that a theory known as the *Emotional/Affective Theory*, most readily associated with Peter Hobson, was also widely cited during the 1980s and, thus, that theory also came under consideration. Indeed, quite why the emotional/ affective theory of autism is no longer deemed important became a key question. Ultimately, some 66 articles and books from the time period were considered, and readings only ceased when it seemed that a natural break (either temporally or in research interest) had been met.

#### *Qualitative interviews*

The use of qualitative interviews within social science research is popular and, perhaps, growing in response to societal change (Gubrium et al. 2012: 1). Nonetheless the decision to ‘get at the action’ with interviews rather

than ethnography, a methodology with such strong traditions in Science Studies (e.g. Latour & Woolgar 1986) and considered by many to be the “gold standard” in qualitative research more generally (Murphy & Dingwall 2003: 54), should clearly not be predetermined. In this instance, the decision was easier than might be the case in other projects. The plethora of viewpoints and competing claims surrounding autism was a determining factor in choosing the condition as a case study through which to analyse models of sociality and the construction of pathology. The extensive time commitments required of alternative qualitative methods such as ethnography would have prohibited the sampling of these wide-ranging views and would have been more appropriate if a single site was of particular interest. Accordingly, the use of interview was deemed to be the most suitable method for the current project.

The claim, seemingly inherent in the decision to utilise qualitative interviews, that the method gives at least a partial awareness of the practises of scientists and their views on various topics, is a claim that needs to be considered in the light of numerous, vigorous critiques. Certainly, the claim regarding the generalisability of interview data has been frequently questioned (Hammersley 2008: 89-100). Indeed, two of the most vocal critics of the qualitative interview as an unproblematic window on participants’ experiences are associated with the institution inside of which this thesis is being written. Robert Dingwall and Elizabeth Murphy have made frequent critiques of interviews along the following lines:

“The interview is an artefact, a joint accomplishment of interviewer and respondent. As such, its relationship to any ‘real’ experience is not merely unknown but in some sense unknowable.” (Dingwall 1997: 56)

Indeed, Dingwall and Murphy believe that there is a good reason to doubt that there is much of a relationship between interview data and ‘real experience’:

"Treating interviews as social interactions, in which all parties strive to present themselves and their behaviors to their listeners as appropriate, has radical implications. Interviews do not yield more or less adequate reports on mental states underlying behavior. We recognize them instead as occasions on which informants are called upon to offer

"accounts" for their actions, feelings, opinions, and so on. In providing these accounts, informants seek to present themselves as competent and, indeed, moral members of their particular communities. Interviews are occasions for informants to display themselves as adequate parents, good patients, well informed citizens, responsible adults, and competent professionals - or to produced socially acceptable explanation of their failure." (Murphy & Dingwall 2003: 95-96)

As Hammersley notes, the essence of the critique put forward here is that "interview data can only tell us what goes on in interviews, or perhaps even just what went on in a particular interview" (Hammersley 2008: 89). This *strong critique* of the qualitative interview, which can be summarised by stating that knowledge gleaned from interview is entirely insular and does not relate to scenarios outside of that interview, is in need of serious consideration for the consequences for the interpretation of the interview data obtained for this project are of some importance.

To a significant extent, the degree to which one thinks that the, or a, truth can be obtained via the qualitative interview depends upon one's general epistemological and ontological commitments, whether one accepts "postmodern trends" (Borer & Fontana 2012: 45) and "Dadaist alternatives" (Hammersley 2008: 128) within research and whether one believes a truth is to be found anywhere. If one does not believe that there is a truth to be found anywhere, then the strong critique of qualitative interviewing is a misnomer; one is actually advocating a strong critique of all knowledge claims, interviews included. These questions of relativism are raising fundamentally different queries, and they will be considered in the final sections of this chapter. Of more immediate interest are critiques aimed specifically at interview methodologies.

Perhaps the most famed methodological traditions to pose specific questions of qualitative interviewing are those drawing influence from ethnomethodology and/or discursive psychology. Potter and Hepburn (2005), for example, have produced a critique specific to qualitative interviewing.

Potter and Hepburn parse that critique of interviewing into two classes; the first class are problems which they see as frequent although ultimately rectifiable errors, the second class as being “endemic and inescapable” (Potter & Hepburn 2005: 282).

The former class of problems, those frequently found within interviews but that are not inherent to the method, can be comprehended under the rubric of a “failure to consider interviews as interaction” (Potter & Hepburn 2005: 291). Potter and Hepburn offer several examples of this failure to contextualise interview data. Firstly, the importance of activities of the interviewer prior to, during, and after the interview are often excluded from research outputs, mimicking the natural sciences’ “gaze from nowhere” that comes under such frequent attack (e.g. Haraway 1988: 581). Secondly, the talk of the interviewee is often made to cohere to the standards of written (academic) text and “rendered as a playscript” (Potter & Hepburn 2005: 285) in blocks of text consisting of formal prose.

The danger of treating data as being outside of an interactional framework, and the possibility for violence contained within that approach, has been considered at length by Donna Haraway (2004). Haraway discusses Sojourner Truth’s famous speech of 1851, *Ain’t I a Woman*, the text of which now ‘adorns the walls of offices in Women’s Studies departments across America’:

"That written text represents Truth's speech in the white abolitionist's imagined idiolect of The Slave, the supposedly archetypical black plantation slave of the South. The transcription does not provide a southern Afro-American English that any linguist, much less actual speaker, would claim. But it *is* the falsely specific, imagined language that represents the "universal" language of slaves to the literate abolitionist public, and this is the language that has come down to us as Sojourner Truth's "authentic" words. This counterfeit language, undifferentiated into the many Englishes spoken in the New World, reminds us of a hostile notion of difference, one that sneaks the masterful unmarked categories in through the back door in the *guise* of

the specific, which is made to be not disruptive or deconstructive, but typical.” (Haraway 2004: 58, italics in original)

Haraway notes that Truth was from New York, not the South, and probably spoke in an Afro-Dutch dialect. Versions of Truth’s speech now exist in such dialects and these exist alongside the original as well as those in more formalised English. In the very act of transcription, then, Truth has become a ‘trickster’:

“...an ideal type, a victim (hero), a kind of plot space for the abolitionists' actions, a special human, not one that could bind up the whole people through her unremitting critical difference - that is, not an unruly agent preaching her own unique gospel of displacement as the ground of connection... The change in the shape of the words makes us rethink her story, the grammar of her body and life. The difference matters.” (Haraway 2004: 59)

When Potter and Hepburn’s criticisms regarding a frequent lack of contextualisation are examined through Sojourner Truth’s example, it seems apparent that those criticisms have some weight and that a failure to contextualise research data has potentially significant consequences.

Nonetheless, the transcripts in this project do not fully adhere to the recommendations of Potter and Hepburn, as the reduction in sample size that even those authors accept would be necessitated by such detailed transcription processes (Potter & Hepburn 2005: 291) was deemed unacceptable, given that a range of viewpoints were being sought. Instead a ‘Jefferson Lite’ system advocated by Potter elsewhere (e.g. Potter & Wetherell 1987) is deployed. Transcripts are not truly cleaned, some pauses and repetitions remain, but there is some formalization of spelling and some of the finest detail is inevitably lost. Such decisions are not being defended here, merely stated for consideration.

With regard to the issues with qualitative interviewing which Potter and Hepburn believe to be ‘endemic and inescapable’, four examples are given.

Firstly, Potter and Hepburn point towards the fact that interviewers, who inevitably determine the framing of any encounter, “flood the interview with a social science agenda and categories” (Potter & Hepburn 2005: 292). This criticism suggests that as interviewers bring various concepts to the interview, those topics inevitably come under discussion and in some sense become reified:

“At its most basic these issues face us with the possibility that a piece of interview research is chasing its own tail, offering up its own agendas and categories and getting those same agendas and categories back in a refined or filtered or inverted form.” (Potter & Hepburn 2005: 293)

A second critique refers to the notion of ‘footing’; are, for example, interviewees and interviewers speaking at any given moment as the representatives of a group, such as autism scientists, or as individual agents? How many roles might one person take within an interview: An individual? An autism scientist? A psychologist? An advocate? A parent? A layperson? In combination with Potter and Hepburn’s third critique, that both interviewer and interviewee always speak from a position of interest and thus have a stake in the data, we are reminded here of Foucault’s definition of power, summarised (appropriately) within an interview:

“...in human relationships, whether they involve verbal communication..., or amorous, institutional, or economic relationships, power is always present: I mean a relationship in which one person tries to control the conduct of the other. So I am speaking of relations that exist at different levels, in different forms; these power relations are mobile, they can be modified, they are not fixed once and for all. For example, the fact that I may be older than you, and that you may initially be intimidated, may be turned around during the course of our conversation, and I may end up being intimidated before someone precisely because he is younger than I am. These power relations are thus mobile, reversible, and unstable.” (Foucault 1997c: 292)

Tracing these changing power relations within a qualitative interview is, according to Potter and Hepburn, a mammoth and perhaps impossible task. For their final critique, Potter and Hepburn hark back, once more, to Dingwall and Murphy and the claim that:

“...we will need to pay attention to the practical and interactional role of cognitive terms [such as think, feel, and so forth] and be very cautious about treating such terms as if they referred to psychological objects of some kind within individuals.” (Potter & Hepburn 2005: 300)

In short, the suggestion that talk refers unproblematically to generalised cognitive activity is an assumption that should be viewed warily.

It is not entirely clear what the solution to these problems might be. Potter and Hepburn argue for the prioritisation of naturalistic methods whenever possible (Potter & Hepburn 2005: 301) and, like others (Rapley 2004: 111), are in favour of the ‘dead psychologist test’ whereby only data that would exist in the absence of the researcher should be considered valid. Despite this claim that naturalistic methods are superior, even Potter and Hepburn seem to suggest that the difference between naturalistic and interview data is a difference of degree rather than kind (Potter & Hepburn 2005: 301) and that, to continue the links with Foucauldian thought, forms of knowledge can never be divorced from power relations. For the reasons mentioned previously, the use of qualitative interviewing can be strongly defended here as being amongst the most suitable of available methods, but this should not stop a reflection upon the local, situated knowledges produced within it.

#### *Qualitative interviews: Topics*

The questions driving this thesis are those concerning the construction of autism within cognitive psychology and social neuroscience and the interview schedules were drawn up with these questions in mind. It was decided that pre-determined questions, formalized in the interview schedules, would be kept to a small number and that questions and topics that arose naturally during the course of the interviews would, to a significant degree, guide the discussion. A schedule consisting of around twenty questions was

initially created. There was an intent to keep questions as general as possible in order to avoid a project which was ‘chasing its own tail’, as Potter and Hepburn might have it, an approach previously utilised in projects such as that conducted by Kate Weiner (e.g. 2006). In much the same way that this project is interested in exploring models of sociality, Weiner’s thesis was concerned with the notion of geneticisation. Despite this interest, Weiner deliberately avoids discussing genetics (Weiner 2006: 124) and when it became apparent that Weiner’s participants did not rely solely upon genetic explanations to explain heart disease this was taken as analytically important (Weiner 2009: 421). It seems likely that, if Weiner had asked questions about genetics she would have received answers about genetics, answers which could reasonably be classed as interview artifacts. Thus, attempts were made here to ask only broad questions about autism and refrain from introducing questions concerning ‘the social’. As discussed above, the experimenter cannot be removed from the research but adopting such a generalist position seems to at least partially mitigate against some of the problems posed by interview methods.

The interview schedule itself can be found in Appendix A (p.236). Broadly, interview questions covered five areas; i) how the participant came to be interested in their research topic, ii) the nature of autism, iii) the nature of the participant’s current research, iv) the impact of social neuroscience, and v) the role of advocacy groups in research. A great deal of flexibility was included however and topics of discussion would be allowed to wander to a quite significant degree. Further, the interview schedule did not remain static throughout the project. For example, the question included on DSM-5 was incorporated in response to the answers of participants interviewed early in the project, all of whom were keen to discuss this topic. Nonetheless, the five areas covered above were usually addressed at least in passing.

#### *Qualitative interviews: Samples*

The intent of this project was to interview researchers with interests in psychology, neuroscience, and autism and who were based in the United Kingdom. In addition to existing knowledge of the field, potential participants

suitable for interviewing were sought through a variety of means; an internet search, discussion with members of the Hamilton Laboratory at The University of Nottingham, and finally by asking interviewees for contacts. Potential interviewees were contacted by e-mail with a brief explanation of the goals of the project and an attached information sheet gave further details (Appendix B: 238). It was made clear to participants that both individual and institutional identities disclosed within interviews would be removed in the act of transcription, that data would be securely stored, and that the project had passed the School of Sociology and Social Policy at The University of Nottingham's internal ethics procedures. Participants were then given the opportunity to ask any outstanding questions and, once satisfied, signed a consent form (Appendix C: 241). If participants had any further questions they were answered informally, again by e-mail.

In addition to a small number of pilot interviews conducted with PhD students at the University of Nottingham, data from which was not used in the thesis, twenty research interviews were ultimately conducted for this project. This sample consisted of 13 women and 7 men, or alternatively 7 Professors, 2 Readers, 1 Senior Lecturer, 1 Associate Professor, 2 Lecturers, and 7 Postdoctoral Researchers of various kinds. Eighteen researchers were interviewed at their place of work, 1 was interviewed at a restaurant, and 1 was interviewed at their home<sup>9</sup>. Twelve researchers declined to take part in the project for a variety of reasons (although none claimed the nature of the project was a determining factor) and an additional 12 researchers were contacted but did not respond. Interviews that were conducted lasted between 38 and 73:07 minutes, with a mean length of 54 minutes and 30 seconds. Approximately one-half of the interviews reached a natural close while the other half were ended at a scheduled time, usually an hour.

The extracts presented in this thesis have been anonymised in the following manner: each participant was allocated a random two letter code (e.g: DF). The number presented alongside these initials refers to the interview number (e.g. DF03 was the third interview conducted), and the longer numbers

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<sup>9</sup> University lone worker policy was adhered to during the visit to a researcher's home.

following the colon refer to transcript line numbers being quoted (e.g. DF03: 430-434).

### *Qualitative interviews: Analysis*

After all of the interviews had been fully transcribed (see above), text was coded by hand into broad, reoccurring themes. For example, themes emerged regarding the fifth edition of the Diagnostic and Statistical Manual of Mental Disorders (DSM-5), autism in relation to normal behaviour, and issues of heterogeneity in autism. Once this initial coding procedure was completed, themes were interrogated to look for particular areas of (dis)agreement in various constructions. For example, it became apparent that ‘autistic heterogeneity’, an issue raised by nearly all participants, was constructed in quite different ways by different participants at different times (see chapter 5, particular p.107). These more nuanced coded sections formed the basis of the data presented in the analysis chapters of this thesis which are based upon interview data. Where interview and literature data are presented alongside one another, as they are on occasion, the interview data exclusively led to a consideration of particular literary sources and not vice versa.

## *2.2 Absent voices*

### *A silent past*

The first analysis chapter of this thesis starts with a critical analysis of cognitive psychology in the 1980s before three subsequent chapters go on to consider notions of autistic sociality within contemporary settings. The claim is that within the laboratories of cognitive psychologists autism is reconstructed and takes on its modern appearance.

Despite this portrayal of construction within the laboratory, Science Studies has taught us that a picture such as this is, at best, vastly oversimplified. Analyses of Acquired Immunodeficiency Syndrome (AIDS) groups of the mid 1980s (Epstein 1995; Epstein 1996), women’s health movements (Anglin 1997; Blackstone 2004; Gibbon 2008; Klawiter 2004), and the French Muscular Dystrophy Association (Rabeharisoa 2003; Rabeharisoa & Callon 2002) have long made us aware that the idea that science is

‘upstream’ from society is grossly inaccurate. As is made clear in the literature review (pp.46-49), within the field of autism research laboratory work is similarly entangled with broader society.

What justification can be provided for silencing the historical, and indeed contemporary, voices of these organisations, especially when we know that the likes of Bernard Rimland blurred or dissolved the line between advocate and scientist, engaging in heated debate with Bettelheim as both peer and parent (Silverman 2004: 118-121)? What justification can be given for considering social neuroscience in the light of cognitive psychology but not the advocacy groups of that era? Any theoretical justification for the exclusion of advocacy groups will be weak. The choice of research topics was driven primarily by a personal interest in researching psychology as a science and was limited by the duration of the thesis. Within the literature review an attempt has been made to consider the impact of advocacy groups in general, and within autism in particular, but given the particular research questions asked in this work, that picture is inevitably incomplete. We must reflexively be aware that this picture is incomplete and any assumed causality partial. Findings must be considered in the light of this partial knowledge base.

#### *A silent present*

One noticeable set of voices completely excluded from the current project are those of individuals who have themselves been diagnosed with autism, an omission which other authors view as particularly problematic. Drawing upon disability studies, Stuart Murray’s text *Representing Autism* (Murray 2008) concerns itself with how autism is represented in various forms across diverse media, giving time to fictional and non-fictional accounts of autism in film, text, advertising, and photography within both contemporary and historical (i.e. prior to 1943) settings. Murray’s central claim is that representations of autism are almost exclusively concerned with the human condition in general, rather than the autistic condition in particular: “Autism is endlessly fascinating, these novels seem to say, but never more so than when we might quickly characterize it and use it to look at something else” (Murray 2008: 98). Thus, for Murray, within a great number of texts concerned about

autism, the individual with the condition "becomes a prop, a prosthetic device, for the discussion of a range of issues - masculinity, family cohesion, adult responsibility - that ultimately have their meaning in non-disabled contexts" (Murray 2008: 163). Within the vast majority of work on autism, then, there is a distinct lack of autistic *presence*. Murray finds the lack of such presence highly problematic, concluding at one point that much contemporary work on autism may ultimately be considered "embarrassing and shameful" (Murray 2008: 134).

It is hard to deny that the current project is guilty of the sins Murray has identified, that autism is in some sense here a prosthetic or a prop used for other purposes. As has been repeatedly stressed this thesis is not only concerned with autism and in some senses apes the natural sciences in using autism precisely because it allows access to other matters, be they constructions of the social or the workings of the cognitive neurosciences. Accordingly, autism is almost exclusively referred to in this work as a condition, a disorder, or (perhaps worse) a concept rather than as an embodied aspect of a unique agent. Without wishing to suggest that this framing (exclusion) is unproblematic, the decision not to interview those with autism was not simply made for reasons of space and interest. Two reasons present themselves for the exclusion of autistic voices from within this project.

The first regards the power dynamics within an interview scenario and making comparisons between interview sessions. If, as discussed above, an interview is 'a joint accomplishment of interviewer and respondent' it seems beyond doubt that interpersonal relationships will have real consequences on the type of data collected. Even granting that power relations are flexible and shift from moment to moment, it seems undeniable that the power relations between a PhD researcher and a senior academic will be vastly different to those between a PhD research and an individual with autism. Thus, to draw comparisons between constructions within these very different interview scenarios seems deeply flawed.

A second critique extends one familiar to disability studies and regards representation. The social model of disability (Hughes & Paterson 1997;

Oliver 1990; Shakespeare & Watson 2002) has sought to give voice to individuals labelled as disabled. But who is free to speak for whom within the context of disability, and who is able to speak at all (Barnartt et al. 2001: 436)? Spivak, in the context of post-colonialist studies with which there are obvious comparisons, has said “the oppressed, if given the chance...*can speak and know their conditions*. We must now confront the following question...*can the subaltern speak?*” (Spivak 1988: 25, italics in original). While there are evidently a great many individuals with autism who can ‘speak and know their conditions’, the “silenced centre” (Spivak 1988: 24) of the condition is quite literally a silent centre. Autism is defined by a triad of impairments in social interaction problems, communication difficulties, and restrictive and/or repetitive behaviours (Baron-Cohen 2000) and between twenty-five and forty percent of individuals diagnosed with autism have been classified as nonverbal (Levy & Bar-Yuda 2011: 343). Thus, while a significant number of individuals with autism are able to put forward their views and eloquently express their opinion regarding their conditions, there are a good number of individuals within the autism spectrum with whom we may draw the comparison of subalterns. Given that interviews and other text-based sources will be fundamental empirical tools within the grasp of current project (for reasons given above) it was concluded that attempts to give voice to autism were doomed to be ‘lost in translation’ (Tsang & Ho 2007) for reasons of both representation and comparison. This is not to claim that representation is not possible (see, for example, the two attempts detailed by Silverman 2008a: 47; 2008b: 327) only that an attempt within this space may lead to “...the unquestioned privileging of elite discourses produced and distributed from sites of power, and the risk of theoretical imperialism” (Tsang & Ho 2007: 640). The hope is that other projects, utilising diverse methods and conducted by diverse parties, will be able to engage with self-advocates productively and inclusively. This project trains its eye firmly upon scientists but, once again, an awareness that an autistic voice is missing is essential<sup>10</sup>.

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<sup>10</sup> It should also be noted that these subalterns are almost certainly under represented/ absent in the psychological experiments which the interviewees of this project conduct, hence they are *doubly* missing from the present analysis: from this thesis itself and from the accounts of

### 3. Personal Closeness

Emotion, affect, and autism: How is one to disentangle these phenomena? In several senses, autism is clearly *about* emotion and affect. Leo Kanner first told us what autism *is* in his paper *Autistic Disturbances of Affective Contact* (1943). This idea is contiguous with Peter Hobson's research, discussed at length in this thesis, which contests that autism *is* a disorder "of affective and social relations – and irreducibly so" (Hobson 1989a: 22). Simon Baron-Cohen's body of work claims that autism *is* a disorder of at least some aspects of empathy (Baron-Cohen 1995). If autism *is* a disorder of some emotion or other, it has also been a consistent claim that autism has a similarly emotional *cause*, be that a biological cause, for instance a broken mirror neuron system preventing the experience of emotion (Rizzolatti & Fabbri-Destro 2010), or a social explanation in which a child is denied affective relations by, perhaps, an aloof mother (Bettelheim 1972).

However, as is made clear within the literature review (p.49), the study of autism is not just *about* emotion, it is *itself emotional*. It cannot pass notice how close so many involved in research are to the 'object' of autism. Many could not be closer; the self-advocacy movement is a significant player (particularly in the United States, e.g. Aspies for Freedom 2002) and this extends into research as those with a diagnosis continue directly to contribute to the scientific research on autism (e.g. Michelle Dawson; Gernsbacher et al. 2005; Dawson et al. 2007). An even greater number of researchers have children or other relatives diagnosed with autism and these researchers contribute to nearly all academic fields involved in autism research; from anthropology (e.g. Grinker 2010), to communication studies (e.g. Nadesan 2005), to history (e.g. Feinstein 2010), to bio-psychology (e.g. Rimland 1964). Nadesan's opening sentence in *Constructing Autism* is "This story begins with my son, Kamal..." (2005: 1) and this is typical, similar utterances being found in all those texts cited above. It is inconceivable that this emotional closeness does not affect the nature of the research produced, for that research may never have been produced if not for the emotion (Silverman 2012: 5).

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scientists that inform it. The effects of scientists 'researching up' the spectrum to the exclusion of those with, for instance, no verbal skills are discussed in chapter 7 (from p. 153).

The study of autism is, therefore, both about emotion and is in itself emotional. This collapsing of the method and the topic, the researcher and the researched, is something that psychology, along with the other human sciences, must contend with as a general rule. If psychology is the study of individual human behaviour, and is conducted by individual humans, then the s/Subject is inevitably discovering and/or creating itself (Gergen 1973). The attempted insertion of a clean break between the experimenter and the subject, the so called repudiation of Wundt (Danziger 1990: 34), has lasted a century and, unsurprisingly, remains an impossibility in the age of neuroscience (Langlitz 2010: 49).

Perhaps the most startling example of this dynamic nominalism, the so called looping effects of human kinds (Hacking 1986a; 1995b), in the history of psychology can be found within the educational system. Danziger (1990: 130-135) has convincingly argued that significant differences in the educational systems of Germany and the United States of America led to very different psychologies developing in those two countries during the early twentieth century. In turn, the emergent psychologies had a profound effect upon the American education system:

"...the Galtonian use of statistics greatly facilitated the artificial creation of new groups whose defining characteristic was based on performance on some psychological instrument, most commonly an intelligence test." (Danziger 1990: 112)

As this psychological research was incorporated into the school setting, therefore, Anglo-American educational systems began to organise themselves around the very groups and subjects that psychology informed them were there to be found; nowhere is this more obvious than in the introduction in the UK of the Eleven Plus exams in 1944 on the basis of I.Q. testing. These changes in the education system would in turn affect subsequent psychological research, *ad infinitum*. If such looping effects are at the centre of the psychological enterprise more broadly, and there are good reasons to suppose this is the case, then a recent emotional turn within the discipline is surely of importance where reflexivity is concerned.

Contemporary psychology, and particularly contemporary neuroscience, attributes an increasingly important role to emotion within the spheres of human conduct. The likes of Antonio Damasio (2005) argue that unreason in the form of emotion is inseparable from reason itself and must logically therefore lie at the heart of any reasonable, scientific psychology. As psychology becomes increasingly *about* emotion, it must be concluded that *it is* increasingly emotional. Once again, therefore, reflecting upon emotional closeness seems important.

Even if Foucault views it as a historical contingency (Foucault 2006a: 414-415), and without delving into the philosophy of emotion, we can probably agree with Hume that distance is crucial in determining our emotional response to an object:

“Here then we are to consider two kinds of objects, the contiguous and remote; of which the former, by means of their relation to ourselves, approach an impression in force and vivacity; the latter by reason of the interruption in our manner of conceiving them, appear in a weaker and more imperfect light. This is their effect on the imagination.” (Hume 2000: 274)

My personal distance from autism and psychology/neuroscience could not be more different from each other. I have never been diagnosed with autism. Neither have any of my family or close friends, at least at the time of writing. Like most people I have spoken to people with a diagnosis and, probably because of this project, discussed at reasonable length autism with numerous parties interested in my views. On several occasions I have volunteered at an autism social group associated with the University of Nottingham. Nonetheless, I have almost certainly spent less time in the presence of people with autism and their families than the vast majority of autism researchers, teachers, or doctors. I am remote from it and this distance may well make autism appear to me ‘in a weaker and more imperfect light’. Certainly it does not hold much vivacity for me. By comparison to my distance from autism, I could certainly be identified as a psychologist. So could members of my family and close friends. I am a trained psychologist to postgraduate level,

have published in the field (e.g. Hollin & Derbyshire 2009; Hollin & Larkin 2011), and retain an affinity to the discipline through one supervisor and her laboratory group. Of course, a conscious act has been taken to distance this project from psychology, both geographically and intellectually, by positioning this project with STS (rather than, say, critical psychology – see below) and under the primary supervision of two qualitative, sociologically-inclined researchers. Nonetheless, twenty-five years' worth of affinities are hard to erase completely.

It may be impossible to discern the impact upon the current project of this simultaneous distance from autism and proximity to psychology. The social sciences in general, and Science Studies in particular, are certainly divided on that matter. On occasion a particular standpoint is seen as being privileged, such as that of the proletariat in classical Marxist thought or women within feminist standpoint theory (Keller 1985). On other occasions, a simple distance is seen to be preferable:

“...’stepping back’ from full engagement in cultural activity which is often said to be emblematic of the ‘sociological attitude’ requires a form of ironic detachment: a disengagement from tribal custom and a heightened awareness of taken-for-granted assumptions.” (Lynch, 2000: 30)

It does not seem wise to issue post-hoc defences of the preferability of either my closeness to psychology or my remoteness from autism given that neither standpoint was entirely under my control once this project had begun. Instead, and continuing a position that is advocated in this chapter, it is merely worth noting this unusual situating and reflecting on how it may have influenced my data collection and analysis. Ultimately, this impact may be determined at the readers' discretion.

#### 4. Critique, the purpose, and status of Science Studies

##### *4.1 What is critique?*

In 1978 Michel Foucault gave a lecture entitled *What is Critique?* (Foucault 1997d). Foucault begins to answer this question by making it quite clear that critique is not, as such, one thing at all:

“...critique only exists in relation to something other than itself: it is an instrument, a means for a future or a truth that it will not know nor happen to be, it oversees a domain it would want to police and is unable to regulate. All this means that it is a function which is subordinated in relation to what philosophy, science, politics, ethics, law, literature, etc., positively constitute.” (Foucault 1997d: 42)

That critique only ‘exists in relation to something other than itself’ and thus cannot be understood outside of a relational framework does not mean that commonalities cannot be found across notions of critique. What becomes clear is that, for Foucault, critique is very tightly bound to the tri-partite nature of experience which is of sustained concern within this thesis (see chapter 2: 23-24 for an overview). Above all:

“...one sees that the core of critique is basically made of the bundle of relationships that are tied to one another, or one to two others, power, truth and the subject. And if governmentalization is indeed this movement through which individuals are subjugated in the reality of a social practice through mechanisms of power that adhere to a truth, well, then! I will say that critique is the movement by which the subject gives himself the right to question truth on its effects of power and question power on its discourses of truth. Well, then!: critique will be the art of voluntary insubordination, that of reflected intractability. Critique would essentially insure the desubjugation of the subject in the context of what we would call, in a word, the politics of truth.” (Foucault 1997d: 47)

Critique is, then, the “art of not being governed like that and at that cost” (Foucault 1997d: 45).

Thomas Lemke has provided a detailed analysis of these links between experience and critique in Foucault’s work (Lemke 2011). Lemke ties the tri-

partite nature of experience to a tri-partite nature of critique. Firstly, Lemke discusses the problematisation of the history of truth, highlighting that, for Foucault, experience is not only something that should be investigated but also provoked, through what Foucault would elsewhere refer to as ‘limit-experience’ (e.g. Foucault 1994: 241-242):

“On the one hand, Foucault clearly indicates an object of analysis. It is something to be investigated to determine its emergence and its conditions of existence. On the other hand, there is another sense of problematization, which refers to the activity of the observer who engages in the process of problematization. He or she problematizes specific experiences and by doing so tries to move beyond the limits they impose. Here, problematization is no longer the object, but rather the objective of the critical investigation.” (Lemke 2011: 32)

Secondly, and in relation to structures dominant in power relations, Lemke highlights the art of ‘voluntary insubordination’, the notion that critique “relies on the existing normative and institutional system while seeking to expose its limits in order to explore ways to transform it” (Lemke 2011: 33). Finally, and in relation to the problem of the subject, critique “exposes one’s own ontological status, it involves the danger of falling outside the established norms of recognition” (Lemke 2011: 36) and confronting the contingency of our ‘ontology of ourselves’ (Meloni 2011: 151).

Despite the fact that Science Studies in general, and actor-network theorists in particular, have a highly ambivalent relationship with Foucault (see, for example, Latour & Crawford 1993: 250-253; Law 2004: 35-36; Mol 2002: 65-66) there are, in two respects, interesting convergences here between the Foucauldian project of critique and the broader endeavours of Science Studies. The first similarity between Science Studies and the work of Foucault is to be found in the relationship between the project and the object of analysis. Judith Butler gives the following take of Foucault’s notion of critique: “Critique will be dependent upon its objects, but its object will in turn define the very meaning of critique.” (Butler 2003: 306). Similarly, Mario Biagioli defines the nature of Science Studies in the following form:

“[T]here are probably as many ways to study science as there are to study literature. But the fundamental difference is that, in the case of Science Studies, methodological disunity does not disunify its subject matter. Its subject matter has become a historical fact and not the product of disciplinary definition.” (Biagioli 1999: xiii)

Both Biagioli and Foucault/Butler give their objects a relational ontology, defining them in opposition to an other (philosophy in the case of Butler, science in the case of Biagioli). It may be that critique and/or Science Studies is purely parasitic, that the host in no way engages with its dependent, or it may be that the relationship is dialectal or symbiotic. In either instance, because the object defines the form of the response, there is always a separation between the activities.

It is in this sense of the relational that the current work is separated from the laudable activities of, for example, the critical psychology network. Within Ian Parker’s key text *The Crisis in Modern Psychology – and How to End it* (Parker 1989), for example, there is just that, the sense of an ending, the suggestion that psychology could be suitably reformed and there would no longer be a need for critical psychology, there would simply be an enlightened psychology. Despite its opposition to dominant thought, therefore, critical psychology is just that – psychology. Such an integration of academic disciplines is clearly the objective of many at a time when academic research is judged by its interdisciplinary aspirations and the impact agenda. This project, however, sides with those (e.g. Armstrong 2012: 20) who remain sceptical about the desirability of consilience. By explicitly defining the ongoing project as being inherently *in relation* to the practices of the psy-disciplines the project will be forever unfinished, regardless of future experiences that come to be constructed. That the contemporary biosciences, in an age of epigenetics and neuroplasticity, look increasingly similar to Donna Haraway’s analyses during the early 1990s, does not signal the end of that particular aspect of the Science Studies project, instead it implies that Science Studies must mutate with science, examining the claims to power, truth, and the subject made within these new disciplines.

A second point of convergence between Science Studies and the Foucauldian project is illustrated in the following extract, again from Judith Butler:

“...the primary task of critique will not be to evaluate whether its objects – social conditions, practices, forms of knowledge, power, and discourse – are good or bad, valued highly or demeaned, but to bring into relief the very framework of evaluation itself. What is the relation of knowledge to power such that our epistemological certainties turn out to support a way of structuring the world that forecloses alternative possibilities of ordering?” (Butler 2003: 306-307)

This dual goal of uncovering the contingency of singular experience and in doing so making space for alternatives is an area of agreement between Science Studies and Foucauldian work. When John Law critiques the metaphysics of science in the following manner, one cannot help but think of Foucault’s ‘historically singular form of experiences’ (Foucault 1984a: 333) and the attempt to move beyond these:

“The problem, then, is that the commitment to visible singularity directs us away from the possibility that realities might in some measure *be made in other ways*. Or, to put it more generally, the presupposition of singularity not only hides the practice that enacts it, but also conceals the possibility that different constellations of practice and their hinterlands might make it possible to enact realities in different ways.” (Law 2004: 66, italics in original)

By placing this project at the juncture of Foucauldian and Science Studies, concerning itself with the practices which others have referred to as ‘the governing of the soul’ (Rose 1999; Rose 2010) or ‘memoro-politics’ (Hacking 1994), it is hoped that the purpose of the present study has begun to become clearer. In considering the possibility that the cohesive condition of autism has been “fabricated in a piecemeal fashion from alien forms” (Foucault 1977: 142) the space is opened for alternative realities of social behaviour and sciences of those behaviour to take form.

#### 4.2 *The demonic machine: Disciplinary reflexivity*

Simply noting that the current project, positioned as a critique from within Science Studies, is distinct from scientific activity does not, however, end the processes of reflexivity. As David Bloor has said (Bloor 1991: 7) any methodology applicable for the studying of scientific knowledge should also be applicable when studying Science Studies, or the sociology of scientific knowledge (SSK). This is especially the case given that research within Science Studies (Knorr Cetina 1999) and the history and philosophy of science (Dupré 1993; Galison & Stump 1996) has largely rejected a monolithic vision of 'science', demolishing the walls between science and other forms of knowledge production, including Science Studies. Positioning research as critique, as defined above, may ensure a continual separation of Science Studies and science, but critique cannot rescue Science Studies' epistemological foundations. Bloor's conclusion problematises any notion of 'selective relativism' to which the social sciences may be exempt. One particular aspect of this 'symmetrical reflexivity', however, has the potential to be highly problematic for Science Studies; the so called *tu quoque* (you, also) problem (Ashmore 1989: 27).

If Science Studies purports to have undermined claims of realism emanating from the natural sciences then on what epistemological basis can we believe the claims of Science Studies themselves to be real? Like post-structuralism (Hammersley 1992: 54), Science Studies potentially suffers from the logical inconsistency that is Russell's Paradox (Russell 1903: 101); if relativity must be applied symmetrically to all forms of knowledge, including this statement, then the initial truth claim of this statement is fundamentally undermined. Reflexivity may then be "likened to a demonic machine that, once set in motion, devours everything in its path and then turns on itself" (Lynch 2000: 46).

This form of *radical reflexivity* (Lynch 2000: 36) has indeed been applied within Science Studies, most notably by Malcolm Ashmore (e.g. Ashmore 1989) and Steve Woolgar (e.g. Woolgar 1989; 1992). Ashmore's *Reflexive Thesis* (1989) is often cited as an exemplar of this type of research.

One of the analysis chapters is an SSK analysis of Collins' *The Seven Sexes*, itself a classic piece of SSK analysis (Collins 1975) and one which Collins claims to have been replicated on five occasions within an issue of *Social Studies of Science* (SSS; Travis 1981; Collins 1981; Pickering 1981; Harvey 1981; Pinch 1981).

Collins' key claim is that within science:

"The reproducibility of scientists' findings is seen as the major epistemological guarantor of scientific validity while the institutionalisation of replication as a behavioural norm acts as an effective mechanism of social control...Collins's version, in contrast, treats replication as a problematic and complex phenomenon, the meaning of any particular instance of which is subject to social negotiation over the relevance of perceived similarities or differences between the events (e.g., experiments) concerned. As no two events can be totally identical, there is always a "space" for this negotiation of sameness/difference. The success or lack of success of any replication claim can never, therefore, rest on the way the world is; rather, it must rest on social agreement in the relevant community." (Ashmore 1989: 115)

Retaining Collins' schema in which there are six stages of social negotiation before a decision regarding replicability is made, Ashmore examines claims regarding the replication of replicability studies and concludes that:

"There is a sense in which the Schema is like a steeplechase with each Stage as a fence...All of our candidate-replicators [those articles appearing in SSS]...appear to have fallen at the third (with the possible exception of Pickering, who might, or might not, have picked himself up and Collins who might have refused!)" (Ashmore 1989: 129)

Replicability within SSK is, then, just as problematic as replicability within natural science. Of course, Ashmore concludes this chapter by repeating the process again, this time with his own preceding analysis of Collins, and concludes his own findings are just as problematic. *Tu quoque* forever.

If one formulates this paradox as a problem (and not all do, see below) then it is a problem with no philosophically definitive solution. It is impossible to dissuade relativists, for no theoretical or empirical claim could be made which does not fit within that philosophy. If, as Collins and Yearley suggested in 1992, relativistic portions of Science Studies and the social sciences more broadly are engaged in a game of ‘epistemological chicken’, standing on the road of relativism longer and longer in the face of oncoming traffic, then it is a game of chicken with no conceivable risk, for nothing is able to hit them.

Numerous theorists take umbrage with the statements of Woolgar, Ashmore and colleagues, however, and view radical reflexivity as a very serious problem indeed. Trevor Pinch has referred to radical reflexivity as “naive, debilitating, and dangerous” (quoted in Ashmore 1989: 70). Harry Collins (who stated that he has “banned reflexivity” (Ashmore 1989: 115)) and Stephen Yearley have wryly suggested that:

“[T]he sociologist knows less that the natural scientist, while the sociologist of science knows still less. Those engaged from day to day with the problem of reflexivity would, if they could achieve their aims, know nothing at all. We might say that SSK has opened up new ways of knowing nothing.” (Collins & Yearley 1992: 302)

Donna Haraway makes her concerns about this particularly clear:

“I, and others, started out wanting a strong tool for deconstructing the truth claims of hostile science by showing the radical historical specificity, and so contestability, of *every* layer of the onion of scientific and technological constructions, and we end up with a kind of epistemological electroshock therapy, which far from ushering us into the high stakes tables of the game of contesting public truths, lays us out on the table with self-induced multiple personality disorder... [W]e ended up with one more excuse for not learning any post-Newtonian physics and one more reason to drop the old feminist self-help practices of repairing our own cars. They’re just texts anyway, so let the boys have them back.” (Haraway 1988: 578, italics in original)

Some who were unsatisfied with ‘new ways of knowing nothing’ offered philosophical (rather than practical) critiques of the relativist programme, despite the fact that these are doomed to failure, at least in the eyes of relativists. Perhaps the most frequent of these critiques is the claim that Russell’s paradox is ontologically important and thus relativism should be rejected for its internal inconsistency. Andrew Sayer, for example, claims:

“Universal relativization involves a performative contradiction which invites ridicule – ‘there is no truth beyond whatever anyone defines as the truth – and that’s the truth!’ Even if we were to say that our own knowledge was also merely a function of our social position it would imply that this was true. The only way we can relativize truth is selectively, by exempting some beliefs, including our own, from the relativization...universally relativizing truth as a function of power leads to incoherence.” (Sayer 2000: 49)

Unsurprisingly, those advocating radical reflexivity laugh this suggestion out of court, preferring to ‘celebrate the monster’:

"He [Ashmore, speaking in the third person] understands that "to respond to the question seriously is to buy into the auspices of the question"... The question asks him to accept that the appropriate thing to do with paradoxes is to (try to) resolve them. He knows that this is a form of destruction and he feels reluctant to join in. Eccentrically as he often thinks, he finds he enjoys paradoxes, even to the extent of taking pleasure in their increasing number." (Ashmore 1989: 16)

### 5. Conclusion

Of course, numerous attempts have been made to move past the dilemmas posed by questions of radical reflexivity. Roy Bhaskar’s critical realism (Bhaskar 1979; Bhaskar 2008) has tried to put ontology back on the table and, within Science Studies, a great many have distanced themselves from post-structuralist thought following the science wars of the 1990s (Fuller 2000: 196-197). Instead of a radical reflexivity, many voices now vigorously claim a ‘3<sup>rd</sup> way’ between the dichotomy of words and worlds, as

epistemology and ontology are collapsed into each other (Latour's response to the science wars, *Pandora's Hope* (1999), is perhaps the fullest articulation of this position, a stance Michael Lynch has recently suggested be called 'ontography' (Lynch 2013)).

None of these suggestions can decisively defeat radical reflexivity. As Ashmore (1989: 69) has stated, "after truth, we should not think of reflexivity as a "problem". It has no conceivable solution." This chapter has respected this tradition, asking for the current project to be understood as a piece of situated knowledge and setting out at least some of the dazzling array of nodes, networks and standpoints that are utterly unique to this project. The reader has been invited to consider the effects of these various positions and the manner in which they condense into particular findings in the light of discourses surrounding reflexivity. Even if we resist running straight towards relativism, that this chapter should end with such an invitation to reflexivity, having been concerned almost entirely bound up with such questions, is itself surely demonstrative of the fact that the form and conclusions of this thesis should be considered as historical, complex, and local.

## **Constructing a social subject: Autism and human sociality in the 1980s.**

### Introduction

The second half of the 1980s is a key time-point for the emergence of the contemporary experience of autism, and the laboratories of cognitive psychologists are prime sites of interest. The period between 1985 and 1989 saw the arrival of three aetiological theories of autism at the psychological level, theories which continue to dominate the field today (Pellicano 2010). Those theories are the theories of Metarepresentations or Theory of Mind (Baron-Cohen et al. 1985), Executive Dysfunction (Rumsey 1985), and Weak Central Coherence (Frith 1989). That these three theories of autism, so markedly different from their predecessors, were developed over such a short time period is intriguing. As Hacking has suggested:

“If a concept is introduced by some striking mutation...there may be some specific preconditions for the event that determine the possible future courses of development for the concept... I am inclined to think that the preconditions for the emergence of our concept ...determined the very nature of this intellectual object.” (Hacking 1975: 9)

It is the goal of this chapter to ask if such preconditions existed for autism research during the 1980s.

A survey of existing literature (chapter two: 32-35) showed that towards the end of the twentieth century, ‘the social’ in social psychology came to mean something distinct and historically novel. Firstly, “cognition is characterized as social merely by virtue of the objects to which it is directed, namely, other persons or social groups” (Greenwood 2004a: 6). This is a ‘social in the shape of a crowd’ (Danziger 1992: 313) whereby social understanding means an understanding of proximal individuals and their intentions. Secondly, “the basic cognitive processes engaged in the perception and cognition of nonsocial objects... are also engaged in the perception and cognition of social objects” (Greenwood 2004a: 6). Thus ‘the social’ is quantitatively and not qualitatively distinct from the nonsocial world and governed by the same cognitive mechanisms. Finally, empathy is positioned

as the quintessential social emotion and believed to be the ‘glue of the social world’ (Young 2012a: 170, 2012b: 414).

These claims, when woven together, constitute a *savoir* of the social for cognitive psychology. In a useful distinction, Foucault used the French terms *connaissance* and *savoir*, both of which are translated into English as ‘knowledge’, to mean very different things:

“Foucault uses *connaissance* to refer to the conscious rules men themselves recognize as justifying a claim of knowledge and uses *savoir* to refer to another level constituting the underlying necessary conditions defining and making possible these recognitions. These underlying conditions are unknown to the knower and his contemporaries and must be produced by the history *ex post facto*...”  
(Kennedy 1979: 271, italics in original)

It is the latter of these knowledges, *savoir*, which Hacking translates as ‘depth knowledge’ (Hacking 1995a: 198) and which Foucault claims to be so important in determining the possible forms of knowledge (Foucault 1972: 15), the highly interconnected “set of rules that determine what kind of sentences are going to count as true and false” (Hacking 1986b: 30).

It is argued here that the three most significant contemporary models of autism rely upon a shared *savoir*, and that an understanding of this depth knowledge is crucial to explaining the contemporary truth of autism. By considering autism in light of a *savoir* of the social in which proximal interpersonal conduct is synonymous with social conduct, the theories of Executive Dysfunction, Metarepresentation, and Weak Central Coherence are all able to position autism as a *social disorder*. Further, by considering social conduct to be only quantitatively different from nonsocial conduct, and governed by the same cognitive mechanisms, these three theories are able to centralise hitherto unexplored and apparently nonsocial symptoms in autism, radically transforming the appearance of the disorder into something significantly greater than the ‘disturbance in affective contact’ described by Kanner (1943). In addition, by describing autism as a *disorder of empathy*, metarepresentations theory in particular was able to centralise autism within

broader discourses of human sociality, positioning the condition as a natural experiment capable of illuminating human nature.

The rest of this chapter will explore these questions of depth knowledge and the construction of social disorder. The theories of Executive Dysfunction, Metarepresentation, and Weak Central Coherence will be examined in turn. Further, a conflict between the protagonists of all three of these theories and a fourth hypothesis, the Emotion/ Affect Hypothesis, which has roots in psychoanalytic theory, will be used to show the incommensurability of competing *savoirs*.

### *Accounts of autism from within cognitive psychology*

#### *Autism as a disorder of executive functioning*

A new foundation for thinking about autism in cognitive terms arose in relation to a key construct from within cognitive psychology, executive functioning, generally assumed to have emerged in the aftermath of the landmark paper on working memory by Baddeley and Hitch in 1974. That a construct founded upon an individual's working memory could be the basis of a model of autism as social disorder demonstrates just how radically autism was redefined to fit within the epistemological boundaries of cognitive psychology.

The psychological concept of executive functioning is contested but, broadly, refers to behaviours where an individual must rely not just upon their immediate environment (a 'bottom-up' response) but also upon their past experiences and knowledge base ('top-down' processes). When discussed within the autism literature of the 1980s and early 1990s, there is general agreement that executive functioning abilities include, among other things, 'flexibility of thought' (Ozonoff & McEvoy 1994: 415; Ozonoff et al. 1991: 1083); the 'inhibition of prepotent but irrelevant responses' (Ozonoff & McEvoy 1994: 415; Ozonoff et al. 1991: 1083; Pennington & Ozonoff 1996: 55; Russell et al. 1996: 673) , and 'planning' (Ozonoff & McEvoy 1994: 415; Ozonoff et al. 1991: 1083; Pennington & Ozonoff 1996: 55; Russell et al. 1996: 673) .

Various tests have been designed to examine executive functioning. Foremost amongst the tests used in the study of autism was the Wisconsin Card Sorting Test (WCST), although by 1996 32 measures had been used in the study of autism (Pennington & Ozonoff 1996: 53). In the WCST four stimulus cards are placed in front of the participant, differing in terms of stimulus colour, shape, and number. For example, one card may show one blue cross, a second card two green triangles, a third card three red circles, and a fourth card four yellow stars (Rumsey 1985: 27). Participants are given a pack of 64 cards which differ from the four stimulus cards along these dimensions of colour, shape, and number (for example, a card from the pack may show one green circle). Participants are required to place the new card next to one of the stimuli cards according to an abstract ordering rule. For example, if the rule is ‘cards of the same colour belong together’ the new card should be placed next to the stimulus card showing two green triangles. Two aspects mark the WCST out as particularly difficult. Firstly, the experimenter does not inform the participant of the abstract sorting rule being employed – only if their card placement is correct or incorrect. Participants must therefore establish the sorting rule by a process of trial and error. Secondly, once the participant has discovered the sorting rule the experimenter changes that rule so that the existing strategy must be rejected and a trial and error process recommenced.

According to Ozonoff et al. (2005) the WCST is generally considered to be a test of cognitive flexibility but:

“To perform well on this task, subjects must be able to discriminate among stimuli, classify them according to abstract principles, inhibit previously reinforced responses, sustain attention to appropriate attributes of compound stimuli, and use verbal feedback.” (Ozonoff et al. 2005: 607)

It should be clear that, in as much as we label anything an individual activity, the WCST is an individual activity. It takes a very particular *savoir* of the social, one in which solitary activities can still be bonded to interpersonal conduct through shared cognitive mechanisms, to conceptualise the WCST as

‘getting at’ social behaviour. The subsequent model of autism would follow this formula; for the first time autism could become a solitary activity.

The first empirical papers examining executive functioning in autism came out of Judith Rumsey’s laboratory at the National Institute for Mental Health (Rumsey 1985; Rumsey & Hamburger 1988; Rumsey & Hamburger 1990). In 1985 Rumsey studied nine adult men with diagnoses of ‘infantile autism’ (according to DSM-III guidelines) who were given the WCST and compared to control groups. Following the WCST, Rumsey reached the following conclusions;

“(1)... High-functioning autistic men, i.e., those with good verbal skills and high IQs, as a group show significant deficits in conceptual problem-solving, (2) that these deficits involve both deficiencies in conceptual-level responding and a tendency to perseverate, (3) that the deficits are, to some extent, independent of Full Scale IQ, and (4) that considerable heterogeneity is characteristic of autism.” (Rumsey 1985: 31)

These conclusions were supported and extended, in similar populations, in two subsequent studies (Rumsey & Hamburger 1988; Rumsey & Hamburger 1990).

In the 1990s the research programme began to gather pace as executive dysfunctions in autism appeared to be differentiated from a control group comprising of children diagnosed with conduct disorder and also attention deficit hyperactivity disorder, a finding which extended executive dysfunction to children, and also appeared to differentiate autism from highly co-morbid diagnoses (Szatmari et al. 1990). Executive dysfunctioning was claimed to be exceptionally widespread, if not universal, in the autism population (Ozonoff et al. 1991) and present longitudinally (Ozonoff & McEvoy 1994), leading to claims that executive dysfunctioning was a “primary deficit” in autism (Ozonoff & McEvoy 1994: 424). By 1996 14 papers had been published on the topic (outlined in the first review on the subject by Pennington & Ozonoff (1996)) and the first papers began to appear in which particular aspects of executive functioning were probed and more specific models (i.e., that the

primary deficit in autism may be an inability to inhibit prepotent responses) began to be formed (Russell et al. 1996).

Rumsey was aware, even in the first articulation of the hypothesis (Rumsey 1985), that something interesting was occurring:

“While impairments in social relatedness are increasingly considered to constitute the core symptoms of autism, cognitive impairments are now recognized as common accompaniments.” (Rumsey 1985: 23)

Rumsey goes on to say that:

“The extent to which cognitive deficits actually underlie or contribute to the social impairments is unclear however.” (Rumsey 1985: 24)

The conclusion of the article is that:

“The shared social impairments in autism may occur on the basis of a variety of deficits – motivational, sensory-perceptual, and higher cognitive deficits – and/or positive psychiatric symptoms.” (Rumsey 1985: 34)

These three extracts all reveal important insights. Firstly, Rumsey, through terms such as ‘social relatedness’, adheres to the model of a ‘social in the shape of a crowd’. Such a construction of the social seems to be a prerequisite to framing autism as ‘social disorder’, something Rumsey does when claiming that deficits in social relatedness are understood as being *primary* to autism (Rumsey 1985: 23). Secondly, the possibility that social impairments are symptomatic of cognitive impairments cannot be precluded (Rumsey 1985: 24), indeed this is the very basis of the Executive Dysfunction account of autism which suggests that cognitive dysfunction may be central with social deficits emerging as a result of these deeper cognitive symptoms (Rumsey 1985: 34). Here too Rumsey adheres to a contemporary *savoir* of the social, positioning social and nonsocial behaviours along a quantitative spectrum rather than as qualitatively distinct arenas.

The implicit theory of sociality inherent in the Executive Dysfunction account, and the operationalisation of that theory within the laboratory, would

be completely alien to a psychoanalytic framework, previously so dominant within autism research. Hitherto ignored, uncovered, or symptomatic cognitive aspects of the disorder are being centralised. In a very real sense, the capacity to form ordering rules is, within the Executive Dysfunction account, indicative of social ability. Whether or not those described as ‘classically autistic’ (Kanner 1943) also had these cognitive deficits, it is now clear that a great many people do have these deficits who are not classically autistic. It was the conceptualisation of these behaviours as social which brought these people under the gaze of autism researchers and began the process of transforming the condition.

#### *Autism as a deficit in metarepresentations*

Arguably the most important paper in the history of autism since Kanner’s initial description (Kanner 1943), Simon Baron-Cohen, Alan Leslie, and Uta Frith’s 1985 work was the first to propose that autism was a disorder of metarepresentations or Theory of Mind (ToM). The theoretical basis behind the metarepresentational theory of autism has origins in a 1978 paper by Premack and Woodruff, ‘*Does the chimpanzee have a theory of mind?*’ According to Premack and Woodruff, the concept of ToM is taken to mean that an individual (of whatever species) “imputes mental states to himself and to others” (Premack & Woodruff 1978: 515).

In extending upon this point, Premack and Woodruff state that:

“It seems beyond question that *purpose* or *intention* is the state we [humans] impute most widely; several other states are not far behind, however. They include all those designated by the italicized term in each of the following statements; John *believes* in ghosts; he *thinks* he has a fair chance of winning; Paul *knows* that I don’t *like* roses; she is *guessing* when she says that; I *doubt* that Mary will come; Bill is only *pretending*.” (Premack & Woodruff 1978: 515, italics in original)

Premack and Woodruff conclude their article by saying that “These inferences, which amount to a theory of mind, are, to our knowledge, universal in human adults” (Premack & Woodruff 1978: 525).

Evoking Premack and Woodruff, Baron-Cohen, Leslie, and Frith (1985) questioned that universality of ToM in the human population in an article entitled *Does the autistic child have a “theory of mind”?* This question of Baron-Cohen et al. (1985) was asked, at least in part, upon the basis of a cognitive-based theory of pretense put forward by Alan Leslie that, while published afterwards in 1987, had clearly been in formation for some time (Baron-Cohen, Leslie, & Frith 1985: 38).

The ability to pretend (for example, that a banana is a telephone) typically emerges in children between the ages of 18 and 24 months (Leslie 1987: 414). Leslie notes that it is in the interests of a given organism for its representations of objects to closely mirror ‘the world’, for this is surely the best way to ensure that individual’s survival. If adults confuse bananas and telephones as a result of pretense they engaged in as a child, they are likely to find life rather difficult. Any being which engages in pretence must therefore possess the cognitive architecture that ensures that representations about the world (primary representations) are not affected by the ensuing act of pretending, avoiding what Leslie calls ‘representational abuses’ (Leslie 1987: 414). Leslie proposes a ‘decoupling’ device which allows such representational abuse to be avoided. Essentially, the mechanism ‘decouples’ the representation from reality and context and instead forms a representation of a representation, or a *metarepresentation*. The decoupling device allows one to say ‘This banana is a telephone, but only in this context, my primary representations of bananas and telephones remain unchanged’ (Leslie 1987: see 419 for an overview).

Metarepresentation theory takes on importance for the study of autism because of the following point made by Leslie:

“The emergence of pretense is not seen as a development in the understandings of objects and events as such, but rather as the beginnings of a capacity to understanding cognition itself. It is an early symptom of the human mind’s ability to characterize and manipulate its own attitudes to information. Pretending oneself is thus a special case of the ability to understand pretense in others (someone else’s attitude

to information). In short, pretense is an early manifestation of what has been called *theory of mind*.” (Leslie 1987: 416, italics in original)

Lorna Wing (1977) and others had already noted that children diagnosed with autism do not engage in much pretend play, and Leslie would propose that a similar deficit may be found in other tasks which required ToM.

In work emerging from his PhD thesis, Simon Baron-Cohen (Baron-Cohen et al. 1985) sought empirical support for Leslie’s (1987) theory of autism through a recently devised (Wimmer & Perner 1983) scenario called the ‘Sally-Anne Test’. The Sally-Anne is presented in a narrative format and is told by the experimenter, who is present throughout. The participant is presented with a desk-top scenario in which two dolls (Sally and Anne) are in a room along with a closed box and a covered basket (Baron-Cohen et al. 1985: 41). Sally has a marble and, to keep it safe while she goes out, leaves the marble in the box with the lid shut. Sally then leaves the room. While Sally is out, Anne takes the marble out of the box and places it in the covered basket. Finally Sally returns to the room. Following a series of control questions for memory and so forth, the participant is simply asked “where will Sally look for the marble?”. We know that the marble is in the basket. If we have access to Sally’s *beliefs* however, that is, if we have ToM, we know that she will look in the box. A participant passes the Sally-Anne Test, therefore, if they correctly identify that Sally will look in the box.

Baron-Cohen et al. (1985) compared the performance of three groups of participants on the Sally-Anne; a group of adolescents diagnosed with autism, a group of chronologically matched participants with Down Syndrome, and a ‘normal’ control group of children aged around four-and-a-half years. Baron-Cohen found that, while the majority of the typical (86 per cent) and Down (85 per cent) control groups were able to pass the Sally-Anne, 80 per cent of the autism group failed the test. Baron-Cohen et al. thus “conclude that the autistic children did not appreciate the difference between their own and the doll’s knowledge” (Baron-Cohen et al. 1985: 43). In other words, the autistic children lacked a ToM.

This initial paper was soon joined by others reporting similar findings utilising a range of different paradigms and making comparisons to different control groups (Baron-Cohen et al. 1986; Leslie & Frith 1988; Baron-Cohen 1989a; Baron-Cohen 1989b; Baron-Cohen 1989c). By far the most important of Baron-Cohen's three papers in 1989 (Baron-Cohen 1989c) was that examining those individuals with autism who had passed the ToM tests presented in previous experiments. That a significant minority (between 18 and 28 per cent; Eisenmajer & Prior 1991: 352) of people with autism could pass ToM tests was a consistent finding. This would seem to suggest that a ToM deficit could not be a primary deficit in autism as it would be possible to be autistic and have an intact ToM (Ozonoff & McEvoy 1994: 415). Baron-Cohen notes that all paradigms thus far investigating ToM deficits had utilised 'first order belief attribution' (FOBA) tests, that is, tests that require one to know that 'X believes this'. A second order belief attribution (SOBA) is the knowledge that 'Y believes that X believes this' (e.g. 'where does the experimenter believe that Sally will look for the marble?'), a significantly more complex ability not usually found in children until around the age of seven (Baron-Cohen 1989c: 288). When a group of 10 individuals with autism (who could all pass FOBA tests) were provided with a scenario in which SOBA was required they uniformly failed, thus restoring the possibility that a problem with metarepresentation was a core, universal deficit in autism.

It is worth considering the model of sociality enacted within false-belief tasks such as the Sally-Anne Test. Firstly, sociality is, again, being directly equated with interpersonal. As belief about Sally's actions requires knowledge of Sally's mental state, it is an *a priori* assumption of the Sally-Anne Test that what is in question is a *social* belief and that an inability to comprehend Sally's motives is indicative of *social disorder*. Further, and as would be expected for a theory dependent upon the contemporary *savoir* of the social, metarepresentations theory still considers social activities to emerge as symptoms of nonsocial cognitive modules. While the relationship between social behaviour and Leslie's metarepresentations is nuanced, the proposed metarepresentation module does not deal exclusively with interpersonal

behaviour. Once again social behaviour is seen as arising from nonsocial cognitive architecture.

Indeed, a diagram in Leslie and Frith (1990: 124) makes this quantitative distinction between the social and the nonsocial clear. The diagram proposes three possible aetiologies for autism; a basic affective disorder, a combined affective/ cognitive disorder, or a basic cognitive disorder. The authors explicitly (Leslie & Frith 1990: 123) favoured the third of these options, stating that affective difficulties were an emergent consequence of this unitary disturbance. While it would be wrong to conclude that ‘affective’ is synonymous with ‘interpersonal’, in the context of the work by Hobson, considered at length below, to which the article in question is a riposte, there is a significant overlap. The dismissal of ‘affective’ processes by Leslie and Frith (1990) (which Hobson explicitly distances from cognitive processes (1993: 79)) is also, to a significant extent, a dismissal of uniquely interpersonal processes. As quoted above, for Leslie the social is about one’s “own attitudes to information” (Leslie 1987: 416).

Authors from the same time period also recognised this aspect of the ToM hypothesis in which the division between social and nonsocial cognition is made quantitative. In 1991, Leekam and Perner conducted empirical work which did not support the metarepresentational account of autism and prompted a theoretical shift from Leslie (Leslie & Thaiss 1992). Leekam and Perner note:

“This suggestion [of change in theory from Leslie] deprives the original idea of one of its most interesting implications, namely that autism might involve very specific problems which do not involve the understanding of human agents in particular.” (Leekam & Perner 1991: 214)

Like Executive Dysfunction, ToM drew upon the *savoir* (Foucault 1972: 15; Hacking 1995a: 198-200) of cognitive psychology and its particular production of the social, reconstructing autism in ways we now know to be hugely significant.

### *Autism as a form of Weak Central Coherence*

The final cognitive model to be considered here is the Weak Central Coherence (WCC) hypothesis. In a sense, WCC belongs not with ToM and Executive Dysfunction theories but with a second generation of theories (such as Empathising/Systemising Theory; Baron-Cohen et al. 2005) that emphasise autism as a social difference rather than social deficit. WCC was the first model of autism to take this step and, analytically, culturally, and politically, that move should not be underestimated. Nonetheless, WCC is suitably close to executive dysfunctioning and metarepresentation accounts of autism, temporally and aetiologically, to ensure it is worth considering alongside those models.

The WCC hypothesis is demonstrably similar to the Executive Dysfunction account in that emphasis is placed upon the ‘non-triadic’ and nonsocial aspects of autism such as savant abilities and a child’s I.Q. profile. While more consideration is given to the ‘triadic’ social deficits found in autism in WCC than in Executive Dysfunction hypotheses, these are explained with reference to the nonsocial cognitive style of those with autism. As with Executive Dysfunction, difficulties in the comprehension of interpersonal relationships are made out to be symptomatic of a nonsocial deficit/difference (Frith 1989: 163).

Uta Frith is, once again, the central figure (along with another of her PhD students, Francesca Happé) in the WCC account of autism. It is the only one of the new cognitive theories which is first elucidated in a book, *Autism: Explaining the Enigma* (Frith 1989), rather than an empirical study. Perhaps accordingly, there is an early emphasis on reinterpreting existing empirical work as well as work conducted further studies. According to both Frith and Happé (Frith 1989: 174; Frith & Happé 1994: 119; Happé 1997: 1) the theory of WCC emerged due, primarily, to the search for parsimony. While a ToM deficit was excellent at explaining the famed triadic impairments of social functioning, communication, and imagination in autism it was less able to explain many of the non-triadic aspects of the disorder. Frith and Happé (1994: 119) include among a list of consistently found non-triadic features in

autism restricted interests, desire for sameness, islets of ability, idiot savant abilities, excellent rote memory, and a preoccupation with parts of objects.

The consistent findings of ‘islets of ability’ and ‘idiot savants’ manifests itself in what have been called ‘spiky’ I.Q. profiles, where some areas of performance are exceptionally good and others exceptionally poor. The Wechsler-Intelligence Scales (W-IS), for example, have ten subtests (Happé 1994b: 1462) in which participants are expected to score reasonably evenly. In autism however, there are consistent peaks, such as in the ‘block design’ subtest, and consistent troughs, as in the ‘verbal comprehension’ subtest. It is hard to explain this pattern of ability with reference only to ToM or metarepresentations. A quest, initially at least, for parsimony therefore seems to have driven Frith to consider ToM deficits to be symptomatic of a larger difference, one that could explain both peaks and troughs.

Frith incorporated research she had been involved in during the early 1980s into the WCC account, firstly research conducted with Maggie Snowling (Frith & Snowling 1983; Snowling & Frith 1986) and secondly that conducted with Amitta Shah (Shah & Frith 1983). With Snowling, Frith seemed to demonstrate that those with autism were less able to take account of word context when reading out loud. Reading in context was tested by examining the number of errors those with autism made when reading homographs, words that are spelt identically but pronounced differently. The findings suggested that while those with autism were more than capable of reading single words, there was also a deficit when ‘reading for meaning’.

In an apparently unrelated test, Shah and Frith (1983) demonstrated that individuals with autism had an ‘islet of ability’ (i.e., a peak in performance) when it came to completing an ‘embedded figure’ paradigm. In an embedded figure paradigm a participant is given a cardboard shape, for example a circle. The cardboard shape appears in a larger picture, and it is the role of the participant to find the location of that shape within the larger picture. For example, our circle may be embedded in a picture of a car as one of its wheels. In the Shah and Frith study (1983) participants were given various tasks of this nature. Those diagnosed with autism vastly out-

performed control groups and, indeed, performed in line with their chronological rather than mental ages (Shah & Frith 1983: 617). Such a finding led to the conclusion that:

“With the present test an effort is needed to resist the tendency to see only the forcefully created gestalt which in itself is a meaningful picture [e.g. the car]...Perhaps they were able to locate the target figure [e.g. the wheel] so easily because the overall meaning of the complex figure (or embedding context) was not relevant or dominant for them.” (Shah & Frith 1983: 618)

In this quote, then, we see both the perceived similarities with the Snowling work and also the beginnings of what would become the WCC hypothesis; that people with autism find it hard to see the wood for the trees.

Frith describes the WCC in the following terms:

"We have now enough evidence to formulate a hypothesis about the nature of the intellectual dysfunction in Autism. In the normal cognitive system there is a built-in propensity to form coherence over as wide a range of stimuli as possible, and to generalize over as wide a range of contexts as possible. It is this drive that results in grand systems of thought, and ultimately in the world's great religions. It is this capacity of coherence that is diminished in autistic children. As a result, their information-processing systems, like their very beings, are characterized by detachment." (Frith 1989: 100)

An important point of interest is that, as of 1989, Frith proposed that WCC could itself explain ToM deficits. As Frith notes:

"Seeing something, expecting something, and being told something can all be equivalent in their consequences on mental states and on behaviour. This draws our attention to the supramodal and hence central nature of mentalizing. Information from different sources, the results of seeing, remembering and telling, are all pulled together in a coherent interpretation of what happened. Because it is a coherent whole, the information content is so simple that any normal four-year-

old can cope with it. If it were not a coherent whole, perhaps because of a weak drive for coherence, but remained a complex set of separate pieces of information, then anybody would find it difficult. This may be the situation for autistic people." (Frith 1989: 163)

Such a grand-narrative allows for the parsimony that Frith sought. Nonetheless, this explanation of WCC as *the* explanation was fairly short lived and in the 1990s Frith concluded that WCC must be a separate deficit to ToM (Frith & Happé 1994, see chapter 5: 112-116).

Again with the WCC account of autism we see a hypothesis which entirely coheres with the *savoir* of late-twentieth century psychology. The, apparently, social and nonsocial aspects of autism are united through a shared cognitive mechanism, erasing the divide between the two domains. Further, we see the social articulated in terms of ‘mentalizing’, or comprehending the actions of those around us. As with both the metarepresentations account and the Executive Dysfunction account we see autism re-positioned away from a condition which is inherently about interpersonality and towards a ‘social disorder’ more broadly defined, to include the capacity to build jigsaw-like puzzles and read for context.

### *A psychoanalytically inspired challenge*

#### *Hobson’s affective theory of the aetiology of autism*

Latour has famously claimed that it is in controversy that the workings of the sciences can most clearly be observed (Latour 1987: 15). Fortunately for present purposes, the advocates of the three models of autism described above engaged in a prolonged and sometimes heated debate with RP Hobson. One need only glance at Hobson’s work, particularly his review articles, to appreciate that this author comes from a very different place intellectually to the cognitive psychologists. Hobson’s work is littered with references to philosophers, most notably David Hamlyn, Maurice Merleau-Ponty, and perhaps above all others Ludwig Wittgenstein (for example, Hobson 1990a: 115-116, 1990b: 208, 1990c: 165, 1991: 35, 1993: 5). Importantly, Hobson is also associated with, and sympathetic towards, psychoanalytic perspectives on

autism (Hobson 1989: 23, 1990c: 176-177, 1990d, 1993: x, 34) particularly those theories based in object relations (Hobson 1990d: 334). In addition to these already notable differences, Hobson is also a medically trained psychiatrist (Hobson 2002: xv).

Hobson states that his theory of autism is that it is a disorder “of affective and social relations – and irreducibly so” (Hobson 1989: 22), an opinion shared by Kanner who was similarly linked with psychoanalysis. While this description of autism coheres with the view of a social in the shape of a crowd, thus keeping Hobson’s theory reasonably close to those emerging from within cognitive psychology, there are also areas of sharp difference. Hobson, basing his work upon the philosophy of Martin Buber (1987), parses human interactions into two categories which he believes to be *qualitatively distinct*; ‘I-Thou’ relations and ‘I-it’ relations. Thus Hobson, basing work from a psychoanalytic perspective, cannot be seen as working from within the same *savoir* as those researchers previously discussed. Generally, I-thou relations refer to interactions between the self and other people (this is the ‘social’ realm) while I-it relations refer to encounters between the self and non-human objects<sup>11</sup>, the nonsocial realm. These two forms of interaction are, according to Hobson, qualitatively different and more or less independent. It is actually in this parsing of interactions into ‘I-thou’ and ‘I-it’ relations that the reason for Hobson’s focus upon affect becomes important, for Hobson believes that all ‘I-thou’ relations are inherently affective, while ‘I-it’ relations do not necessarily contain this component. As one of Hobson’s titles (Hobson 1993b) suggests, there are “emotional origins of social understanding”.

It is not always entirely clear what Hobson means when he talks of ‘emotion’ and ‘affect’. At times ‘affective’ operations seem to be qualitatively different from ‘conative’ or ‘cognitive’ operations (Hobson 1984: 86), at other times they are not (Hobson 1993b: 228; Hobson 1993: 4). At times a deficit in affect is “pivotal” (Hobson 1989a: 23) to the theory and apparently synonymous with (or at least inseparable from) the social (Hobson, Ouston &

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<sup>11</sup> This lack of complete harmony is found because it is possible to treat objects ‘as’ people – for example a pet, or a teddy bear – and people ‘as’ objects – for example when taking their visual perspective (Hobson 1984).

Lee 1988b: 441; Hobson, Ouston & Lee 1988a: 919; Hobson 1989a: 23; Hobson 1989b: 198) at others affect seems less important (Hobson 1986: 339) and divisible from the social (Hobson 1989a: 36). Certainly the issue of what exactly affect *is* warrants further attention than Hobson gives it. It is tempting, and perhaps useful, to consider Hobson's infant with 'affectively charged perceptions' in Lacanian terms, as a collection of drives without a unified sense of self, the pre-social ego before the mirror stage (Lacan 1968). The fact that Hobson, like Lacan, compares infants' drives with the pre-symbolic affect of animals further renders this comparison tempting (see below). However it should be remembered that the Lacanian branch of psychoanalysis is never explicitly mentioned by Hobson and must be considered to be aiding our understanding, rather than his. Considering all of this, and while remaining in keeping with the authors of the time (e.g., Baron-Cohen 1988; Leslie & Frith 1990<sup>12</sup>) and referring to Hobson's theory as the 'Affective Theory' of autism, it is important to remember that the role of affect in Hobson's thesis is particularly nuanced.

It is against this theoretical background that Hobson's theory of autism can be articulated. Hobson believes that we do not come into the world with a repertoire of I-thou and I-it modules ready to draw upon at suitable moments (as might be suggested in purely cognitive accounts), rather this understanding of the world develops over time based upon our intersubjective experience:

“In order to acquire the concept of persons...one needs to have felt what it is like to be a person in relation to others. One must have had emotional contact with others, and have apprehended what it is like to share experiences with someone else. Without the kinds of interpersonally co-ordinated feelings that make 'sharing' of experiences possible...one would inhabit an impersonal world.” (Hobson 1993b: 229)

It is in this vein that Hobson titles his book *Autism and the Development of Mind* (Hobson 1993a); a mind is not give, it is something that

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<sup>12</sup> Ozonoff et al. (1990) prefer 'Emotional Theory'; affect and emotion are treated synonymously by all.

emerges slowly over time. According to Hobson individuals with autism have, due to more or less severe innate biological deficits, been denied the opportunity to share in the interpersonal (social) world and form a complete concept of ‘persons’. The biological deficits proposed here are far more rudimentary than, for example, the faulty decoupling mechanism proposed by Leslie (1987). These deficits are pre-conceptual, and are “affectively appraised in the very act of perception” (Hobson 1993b: 240). A dog does not need to possess a detailed concept of ‘fear’ to retreat from its owner’s harsh voice, but such a perceptual response (in humans at least) may be a precondition to the subsequent development of the concept of fear. If individuals with autism did not innately possess emotionally charged perceptions when relating to other people then they would be denied the opportunity to form the appropriate concept of persons. This is Hobson’s theory.

Hobson lists a wealth of examples of “interpersonal-affective coordination” from very early childhood which emerge in normally developing children (e.g. Hobson 1993a: 33-52) and which may be impaired in the autistic child, from the development of a social smile at 6 weeks of age to the tendency to engage in joint-attention. The specific mechanisms relevant to autism do not seem to be of much interest to Hobson, and his focus on other conditions which also deny some access to the ‘I-thou’ sphere of relations (notably the personal pronoun confusion sometimes found in young children with congenital blindness (Hobson 1993a: 203-206; Hobson 1990c: 117-119)) demonstrate that Hobson’s focus is less upon these biological pre-cursors themselves and more upon the subsequent and crucial development trajectory.

#### *Hobson’s experimental studies of emotion*

As shown above, a focus upon affect/ emotion, in a simplistic sense, is somewhat misleading and critics who focus upon this aspect of Hobson’s work (e.g. Baron-Cohen 1988) are prone to miss the mark. Nonetheless, the affective portion of Hobson’s work does retain particular importance, for it is affective relations which he sought to test in a laboratory environment. As noted above, many aspects of Hobson’s background may have been very

different to the cognitive psychologists, but he was and is primarily an experimental psychologist and this is crucial for if he was not, it seems unlikely that Baron-Cohen, Frith, Leslie, or Ozonoff would have spent as much time as they did concerning themselves with Hobson's philosophical musings, despite his direct attacks upon their theories (e.g. Hobson 1991a).

Broadly speaking, Hobson's experimental methods fall into one of two paradigms; 'post-box' experiments (e.g. Hobson 1983; Weeks & Hobson 1987) and matching experiments (e.g. Hobson 1986a; Hobson 1986b; Hobson 1987; Hobson, Ouston & A Lee 1988a; Hobson, Ouston & A Lee 1988b; Hobson et al. 1989). It is immediately apparent that even within a laboratory environment the stimulus always features another person, in clear contrast to a great many of the cognitive experiments described above. Once more this gives insight into Hobson's constructions of the social; it *must* concern interpersonal conduct. The post-box design utilised in Weeks & Hobson (1987) was used to investigate the salience of facial expressions for children with autism. Two post-boxes were placed in front of the children and on each post-box there was a photograph of an adult's face. These photographs differed from one another in three ways; the sex of the model (male/female), the emotion of the model (happy/neutral), and the type of hat worn by the model (wide brimmed/woolly) (Weeks & Hobson 1987: 142). The participants were presented with a stack of cards which featured similarly attired models; their task was to post the photograph into the matching post-box. Imagine our two post-boxes feature a (1) happy man wearing a woolly hat and a (2) neutral woman in a wide-brimmed hat. If a stimulus photo was of a neutral man in a wide brimmed hat it would be perfectly reasonable to sort this photograph into either post-box; if we sorted by sex we could place the card in post-box (1), if we sorted by emotion or hat we could place it in post-box (2). By examining decisions made across the entire stack of cards, Weeks and Hobson sought to establish which feature was most salient for both autistic and control participants. Both groups sorted by sex first (Weeks & Hobson 1987: 144) but when the sex was removed as a variable (e.g. by changing the photograph on (1) to a happy *woman* wearing a woolly hat) it was found that the control group next sorted by emotion, while the autistic group sorted by hat

type. Indeed, only 6 of 15 autistic participants could sort by emotion in the absence of all other variables (Weeks & Hobson 1987: 148), leading to the conclusion that individuals with autism might have particular problems with the processing of emotional/ affective stimuli.

A series of experiments were conducted by Hobson and colleagues in the late 1980s (e.g. Hobson 1986a; Hobson 1986b; Hobson 1987; Hobson, Ouston & A Lee 1988a; Hobson, Ouston & A Lee 1988b; Hobson et al. 1989) utilising a matching paradigm and the first of this set (Hobson 1986a) is typical. In this experiment Hobson wanted to examine the ability to “understand the ‘meaning’ of different bodily expressions of emotion, such as a happy face or gesture, or vocalisation.” (Hobson 1986a: 323). Children from both autism and control groups were asked to watch a short video clip of an actor ‘acting out’ a particular emotion. In the video, the actor’s face was covered so that only the body could be seen. When the video finished, participants were presented with a drawing of a figure reproducing the final pose of the character in the video. Alongside the posing figure were placed five faces expressing different emotions. The participants were immediately told “*There* the person is. Now what face goes with *that* person?” (Hobson 1986a: 327, italics in original). While the statistical conclusions appear far from clear-cut (Hobson would later suggest this is because of the inherent difficulty in testing for emotions in a laboratory setting (Hobson 1991b)), Hobson concludes that:

“The results from these experiments are unequivocal: compared with young normal children and with same-age retarded children...autistic children demonstrated a marked impairment in choosing the appropriate drawings and photographs of facial expressions of emotion to “go with” videotaped gestures, vocalisations and contexts characteristic of happy, unhappy, angry and fearful feelings.” (Hobson 1986a: 336)

Future studies would replicate these findings when videos were compared to photographs rather than drawings (Hobson 1986b), when participants were asked to differentiate stimuli in terms of age and sex (Hobson 1987), and when

participants were asked to match photographs of emotional people to different vocalisations – both in forced (Hobson et al. 1988a) and free (Hobson et al. 1989) recall scenarios. Conclusions remain the same throughout however; that there are “emotion recognition defects in autism” (Hobson et al. 1989: 249), and with emotion comes an inherently interpersonal aspect to the disorder.

In the context of his body of work, we can now fully comprehend Hobson’s conclusion that metarepresentational theory is “nondevelopmental, nonsocial, and restrictively cognitive” (1990: 114) and his declaration that he is ‘against the theory of theory of mind’ (1991). In return, Leslie and Frith fervently deny their theory to be nonsocial, somewhat dismissively stating that the “theory can hardly be considered non-social” (Leslie & Frith, 1990: 122). The discourse in this debate is often stark. ‘It’s cognitive or affective’ says Simon Baron-Cohen (1988). Occasionally it gets personal; Leslie and Frith (1990) spot the opportunity for a pun and suggest that there is really only a ‘Hobson’s choice’.

It is apparent that at no point is a decisive empirical test established to falsify any of the theories involved in these disputes; within twenty years references to Peter Hobson’s work, and certainly to his theoretical framework, have simply fallen from the field while Executive Dysfunction, ToM, and WCC remain on the table. Given the analyses presented here and in chapter 2 it becomes easier to see why this is the case. This was not a debate over empirical findings, over *connaissance*. This was a debate over *savoir* and a pitting of the psychoanalytic conception of the social which inspired Hobson against the mainstream social psychological reading of Executive Dysfunction, Metarepresentation, and WCC. As history has demonstrated it is perfectly conceivable for different constructions of the social to be investigated within an experimental paradigm. But, just as the gestaltian analyses of Kurt Lewin were not compatible with the changing constructions of psychology during the 1950s (Danziger 2000: 342; see chapter 2 (pp.32-35) for an overview), neither were Hobson’s with the 1990s. Autism would emerge from the 1980s reflecting the image of only one, very particular, *savoir* and with a similarly particular trajectory determined for the disease concept.

## Conclusion

This chapter has made several arguments. Firstly it has been claimed that the groundwork for the contemporary experience of autism, as related to three key theoretical frameworks (Metarepresentations, Executive Dysfunction, Weak Central Coherence), was laid down over a remarkably short period of time in the latter half of the 1980s. Secondly, focusing upon the notion of *savoir* it was claimed that a particular production of autism became available to questions of truth and falsity following a radical reconstruction of ‘the social’ in which human sociality was considered to be both intra-personal in nature and continuous with nonsocial cognition (Greenwood 2004a). The *savoir* of the three dominant cognitive theories was brought into relief through an examination of Peter Hobson’s Emotional/Affective Theory of autism which did not share this depth knowledge.

As Hacking has said:

“When new descriptions become available, when they come into circulation... then there are new things to choose to do. When new intentions become open to me, because new descriptions, new concepts, become available to me, I live in a world of new opportunities.” (Hacking 1995a: 236)

The current chapter has argued that the *savoir* of cognitive psychology offered new descriptions of the social to autism researchers in which the social extended to a range of (what would previously have been seen as) nonsocial acts. It thus became possible to see social disorder in new places, to perform novel actions, find novel truths, and construct novel autisms.

Yet it was not just the construction of autism which changed during this time; the relationship between autism and the rest of humanity also changed radically. In an ongoing project, Viney (e.g. 2013) has begun to think about how some human classifications come to be seen as ‘inherently useful’ “not for what they do,” within a laboratory for instance, but rather “simply for what they are” (Viney 2013). Viney’s own work centres on twins; of interest since antiquity but especially since monozygotic twins were seen to constitute a

natural experiment into Mendelian genetics and, thus, human nature more broadly.

Given that we are in an era of ‘brainhood’, whereby the self is frequently aligned to a particular brain state (Vidal 2009) it is perhaps unsurprising that some individuals with particular types of brain are articulated as being of inherent scientific value. Allan Young has described how neuroscience has broadly adopted the notion of a ‘Jacksonian brain’ (Young 2012: 165), named after John Hughlings Jackson. In the Jacksonian brain, there are posited to have evolved numerous more or less independent control centres. Crudely, the reptilian sub-cortical brain evolved first, with the mammalian cortex evolving next, and the human neo-cortex evolving last. Each of these centres has (literally) top-down control over existing regions of the brain. When a brain region is damaged (because of alcohol, disease, or some innate disordering) then the inferior brain regions are ‘released’ and we behave in a manner akin to our evolutionarily distant relatives (Young 2012: 165). In such instances a natural experiment into human nature has commenced. It is this belief that the personality of an individual is essentially additive (rather than a non-localisable whole) assembled brain-region-by-brain-region that allows individuals such as Phineas Gage to become of interest to psychology; one man’s disorder reveals humanity’s order.

Allan Young has described how human nature became socialised when it was posited that our brain evolved in the manner it did not simply in response to past physical environments but also past interpersonal (social) environments. What is more, empathy came to be seen as sitting at the heart of this interpersonally determined evolution, a crucial element in determining what it means to be human (see chapter two: 36-38). Within this context, the Metarepresentations account of autism, which articulates a social disorder existing *within* the individual (rather than developmentally determined, *qua* Hobson) and *specifically* relating to empathy and ToM, positions autism as being absolutely at the heart of human nature, a natural experiment into human-minus-social. This is a remarkable status to give to a condition that, until the 1940s, had never been described. The rest of this thesis describes

how that condition continues to be performed and constructed within the discourse of cognitive scientists working today.

## A coherence of alien forms: order and disorder in autism

### Introduction

The previous chapter contains a description of how a very particular *savoir* of the social, with a history from within the social and cognitive psychologies, provided the conceptual framework to allow for a singular, novel construction of autism to emerge during the latter half of the 1980s. Nonetheless, at the level of *connaissance* and within the talk of contemporary psychologists and neuroscientists interviewed for this project, autism appears to be a classification in crisis. Autism is now described by one interviewee as: “...a multi-faceted thing that is quite hard to describe with, based on just one core, one variable” (JH08: 59-60). This multiplicity means that it is, to give the title of one of the most important papers on autism in the last decade, ‘time to give up on a single explanation for autism’ (Happé et al. 2006). This view is echoed by BG, an Assistant Professor, who thinks that the search for a grand theory of everything, such a central narrative to debates in the 1980s, is unlikely to be successful:

BG: Well there is no one (.) model like the physicists have, there is no, er, there is no one model that explains er, everything. Er, and I don't think that we'll get one model that explains everything... (BG06: 120-123)

BG, who has a background in the natural sciences, makes a stark point. Unlike the objects of physics there is no unitary essence to autism, ‘there is no one model that explains everything’. It might be possible for one scientific theory to capture completely the essence of atoms but something in the nature of autism resists such classification. In the extract below a second interviewee, a Lecturer, makes a similar point. EC explains how they are currently teaching undergraduates about the history of autism research and how there had previously been a search for a “holy grail” (EC11: 92) in psychology, one cognitive theory that would explain all the behavioural manifestations of autism. This, claims EC, is a search which has been largely abandoned:

GH: So, (.) you do, you, so it's not the case, you do think that there's, there isn't a holy grail out there, do you think? It's, it's not.

EC: Oh, I don't think we're going to, well, it's just my personal opinion, I don't, you know, I don't think there's going to be one neat cognitive explanation. (EC11: 109-114)

While EC is careful to frame their answer as 'just their opinion' it is noticeable how closely the statement mirrors that of BG; that autism is going to resist 'one neat explanation'. MW, a Professor, makes a similar point. Not only has the search for the 'holy grail' of autism, the unitary core at the cognitive or neurological level, failed up until this point, in all likelihood MW believes that future attempts will also be futile:

MW: ...And there are clearly going to be, well there must be many, many different causes in different individuals on the autism spectrum. Erm, whether those will unify down to a final common pathway in terms of some brain systems or some (.) interrupted neurodevelopmental processes (.), erm, again I wouldn't, hmm, I don't know how much money I would bet on that ((laughs)). (MW18: 432-438)

Descriptions of this type, referring to a radical uncertainty in autism, are almost entirely missing from the research conducted during the 1980s. Indeed, several interviewees claim that these new issues of *heterogeneity* are among the most important contemporary discussions in the field:

DF: ...I think one issue is whether or not these, erm, t', t', whether or not the behaviours in autism, the behavioural features of autism are:: dissociable, so whether you do have just, whether, basically, autism is made up of kind of a problems in social interaction, problems in communication and repetitive behaviours, that somehow get kind of, basically these people get, quote unquote, unlucky, erm, and get all three and that causes autism, erm, I'm (.) and that obviously leads onto, I mean if you look at the cognitive level then the major debate would be well you've got three different things causing those, each of those distinct behaviours, so you've got something causing the, erm, some cognitive atypicality causing the, erm, social communication problems for example, and then some other cognitive atypicality causing the repetitive behaviours for example... I don't know if it is a zeitgeist, maybe it is, kind of the prevailing view maybe... (DF03: 206-220, 248-249).

DF, a Reader, is clearly in agreement with EC, JH, BG, and MW who are quoted above: there is a 'zeitgeist' or perhaps a 'prevailing view' that the core symptoms of autism are, behaviourally and cognitively, 'dissociable'.

While the word ‘zeitgeist’ is not used in the extract presented below, a very similar level of importance is clearly attributed to the notion of heterogeneity. Prior to the extract presented below the interviewee (MN, a Research Fellow), who strongly refutes claims of heterogeneity, has stated within in the interview that they expected to find heterogeneity in their research sample but in fact did not. MN was asked if they believe such findings of homogeneity to be typical of the field in general:

GH: Saying, you know, heterogeneity at the core of this disorder. Do you think a kind of (.) that idea’s being rethought so that there is a general move away from heterogeneity?=-

MN: =No::: everybody, heterogeneity is the current thing in autism, like everyone loves it, we’ve got, we’ve got this wonderful the autisms term that everybody’s talking about and, no it’s the current very in thing, but I prefer to be working against, [against the] ((laughs)) (.) erm, status quo.

GH: [[[Laughs]]]. (MN05: 292-301)

While MN ‘enjoys to work against the status quo’ and thus refutes notions of autistic heterogeneity, that statement in itself places the concept of heterogeneity at the very centre of *connaissance* of autism, as an accepted and known fact. From these extracts, coming from interviews with six different researchers at six different institutions and covering all levels of seniority from Post Doctoral (JH) to Professorial (MW), there is the clear suggestion that ‘everybody loves’ heterogeneity, it is ‘*the* current thing’, to the extent that some researchers now talk about ‘autisms’ rather than autism.

Extracts such as those presented above stand in direct contrast to the literature from the 1980s, which was the focus of the previous chapter, when it was the norm to discuss the ‘core’ deficit at the heart of a ‘universal’ and ‘cross-cultural’ vision of autism<sup>13</sup>. This chapter will explore notions of heterogeneity in autism research. It will be argued that the construct of autistic heterogeneity is itself heterogeneous and, at different times, refers to

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<sup>13</sup> Direct comparisons between published literature and interview data should of course be made cautiously, given that ‘local uncertainties’ may well be transformed into ‘global certainties’ through the act of scientific publication (Star 1985). Nonetheless, uncertainty is also present in the contemporary published literature in a reasonably novel manner (see section *Hackers and Passers*, below) and so some tentative temporal conclusions might be hazarded.

heterogeneity at the level of epistemology, ontology, individual, and population. The chapter then goes on to consider a lingering *experience* of social disorder and argues that it is this ordered experience that facilitates continuing scientific research in the face of a disordered *connaissance*.

### Heterogeneities

When discussing heterogeneity it is important to consider that the term could mean a number of different things, and indeed many of these options are present in the interview data. A key split in concepts of heterogeneity can be illustrated by comparing autistic heterogeneity with the notion of probability. In *The Emergence of Probability*, Hacking (1975: 11-17) argues that the concept of probability has an inherent duality. Firstly, *epistemological probability* indicates that a lack of knowledge about a given event/object ensures a degree of uncertainty. For example, when it is claimed that ‘Socrates probably died in 399BC’ the ‘probably’ does not refer to the nature of Socrates’ execution but to knowledge about the event. Socrates is not usually thought of as being like Schrödinger’s Cat; a person called Socrates either did or did not die in that year. By comparison, *aleatory probability* refers to a probabilistic quality inherent in an object. When it is claimed that ‘the die will probably land on a number greater than one’ there is reasonable certainty regarding the knowledge of long-term frequency distributions of dice, and what is being referred to instead is the uncertainty inherent in the landing of the die itself. Hacking has argued elsewhere (Hacking 1983; Hacking 1990) that it was an emergence of a biopolitical knowledge with aleatory conceptions of human populations at its heart which revolutionised constructions of the normal and abnormal in the nineteenth century (see chapter two: 20-22). Such a duality may immediately be applied to concepts of autistic heterogeneity. Epistemic heterogeneity would refer to the claim that autism *appears* heterogeneous in the light of uncertain (scientific) knowledge. Aleatoric heterogeneity would refer to the claim that autism *is* heterogeneous and that knowledge of this heterogeneity is sound.

### *Epistemic heterogeneity*

While rarely explicitly understood as statements about scientific *connaissance*, claims of epistemic heterogeneity took several forms within the interview data collected for this thesis. Firstly, heterogeneity could be seen as an artefact of poor disease classification. That the American Psychiatric Association's (APA) Diagnostic and Statistical Manual of Mental Disorders (DSM) constructs psychiatric disorders as categorical rather than dimensional in nature has, of course, been an issue of prolonged debate (e.g. Anckarsäter 2010) and this is a discussion from which autism has certainly not been exempt (e.g. Skuse 2012). Indeed, the idea that there is 'no such thing as autism', and that disparate subgroups of quite different individuals were being lumped together was a frequent claim within the research interviews. HB, a Reader, for example states that:

GH: So we're going to get to the real crux of it and what is autism?

HB: Well I don't think there is one autism so to say what, what autism is, is I think a bit of a (.) non-starter to be honest. (HB01: 184-188)

HB is here advancing the viewpoint previously articulated by MN, who mocked 'this wonderful the autisms term' that has obtained popularity within autism research. In HB's extract it appears that 'autistic' heterogeneity is an epistemic artefact of grouping different 'autisms' together. If scientific *connaissance* of autism improved, heterogeneity could conceivably disappear. The same point is made even more bluntly below by MC, a postdoctoral Research Fellow:

GH: ...if I was going to ask you what you think autism is, erm, how do you think it's best described?

MC: (.) Erm. I honestly think that it's, at the moment, what we call autism is just a collection of many different subgroups. I don't know, maybe this is controversial, I don't even know if there is such a thing. (MC12: 169-174)

The argument being advanced here, as with HB, is that it is unlikely that there is 'such a thing' as autism; this is once more an epistemic claim about heterogeneity, that it is a faulty classification which leads to a number of diverse groups of individuals being placed together. It is conceivable that a reorientation away from behaviour and towards, for example, genetics could

bring about the dissolution of autism and heterogeneity; the introduction of classifications based around single, known genetic abnormalities, for instance *CATNAP2 condition* (Peñagarikano et al. 2011) or *Shank3 syndrome* (Peça et al. 2011), may well solve this problem of disorder.

A second form of epistemic heterogeneity stems from an attempt to integrate numerous diverse scientific methods. For example, since Baron-Cohen first utilised the Sally-Anne test to examine autism in the 1985 (Baron-Cohen et al. 1985), the number of theory of mind tests available has exploded. The sheer range of tests, and the differences between them, may lead to contradictory findings and apparent heterogeneity. AO, a Research Fellow, states that:

AO: ...I mean the experiments that are used are hugely mixed, erm, the samples that are used varies, the mental state that you're looking at varies, whether you're look at something like a belief versus an intention. So it's really not surprising that there's not a huge amount of agreement. (AO04: 549-554)

According to AO heterogeneity is a performance artefact; it has been the assumption of the sciences that examining different 'mental states', such as 'belief' and 'intention', and doing so through a 'hugely mixed' set of experimental paradigms should yield uniform results. If science could order its epistemic tools then order could likewise emerge in autism. This is an epistemic claim repeated by EC, a Lecturer, who states that "...you look at other areas and it becomes much more higgledy-piggled what people have used, and the task they've used and how they've measured it" (EC11: 932-934). Cognitive psychology has thus become 'much more higgledy-piggled' and the 'hugely mixed' experimental paradigms ensure a certain lack of homogeneity.

Diversity has not only increased within cognitive psychology. Since the 1980s a vast array of new methodologies has been applied to the study of autism, including a number of emerging technologies allowing researchers to directly integrate biology. The twenty psychologists spoken to for this project claimed an interest studying autism while utilising techniques as diverse as animal modelling, Bayesian Decision Theory, behavioural experimentation,

behavioural genetics, classical conditioning, cognitive experimentation, electroencephalography (EEG), epidemiology, event-related potential (ERP), eye-tracking, focus groups, functional magnetic resonance imaging (fMRI), galvanic skin response, longitudinal research, magnetoencephalography, near infra-red spectroscopy (NIRS), molecular genetics, optical brain imaging, proteomics, psychophysics, quantitative content analysis, resting state magnetic resonance imaging (rsMRI), screening studies, stem cell research, structural magnetic resonance imaging (sMRI), surveying, and tensor diffusion imaging (TDI). Perhaps unsurprisingly, attempts to integrate these forms of analysis have proven difficult and, on occasion, have led to conflict and contradictory findings. This conflict, the struggle to homogenise findings and work between forms of analysis, is perhaps best expressed in the following metaphor, where a Professor discusses attempts to integrate neuroscience and experimental psychology:

ST: I mean I think there's a kind of cultural, erm, narrative that says somehow or other if we can nail it to bits of the brain that we understand it better, er, but (exhales). Er, in fact that nails are not very tight, or deeply driven in, er, they're, they're more speculative nails. (ST07: 696-701)

The inability to drive nails between different levels of analysis may thus give the impression of heterogeneity and this, presumably, is a failing of psychology's 'cultural narrative', the assumption that biological levels necessarily ensure that 'we understand it better'.

#### *Aleatory heterogeneity*

Alongside these constructions of epistemic heterogeneity runs a quite different narrative which considers issues of heterogeneity at the ontological level, whereby chance and uncertainty are inherent to the autistic condition itself. It is repeatedly stated by interviewees that autism is "a heterogeneous disorder" (SP16: 214, 773), "autism is heterogeneous" (CG09: 586), "...autism is so heterogeneous." (PC20: 125), "...I mean autism is a very heterogeneous condition, okay? I mean I think that's pretty well accepted now, it didn't used to be..." (DR13: 295-297). Within these four brief extracts, again coming from researchers spanning various institutions and seniority, we can see

heterogeneity being something which has come to scientific attention recently and then been positioned within autism, as an essential aspect of the condition itself; a condition determined by its indeterminacy.

One interviewee states: "...I think one of the things we do know about autism is how heterogeneous it is, I mean it's just ridiculous" (EC11: 117-119). According to EC (a Lecturer), therefore, it is not the case that the apparent failure of the cognitive sciences to locate the unitary essence of autism is a failing on its part, as suggested by notions of epistemic heterogeneity. Rather, because "...it's quite hard to tar everybody with the same brush in something that's so, erm, heterogeneous as autism" (YC19: 424-245), it is in the nature of autism to elude the descriptions of psychologists. CG, a postdoctoral researcher, makes such a statement in the following extract:

GH: Mm hm, yeah. So, I mean, it's quite striking I think that, still, er, despite that we have these, clearly very sophisticated, models and some very sophisticated methods as well, that there's still a lot of disagreement about, erm, what causes autism and what autism is. Erm, what, why do you think that there remains such a degree of disagreement, erm, when we've stud', studying this condition for quite a long time now?

CG: (.) Yeah. (.) Yeah. Well (.) I think one thing about it is the heterogeneity... (CG09: 494-503)

Again and again when asked why psychological research has so frequently contradicted itself in terms of research findings, it was claimed, as Lecturer EC does here, that: "... I think the heterogeneity's probably the biggest barrier and [the biggest explanation for why it's messy]" (EC11: 934-936). Here, heterogeneity is not a construct of science but 'the biggest barrier' to science, clearly locating heterogeneity in the essence of autism itself. This is made abundantly clear in the following extract from MN, a Research Fellow:

MN: ...So::, (.) yeah I guess because of all of that heterogeneity (.) that just makes it a much harder disorder to work with. Erm, and harder to get to the core component, whatever level they happen to be at, erm::, because of the heterogeneity, it's really just getting in the way of us advancing so, although we've got a hu::ge amount of autism research in the literature now and a huge amount of money's been pumped into it in the last ten, twenty years, I think we still don't have enough to really (.) we haven't gone far enough with it. (MN05: 787-796)

MN claims here that heterogeneity is ‘getting in the way of us advancing’ and making it ‘harder to get to the core component’ of autism. It is, once again, in the nature of autism to resist science.

*Hackers and Passers: Aleatoric heterogeneity in the 1990s*

These narratives of aleatoric heterogeneity in autism have a history which extends to the early 1990s, just after the period examined in the previous chapter. In particular, a paper by Dermot Bowler published in 1992 is a key reference point in contemporary debates over heterogeneity. Bowler sought to examine theory of mind abilities in an adolescent and adult population diagnosed with Asperger’s Syndrome. The unique aspect of Bowler’s study was the fact that the fifteen individuals diagnosed with Asperger’s who comprised his experimental group were both significantly older and had significantly higher I.Q.s than the SOBA passing group in Baron-Cohen’s 1989 study (Bowler 1992: 883, see chapter four (p.89) on SOBA tests).

Surprisingly Bowler found that a high percentage of individuals with Asperger’s Syndrome were able to pass SOBA tests, and that these pass rates were indistinguishable from those of control subjects. This finding was more interesting still when taken alongside two further pieces of information collected by Bowler. Firstly, when participants were asked to provide justification for their answers, it was found that even those who passed consistently provided explanations without reference to second-order beliefs. In other words, justifications of the sort “because Mary thought that John thought” were virtually never uttered. This finding suggested that alternative routes, not requiring the understanding of agents, could be taken to arrive at the correct solution to SOBA tests. Secondly, Bowler found that the parents of the Asperger’s group, when presented with a retrospective questionnaire, recalled little or no pretense or imaginary play during their child’s infancy, hypothesised key markers of a metarepresentational deficit.

Bowler is stinging in his criticism of the metarepresentation theory in the wake of these findings (Bowler 1992: 888-890). Firstly, claims Bowler, the seeming ability to pass SOBA tests without the expected mind-based justifications suggests that the “ability to solve problems that involve a second-

order theory of mind does not strongly depend on having developed either joint referencing or symbolic play skills” (Bowler 1992: 886). Instead, it may be the case that effortful, logical, cognitive processes can also lead to the correct answers on these tests. If nothing else, this conclusion appears to undermine the primary piece of empirical evidence for a metarepresentation deficit in autism; the deficit in decoupling thoughts about the world from the world itself could just as easily be conceptualised as a deficit in problem solving abilities. Secondly, notes Bowler, in the metarepresentation account pretend play and ToM abilities should be indissociable<sup>14</sup>, as the same cognitive capacity is believed to govern both behaviours (Leslie 1987). That this Asperger’s group did not, according to their parents, engage in pretend play but could pass ToM tests threatened to reduce Leslie’s metarepresentational account to a worthless tautology, the only evidence of a metarepresentation module coming from ToM tasks, and ToM task ability explained with reference to metarepresentation (Bowler 1992: 890).

Partially in response to this research, Francesca Happé, today an esteemed Professor, commenced a thesis in 1991 concerned, primarily, with two issues. Firstly, the repercussions for metarepresentation theory of the finding that some individuals with Asperger’s/ autism were able to pass SOBA tests (Bowler 1992; Ozonoff et al. 1991). Secondly, the proposed relationship between WCC and metarepresentation deficit, whereby metarepresentational deficit is proposed as a consequence of WCC (Frith 1989: 163). The empirical work from this thesis would be published in the years immediately following Happé’s thesis submission and both would suggest cognitive heterogeneity.

Happé claimed that there were two possible reasons as to why individuals with autism would be able to pass SOBA tests. The first, and most obviously damning possibility, was that at least some individuals with autism were ‘really’ able to understand the mental states of other individuals. The second possibility, however, was of methodological rather theoretical importance. In his paper, Bowler states the view, widely recognised within developmental psychology, that failure on a given test can never definitively

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<sup>14</sup> This aspect of the metarepresentation account is discussed at in the previous chapter (p.87-88)

be claimed to demonstrate lack of *ability* it may, rather, demonstrate lack of *performance* (Bowler 1992: 890). For instance, it is widely believed today that both young children and non-human animals may fail tests such as the Sally-Anne not because they have no ToM (lack of ability) but because they are unable accurately to report their beliefs verbally (lack of performance) (Krachun et al. 2009: 521). When considering Bowler's finding that some individuals succeed on SOBA tests, Happé applies the reverse logic; passing may demonstrate a specific performance, rather than a specific ability.

In a phrase which is traced back to a paper by Frith et al. in 1991, but which is first used in press<sup>15</sup> in 1994 (e.g. Frith et al. 1994: 110; Happé 1994: 130), an ability to circumvent ToM tasks with the use of effortful logic based processing is called 'hacking out':

“[Autistic individuals'] success could be seen not as proof of theory of mind ability, but rather as evidence of the “hacking out” of some strategy for solving the tasks.” (Frith et al. 1994: 130)

The question to be asked was, therefore:

“Are they [autistic individuals] simply better problem-solvers, more able to devise a strategy to answer theory of mind questions – thanks perhaps to more experience, higher IQ or a more social disposition?” (Happé 1993: 115)

It was proposed that ““hacking” would enable individuals to solve false belief attribution tasks, but probably would not generalize to the large variety of mentalizing situations in real life” (Frith et al. 1994: 118). However, when eight individuals with autism who could pass FOBA were quizzed over the 'interactive sociability' skills it was found that three individuals did indeed score particularly highly (Frith et al. 1994: 118). This finding, along with others like it (e.g. Happé 1993), is a crucial moment in the emergence of heterogeneity. The authors conclude that:

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<sup>15</sup> Happé also uses the term in her doctoral thesis (e.g. Happé 1991: 78)

“Our results, then suggest the existence of subgroups within the autistic spectrum. The majority have no understanding of other minds, and demonstrate “mind-blindness” in the laboratory as well as in everyday life. Then there are those who have learned limited strategies sufficient to pass highly structured artificial tests of theory of mind, but still show no evidence of mentalizing in real life. In addition, our results suggest that there is a third subgroup who appear to be able, to some extent, to represent mental states. They show evidence of this not only in the laboratory, but also in real life.” (Frith et al. 1994: 118)

What is particularly of note here is that metarepresentation theory is not rejected outright, it is still seen as relevant for some (but certainly not all) autistic individuals. This is the essence of cognitive, aleatoric heterogeneity.

Alongside these studies examining ToM ability in individuals with autism, Happé was also concerned with examining the relationship between ToM and Weak Central Coherence (WCC). Simply stated, if WCC is able to account for ToM deficits (more WCC equals greater difficulty with ToM tasks), as suggested by Frith (1989: 163) then individuals who perform particularly well on WCC tasks should perform particularly badly on ToM tasks. Happé found no relationship between WCC and ToM skills and thus proposes that:

“The independence of ... theory of mind tasks success suggests that the postulated weak central coherence must be thought of as separate from the mentalising impairment in autism. This is a change from Frith’s (1989) original position, and raises the interesting possibility that weak coherence may be able to explain persisting handicaps in even those subjects who appear to be able to represent mental states.” (Happé 1994b: 1469)

This is the crucial conclusion for the emergence of a heterogeneous autism and it is worth considering carefully, and in the context of the previous discussion of Happé’s work. Firstly, metarepresentational deficit is not found in all autistic subjects *ergo* it can no longer be considered the universal, core deficit at the heart of autism. Secondly, WCC is found in all autistic subjects and

therefore could be at the centre of a cognitively homogenous account. However, WCC ability does not correlate with ToM ability, and so WCC can only offer a homogenous account of autism if the theory of metarepresentational deficit is dropped entirely. In a paper entitled ‘autism: beyond “theory of mind”’, however, Frith and Happé state:

“At present, all the evidence suggests that we should retain the idea of a modular and specific mentalizing deficit in our causal explanation of the triad of impairment in autism. It is still our belief that nothing captures the essence of autism so precisely as the idea of mindblindness.”

However:

“...this explanation alone will not suffice. Therefore, our present conception is that there may be two rather difference cognitive characteristics that underlie autism.” (Frith & Happé 1994: 126)

This conclusion, too, absolutely captures the notion of an aleatoric heterogeneity in autism.

#### *Intra- and inter-individual heterogeneity*

Within these findings of the 1990s, where Happé first puts forward the notion of aleatoric heterogeneity in autism, it is possible to see a further subdivision, with two separate types of heterogeneity coming under discussion. When, following Bowler, Happé suggests that some individuals with autism may have impaired ToM while others do not, a model of *inter-individual* heterogeneity is being advanced. By comparison, when Happé suggests that WCC may be core to autism but may not be able to explain all autistic symptomatology, a model of *intra-individual* heterogeneity is advanced, whereby more than one theory is required to explain that appearance of autism in any given individual. These two constructions of autism, that it is aleatorous both within and between individuals are found throughout the interview transcripts.

The concept of an inter-individual heterogeneity refers to the idea that individuals with autism look very different to one another, genetically, neurologically, cognitively, and behaviourally. This was a claim made repeatedly throughout the interviews. For example PC, a Professor, states that:

PC: ...But it just seems to me that autism is so heterogeneous. You know that, that, that you've got, you know, in order to get, you know, you can give a diagnosis of autism using ADOS to someone who's a Professor at Oxford and somebody who sits in the corner and hand flaps. (PC20: 125-129)

In this extract we can see the now familiar claim that autism is heterogeneous. However, we can also see that this heterogeneity is constructed interpersonally, between a 'Professor at Oxford' and 'someone who sits in the corner and hand flaps'. A similar idea, that 'everybody with autism is completely different to everybody else with autism' is expressed by HB, a Reader, below:

HB: ...when I've students and, and they come and work with me and say what have you learned about autism they say well (.) all I've learned is that everybody with autism is completely different to everybody else with autism ((laughs)) so how can you call it all the same thing and, and I think that's a really interesting point... (HB01: 195-200)

These extracts both unambiguously concern themselves with the differences *between* individuals with autism. It may be that those interpersonal differences are theorised in very different ways. Differences may arise because individuals have qualitatively different symptoms to one another, for example, the presence or absence of repetitive behaviours or sensory difficulties. Such a situation is described by DR, a Professor who acknowledges that 'sensory sensitivities' do 'aggregate' with the 'problems in social reciprocity and communication' typically found in autism, but also insists that there are a number of children who are part of the 'domain of autistic behaviour' who do not have these problems:

DR: ...I mean, you get that as well, you, you get these sensory sensitivities which are now recognised in DSM-5, auditory hypersensitivity, what the hell's that got to do with anything? (.) Maybe that's a damping issue, maybe that's something to do with inhibitory feedback that's not working properly (.), pfft, don't know. But I mean you

do get this, these things (.) they tend to aggregate (.) but you, you, we, we would argue, we, we also see a lot of kids with very typical problems in social reciprocity and communication, erm, who don't have the restrictive, repetitive behaviours and specific interests and s', sensory sensitivities and so on, that use, that are, are, p', p', part of the, you know, of that domain of autistic behaviour. Erm, (.) so, anyway, whatever's going on I would suggest it's got multiple aetiologies... (DR13: 409-423)

Autism may, then, look qualitatively different in different individuals. Alternatively, interpersonal difference may be theorised as an issue of severity, individuals being on different 'parts of the spectrum'. Regardless of the particular hypothesis, the defining issue here is of differences between individuals.

By comparison, intra-individual heterogeneity refers to the claim that there are different causes of autism *within* an individual person. For example, it might be claimed that the causative factors behind communication deficits are fundamentally different to the causative factors behind repetitive behaviours; this is the story being told when Happé claims that neither WCC nor ToM can explain all autistic symptomology. Such a discourse of intra-personal heterogeneity was also prominent within the interviews and is nicely expressed in the following extract. BG, an Assistant Professor, has advocated a theory of autism known as the social reward hypothesis (see Chevallier et al. 2012 for details) which argues that, rather than being unable to understand other agents (*qua* theory of mind), individuals with autism are simply not motivated to do so. BG is asked how such a hypothesis might explain 'non-triadic' aspects of autism:

GH: Erm, how does this, er, how do you see repetitive behaviours fitting into, erm, for example, theories about social reward, I mean how, how do these come together?

BG: (.) Well (.) the, there is no direct, erm::, there is no direct way where the social, er, reward hypothesis will have a prediction for higher or lower social, er, lower repetitive behaviour... it appears that these two aspects of autism may have different aetiologies so they happen to co-exist in various cases but, erm, there wouldn't be a single phenomenon that could explain both at the same time, erm, (.) from the looks of it, from, from the looks of the factor analyses that have been done... I mean I think it's, it's almost a wrong question to ask, whether, say, for example, the enhanced perceptual function model which, er, erm, l, which erm, erm,[Laurent] Mottron, er,

and Kate Plaisted-Grant, er, talk about (.) whether, to ask those perceptual based models that whether they can explain the social phenomenon or to ask a social motivation type, a social based model to ask whether they can explain the perceptual, I mean, why do we need a single explanation? We, we don't. (BG06: 334-340, 341-347, 358-366)

We can see here that, despite also advocating a form of aleatoric heterogeneity, the form of heterogeneity is quite different to those described in the previous extracts. Here, individual differences are not mentioned. Instead, 'two aspects of autism' – 'social' and 'repetitive' behaviours – have 'different aetiologies' but 'co-exist' in certain individuals. Indeed, the study of autism has advanced to the point where the question has to be asked 'why do we need a single explanation' – the answer is that 'we don't'. Exactly the same point is made by a Research Fellow, MC, in the extract below. MC suggests that there are 'two distinct things' within an individual with autism:

MC: ...I find it hard to explain all of the social components with non-social components so I wonder whether there's two distinct things, erm, that have to coincide for you to have autism but they are distinct. (MC12: 223-226)

These findings of intra-personal heterogeneity have led many researchers to reach the conclusion that there is no 'single psychological deficit' and that 'more of a mix' of theories is required to explain autism. AO, a research fellow, states that:

AO: ...people are more accepting of the idea that you don't have a single psychological deficit. So:: the days of Weak Central Coherence versus Executive Functioning versus Theory of Mind seem to be gone, and people are understanding more that there's much more of a mix. (AO04: 508-513)

Heterogeneity has, then, been constructed as both intra-individual and inter-individual in nature.

#### *The nature and nurture of heterogeneity*

It has been argued within this chapter that one of the most striking changes in autism research since the 1980s is that autism is now constructed as a 'heterogeneous' condition. This is true to the extent that heterogeneity has become a rallying-call for those working in fields relating to autism; 'if you

know one person with autism, you know one person with autism' is the autism advocacy sector's unofficial slogan.

This chapter has gone beyond existing analyses of autistic heterogeneity by critically examining how cognitive heterogeneity is constructed by autism researchers within the context of research interviews, and how these constructions relate to the history of autism research. It has been argued that 'autistic heterogeneity' as a concept does not easily cohere, that at different times heterogeneity refers to differences between people and at others times to differences within individuals. At yet other times heterogeneity is located not in the condition of autism itself but at the level of epistemology, as an artefact of classification or methodology. This conclusion regarding the incoherence of autistic heterogeneity is reminiscent of Foucault when he states that:

“...if the genealogist refuses to extend his faith to metaphysics, if he listens to history, he finds there is “something altogether different” behind things: not a timeless and essential secret, but the secret that they have no essence or that their essence was fabricated in a piecemeal fashion from alien forms.” (Foucault 1977: 142)

The findings within this chapter concur with such conclusions, at least in the case of autistic heterogeneity: the disorder within the disorder of autism is itself disordered.

The striking references to heterogeneity within the fields of autism research have not escaped the attention of social science scholars (e.g. Fitzgerald 2012: 58-84; Verhoeff 2012). Indeed, Verhoeff (2012) makes the strong claim that autism researchers concerned with heterogeneity cling to a belief that autism is a 'natural kind' whilst simultaneously, and apparently contradictorily, stating that there is no single autism. Verhoeff is here arguing that autism is *epistemologically heterogeneous*. As has been shown above, autism researchers too make this claim on occasion and construct heterogeneity as an artefact of, for example, diagnostic or methodological procedures. However autism researchers also contest that autism has an aleatoric heterogeneity, both intra- and inter- personally, and this is the

construction which Verhoeff, understandably, disagrees with. Simply to dismiss autistic heterogeneity as incorrect, however, surely misses much of heterogeneity's productive value. If autism had not been constructed as quite so heterogeneous, if it had not been possible to domesticate the social behaviours of the 'Oxford Professor' and the adolescent who 'sits in the corner and hand flaps' under the banner of autism, then it seems unlikely that the disease construct could have radically expanded in quite the manner it has over the past thirty years. Indeed, it seems likely that the autism epidemic and heterogeneity are in some senses mutually constituting phenomena; as the autism epidemic 'revealed' very different people to be autistic it became apparent that autism itself was very heterogeneous, thus enabling more people to be labelled autistic, and so forth.

Nonetheless, the question of autistic heterogeneity does raise interesting questions for autism research, namely; how is autism research able to continue, and indeed increase, given that the scientific *connaissance* of autism seems to be so poorly ordered? How *has* autism managed to domesticate the Oxford Professor and the profoundly disabled adolescent given that no laboratory test has thus far been able to unite them? A heterogeneous condition may be productive, it may lend itself to increased rates of diagnosis, but as Verhoeff demonstrates it leaves the scientific research open to ridicule. By further examining interview transcripts, the following section will argue that autism's order is found when researchers report an unmistakable experience of social disorder in autism. For the researchers, it is this experience of autism that in some sense proves that autism is 'a real thing' which deserves to be named as such despite its surprisingly heterogeneous nature.

#### *A singular experience of autism*

Throughout the interviews, discussions surrounding a fascination with the otherness of autism were present, arising during two specific points in particular. Firstly, narratives of otherness occurred during a number of researchers' origin stories. The following extract is typical in this respect. CT,

a Senior Lecturer, described their first contact with autism as a teenager volunteering in a psychiatric hospital:

CT: ...that experience of working with these children with autism stuck in my mind, erm::, I just found it very, very compelling and fascinating, of course there wasn't nearly as much known then about autism as there is now, but I just, there's something about the:: kind of mysterious nature of the way they are and I remember the, the kind of, this is from way back when I was an undergraduate, but I remember this kind of experience of having this child take me by the hand and use my hand to get things that he wanted and (.) I didn't then go on and do a PhD on autism and I'm not really quite sure why. (CT02: 23-27)

The 'mysterious nature of the way they are' was 'compelling' and 'fascinating' and the experience with a child, perhaps using CT as an object, is certainly striking. A similarly profound and deeply moving experience is reported by MW, a Professor:

MW: ...I went and during the summer holidays, er, went and collected data for them [two researchers] from people with autism, children mainly, some adults, er, who had extraordinary memory skills. And then, erm, other children and adults with autism who were:: matched for ability but didn't have memory skills. And so that was my first experience of really what autism was as opposed to reading about it. And it really, really blew my mind actually ((laughs)) how, how different the reality was and to go into some of the special schools then and see, you know, a playground full of children all:: moving and all making sounds, often very unusual sounds, and not usually playing together and not (.) you know responding to you as somebody coming in in the way that you would expect, you know, an ordinary child, or a child with intellectual disabilities too. And it's just completely fascinating. And, erm, and after that I thought that autism was utterly fascinating but so upsetting that I was going to do a PhD... (MW18: 28-45)

MW articulates the 'reality' of autism, being able to 'see' it rather than simply 'read' about it as a crucial formative experience, it 'really blew my mind actually', it was 'utterly fascinating but so upsetting'. It can be seen even within this extract how the 'real' autism is something that is seen and experienced, rather than merely read about in scientific tomes. The description of 'a playground full of children all moving and all making unusual sounds', as being so obviously socially disordered, is described as being a prime motivating factor in embarking upon a career in autism research.

The second occasion on which these narratives of otherness and being were called upon during the research interviews is perhaps more striking still. One of the first questions asked, in one form or another, was what exactly the researcher thinks autism *is*. Among the textbook answers (e.g. that autism was a ‘neurodevelopmental condition’) a particular subset of interviewees once again described autism as a ‘way of being’ or as a particular form of existence. There was no indication that the interviewees who comprised this group were linked in any sense in terms of seniority or institutional affiliation. These discourses on ‘being’ are striking coming from senior scientists from whom one may expect earthy, material explanations of their subject matter. The following two extracts, the first from a Lecturer and the second from a Reader, are typical of the ‘way of being’ articulations found within the interviews, the sense that autism is an ‘understanding about the world’:

GH: So if, if I was going to ask what autism is, how do you think it’s best characterised?

FC: Erm:: (.) just a, being slightly different really. ((Laughs)) Being a::, having, er, understanding about the world... But, er, yeah, it’s just about (.) being, interpreting the world differently. (FC15: 308-312, 320-322)

And:

DF: ...I think it’s a way of being, erm, erm::, a way of being that is ob’, obviously different from (.) the norm, or what we like to think is kind of normative, erm::, but that has a pattern of, erm, there’s, it has, it has a skill set that is characteristically different from, from non-autistic people... (DF03: 211-216)

ST, a Professor, used the same phrase, ‘way of being’, on numerous occasions throughout the interview. Before the following extract ST had been musing on an individual diagnosed with autism who they know particularly well. This individual in question lives alone but does not work, preferring instead to play computer games. The ethical dilemma, for ST, is whether this individual should be forced into employment or whether they should be supported by society: “...I’d be inclined to say well we have to support them because he has a different way of being” (ST07: 452-454). The same phrase is used again when considering if scientists should dispute the neurodiversity movement’s claim that autism is a difference rather than a deficit:

ST: ...But I'm quite happy to say that, er, you know, I, I, I'm quite happy with the view that we just (.) recognise it as, erm, a way of being and help that individual to achieve a level of comfort that they're happy with, er, and leave it at that. (ST07: 791-794)

The extracts presented above continue a long trend of metaphor use around autism which is vast and replete with examples that suggest an altered state or another way of being. Sometimes these constructed figures arise from within science, for example the 'enigma' of Frith's (1989; 2003) Rapunzelian character occupying the centre of Bettleheim's 'empty fortress' (1972). On other occasions it is from within the autistic community itself that metaphors arise (Silverman 2008b: 333) and it is from here that metaphors of aliens (see Hacking 2009b), extraterrestrial planets, and Harawayian cyborgs (see Nadesan 2005: 128-132) seem to emerge. For better or worse, the thread running through all of these descriptions is autism as an altered state, as unknowable, and as other.

What is clear, however, is that despite constructing autism as being 'unknowable', interviewees consistently claimed that autism could be seen, or experienced, in quite a vivid manner, as suggested by MW's experience in the playground and CT's experience in the hospital. As one particular interview was concluding, an interviewee was asked "...is there anything else which you'd like to add or that you think we've not discussed, any bits of your research which you think are interesting?" (YC19: 824-826) The response was the following:

YC: "Erm, one thing I did do is I got a second rater to look at my videos and code them in terms of, erm, quality and quantity of facial expression use and things like that and he was a si', a very proficient sign language user [the children in the study were deaf – GH]. And I didn't tell him which groups were which, I just, you know, er, kept everything [kind of (.)]

GH: [Mm hm, mm hm.]

YC: You know, anonymous. Erm, well, as anonymous as you can when you're looking at someone, but he didn't know the group information at all.

GH: Yeah.

YC: And I asked him who, like, who do you, can you, just out of interest can you tell me who you think is in the kind of ASD group. And he i', he was able to, even though they're not coming up as massively different in a lot of their communication he was able to say they were the autistic children and they were the ones who didn't have autism so it's, there is this something that seems to be there that doesn't necessarily come up that makes you kind of, have that kind of gut instinct. And I know that's only one person looking at videos but there was something I felt I couldn't put my finger on with those children that you knew just, kind of looking at their communication, something that comes across. And I've heard this with quite a lot of people talking about individuals with autism, that you just get this kind of, you, you know but you don't know, erm, (.) you can't really put your finger on what it, what it specifically is..." (YC19: 831-858)

Several themes are prominent in this extract. The "historically singular form of experience" (Foucault 1984a: 333) of autism, discussed in chapter 2 (pp.24-25), is evidently present. Two central elements of that experience, that 'way of being', are captured here. Firstly, despite the fact that, on the particular emotion recognition task under discussion, the children diagnosed with autism were performing more-or-less identically to those without a diagnosis, 'there is this something that seems to be there that doesn't necessarily come up', a 'gut instinct', a sense that 'you know but you don't know'.

This sentiment, that the essence of autism somehow eludes scientific description, is echoed in a second interview with PC, a Professor. Once again, empirical evidence in any formal sense is not required to experience autism:

GH: And: (.), er, how do you fee', how do you feel about the ADOS [Autism Diagnosis Observation Schedule] as a diagnostic technique?

PC: Er: (.) it's probably the best thing we've got. I mean I, you know, I think it's (.) I, I, ha', I like the:., the child versions better than the adult version. I think that it's q', I think the adults that are very able, that have done a lot of developing.

GH: Mm hm.

PC: Especially the ones that come in here because they travel around on their own, a lot of them live independently, and I think that some of them don't meet criteria using ADOS and they're clearly autistic. (PC20: 544-555)

PC describes some of her participants as 'clearly autistic' even though the ADOS, the formal, gold-standard, diagnostic measure does not mark them as

such. PC makes the same point again later in the interview when a complaint from PC about a lack of scientific publications concerning aging in autism was followed up:

PC: ...I mean if you look at the number of papers that are published on adults there are really not that many.

GH: And why do you think that is?

PC: Well from my experience it's because, erm, ((laughs)) well certainly on the auditory, erm, work we've done is that they don't really perform very, very differently to adults without autism. (PC20: 597-603)

It does not seem occur to PC that an individual who does not 'meet criteria' on the ADOS, who does not 'perform very differently' to 'adults without autism', might in fact not have autism. This possibility does not occur because, for these researchers at least, autism is a 'way of being' that appears as obvious and given, they know even though they don't know.

Returning to the extract above from the interview with YC, a second notable feature of the experience of autism is that it is presented as being not only available to the autism researcher, but also to the second-rater who is a non-expert. This idea that experts and non-experts alike experience autism as immediate, obvious, and given was repeated across interviews and used as evidence for the reality of autism. MW, a Professor, states that:

MW: And there's no denying that, erm, within this great range of the autism spectrum there's a, a big chunk where autism is enormously recognisable. I mean, you know, the, what people will say fairly flippantly is that for clinicians their, the person in the reception, er, who's manning reception, can tell you already whether they're going to get a diagnosis or not. Or, you know, from seeing them walking down the street towards the reception door they can tell. ((Laughs))

GH: ((Laughs))

MW: So there's a sort of sense that autism, the core autism is really very, very recognisable. (MW18: 396-407)

In this extract, MW claims that 'a receptionist' would be able to identify correctly individuals with autism before they have spoken, before they have even entered the room. This experience that autism is 'enormously

recognisable' understandably leads a great number of researchers to the conclusion that "there must, must be something in it." (CG09: 102). That sense is well captured again in the following extract with MD, a further Professor:

MD: ...Clinically I, I sort of think there is something quite striking because it seems to be the thing that lots of us who've been, you know, been involved in clinical work with, you know, children with autism for more than twenty years and research for the best part of, you know, twenty-five years so::, erm, clinically there is a sort of notion that when you see that constellation of developmental and behavioural characteristics together (.) it, you know, it, it, it seems to one like, you know, like a thing.

GH: Mmm.

MD: Er::, you know, so it belongs in some n::osological sort of system... So some sort of notion that, erm, er, erm, erm, the medical model, you know, erm, (.) y::, you know, is, erm, sort of demonising sort of, you know, individuals in a way that is going to be disadva', 'vantagous, sort of, to them, to some sort of notion that disorders like autism are, are (.), erm, primarily a sort of social construct are both rather silly I think, you know, I don't think people, I think probably most sensible people wouldn't think, hold either of those extreme sort of views. (MD17: 454-466; 495-503)

Twenty years of clinical 'experience' leads to the conclusions that autism is 'a thing', that to claim that autism is a 'social construct' is 'rather silly' and something that 'sensible people wouldn't think'. Once one has *experienced* autism in the clinic, denying its reality becomes untenable.

### Conclusion

The latter half of this chapter has argued that autism researchers do not cling to the notion that autism is a 'natural kind' in the absence of any evidence. Instead, the striking evidence that researchers have for the reality of autism is found outside of experimental research, in the experience of meeting an individual in the street, playground, hospital, clinic or, indeed, laboratory who is socially disordered. It is when an individual is encountered within society and, for whatever reason, their behaviour deviates from the normative framework prescribed by that society, that they become unambiguously experienced as autistic. It is these intense experiences of social disorder which reportedly persuade research scientists of the singular reality of autism and

provides the means through which they are convinced of the coherence of autism.

This finding, that experiences that exist outside of the laboratory are crucial in constructions of autism, is consistent with existing hypotheses from within the social sciences regarding knowledge production and autism. Silverman, for example, has argued that love “as a way of knowing about bodies [and] persons” (2012: 3) is central to contemporary articulations of autism. Similarly, Fitzgerald argues that scientists’ “unambiguously affective and emotional labour” (2012: 116) is central to accounts within biomedicine. Fitzgerald’s point in particular, that emotional labour is important *within* biomedicine, is crucial to understanding autism. What these accounts have in common is the finding that scientific knowledge about autism includes far more than is readily allowed within the scientific method. In essence, Fitzgerald echoes one of the foundational claims of STS, that it is “possible to speak about that which cannot be spoken” (Collins 1974: 184), and that science does not progress *in spite* of tacit and ambiguous knowledges and experiences but, in the case of autism at least, *because* of them (Fitzgerald 2012: 84). That autism cannot be reduced to a single act, a specific cognitive profile, or the *connaissance* of laboratory science does not seem to undermine these moving, troubling and profound experiences of confronting social disorder.

It might also be hypothesised that a combination of a heterogeneous *connaissance* and a singular experience also holds significant productive value for autism science. Inter-personal heterogeneity ensures that any two individuals with autism do not need to appear similar - cognitively, behaviourally, and so on - to each other in order to procure a diagnosis of autism. Similarly, intra-personal heterogeneity ensures that a single causative factor is not required to explain the presence of autism within a given individual. Once autism has been constructed in this heterogeneous manner there is significant scope for diagnostic increase, as no limits have been imposed upon what may count as an autistic symptom. The problematic aspect of heterogeneity, as highlighted by Verhoeff (2012), is the accusation of conceptual incoherence. However, as the scientists interviewed for this project

have demonstrated, the accusation of incoherence can be refuted with recourse to an experience of autism in which autism ‘seems like a thing’. This, it could be argued, is a very powerful combination.

What must be remembered, however, is that whatever status is given to the nosological category of autism we can be certain its *experience* is historically novel; it is a fact that no one experienced autism like this 100 years ago. The Foucauldian perspective outlined within chapter 2 (pp.24-25) questions the immediacy of experience, instead arguing that experience should be understood as the outcome of particular processes, of particular “grids of visualization, vocabularies, norms and systems of judgement” (Rose 1996a: 130) which come together to produce a particular type of subject based upon “...forms of possible knowledge, normative frameworks of behavior, and potential modes of existence for possible subjects” (Foucault 2010: 254). Chapter four began to describe the forms of possible knowledge, a *savoir* of the social from within cognitive psychology, which was required in order for the experience of autism as a social disorder to take place. However, the experience of autism within a society, how the experience of autism relates to ‘normative frameworks of behaviour’ has thus far been left broadly unexamined. The normativity inherent in notions of social disorder will be one of the themes of the following chapter.

## **Autism, the social, and society**

### *Autism and society*

Within the interviews conducted for this thesis there was a great deal of technical discussion of ‘the social’ as an object of psychological investigation, for instance as something to be demarcated from the ‘nonsocial’, as something with neural underpinnings, as something that is exposed as being deficient in individuals with autism through cognitive experiments such as the Sally-Anne Test, and so forth. Simultaneously, however, the social seems to be used to mean something quite different. For example, one Lecturer, when discussing how they came to be interested in studying autism, stated that their interest stemmed from an undergraduate research project; “For my third year undergraduate project I was writing about Theory of Mind in terms of social theories versus biological theories (.) of, of how this deficit might occur” (AO04: 19-22). There is something curious in the concept of the social as it is being used by AO here; the possibility of a social theory of social understanding. This social, positioned as something that is ‘versus’ biology, does not seem to be being used in a particularly technical sense but, instead, has a more every day, lay meaning. Donna works in the social sciences and not the biological sciences. Sheila is not good in social situations. Similarly, an eminent Professor, again discussing how it is that they came to be interested in their research topic, juxtaposes autism with ‘social disorders’:

DR: I was interested in the neurodevelopmental disorders in general, erm, so disorders where there might be abnormalities in brain development as opposed sort of social disorders, ((laughs)) if you like. (DR13: 28-31)

Lurking behind DR’s claim is, as with AO, the sense of an opposition between nature and nurture, ‘neurodevelopmental’ and ‘social’ conditions. This contrast between ‘biological’, ‘cognitive’, and ‘social’ factors is again considered by YC, a postdoctoral researcher:

YC: So that’s kind of using a little bit of the approach of kind of biological versus cognitive, erm, and social, because the families or carers are usually there as well so you’re getting the kind of really broad picture of what’s going on with the person. (YC19: 642-646)

Autism is perhaps *the* archetypal social disorder examined and addressed within the psy-disciplines. And yet at particular times for these researchers, who have been researching autism for periods of time ranging for a few years (YC) to several decades (DR), the social means something quite different and, certainly for DR, autism is of interest precisely because it is *not* social but rather biological.

These lay conceptualisations of ‘the social’ are also drawn upon when considering intervention within autism. In the extract below SP, another Professor, is discussing the changing field of autism studies. SP has previously discussed advancements within the sciences, considering proteomics and stem cell research among other topics. SP then goes onto to discuss ‘exciting things’ that are ‘going on’ in the ‘social domain’:

SP: Other important and really exciting things I think are going on, in the social domain are:: how some employers are coming and actively saying we want to help out, we want to help out with the employment and retention of individuals with autism because we think they’ve got special skills, which again is something that you would never have thought of five years ago... (SP16: 716-722)

In this extract, when SP discusses ‘the social domain’ it is quite clear that they are considering factors different to those discussed within the technical literature. The social under discussion here seems to have little, if anything, in common with the social of the ‘social brain’, or the social being fought over by Peter Hobson and Alan Leslie in debates over metarepresentations. Instead, this social is about employment and working conditions. This looks more like the social of political science and economics than it does psychology and neuroscience; society rather than the social.

It is not, of course, surprising that scientists are also people and speak as such. Following on, both from these extracts and the discussion of an experience of social disorder presented in the previous chapter (p.121 onwards), a question of interest is quite how effectively these normative versions of the social are parsed out from the apparently objective notions of the social which underpin scientific endeavours into autism. Bennett and Hacker have discussed the implications for the neurosciences of adopting

terminology, traditionally applied to individuals and societies, and projecting these terms onto brain regions or cognitive faculties (Bennett & Hacker 2003). Following Wittgenstein, Bennett and Hacker refer to this tendency as the ‘mereological fallacy’ - the attribution of qualities to the part that should only be attributed to the whole – and deem such mereological usage meaningless. Another approach, however, would be to follow Giddens and consider the potential of a double hermeneutic (Giddens 1987) whereby lay and technical uses of a term loop and mutually inform one another. If such hermeneutic practices were present within autism research it may suggest significant consequences. Firstly, looping would suggest that the ‘objective’ social of autism research is itself normative and inflected with the values of the contemporary Western societies in which the concept of autism emerged. Secondly, it would open the possibility that contemporary lay notions of the social are themselves taking on the appearance of autism. This chapter will consider those possibilities.

#### *The social as an ethical substance*

As discussed at length in chapter 4, within the psychological literature there has been an increasing emphasis upon ‘nonsocial’ aspects of autism, such as processing styles and sensory difficulties. Several interviewees explicitly stated that they were not interested in, or did not focus upon, the social difficulties experienced by individuals with autism. ST, a Professor, was one such researcher. Much of ST’s interview was concerned with the assertion that social impairments in autism were not caused by any deficit in social processing per se, but rather an inability to understand complex relational processing in general. Given this position, ST was asked if it was incorrect to think of autism as a ‘social disorder’:

ST: Erm, ((exhales)) well can’t stop thinking about it as a social disorder because pra’, perhaps the bit that poses most problems both for the individuals themselves and for the people that have to deal with them in one way or another, either live with them, love them, or:: educate them or whatever. Erm:: but (.) we say it is all these things including a social disorder, rather than saying all it is is a social disorder and everything is secondary to that. (ST07: 722-729)

There are two features of this extract which are worthy of consideration. Firstly, despite ST's earlier claim that there is no specific deficit in social processing, it is contested within this extract that it remains reasonable to classify autism as a social disorder. Continuing the theme discussed above, this 'social disorder' is quite different from a disorder of the social brain; autism is a social disorder because it is within social contexts, within a society, that the disorder becomes apparent. It is a social disorder because society is 'the bit that poses most problems', not only for the individual but for those who 'have to deal with them' be those individuals their loved ones or state officials. This is one of the first instances, which will be considered in more depth below, of a normative conceptualisation of societal functioning being localised and reified within an individual. Secondly, there is the assertion from ST that we 'can't stop thinking about it as a social disorder' precisely because of these societal difficulties. The social is here being positioned as an "ethical substance" (Foucault 1984a: 26; Foucault 1997a: 264), it is the part of autism that is ethically relevant and there is thus a requirement for scientists to continue to act on it.

Similar issues are present in the extract with MC, a Research Fellow, below:

GH: So do you see those nonsocial aspects as being really central [to the disorder?]

MC: [Yeah I do. Yeah.]

GH: And do you think that's been a, like a misplaced emphasis, in the past, or, erm, do think increasing the emphasis on the nonsocial aspects is?

MC: I think there is an increasing emphasis. (.) But, I'm not sure if it's misplaced because I think the social deficits are very important as well. And they're the ones that are most obvious when you:: meet someone. So, maybe that, it is necessary to have emp', emphasis on the social impairments for diagnosis and also for helping people interact better in the real world because that's probably the biggest difficulty for people with autism, like on the face of it.

GH: And, sort of theoretically do you see, erm, so you mentioned Weak Central Coherence

MC: Yeah.

GH: Do you see:: one underlying (.) cause being responsible for nonsocial and social deficits or do you think they're independent? Or what's the relationship there?

MC: Erm:: (.) I'm going to cheat and go with someone else's interpretation which is that the nonsocial stuff feeds into the social but not the other way around. (MC12: 200-222)

As with ST, MC does not position autism as a social disorder in the expected sense, stating that the 'nonsocial stuff feeds into the social but not the other way around'. Nonetheless, and again like ST, MC resists the claim that social factors should be downplayed because, again, there is an ethical requirement to help individuals with autism overcome their 'biggest barrier' to functioning 'in the real world', that is, to adhere to the demands made of them by society.

This ethical requirement to act on the social difficulties found within autism, because it should be defined by its place within society, is perhaps most starkly stated by MW, a Professor, here considering the ethics of intervention:

MW: ...I would say look giving people more social insight is probably a good thing, you know?

GH: Mm hm.

MW: They can then choose whether they want to be hermits, whether they want to engage with the social world, but actually not being vulnerable to other people being able to put one over on you because you don't have a concept of deception is probably not a nice way to be in the world. Erm, and on the other hand changing people's detail focus, we have absolutely no right to, to meddle with that. (MW18: 675-681)

As in the previous extracts, MW is again using the word 'social' to do a lot of work. The individual with autism simultaneously has a social deficit – they don't have a 'concept of deception' – but must also be in a 'social world', in a society. Further, there remains an ethical requirement in relation to the social; 'giving' more social insight is a 'good thing', science and society *ought* to intervene, it is *right* to do so. Detail focus (an aspect of Weak Central Coherence), however, is quite different, it just *is*; 'we have absolutely no right to meddle with that'.

It should be noted that, as might be expected of ethical problems, there is not necessarily agreement between autism researchers on how they believe we ‘ought’ to proceed with regard to social issues in autism. The following extract from an interview with DF, a Reader, makes this clear:

DF: ...I think the emphasis on, on the social ((exhales)) (.), ahh, the emphasis on problems in social communication in autism, ahh, kind of stresses the (.), the:: erm (.) maybe just illustrates this norm, the normative framework that we live in, you know, we are social and we want people who aren’t social to be social like us, erm, and (.), ahh, I, I think I’m, I think I’m genuinely conc’, generally concerned about that approach, um, ‘cause (.) I don’t necessarily want anyone ((laughs)) to make someone else like me or like or, yeah, us generally... (DF03: 345-354)

This extract shares a great deal with those that precede it. Like the previously presented extracts from ST, MW, and MC, social functioning is positioned as an ethical substance. Like those extracts, too, there is a sense that research into social functioning is based upon an intention to make individuals fit into broader society. DF, however, reaches a different conclusion to other researchers stating that we ought not to intervene in ‘problems of social communication’ precisely because it is an attempt to make individuals cohere with a ‘normative framework’. This is exactly the type of disagreement one might expect over an arena constructed as ethical in nature. The social in these discussions of autism is not, or is not only, a scientific object but an ethical substance related to being a particular type of individual in a particular type of society.

### *Transforming ought into is*

The preceding discussion has been attempting to elucidate how, within various interview extracts, the social is enacted as an ethical substance which scientists *ought* to consider and *ought* to act on differently to the other objects which come under their consideration. The discussion has also sought to show that, within these extracts, the is and the ought of the ‘socially abnormal’ are brought into close proximity with each other; close enough that, on occasion, it is quite hard to tell if the ‘social disorder’ under discussion refers to the functioning of an individual-in-society who has certain demands thrust upon her by educators, parents and the like, or if it refers to the isolated properties of

a biological organism. The following sections will attempt to show spaces where ought is transformed into is, where the normative requirements of society become the objective items of scientific study. Four such spaces will be considered: *i) in diagnostic criteria ii) in diagnostic practice iii) in laboratory practice and iv) in narratives of neuroplasticity.*

*i) Diagnostic criteria*

June 2013 saw the publication of the 5<sup>th</sup> edition of the American Psychiatric Association's Diagnostic and Statistical Manual of Mental Disorders (DSM-5; American Psychiatric Association 2013). Within what is a hugely controversial text, the revised criteria for autism remain among the most controversial. In DSM-IV, text revisions (DSM-IV-TR; American Psychiatric Association 2000) 'Autistic Disorder' was one of a gaggle of 'Pervasive Developmental Disorders' that also included 'Rett's Disorder', 'Childhood Disintegrative Disorder' (CDD), 'Asperger's Disorder', and 'Pervasive Developmental Disorder, Not Otherwise Specified' (PDD-NOS). Within DSM-5 this classification has changed radically. The subgroups Autistic Disorder, Asperger's, and CCD have been removed and replaced by the single, dimensional classification of 'Autistic Spectrum Disorder' (ASD). It is intended that this classification will also include the overwhelming majority of individuals currently diagnosed with PDD-NOS, a classification which has also been removed. Any individuals who would previously have been diagnosed with PDD-NOS and who do not meet the new criteria for ASD will in all likelihood be diagnosed with a further new condition, 'Social Communication Disorder' (SCD), which is not directly linked to ASD but is rather included as one of the communication disorders. 'Rett's Disorder' has been entirely removed from DSM-5, intriguingly because its genetic basis has been determined (Kurita 2011: 609).

The formalisation of an autistic spectrum has also necessitated the advent of 'severity scores' which place an individual at a particular point on that spectrum. Further, the symptoms required for this base diagnosis have also altered. Sensory abnormalities have been included as a possible symptom and included amongst the category of restricted interests and repetitive

behaviours (RRBIs), a category afforded increased importance by transforming the existing triad of impairments (social impairments, communication impairments, RRBIs) into a dyad; achieved by a merger of social and communication impairments into socio-communicative impairments. It is also possible to have a diagnosis ‘modified’ by one of another range of factors, from intellectual disability to normal language onset (christened ASD, Asperger’s type).

As noted above, critics have questioned numerous aspects of these changes in diagnostic classification. Firstly, it has been asked whether the amended criteria sacrifice sensitivity in exchange for their undoubtedly specificity (Skuse 2012), with a particular concern that high functioning individuals may no longer meet criteria (McPartland et al. 2012). Secondly, the justification for SCD has been repeatedly questioned (e.g. Tanguay 2011). Finally, perhaps the most hotly contested change has been the removal of Asperger’s Syndrome (Mattila et al. 2011) which has become central within contemporary discussions of ASD.

The research interviews for this project took place between August 2012 and February 2013 and, over this period, the DSM-5 classification went from a stage of ‘final proofing’ to having its composition confirmed. Perhaps unsurprisingly most, although certainly not all, researchers held strong and usually nuanced opinions about DSM-5. Three areas of concern were repeatedly raised when researchers discussed what they thought DSM-5 ought to achieve. These three areas of concern to researchers within DSM-5 were those of scientific validity, socio-economic politics, and identity politics. It should be noted, however, that exactly where one of these arenas ended and another began was rarely straightforward. What is noticeable, once again, is how easily normative prescriptions and scientific descriptions merge. The following extract from AO, a Research Fellow, discusses the manoeuvring surrounding Asperger’s within DSM-5:

AO: I mean Asperger syndrome, there was a lot of, erm, objections to the possibility of that going because there’s a very strong Asperger identity, which I completely understand and that the committee I think completely understand and accept which is why they’re keeping that as a qualifier I think [is the word they use. (AO04: 377-382)

To provide context to this extract, AO has previously stated that there “isn’t the research evidence that consistently backs up that it [Asperger’s] is a different diagnosis” (AO04: 123-125). Nonetheless, AO believes that there have been ‘objections’ to the possibility that Asperger’s might be removed because of the ‘strong Asperger identity’ which has formed around it. These are objections that AO is able to ‘completely understand and accept’, and AO seems reasonably happy for the inclusion of a qualifier to pacify these needs.

CT, a Senior Lecturer, makes a similar point. It is particularly noticeable in this extract the extent to which the line between scientific evidence and identity politics is repeatedly blurred:

CT: The:: rationale for the changes in the diagnostic classification is indeed that the reliability of the sub-diagnoses, you know, between Asperger’s and High Functioning Autism and so on was low, so a child who was diagnosed with Asperger’s Syndrome in one setting or one, by one clinician might go to another with, showing the same symptoms and so on, and receive a diagnosis of High Functioning Autism so the sub-diagnoses were not sufficiently reliable. That’s the rationale for changing the way the diagnosis is done. I do have reservations about these changes, I don’t know whether this is a pre, prefiguring, or prejudging what you’re going to ask me next, but do you want me to [carry on?]

GH: [Oh no, go,] go ahead

CT: Well, erm, (.) I think for one thing in terms of thinking, thinking of the interests and needs of people on the autism spectrum that Asperger’s Syndrome is a kind of badge of identity, erm, for people on the Autism Spectrum, people with Asperger’s Syndrome, they see it as sort of maybe less stigmatising than a diagnosis of autism, erm, it’s been around for a while, erm, it’s very much part, part of the way we think and con’, think about and conceptualise autism. So I think that’s going to be:: a big shift, erm, I think it might be a bit premature. No. It may be that the ways in which Asperger’s Syndrome is differentiated from (.) autism proper, so to speak, are not sufficiently reliable at the moment, it doesn’t necessarily mean you throw out that sub-diagnosis. (CT02: 239-265)

CT begins this extract by explaining the ‘rationale’ for the changing diagnostic criteria in terms of reliability, considering how individuals diagnosed as having Asperger’s ‘in one setting or by one clinician’ may receive a different diagnosis within another setting, or with another clinician. The rationale for

DSM-5 is therefore laid down broadly in terms of the reliability and validity of diagnoses. CT ‘has reservations’ about these changes, however, principally that the ‘interests and needs of people on the autism spectrum’ may not be met, that the loss of the ‘less stigmatising’ ‘badge of identity’ that is Asperger’s may be unduly damaging. These reservations, therefore, broadly concern issues of identity. Within a sentence, however, CT reverts to considering the re-formulation of diagnoses in terms of reliability and concludes, apparently on the basis of the identity politics of Asperger’s, that while sub-diagnoses are ‘not sufficiently reliable’ it ‘doesn’t necessarily mean you throw out that sub-diagnosis’. In CT’s talk we are not seeing normative, value-laden concepts intrude into the ‘objective’ world of diagnosis; the door is thrust open and they are being actively encouraged in. As AO notes, this is a view that the working committee for DSM-5 seem to have held as well, with original drafts excluding Asperger’s entirely (Happé 2011) only for it to be re-introduced following public consultation.

Similar claims, that DSM-5 *ought* to consider the societal consequences of diagnostic criteria, were made in relation to the socio-political effects of diagnostic change. For example an Assistant Professor, BG, made the following point:

BG: ...the manifestation of the symptoms may not be very distinguishable between Asperger and high functioning autism. However, it has a larger impact to keep in mind, in terms of clinical and service provide, service users because, erm, you can, y, I mean, one needs to think about what will happen to all those people who have a Asperger’s Syndrome diagnosis from, from 2014 will they, will they go into SCD or will they go into ASD because some Asperger’s Syndrome patients may not have had, er, mm, a repetitive behaviour issue. Now, that would be true also for ASD, I mean, they may not have had repetitive behaviour issues, so:: (.) that in, in that sense, as long as the service users can be protected and those people who need services can be, can, can continue to access them, then:: that, that, then the, otherwise this general move from, er, creating separate categories called Asperger and autism into one category called ASD I think is a generally good move... (BG06: 430-446)

BG agrees with the DSM-5 committee that ‘the manifestation of the symptoms may not be very distinguishable between Asperger’s and high functioning autism’ and accepts that the merger of these diagnoses into one category is

‘generally a good move’. DSM-5 is here applauded for supporting recent scientific evidence. This merger would only be appropriate, however, if the consequences for ‘clinical and service providers’ and ‘service users’ were considered. It is of paramount importance for BG that ‘service users can be protected’ and that ‘those individuals who need services can continue to access them’. If, and only if, these conditions are met should the diagnostic criteria change.

ST, a Professor, is particularly explicit about the requirement of DSM-5 to protect the imagined socio-political futures of individuals with autism. ST states that, within their own research, sub-diagnoses have never been used. This is at least partially because a diagnosis of Asperger’s is dependent upon a retrospective interview which seeks to establish the onset of communication difficulties, a process which ST believes is simply too problematic. When asked if the move within DSM-5 to ‘make that official’ was positive ST states that:

ST: Erm:: yeah. I think it’s broadly a sensible one because dia’, diagnosis is generally to help the individual, erm, be managed and cared for appropriately, and I’m not so sure that, er::, an individual who has perfectly good language now and a perfectly good level of intellectual functioning, er, it, it doesn’t make any difference whether their language is a bit delayed when they were, er, three or whatever. (ST07: 382-389)

It is immediately evident here that ST does not frame the change within DSM-5 as positive because it aligns with research requirements; rather it is positive because it should ‘help the individual’ be ‘managed and cared for appropriately’. ST goes on to accept that the removal of Asperger’s is problematic for those who have forged identities around that label but states that:

ST: ...well, you can start to think about this in various different ways, you can start to say, erm, why not keep, why not keep the labels? Fine, but then, er, er, services agencies are going to want to look for reasons not to spend money, erm, and so they’ll say well we’ll only give this service to people with Asperger’s Syndrome and we won’t give it to people who’ve got autism, er, but you take, pick two individuals and they’d be indistinguishable apart from something that happened when they were three, erm, and you’d say well this guy’s going to get services and this guy isn’t and

that's not fair, whatever about this guy's identity and this guy's identity and so there, there's complex issues which I'm not competent to, to talk about even though I'm a, a, aware of them as, a, important issues for the individuals themselves, I mean it's one, it's quite something to say to somebody well we know we've always called you Asperger but, you know, you're not anymore, you're just, you've got Autism Spectrum Disorder, maybe, of the Asperger type or something like that, because they can, they can put qualifications into the diagnosis now on the basis of DSM-5. (ST07: 403-422)

It is clear that, for ST, service provision trumps identity politics. The fact that 'service agencies are going to want to look for reasons not to spend money' means that it is simply not feasible to have autism and Asperger's co-exist as it is possible that this there will be a 'service to people with Asperger's Syndrome and we won't give it to people who've got autism'. This, quite simply, 'is not fair' and fairness must be considered within the diagnostic manual. The addition of qualifiers, allowing somebody to be 'of the Asperger's type' is a nod towards the importance of identity politics, but for ST it is service provision which should drive DSM-5.

It is interesting that, despite clearly giving the issue a great deal of thought, ST states that 'they are not competent to talk about' these issues'. And yet the DSM-5 working committee was comprised entirely of British and American psychiatrists and psychologists with similar knowledge bases. One Lecturer stated that:

EC: In a sense you, you're trying to describe and understanding what people with autism and families with autism think is the core of autism, what they think is problematic, what they're finding difficult. (EC11: 169-172)

This position nicely captures the general sentiments expressed with the interview data; diagnoses *ought* to be normative, they should 'describe and understand' what 'people with autism and families with autism think is the core', 'what they're finding difficult' and 'what they think is problematic'. Historical analyses of the DSM have revealed that socio-political matters have been always been crucial in the determination of mental disease classification (Pickersgill 2012a). What is added to that analysis here is that within these interviews scientists state that DSM should be a socio-political act, it should be

working to ensure the best possible future for those deemed to be in need of care. What is not acknowledged, however, is that what ought to be within a research setting, that societal values become social truths, and that a classification which only makes sense within a particular historical-cultural moment is localised and reified within an individual.

*ii) Diagnostic practice*

The autism researchers interviewed repeatedly claimed that, in order to be published in the ‘top journals’, it was essential for participants to be given a score on the Autism Diagnosis Observation Schedule (ADOS; Lord et al. 1989; Lord et al. 2000) and, thus, despite complaints over costs of both training and the kit itself, most interviewees had qualified to administer the ADOS and/or had students who were.

The ADOS itself takes the form of a highly structured observation. A researcher sits one-on-one with an individual who potentially has autism. Over the next 45 minutes or so, and following guidelines, researchers run through a series of fifteen different tasks, including telling a story from a book, an attempt at a back-and-forth conversation, questions concerning work and school, and a demonstration task whereby the participant must pretend to brush their teeth with a finger. The encounter is video recorded and, following completion of the observation, the researcher watches back the interactions. The researcher then gives the individual a score, between 0-3 for example, on various measures seen to be indicative of one portion of the triad of autism symptoms. For example, RRBI is broken down into “unusual sensory interest in play” (this may take, for example, the form of repeatedly brushing a toy against one’s cheek), “hand and finger and other complex mannerisms”, “self-injurious behavior”, “compulsions or rituals”, and “excessive interest in or references to or highly specific topics or objects or repetitive behaviors”. The total scores are then tallied and any individual who scores above a particular cut-off is, for the purpose of research at least, classified as autistic.

Reading through the ADOS procedure, or better, watching the ADOS being performed with a teenager suspected of having autism, it is remarkable quite how (hetero)normative a process it is. Formal questions include “Do you

have a job”, “What about money? Do you take care of your own money?”, and “Do you have a girlfriend/boyfriend? What is her/his name?”<sup>16</sup> While it is not (quite) as straightforward as a failure to answer these questions ‘correctly’ leading to a diagnosis of autism, it is striking just how readily societal norms are introduced into this supposedly objective diagnosis.

There is also something of a semantic sleight of hand in the title of Autism Diagnosis *Observation* Schedule because it is quite clear that the ADOS is an *encounter* and not an observation. Bruno Latour has claimed that scientists “should never speak of “data” - what is given - but rather of “sublata”, that is, of “achievements”” (Latour 1999: 42) and that is certainly the case with the ADOS. The objective scientific finding in the ADOS, that an individual has autism, is not given to the researcher but only becomes apparent following a significant amount of work on their behalf; a diagnosis is not revealed, it is achieved. MC, a Research Fellow, for example, says:

MC: It [the ADOS] is, it is useful. And it’s also a useful practice that it, it really means you do up, get to understand that person. Erm and it’s a good way of eliciting the various behaviours that you’re, you’re trying to look at. So yeah, I think it’s, the measure itself is very good... (MC12: 618-623)

MC here states that the ADOS ‘is useful’ and that ‘the measure itself is very good’. However the claim that the ADOS is a good way of ‘eliciting’ various behaviours ‘you’re trying to look at’ is quite striking, reflecting the fact that autism only becomes visible following the activities of the researcher. That claim is put even more starkly by EC, a Lecturer, discussing the utility of the ADOS for examining social symptoms in autism as compared to RRBIs:

EC: Well we can, you can provoke a child to be crap at social interaction, excuse my language, by trying to have a conversation with them and they just fall apart. But you can’t really provoke a child into showing t’, t’, their like, you know, repetitive behaviour or something unless they. You know, it’s just much more difficult to elicit that. So on that, it’s not good at that at all. (EC11: 972-978)

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<sup>16</sup> This question is expanded upon with “How do you know she/ he is your girlfriend/boyfriend?” and “Do you ever think about having a long-term relationship or getting married?”

Once again we can see that EC talks about the ADOS as a place where it is possible to ‘elicit’ social abnormality. The term ‘provoked’, however, seems blunter still. The idea that you can ‘provoke a child to be crap at social interaction’ seems, if anything, quite sinister. Despite this, EC would go onto say that the ADOS was a space where you can “see the autism really staring at you” (EC11:1015). This is the central paradox within the ADOS and within autism – it is a space where autism is seen but only when it is ‘provoked’ and ‘elicited’, forced blinking from the shadows into the blinding light of truth, as Foucault says of madness (2006a: 79).

To a significant extent, autism researchers know this of the ADOS, that as within DSM-5, autism only makes sense within a normative framework, within a society, that the social of cognitive science cannot truly be separated from society. For example HB, a senior Lecturer, states that:

HB: Well, (.) Hmm.(.) Well I think for a start it’s [the ADOS] quite verbal, the, there’s lots of issues. So one is that there’s not particularly great developmental research using things like the ADOS so, if you wanted to say (.) you’re kind of rating kids on things like do they understand marriage or friendship and, and yet we don’t really have any kind of hard and fast norms for how typical kids understand those things or how much eye contact is appropriate, or how likely are teenage boys to engage in a conversation with a young woman they’ve never met before about, about their friends or their experiences you know so (.) and the modules, so module three for example goes from age four to fourteen and you would expect all of those things to be developing over time but you use the same set of codings for that entire age range so people have to kind of internally adjust what their expectations are according to what they think typical kids at that age might do which is quite hard, I think, and so a lot of it is still quite subjective. (HB01: 276-293)

HB understands here that the ADOS is attempting to ‘rate kids’ on things like ‘marriage and friendship’ to determine if they are normal or ‘typical’ and that these ratings depend upon the ‘expectations’ of the researcher. There is also an understanding that much of an individual’s response is determined by factors not accounted for within the ADOS, such as the age of the child and the relationship between the researcher and the participant. As hinted at by HB, the majority of individuals being assessed are young boys while the majority of autism researchers are young women and this relationship is undoubtedly

influential and dynamic both within and between ADOS sittings. The conclusion from these extracts is that, within the ADOS at least, autism is never ‘just’ seen, it is ‘elicited’ and ‘provoked’ into appearing by the researcher when they interact with the individual in question. That specific dynamic, the relationship between the researcher and the child is key to determining the form that autism will take, and the researcher’s idea of what ‘typical kids’ would do in that situation goes a long way to determining whether or not a diagnosis is appropriate. As with the researchers’ discussions over DSM, what a child *ought* to do in a given situation is the basis of deciding what that child *is*.

*iii) Laboratory practice*

Continuing with the theme that seeing autism should be considered a mutual achievement, participants repeatedly constructed the social as something that was very hard to measure objectively within a laboratory setting. In the extract below DR, a Professor, considers individuals with Turner’s Syndrome, a genetic condition which DR believes predisposes individuals to autism. Recognition that this condition is linked with autism has been, according to DR, slow coming, at least in part because many of these individuals are able to complete traditional cognitive tests of social functioning:

DR: ...they can do Theory of Mind tasks, they can, you can chuck a whole bunch of different tasks at them and they’ll be able to pass them. So you say well why are they so socially incompetent? Well because actually measuring social competence is something which is very, very difficult to do in a s::, you know, in a scientific way.  
(DR13: 373-378)

Most interesting here, for present purposes, is DR’s claim that ‘social competence’ is ‘very, very difficult’ to measure in a ‘scientific way’. What this statement reflects is a frequently stated position of autism researchers that ‘the social’ is something which is hard, if not impossible, to contain within the decontaminated realms of the laboratory.

That struggle to get the social to cohere to the demands of an experimental regime is remarked upon by MN, a Research Fellow:

MN: ...social behaviours just are such a (.) awkward thing to define compared to, like, dyslexia where you get them to do a reading test and then that's you're behavioural test. Erm, social behaviour's really hard to assess and test because it's interactional, so whatever the thing is that you're interacting with is also variable so, it's just quite complex to test it. (MN05: 781-787)

MN here focuses upon two themes that would reoccur throughout the interviews. Firstly, 'social behaviours' are particularly hard to define. Another interviewee would say, while problematising the notion of the social brain, that "...almost anything we can do can be construed as social under certain circumstances" (ST07:961-962) and that sense comes across clearly here. Secondly, and following DR, there is the claim that because social behaviour is 'interactional' it is 'quite complex' to examine. This problem is further considered by EC, a Lecturer, below:

EC: Erm, so ((exhale)) I think the problem with autism is that when you're ca', you're capturing something about a social dynamic and it's, it's about somebody's abilities falling down within a social setting, well experimentally that's quite difficult to replicate, so I think, I suppose the other way of looking at it is if you can, to think better about capturing real life in an experimental setting because maybe they're bad at, erm, they're bad at recognising emotion when it's in the context of something very dynamic that's happening in a short period of time in a real life interaction, whereas if you give something and they have, you know, five seconds to work it out and it's a still image they're going to be fine... So there's so much data that's contradictory and not well understood and I think a big problem is that it's not (.) it, it, yeah, it's something about the social context that we just not, you don't have inherent in an experimental task, you know? (EC11: 1069-1081, 1088-1092)

As with the previous extracts, EC here considers the possibility of 'capturing something about a social dynamic' within a laboratory setting. Experimentally, this social dynamic is something which is 'quite difficult to replicate', indeed it may be that the 'social context' is something that just isn't 'inherent in an experimental task'. This is quite a claim, that the defining features of autism resist the dominant methods of the human sciences. It is a claim, however, that clearly resonates with both the views of MN and DR, above, as well as some of those discussed in a previous chapter on heterogeneity, where researchers claimed that there was something about autism which made it hard to grasp scientifically (see in particular pp.124-

126). It also resonates with the above discussion concerning the ADOS in which social abnormality had to be ‘elicited’ or ‘provoked’ by experimenters. It is also interesting that EC seeks to overcome this problem by calling for an attempt to ‘think better about capturing real life in an experimental setting’. Utterances such as these continue to reframe the relationship between science and society within autism research. Rather than position the laboratory as an a-geographical, a-temporal space, there is the possibility suggested at here that one should deliberately attempt to bring society into the experimental setting. It is, of course, “lesson number one of the sociology of scientific knowledge” (Shapin 2012: 171) that society is present within the laboratory, but the suggestion that society should be introduced deliberately is perhaps more surprising.

*iv) neuroplasticity*

Placing narratives of neuroplasticity within contemporary neuroscience remains complicated. On the one hand, and as others have noted (Callard & Margulies 2011; 228; Papadopoulos 2011), it seems undoubtedly the case that the possibilities of co-opting science into the attempt to eradicate the boundaries between material and semiotic, inner and outer, nature and culture have lead to a certain overexcitement within the social sciences, and contemporaneous dearth of critical thinking on the matter. Similarly, it cannot be denied that the Nature Publishing Group alone have published hundreds if not thousands of articles on the plasticity of biology over the last decade. With reference to autism in particular, there is an interesting aspect to debates over neuroplasticity as there is the suggestion the societal norms may indeed be written into the biology of the social brain, thus society enters the laboratory within the heads of participants. RS, a Professor, notes the following:

RS: ...when you constitute small groups for say brain scanning and find homogeneity that suggests to me that it’s (.) certainly plausible that what you’re seeing is a reflection of a developmental outcome of what might be diverse stories that correlates with the autism that you’re seeing but may indeed not be the underpinnings of it, there may be different underpinnings, because the idea that I would suggest is that you explain the heterogeneity leading to homogeneity by a final common pathway in the

psychological level of explanation in terms of the development of these children  
(RS10: 348-357)

According to RS, it is ‘plausible’ that neuroimages are a ‘reflection of a developmental outcome’ rather than images of innate biology. In other words there is the possibility that differences in the social brain reveal not so much the ‘underpinnings’ of autism but rather the outcome of having autism within a particular societal configuration. CG, a postdoctoral researcher, bases much of their research around this relationship between the self and society:

GH: So what are the, yeah, so I mean the mechanisms which are underpinning, er, (.) the social cognitive behaviours I guess, or (.)

CG: Well (.)

GH: (.) Or that might be, er, atypical in an autistic population?

CG: (.) Yeah (.) What is atypical in the autism population. Erm, (.) that’s part of the reason why I’m here to study this developmental neuroscience, to understand autism. Because autism is heterogeneous but it’s, tends to show, er, pattern of, like, developmental trajectory. So::, any understanding, to understand social, and then there’s, social cognition is also the (.), erm::, the type of cognition which adapts to, the:: human world through experience and interaction, through the peer to peer interaction, through the interaction with wider members of the society. So:: I think that the key question is like, how they learn, how this brain like appeared in the real, erm, the world. (CG09:577-594)

CG is interested in studying ‘developmental neuroscience’. ‘Social cognition’, which CG takes as central to understanding autism, ‘adapts to the social world through experience and interaction’, it looks the way it does because of ‘interaction with wider members of society’. The question of interest is how the brain looks ‘in the real world’ having been shaped, at least partially, from the society in which it is embedded. CG is not alone in asking these questions, EH, another postdoctoral researcher claims that “...I’m going to be looking at EEG responses to social and nonsocial stimuli, that kind of thing to look at how, how the brain’s developing, how the social brain’s developing” (EH14: 483-485). Again, the emphasis here is upon development, ‘how the social brain is developing’ and comes to look the way it does because of the environment in which it finds itself.

In discussions over DSM-5, the ADOS, and laboratory practice, there is a folding of ‘the social’, in a rationalistic, scientific sense, into a political, institutional, and economic ‘society’ where identity politics are to the fore. These discussions reach their logical end point at neuroplasticity where society is literally written into the social brain, where the social disorder under examination has developed in the manner it has because of the society in which it formed. In all of these narratives, the divisions between is and ought has been completely erased.

### Conclusion

Throughout this chapter, Canguilhem’s claim that "the sick man is not abnormal because of the absence of a norm but because of his incapacity to be normative" (Canguilhem 1991: 186) has been endorsed. By examining discussion of the social in the diverse contexts of DSM-5, the ADOS, laboratory settings, and the concept of neuroplasticity, it has been claimed that this object of ‘the social’ that emerges within scientific discussion has several properties. Firstly, the social is not given, it is *achieved* in a local encounter between a scientist and a participant. In this sense, this chapter follows Mark Rapley when he claimed that, with reference to intellectual disability:

“...whether we look at formalised quality of life assessments conducted by psychologists; informal interviews...; or at the mundane interaction of staff members and the people they support in community based homes – that what is to count as (in)competence is negotiated and *constructed* locally, and for local purposes, by local means.” (Rapley 2004: 202, italics in original)

This argument has already been made with reference to autism (Stribling et al. 2009; Muskett et al. 2010). Secondly, but far from independently, the social of cognitive scientists is inextricably tied to notions of society. Chapter two (pp.31-32), in examining the Allports’ constructions of normal and pathological social behaviours in the 1950s, has shown that this has been the case throughout the history of social psychology. The ‘objective social’ of the cognitive neurosciences only makes sense in relation to societal norms at a particular historical, philosophical, and societal juncture and, hence, the

experience of autism as social abnormality, likewise, could only have occurred at a particular moment. Finally, the social is, and unlike other aspects of autism symptomology, constructed as an ethical substance, something that scientists *ought* to be concerned about; there is a duty of care and a compulsion to act upon this aspect of autism. The conclusion of this analysis has been accurately captured by Nikolas Rose. Musing on the claim of Canguilhem (considered in chapter 2:22-23) that ‘health is life in the silence of the organs’, Rose claims that there is a problem for psychology when:

“...what counts as abnormality is set by a norm of adaption to conventions of a socio-economic order. Health, for the psychology of the individual, is not so much life in the silence of the organs as life in the silence of the authorities.” (Rose 1985: 231)

This is absolutely the case for autism. Without institutions (including science) to ‘provoke’ and ‘elicit’ autism it would simply not be seen.

There is one final point which is worthy of consideration. If ‘social disorder’ refers to an incapacity to be normative rather than the absence of a norm, as Canguilhem claims, then attempts to study autism within diverse cultural settings with diverse norms is necessarily problematic. BG, a Senior Lecturer, made the following comment concerning an ongoing project seeking to examine autism within an international context:

I mean, that’s the problem of a behaviourally defined disorder, especially a social behaviourally defined disorder, it’s not a, I mean, it’s not a blood test, it’s not something that, that’s more, so, I think that, that has not been dealt with very carefully or, or in much detail. I mean so far people have been just translating, er, the Western criteria into various regional languages and, er, and trying to use the same criteria (.) and recently there was a Korean study that found a significantly high proportion of, of the population with autism. Now, whether that is due to the fact that the Western criteria are not well suited to identify, er, or are picking up abnormalities than normal, than, than, than, er, we don’t know. I mean I think, er, we, we have to have a more concerted approach towards, er, intercultural in, in diagnosing autism.

These concerns of BG, with regard to ‘picking up abnormalities’ with the use ‘Western criteria’ in general, and an infamous Korean study (Kim et al. 2011) in particular deserve careful consideration. Indeed Hacking has described this

research, led by Roy Grinker, which concluded that 1 in 38 South Korean children were diagnosable with autism as a “B52 attack” (Hacking 2013). This is a worryingly apt description. If, as is suggested in this chapter, the experience of autism is forged within a social encounter, then the risk of a pathological imperialism is quite evidently present.

## **Ontologies of our social selves: Normalisation and pathologisation in autism**

### Introduction

The previous chapter was largely concerned with the ways in which the condition of autism has been shaped by broader society. On a local scale, it was argued that the individual scientist ‘achieves’ autism by ‘eliciting’ social disorder within, for example, a diagnostic encounter. Globally, it was argued that interviewees describe autism as becoming visible within particular societal configurations, that notions of how individuals ought to behave or ought to be cared for in some sense determined what those individuals were. This chapter moves on from that discussion and begins to consider not only how autism has been shaped by society, but how autism itself has the capacity to shape society.

As was argued in chapter 1 (p.8), autism has become “the condition of fascination of the moment” (Murray 2008: 5), not (only) because it is interesting in and of itself, but also because autism has been constructed in such a way as to convince us that the condition reveals something fundamental about humanity *in toto* (Murray 2008: 16). It has been argued in chapters 2 (p.39-40) and 4 (pp.102-103), and following Allan Young’s analyses (Young 2012a; Young 2012b), that such a positioning of autism within discussions of the human condition is related to the recent centrality ascribed to empathy in both discourses concerning autism and those regarding humanity more generally.

The first portion of the present chapter will examine the relationship between autism and the rest of the human population. It will be argued that there are a number of contemporary research streams which attempt to ‘normalise’ autism; these endeavours both reconstruct autism, ushering in new possibilities, and cease to treat autism as a qualitatively distinct disease experience which reveals something about humanity through what is lacking (human-minus-social). Instead these normalising research practices transform the condition into a quantitative variation which is *itself* fundamental to an understanding of the human condition (‘we’re all a little bit autistic’). If, as Canguilhem claims (chapter 2: 22-23), health has no voice except that of

disease, then the quantification and normalisation of autism provide the opportunity for non-pathological behaviours found across the human population to be given voice. Thus, autism not only looks the way it does because of constructions of the social, autism is itself an increasingly key factor in contemporary constructions. This chapter concludes by considering whether this new vision of autistic sociality may be influenced by novel ‘resting state’ research methodologies and the new populations these methodologies attempt to surveil.

### *Researching up the autism spectrum*

The researchers interviewed for this project agreed that the last thirty years has been a period of intense change for autism. Perhaps the single ‘biggest change’ has been a vast increase in diagnosis:

ST: There were a lot of changes I mean the biggest change was the acceptance that autism wasn’t this little circumscribed set of disorders affecting, what is it, four children in 10,000, er::, ((coughs)) but that it was this much larger collection of conditions that affected about one percent of the population. (ST07: 63-68)

The diagnosis rates offered by ST, a Professor, reflect those presented in the literature, with a widespread claim that there has been an increase in autism prevalence from around 4:10,000 individuals in 1978 to around 100:10,000 in 2009 (Baron-Cohen et al. 2009: 500). Autism is no longer a ‘little circumscribed set of disorders’ but one of the most prevalent of all neurodevelopmental conditions, and certainly the one garnering the most attention.

It is worth noting, however, that interviewees saw a further movement within this broader trend for increased rates in diagnosis, and that is a focus upon the ‘high functioning’ end of the spectrum. One Professor noted that “there’s been a secular trend to find smaller and smaller percentages of the autism population having very low I.Q.” (MW18: 823-825). This claim is supported by DR, a further Professor, who goes as far as to state that “in my opinion autism is not a condition of mental retardation” (DR13: 293-294), claiming that, ‘in their opinion’, researchers have been ‘mislead’ because of

the failure of two key diagnostic instruments, the Autism Diagnostic Interview and the Autism Diagnosis Observation Schedule, to correctly diagnose individuals with autism who have a high level of functioning<sup>17</sup>.

MW, a Professor, says that at the beginning of their career they “...met very few, very few high functioning people, I mean there were very few high functioning people identified...” (MW18: 986-988). This suggestion is repeated by EC, a Lecturer:

EC: ...And the early days of autism research was always these low functioning kids because people didn't realise you could be a lot brighter and have autism ((laughs)) because they were the quirky ones in mainstream schools. (EC11: 347-350)

People ‘didn't realise’ that you could be ‘brighter and have autism’ and, thus, the medicalisation of ‘quirky’ behaviour could be conceived of as an act of consciousness raising on the part of autism researchers. Interestingly, while noting ostensibly the same phenomenon, CT reaches a quite different conclusion:

CT: ...over the last twenty years there's been a huge, I think, revolution in the way that autism is considered, erm, and some of the pathologising, erm, has gone, for better or for worse, you know there's been a swing towards, erm, looking at, erm, the high functioning end of the spectrum and, you know, celebrating some of the skills of people who are high functioning and so on, erm, so yes I have, I guess, you know, been able to witness a massive evolution. (CT02: 67-74)

CT, a Senior Lecturer, claims that a shift to the high functioning end of the spectrum has removed some of the ‘pathologising’, and that this ‘huge revolution’, or ‘massive evolution’, is of great significance. The difference in the conclusions reached by CT and EC is at least partially explicable in terms of focus; EC's statement concerns individuals, some of whom have certainly been ‘pathologised’, while CT's statement is concerned with the population and its perception, which may indeed have been normalised. This dual move of

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<sup>17</sup> For a consideration of the inability of the ADOS to diagnose high functioning autism see chapter 6: 125-126

pathologising the individual while normalising the population will be a running concern throughout this chapter.

What seems certain is that a shift in focus away from individuals with lower levels of functioning and towards individuals with higher functioning is seen as a hallmark of the contemporary field. This is captured nicely in the following extract by DR:

DR: ...I would say a particularly, erm (.), forward looking state, erm, the, the deep south seems to have much more traditional views, which is a sort of interesting geographical er, erm, er, split, er, but Utah, the new cases about eighty, eighty-five percent are high functioning. Erm, newly diagnosed cases. Down in, you know, South Carolina it's like twenty percent which is what it was thirty years ago, twenty years ago. (DR13: 318-325)

Here DR, a Professor, suggests that the 'forward looking' American states, such as Utah, divorce autism from intellectual disability while the 'traditional' 'deep south' continues with a position advocated twenty-to-thirty years ago, believing that autism is associated with intellectual disability. The disassociation of autism from intellectual disability is here directly positioned as modern and progressive.

Taking the increase in the diagnosis rates in conjunction with the shift towards higher functioning individuals with autism, it seems that a quite spectacular transformation in the concept of autism has taken place over the last thirty years, a change which, as CT states, researchers have 'been able to witness'. While the precise numbers are not of particular importance, let us assume that there is some validity to the oft quoted claim that autism prevalence in 1978 was approximately 4:10,000. DR claims that, around 1978, twenty per cent of individuals with autism were high functioning<sup>18</sup>. This would mean that prevalence rates of high functioning autism in the general population were approximately 1:12,500 in 1978. Prevalence rates in 2009 have increased to approximately 100:10,000, or one instance of autism per hundred individuals in the population, and DR notes that eight-five percent of

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<sup>18</sup> This may be an overestimate, although it is similar to the figure offered in the 1990s (Charman et al. 2011: 619).

individuals diagnosed with autism in Utah, in 2009 are high functioning<sup>19</sup>. If this were the case, as is suggested above, prevalence rates of high functioning autism would have jumped to around 1:118. This is a staggering transformation.

The changing nature of the autistic population has gone hand-in-hand with a changing research programme. The ability to study autistic symptomology in highly skilled individuals reflects the experimental requirements of cognitive psychology. The cognitive paradigms of interest since the 1980s required individuals to act upon verbal instructions beyond the capability of the vast majority of individuals diagnosed as autistic at the time, and who would now be thought of as ‘low functioning’. These prohibitive experimental requirements have ensured a literature seeking to make inferences about autism as a whole on the basis of samples consisting entirely of individuals with autism who have exceptionally high I.Q.s. One interviewee, for example, states that:

DR: ...We certainly got some very nice data from neuroimaging we’ve done recently, it’s the first study that’s been done, erm, of its type in high, high functioning adults, erm, where we’ve shown white matter tract anomalies all over the brain, it’s not in one area, it’s all over the darn place. But, very high ability individuals, an average I.Q. of around one hundred and twenty five or so, higher than the average student at [a large British university]. (DR13: 464-471)

There is a claim here that general models of connectivity in autism can be examined with reference only to this particular high-functioning sub-group. This is not an unusual claim, with other interviewees stating that “we just deal with high functioning groups” (FC15: 402-403). As stated above, such high functioning groups are attractive to researchers as they are able to complete more complex tasks, are easier to match with control groups comprised of undergraduate students, and so forth. Perhaps because of these abilities,

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<sup>19</sup> The figures quoted by DR would certainly be disputed in the UK, where a recent epistemological study claimed that around 50 percent of individuals with autism are free of intellectual disability (Charman et al. 2011). Some researchers (e.g. Skuse 2007) claim that this continuing association between ID and autism is a result of a clinical bias rather than the nature of autism. However, even this lower figure of 50 per cent denotes a quite remarkable increase in the prevalence of high functioning autism.

research with high functioning individuals has a long history, with a stretching of the spectrum away from the ‘core’ autism identified in Kanner’s initial study continuing since at least the 1970s (Baron-Cohen et al. 2005: 628).

*Researching autism outside of the spectrum*

The trend of ‘researching up’ the spectrum is not constrained to the analysis of individuals diagnosed with autism who are particularly high functioning. Two other trends are also discernable. First is the consideration of autistic symptomology within sub- or non-clinical populations. Within sub-clinical populations, individuals within the ‘Broader Autism Phenotype’ (BAP) are deemed to be in a group with:

“...‘sub-threshold’ social skills and communication traits and unusual personality features that are frequently found in the relatives of people with autism and which are believed to be milder manifestations of traits characteristic for clinically diagnosed autism.” (Sucksmith, Roth, & Hoekstra 2011: 360)

The concept of the BAP is believed to have emerged around 1998 (Sucksmith et al. 2011: 360) and is now used to examine autistic symptomology in research concerning, for example, hormonal (Ingudomnukul et al. 2007), neurological (Elsabbagh et al. 2009), and cognitive (Briskman et al. 2001) phenotypes in autism.

The work of Angelica Ronald and colleagues (e.g. Happé, Ronald, & Plomin 2006; Happé & Ronald 2008; Ronald, Happé, & Plomin 2005; Ronald, Happé, Price, Baron-Cohen, & Plomin 2006), investigating the so-called ‘fractionable triad’ of autistic symptoms, is the most notable project considering autistic symptomology within an almost entirely non-clinical population. Ronald’s work forms part of the ongoing Twins Early Development Study (TEDS), a longitudinal project featuring a representative sample of 12,054 twin pairs born in England and Wales during the years 1994, 1995, and 1996 (Kovas, Haworth, Dale, & Plomin 2007: 14), based at the Institute of Psychiatry, and funded by the Medical Research Council. Different subsets of that overall sample have been involved in different

projects and, for various portions of Ronald's work, between 3-4,000 twin pairs aged 7 were involved (Ronald, Happé, & Plomin 2005: 446; Ronald, Happé, Price, Baron-Cohen, & Plomin 2006: 692). Only a tiny proportion of individuals included in the studies had been diagnosed with autism (e.g. 37 in Ronald et al. 2005) and this is important to note; this study is entirely concerned with autistic symptoms in the general population.

Within the interview data, this consideration of symptoms in a 'general population' is, like the emergence of the BAP, seen as novel, part of a 'secular time trend':

MW: ...one can see a sort of secular time trend in that, that we, that there's a change from, historically, for studying autism in groups that are diagnosed, erm, to studying autism in: a general population group that aren't diagnosed and taking, sort of, trait-wise approach. (MW18: 469-473)

This body of research, then, in studying autistic symptoms within both the BAP and the general population, seeks to draw conclusions about the nature of autism from an analysis of groups outside of that diagnostic sphere. This ability to study autism in the general population seems to represent a distinct shift in the relations between those with and without a diagnosis. In the 1980s cognitive abilities known to be present in the general population (Theory of Mind, Executive Functions, and so forth) were taken to autism in order to reveal what was deficient in those individuals. In the 2000s it is conceivable to move in the opposite direction; to take traits known to be characteristic of autism and then apply them to the general population. Such a move demonstrably requires autism to be understood as a quantitatively, rather than qualitatively, distinct state.

#### *Researching autism through exceptional abilities*

Stuart Murray has claimed that there is a "current obsession with the savant figure" (Murray 2008: 83) within fiction concerning itself with autism, and it is certainly the case that a further area of 'researching up' within autism studies is the body of research that considers autism in the light of Savant Syndrome, where an individual shows "islands of genius" which vastly surpass other

abilities (Treffert 2009: 1351). Several interviewees had an interest in autistic individuals with such skills, with one interviewee stating that “...pretty much all my research is looking at, erm, increased ability in autism.” (MC12: 361-262). MC used this information regarding exceptional abilities to draw inferences about autism as a whole, claiming that the particular cognitive model developed within this research field explained deficits as well as strengths in autism:

MC: ...I would argue that that underlies a lot of the superiorities but also the deficits because, erm, if you’ve got extra capacity it spills over automatically whatever you’re doing and in some cases that’ll be relevant so it’ll be an advantage but in other cases it’ll be:: a disadvantage because it’ll be irrelevant information... (MC12: 379-385)

While the exceptional abilities studied by MC were those manifested within particular cognitive experiments, CT researched savant skills of more general interest, being concerned with the work of artists with autism such as Jessica Park and Jayson Valles who produce well recognised cityscapes and scenes from life in Manhattan. What CT, a Senior Lecturer, does have in common with MC, however, is a belief that a consideration of exceptional abilities will inform about autism more broadly:

CT: ...I’m actually much, much more widely very interested in the notion of imagination in autism, erm, and in (.) erm, I guess people with, people with talents and what that can tell us about, erm, the rest of the autism spectrum and what it tells us about, erm::, imagination more widely. (CT02: 538-543)

Once again we see from CT the claim that an examination of ‘talents’ can inform about ‘the rest of the autism spectrum’. There is also the additional claim that researching autistic savants will also inform about the non-autistic population; it is a goal of CT to study talents in autism in order to learn about ‘imagination more widely’.

PC, a Professor, who considers exceptional musical ability within individuals diagnosed with autism, makes a similar claim to CT regarding the capacity to learn about autism generally from the study of exceptional ability:

GH: So what, what do we think that might be telling us about what, about the nature of autism?

PC: I think that, I mean my thinking about autism is that autistic people, erm, have got a capacity to develop quite a unique type of intelli', I think intelligence is very different in autism... Erm, it's a kind of fluid intelligence that it is just quite different to the sort of nature of intelligence in neurotypical people.

GH: Mmm.

PC: Erm, and I think that the thing about savant syndrome that's interesting is that savant skills are clustered in domain. So there're obviously domains where having that kind of intelligence will make you really good at it. (PC20: 288-293; 309-311)

For PC, savant skills reveal a 'unique' 'autistic intelligence' quite different from that in 'neurotypical people' and it was only through the study of autistic savants that this fluid, autistic intelligence could be observed.

### *Canguilhem and autism*

These three research poles – studying autism in non-clinical populations, savants, and individuals with exceptionally high I.Q.s - are more or less independent experimental developments and, indeed, a single participant could not meet the criteria to be in two groups; one either has autism and an I.Q. within the normal range (high functioning autism), has stand-out abilities (Savant Syndrome), or does not have a diagnosis (BAP/general population). Nonetheless, beyond merely driving research 'up', these three poles share some notable effects which are worthy of consideration. Firstly, these studies conducted with individuals who have a high level of functioning do not only reflect the requirements of the cognitive neurosciences, but usher in new possibilities. As well as allowing quantitatively harder tasks to be utilised with autistic individuals (for instance, those requiring second order belief attribution, see chapter 5: 112-113), the presence of new highly intelligent populations means that it is now feasible to examine the autistic experience in qualitatively different ways, for example by interviewing

individuals with autism (e.g. Bagatell 2007) or considering works of art, poetry or autobiography (e.g. Roth 2008) produced by those same individuals.

A second consequence of the stretching of the spectrum comes from the fact that individuals with autism are no longer simply the objects of scientific research, but also the producers of scientific research. As Steve Epstein has famously shown (Epstein 1995; Epstein 1996) some groups are preferentially situated to inform scientific practice. While advocacy groups have long held an influence over autism research (see Silverman 2012 for a comprehensive analysis) the emergence of *self*-advocates able to speak in the language of science has been a relatively recent phenomena, leading MD, a Professor, to claim that we're in a 'period where the autistic voice is beginning to be heard':

MD: ...of course we're in that period as I'm sure you're extremely aware where, where, where, you know, the autistic voice, you know, erm, i::, you know, is, erm, (.) beginning to be heard... (MD17: 512-514)

It is now not uncommon for individuals with autism to be directly linked with laboratory work, a trend most famously illustrated by Michelle Dawson, an individual with autism who works within the laboratory of Laurent Mottron in Montreal (Mottron et al. 2006; Dawson et al. 2007). At least one interviewee wanted to replicate this scenario within their own laboratory:

ST: ...there's one guy and we're hoping to get him on our, on our kind of steering group in the lab here because he was genuinely insightful and thoughtful things to say about the inner experience of what it's like to, for one person to have autism. (ST07:1032-1036)

The incorporation of the autistic voice into autism research is certainly not complete, and other researchers bemoaned the lack of opportunity to further consult individuals with autism about research directions, but there are undeniably many new spaces emerging in which high functioning individuals with autism have become able to contribute to the autism research agenda. Neither, and as discussed in chapter three (pp.65-66), is the emergence of an 'autistic voice' entirely unproblematic in terms of providing a representation of

autism. What can certainly be said, however, is that the emergence of autism self-advocates is of potential importance.

These consequences of stretching the spectrum seem like classic instances of looping (Hacking 1995b). The experiments of cognitive science required a new population of individuals who were self-aware and able to engage verbally with scientists. As the gaze of the sciences shifted towards this top end of the spectrum the number of individuals found who met the necessary criteria expanded enormously. Once the scientific gaze had uncovered this wealth of potential subjects, the subjects themselves held numerous opportunities to influence scientific activity, from the possibility of new methodologies which required even higher levels of ability, to new forms of engagement where the ‘autistic voice’ was heard within the laboratory. Thus, these newly diagnosed individuals would be able to feed back into the cognitive science and alter its make-up in turn.

Further effects of researching up the autism spectrum can, as discussed above, perhaps best be understood by thinking about the research body in relation to Georges Canguilhem, whose work was considered at length in chapter 2. Canguilhem made two related arguments which are of relevance here. Firstly, it was claimed that during the 19<sup>th</sup> century it became possible to think of normal and pathological states as being quantitatively rather than qualitatively distinct (Canguilhem 1991: 41-42). Secondly, Canguilhem argued that ‘health is life in the silence of the organs’ (Canguilhem 1991: 100-101), that normalcy is defined in relation to pathology and not *vice versa*. Considering the possibility that ‘normal’ behaviours may be articulated, indeed may become visible, only after it has been framed by the discourse provided by a pathological state brings with it the possibility that, through processes of subjectification (Rose 1996a: 130), various forms of ‘social’ act find a voice through autism: autism may now be able to contribute to an ontology of our social selves.

The first portion of this chapter has suggested that these dual Canguilhemian processes are occurring, at least in the talk and activities of research scientists. The research into the BAP and non-clinical groups detailed

above has demonstrated that autism scientists have been able to take the vocabulary of autism and apply it in such a way as to understand the behaviours of the rest of the population. This move has, undoubtedly, been facilitated by research into autistic savants and individuals with high functioning autism who begin to bridge the gap between the pathological and the normal. That the researchers involved in the TEDS study then sought to re-apply their findings in the normal population back to autism further confirms the dialogue between the normal and the pathological in the case of autism. These findings suggest the possibility that the autistic experience, of concern throughout this thesis, may increasingly be an experience applicable to all subjects and not only a small proportion of individuals who have been diagnosed as socially disordered.

If the first half of this chapter has been concerned with ‘researching up’, bridging the division between the normal and pathological populations, the second half of the chapter is concerned with a second contemporary trend; to ‘research down’ with the aid of novel technologies. It is suggested that these technologies, which focus upon the subject at rest, may be facilitating further changes in the construction of social disorder with an increasing focus upon the body, rather than the mind.

#### *Putting the subject to rest*

Callard and Margulies (2011) have claimed that there have been ‘three poles’ within the cognitive neurosciences that have particularly excited neuroscientists and social scientists alike. Those poles are the concepts of: i) neuroplasticity/neurogenesis, which reconceptualises the brain as an open system (Papadopoulos 2011: 439); ii) analyses of emotion/affect which are taken to challenge a rationalistic view of the self, a field of research characterised as ‘biology’s gift’ to the humanities by Papoulias and Callard (2010: 33) and iii) a consideration of mirror neurons and the broader concept of the social brain (Young 2012b).

As has been discussed throughout this thesis, poles ii) and iii) have been of longstanding interest in relation to autism; from Peter Hobson’s Emotion/Affect Theory of autism during the late 1980s and early 1990s

(Hobson 1993a; see chapter four, p.94 onwards) to, more recently, the ‘broken mirror’ hypothesis of autism which contests that autism is caused by a dysfunctional mirror neuron network (Ramachandran & Oberman 2007). With regards to the remaining pole, that which is focused around issues of neuroplasticity, within the interviews there appeared to be a sense of increased openness towards the impact of ‘environmental’ factors and ‘development’ more broadly from within the cognitive neurosciences. ST, a Professor, for example noted the following:

ST: ... And I, I often wondered and I often speculated to myself, like, maybe even care giving practices and of course it’s, as you know, it’s very difficult to raise issues like this because of the whole refrigerator mother type of thing.

GH: Mm hm.

ST: But actually (.) erm, maybe, certain kind of, erm, things you would do with a typical baby are things which will drive an autistic child, you know, (.)

GH: Mm hm.

ST: Kind of, into its autism a little bit more. Er, whereas if you kind of hold back and are a little bit more aloof with the child maybe they just kind of are a little bit better. Who knows, who knows. But I, I’m kind of gratified to see that the, people are beginning to think about ways of pushing people around the autism spectrum a little bit so that they’re less handicapped by their autism. (ST07: 765-781)

ST does not here frame the turn towards the environment in neuroscientific language but the position is consistent with it, and ST claims that they have long believed that the ‘neuro-’ should not necessarily be privileged over the ‘-developmental’ in autism, and that they are ‘gratified’ that there has been a shift in this direction more broadly. EH, a postdoctoral researcher, notes a similar occurrence. When asked why it might be that siblings of children with autism are at an increased risk of diagnosis themselves, EH replied:

EH: Well:: people have traditionally thought that it means that it’s genetic or all of it’s genetic, right, erm, and so:: siblings have a higher risk because they have an older sibling and they share the same genetic background. Erm, but environmental theories of autism have become a lot more popular recently

and of course it could be that there's an environmental factor that's affecting that family that means that subsequent children are more likely to develop autism. (EH14: 401-408)

As with ST, EC sees the possibility of an 'environmental factor that's affecting the family' as a possibility in determining the aetiology of autism, and believes such perspectives have become 'a lot more popular recently'. As ST notes with reference to Bettelheim's (1972) notion of the 'refrigerator mother', these topics of environment and family practice have been, and continue to be, difficult for autism researchers to approach (see chapter 2: 45). Nonetheless, championed by researchers like Annette Karmiloff-Smith (e.g. Karmiloff-Smith 2009), there has been something of a move away from the innatist theories of Alan Leslie and colleagues which dominated during the 1980s and towards a consideration of neuroplasticity (e.g. Dawson 2008; Mundy & Neal 2000)<sup>20</sup>.

The central point of Callard and Margulies' article, however, is to suggest that:

"It is our contention that the brain – as it is being conceptualized and modelled by the neurosciences, as well as disseminated beyond them – is in the process of being reframed by a fourth potent arm of research, that on the brain's *resting state*..." (Callard & Margulies 2011: 228, italics in original)

It is hard to overestimate the importance of the movement identified by Callard and Margulies. The authors themselves claim that:

"...there appear already to be indications that the resting state and the DMN [Default Mode Network] might be installed as a new foundation of the self... it is the subject 'at rest' that – by dint of her default mode brain activity – potentially holds the key to subjectivity *tout court*." (Callard & Margulies 2011: 244, italics in original)

Callard and Margulies' analysis specifically and exclusively examines the influence of resting state magnetic resonance imaging (rsMRI). Without

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<sup>20</sup> See chapter 6 (p.147-149) for a further consideration of narratives of neuroplasticity

wishing to question to importance of rsMRI as a qualitatively distinct method with distinct consequences, it is also possible to broaden out an analysis and consider rsMRI as the archetype for a set of methods which seek to reduce task demands to a minimum and address autism within its ‘natural state’. For the current analysis, the defining characteristic of these methods is that they are constructed as requiring no explicit response from the participant.

*Seeing the real autism: resting state technologies and autism*

GH: ...So how, how do you think all the, or how do you think that (.) imaging and the neurosciences are (.) contributing to (.) issues around autism?

MC: Erm:: (.) I think they’re contributing a lot. I think, erm, so my personal view is that (.) one, one of the best bits of research that’s come out at the moment is looking at increased noise in the neural networks in, erm, in people with autism. And I think that finding a neural marker is so important because it means that it can be seen irrespective of tasks so, and also in whatever age and whatever level of functioning. So I think a lot of the problem with behaviour is, erm, you just don’t know what’s behind it and you also, you don’t know whether task demands are stopping you see the real, the true abilities of people. Whereas if you put them in the scanner then, and especially if you can put them in the scanner at rest or asleep and be able to see a difference between autism and typical populations then you have an amazing ability to diagnose. (MC12: 705-722)

The above quotation from MC, a Postdoctoral Fellow, captures so much of the hype and the hope surrounding resting state technologies. Autism research is constructed here as being fundamentally hampered by reliance upon behavioural measures, the results of which may not reflect ability as much as they reflect ‘task demands’, for example, the inability of participants to follow verbal instructions (see chapter 5: 113-114). Neuroscientific technologies, especially those that can be used while the participant is ‘at rest or asleep’ provide the opportunity to ‘know what’s behind’ behaviour and ‘see the real, the true abilities of autism’. This capacity to see ‘the real autism’ in individuals of ‘whatever age and whatever ability’ proffers an ‘amazing ability to diagnose’. These are significant claims to be making of a new technology. One can immediately identify the model of subjectivity commented upon by

Callard and Margulies whereby rest is ‘installed as a new foundation of the self’ and of the autistic condition.

There are two further points which need to be made with regard to the above extract. Firstly, MC is not alone in stressing the importance of these emerging methods which get ‘beyond’ or ‘behind’ task demands. MD, a Professor, for example, says that:

MD: ...I think that’s sort of quite a interesting sort of methodology because it suddenly doesn’t rely on you, erm, (.) asking someone a question, you know, erm, and of course a’, you know, there’s a long history it’s also true in infant, you know, experimentation where you do things like habituation tasks that, or looking time tasks, or whatever it is. But this is a sort of, you know, different sort of methodology. (MD17: 1000-1007)

Like MC, MD here focuses upon new types of methodology which ‘suddenly’ don’t require the experimenter to ask ‘someone a question’, thus reducing task demands. Once again, therefore, this new direction for understanding autism is constructed as both possessing quite a significant degree of importance and as being distinctly different from existing methods. A second point made by MD is that, by relating these new technologies to an existing literature within developmental psychology, methods which don’t ‘rely upon asking someone a question’ are not the exclusive domain of the neurosciences. In much the same way that the vast majority of fMRI, for example, has been task dependent<sup>21</sup>, it is possible, albeit in a more limited sense, to develop behavioural or cognitive experiments which are not task dependent. Eye-tracking experiments, for example, are behavioural experiments that have proven to be exceptionally popular within the field because the:

CG: ...data set is very rich so you can finish the experiment very quick with it, you don’t have to get hundred trials, you maybe have ten trials and get, of course, in a sample, sampling (data) could be like two hundred and fifty trials

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<sup>21</sup> The majority of the autism literature utilising fMRI continues to give participants a task to complete but analyses brain rather than behavioural responses to that task.

or could be a thousand trials, so you get like a thousand data points in, in, [in a matter of seconds. (CG09: 483-488)]<sup>22</sup>

Eye-tracking is an ideal technology for reducing task demands; it is perfectly feasible to ask how individuals “...visually attend to the world, and we look at their attention, memory mechanisms, erm, social attention, those kinds of things...” (EH14: 130-132) without requiring participants to either comprehend instructions or give deliberate responses. While the neurosciences, and particularly rsMRI, are primary movers in the examination of autism in its ‘natural state’, it is important to remember that other forms of experimentation are frequently conducted in which the participant is constructed as being entirely passive.

Regardless of the particular methodology being utilised, two key populations were repeatedly identified as being available for experimentation which, prior to methods which did not require one to answer a question, would have been much harder to examine. These two populations consist of individuals with low functioning autism and infants.

### *Infants*

One Professor, RS, mused upon what they saw as the field’s misplaced emphasis upon innate modules following the work on ToM conducted during the 1980s and early 1990s. Considering the importance of those errors within the field today RS concludes that:

RS: In fact, what’s happened, as far as I can see, (.) is that, er::, empirical changes have somewhat ((coughs)) modified the whole field anyway so it hasn’t been a great disadvantage, er, and in particular as people have looked at, er, studies of younger and younger and their siblings really you get into a realm where it’s clearly an issue of non-verbal communication that’s, that’s, er, at stake, and so things like joint-attention, which was kind of assimilated to a theory of mind picture, actually itself needs to be understood, erm, not in terms of what follows from it but in terms of its, what, its own standing, its own importance, and indeed what it derives from. (RC10: 131-142)

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<sup>22</sup> Eye-tracking is also cheaper than neuroimaging to several degrees of magnitude.

According to RS, ‘empirical changes’ have ‘modified the whole field’ of autism research since the 1980s. The most significant of these empirical changes has been the tendency for research to focus upon participants who are ‘younger and younger’, a change which was shifted attention towards ‘non-verbal communication’. As noted in the section above, these empirical changes have largely come about due to the emergence of technologies (rsMRI, eye-tracking, and so forth) that do not require participants to actively engage with the experimental scenario. This point concerning the importance of research into infancy is expanded upon by MD, a Professor, below:

MD: ...One [important strand of research] is groups who:: are running these at risk sibling or pre-term, that’s another design where you can look at pr’, you know, kids born prematurely, erm, because they’ve compromised development not just for autism but for a whole bunch of other neurodevelopmental outcomes. Erm, you, and, erm, (.) and, and, and then that overlaps with the group of people who use new technologies. So they use, you know, erm, eye-tracking and::, you know, erm, EEG and n’, n’, nears, n’, n’, near infrared spectroscopy and other, other sorts of, you know, w’, w’, well now some of the groups now do MRI, including functional MRI in four month olds. But anyway, there’s story, obviously I’m not an imager but, you know, but it’s quite cool. Erm, I didn’t know you could do functional MRI with four month olds, but you can.

GH: ((Laughs)).

MD: Erm, erm, which is great. Erm, well you can do, you can do functional ((laughs)), erm, MRI with foetuses but that’s another story too. And as I say it’s not my story, but erm, erm, and then there’s a group who are involved in, erm, screening studies across Europe and, erm, or have been and, and a group involved in early intervention. (MD17: 252-272)

Within this extract there is the explicit linking of research into infants at risk of autism and ‘the group of people who use new technologies’; specifically technologies mentioned include ‘EEG’, ‘near infrared spectroscopy’ (NIRS), ‘fMRI’, and ‘eye-tracking’. The type of study mentioned by MD, that which examines ‘at risk infant siblings’ is by far the most common form of method

examining autism in infancy. This type of study is possible because, out of those children who have an elder sibling with autism:

EH: About twenty percent end up with autism, erm, and about twenty percent end up with some other difficulty so social problems or language problems, erm, or just general developmental delays, erm, and then the rest tend to be typically developing. (EH14: 392-396)

While there will still be a significant attrition rate in experiments utilising at risk infants, therefore, a sample of one hundred infants might be expected to yield a sample of around twenty individuals who will go on to develop autism. This is a sample sufficient for many statistical analyses.

It should be noted that there are at least three independent developments which have made such study designs conceivable. The first is a conceptualisation of autism whereby signs of the condition are, even if only marked out as such retrospectively, conceived as being diagnosable in an apparently non-symptomatic individual. Continuing the Canguilhemian theme, this conceptualisation of autism is further suggestive of a quantitative rather than qualitative division between healthy and ill or normal and abnormal, in the case of autism and immediately makes us consider the literature on risk within the biosciences and its application to human kinds within, for example, healthcare (e.g. Shostak 2010) and judicial (e.g. Rose 2010) settings. Secondly, a diagnosis rate of between fifteen and twenty percent makes infant sibling studies ‘efficient’. In the 1970s, such efficiency would have been drastically reduced with diagnosis rates being so much lower; the autism epidemic of the 1990s has certainly facilitated this type of research. Finally, and of most relevance to the current discussion, methodologies are required with task demands sufficiently low that even very young infants are able to be examined. Indeed, as MD notes, technologies which ‘don’t require a question to be asked’, such as fMRI, rsMRI, NIRS, EEG, and eye-tracking go ‘hand in hand’ with these experiments.

The importance of new technologies to the developing field of infancy research in autism was stressed repeatedly by participants, including EH, a postdoctoral researcher:

GH: And it perhaps goes hand in hand with the, erm, (.) recruiting younger and younger infants because there are methodologies which allow you, er, to test hypotheses in different age groups than behavioural tests?

EH: Yeah, exactly so we test four month olds and, you know, there's so little you can do with a four month old, you can basically look at what they look at and you can look at what their brain does. (EH14: 1040-1047)

A range of methods are available to 'look at what their brain does' and these, by definition, are neuroscientific technologies. A further postdoctoral researcher, JH, for example describes how they use a range of neuroscientific technologies to see 'how the brain responses' and 'develops' in very young infants:

JH: ...We're using, er, ERP, EEG, so how the brain responses to:: novelty, to repeated sounds, to:: gaze, er, we're also doing fMRI to look at, again, whether, how the specialisation of brain areas develops to social stimuli versus nonsocial stimuli. So we are doing (.) a lot of things.

GH: And are you ((laughs)), yeah, and are you able to use those, erm, (.) neuroscientific tools on very young children, or=

JH: =Yeah so we can, the youngest we are seeing in this project is four months and we do, er::, MRI on that age, structural MRI, just to look at the structure of the brain, to look at this issue of connectivity. Erm, and they're asleep in the scanner so they can't be seeing things but we also present some auditory stimulation. (JH08: 340-354)

The methodologies mentioned by JH are very different to one another. Some, such as fMRI and EEG examine brain function while others, such as sMRI examine brain structure. What these methods have in common is that there is no comprehension required on behalf of the infant; stimuli (say, 'auditory stimulation') are presented and neural response is measured. The ability to 'see' the difference between social and nonsocial stimuli which are inscribed onto the brain in the absence of any task demands directly facilitates this form of knowledge.

While, as MD noted above, there is a long history within developmental psychology of 'looking at what infants can look at', eye

tracking technologies are now frequently used to give particularly fine-grained analysis. As JH, a postdoctoral researcher, says of their work with eye-tracking:

JH: ...we could be looking at attention, for example, how are, whether they are shifting attention from one object to another. Again you can do this with the young, very young infants just showing something on a computer screen, making them look at this object and then there's something else appearing, how fast are they to orient. (JH08: 322-328)

These technologies do not require subjects to be 'at rest' in the strict sense, as they are in rsMRI; there is still a process of 'making' the infant look at a computer screen. Nonetheless, this 'making' is done in the absence of specific instruction and, as in rsMRI and the other neuroimaging technologies considered above, there are no specific demands made of the participant. It is only because no demands are made that infants are able to be examined and used in experiments concerning autism.

#### *Low functioning autism*

A second population to come under (renewed) scrutiny following the proliferation of methodologies allowing experiments in which task demands are minimised consists of individuals with low functioning autism. Research into low functioning autism was discussed far less frequently than research into infant populations during the interviews. Indeed, a concern at the lack of research into individuals with low levels of functioning was itself a reoccurring theme within the interviews. Considering a finding that suggested that individuals with autism were less able to complete paired-association tasks than control subjects a Professor (ST) concluded that:

ST: Now (.) erm, that's important because (.) it tells us about particular processes that you can test out using much simpler paradigms that could be used with very severely autistic individuals. Very severely autistic individuals are under researched, they're almost scandalously under researched. (ST07: 512-517)

That individuals with low functioning autism, or who are ‘more severely autistic’, are under researched is, of course, the flip-side of the trend, identified above, to ‘research up’ the spectrum. Researchers gave numerous reasons for this lack of research, from individuals’ inability to complete the tasks that would be required of them, to an inability to find suitable control groups:

GH: So, so I guess maybe, so as you say, if there’s more of a focus on high, on the high [func]tioning end

EC: [Yeah].

GH: do you think by incorporating lower functioning [and]comparing that across a broad range of people, I mean

EC: [Yeah. Yeah.] (.)

Yeah, I mean I think there’s a huge value to that, I think it’s, mmm, you know, I think it’s really remiss of, I mean, it totally makes sense why it hasn’t been done and the biggest issue with testing low functioning children, and it’s the same problem that we had, is that how do you find a good match group?  
(EC11: 334-345)

While the issue of control groups may not be resolved by emerging technologies, there is a broad discourse that new technologies will be able to facilitate a return, within cognitive science, to the study of those individuals with lower functioning autism. As MD, a Professor, states:

MD: Well potentially yeah, yeah potentially into, and I mean it’s also true that you know, you, you sometimes you, these are paradigms you can use with, erm, people with intellectual disability because you’re not requiring them to understand an instruction or produce a response so if you can keep them still which is challenge as far as I’m, you know, I’m not the person who does the imaging but if you can keep them still, which is a challenge, erm, an::d erm get them to look at a monitor and be interested in the thing that’s in front of them then you can, erm, you know, you can be doing experiments with people, erm, you know, with a very w’, a wide sort of range of I.Q.s which I think has also not been done as much as it sort of should be. (MD17: 1037-1049)

As with research conducted with infants, the capacity to use ‘paradigms’ where the research is ‘not requiring them to understand an instruction or produce a response’ opens the possibility of conducting research with this novel population.

In the following extract, EH makes it clear that neurotechnologies, and specifically EEG, are ideally situated to offer experimental paradigms of utility for the study of individuals with low functioning autism; those ‘without many language skills’ or with little understanding of what is required of them within an experiment:

EH: I think, and again for kids with autism, for lower functioning kids who have, erm, who say don’t have very many language skills or who (.) don’t have many cognitive skills again for them behavioural tasks can be really difficult, sort of pressing a button can be really hard, erm, you know understanding that they’ve got to press a button whenever something happens or (continues), just that kind of stuff. So something like EEG where you can show them stuff and look at how their brain responds without having to do something, erm, can actually be a really good technique. (EH14: 1060-1078)

It is worth noting that EH mentioned EEG specifically and not the full gamut of neuroscientific technologies mentioned in relation to the infant literature. EH expands upon this point in the following extract:

EH: And again, fMRI really is almost impossible with low functioning kids because they don’t like lying down in the scanner and it’s dark and it’s noisy and it’s, whereas EEG they can sit up and it’s light and they can watch a video and they have the hat on and, so in terms of practicalities you can get, collect data from children right across the spectrum which is very hard with other methods. Erm, and I think that’s important in autism research to try and include children and individuals right across the (.) functioning spectrum. (EH1069-1078)

This extract reminds us that, for all their similarities, neurotechnologies remain diverse tools and come coupled with a range of contextual factors that are essential to understanding their knowledge production (Joyce 2005). Nonetheless, it is not only EEG that is suitable for the study of individuals with

low functioning autism. Just as for eye-tracking in infancy, technologies can be used to make behavioural experimentation feasible in this population:

ST: =They're much harder to test and you can't do things, and you know, a lot, a, a, a classic paradigm that we've used in our memory research is, you know, asking you to, erm, describe, er, imagine the last time you went to the movies with a friend and just describe this to me. Well you can't do that with a kid who's just running round and flapping and rocking and is minimally verbal and he's not going to give you any kind of coherent account if he can speak at all. Whereas if you can just have a series of things where you present simple, er, shaped stimuli, and combinations of stimuli and so on and reward them for peck, picking one rather than the other and the touch screen and technologies have improved enormously as well, er::, then, you know, by structuring those kinds of investigations you can get an, an enormous amount of information about what kinds of very simple, er::, psychological tasks are possible and what aren't in kids like that. (ST07: 542-558)

Here, new 'touch screen' technologies have aided researchers in being able to work with new populations, reducing task demands, and uncovering the 'true autism' that lies beneath them.

### *The performativity of rest*

There are two narratives running through the research into infancy and low functioning autism that has been depicted above. The first narrative is that, due to the ability of various technologies to remove or reduce task demands, to get 'behind' behaviour, and see 'the real' or 'the true' autism, emerging neurotechnologies offer an 'amazing ability' to diagnose and examine autism in all its forms, in new populations, and in new ways. The second narrative stands in direct contrast to the first. The inability to domesticate<sup>23</sup> research subjects and make them conform to the demands of these methodological innovations radically constricts the type of questions that can be asked. Looking at the extracts above, there are repeated references to the fact that there is 'so little you can do with a four month old', that research can only progress 'if you can keep them still which is challenge' because 'they

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<sup>23</sup> The term 'domesticate' is taken here from Prasad (2005) and his analysis of attempts to make the body cohere with the demands of the magnetic resonance scanner.

don't like lying down in the scanner and it's dark and it's noisy' and even if they're in the scanner the researcher needs to 'get them to look at a monitor and be interested in the thing that's in front of them'. These limitations, of course, drive the questions which are asked within this form of research:

GH: And does a lot of this [research] focus around the area of attention? Is that a kind of

EH: Yeah, it's one of the key areas. Because, because the things that ((laughs)), infants can't, I mean they can do a bit, but there's, there's, there's not a lot they can do, erm, but I think focusing on the more, sort of, domain general mechanisms like paying attention to things, which they can do, they can learn, erm, to habituation (.), erm, they can show preference to a social stimuli, which is important in terms of them getting enough social input, they can, you know, basic vocalisation which is important in terms of getting, so those sort of basic learning processes that, that all infants have to an extent, but to a varying extent. (EC10: 513-525)

In this extract the topic of attention is, apparently, not so much of interest *a priori* but rather a psychological construct that happens to match the ability levels of infants and the requirements of the technology. Infants can 'learn', 'habituate', and 'show preference' but, as EH noted elsewhere, "they're not going to point at something" (EH14: 1049).

What these resting state technologies reveal is a partial picture, but this is a partial picture that is presented as being 'behind' methodology, whole, as 'the truth', as 'the real autism'. This leads to a conceptualisation of autism made in the image of scanner, where a significant number of behaviours are 'othered' as they do not conform to the requirements of these methods. It is in this sense that these technologies can be described as productive, forming new visions of autism and social behaviour. This performance of autism can be described as a somatisation.

Within a somatised autism, there is an increased importance attributed to the body and its 'natural state', as it is the 'subject at rest' that comes under the gaze of emerging technologies. Secondly, there is decreased importance attributed to language. When these scientific studies seek to present social

*stimuli* those stimuli are based upon eyes, faces, or whole bodies presented in the absence of speech, for it is these bodily responses that can be turned into data within the bounds of the neurosciences. Similarly, when one wishes to examine social *response* what is examined is not a verbal report or anything which might require some form of internal monologue based upon rumination, contemplation, conscious understanding or the like. Instead, social understanding is based upon one's bodily responses, either external movements (e.g. eye saccades), or internal changes (e.g. particular activations of the social brain network). These responses are both extra- and pre-discursive in nature. These responses are pre-discursive in the sense that social (in)ability can be measured in those without language abilities; this category includes infants, animals, and those individuals (with or without autism) who may never be expected to gain significant language abilities. Responses are extra-discursive because social (in)ability can be measured without recourse to language, thus ensuring that individuals with competent language use can still be classified as socially abnormal.

### Conclusion

This chapter has been concerned with two trends within contemporary autism research. Both of the identified novel trends have the capacity to reconstruct autism and broader descriptions of social behaviour. The first half of the chapter was concerned with three research strands into high functioning autism, savants, and the BAP/normal population. It was argued that all three of these strands have engaged in a process of normalisation and extended the concept of autism towards the non-clinical population. It has been argued that such research practices are indicative of a changing relationship between the normal and the pathological in the case of autism. Existing processes of subjectification (detailed in the preceding chapters) seem to show autism taking on an appearance based, at least in part, upon experimental psychology's *savoir* of the social and particular requirements made of an individual within a given society (autism in the shape of society). By comparison, these normalising research programmes begin to understand society through the lens of autism (society in the shape of autism). The argument, here, is that these regimes have brought about a form of

subjectification whereby ‘healthy’ social behaviour has become explicable in terms of a discourse based upon autism, and that autism is being incorporated into an ‘ontology of our (social) selves’. We are all, increasingly, ‘a little bit autistic’.

The second portion of this chapter has been more directly concerned with reconstructions of ‘autistic sociality’ within populations diagnosed, or at risk of being diagnosed, with autism. It has been argued that research into two new populations – infants and low functioning individuals with autism – has intensified as new technologies have facilitated research in which the participant is able to be ‘at rest’ and is not required to have any particular comprehension of the experiment taking place. It has been argued here, and elsewhere (Callard and Margulies 2011), that resting state technologies are becoming increasingly influential sites for self-making. In particular, the claim is that, because of the perceived abilities of the individuals engaged in these research practices and the requirements of the methodologies themselves, this research utilising resting state technologies has privileged the body to the exclusion of discourse. The social has thus been ‘somatised’, with an increased focus upon automatic and unconscious bodily states and a decreased focus upon intentionality, language, meaning, and the abstract symbolic world.

What is it that unites these two research trends, seemingly heading in opposite directions? One Professor, MD, speculated that emerging technologies might offer one further possibility for understanding autism:

MD: ...you can actually get ex’, the same experiment which would rely on new technologies like, erm, (.) eye-tracking or:: EEG or ERP that you can use with infants and adults and y:: so, you know, you can’t do that with behavioural experiments, you have to have different experiments y’, erm, erm, there’s an even issue, even an issue of whether you can really sort of do it with infants and sort of adults. But, but you can get parallel versions at the very least and possibly even completely identical sort of versions. Which I think opens up all sorts of possibilities in terms of, you know, erm, investigating things across a wide sort of span of development which I think is, erm, good. (MD17: 1011-1022)

‘New technologies’ may allow for ‘the same experiment’ to be deployed with adults and infants, thereby facilitating comparisons between those populations. It has been argued, above, that the resting state experiments conducted upon infants and low functioning individuals construct a novel ‘somatised social’ following the forced ‘othering’ of behaviours which cognitive experiments upon higher functioning individuals routinely investigate, including the use of language, conscious understanding, and so forth. If it is also the case, as this chapter has argued, that a general process of subjectification has begun to take place whereby autism is incorporated into an ontology of our social selves, then the possibility for that ontology to be likewise somatised must be considered.

## **Conclusion: Autism, history, and socio-emotive politics**

Across four analytic chapters, this thesis has sought to examine the emergence and subsequent enactment of a particular type of socially disordered subject, the individual diagnosed as having autism, within the discourse of cognitive (neuro)scientists. There has been an attempt to examine the constructions of autism during the 1980s, when the contemporary vision of autism emerged within the cognitive sciences; in discourses discussing heterogeneity; with regards to broader constructions of society; and finally in relation to emerging neurotechnologies. Across all of these chapters there has been an interest in how constructions of ‘autism’ and constructions of ‘the social’ are related, and whether the concepts are in some sense dependent upon one another or indeed mutually constitutive. This conclusion will discuss the findings of this thesis; firstly on a chapter-by-chapter basis, and then by drawing together overarching themes and asking questions for possible future research. The limitations of the project will also be considered.

### *Chapter findings*

#### *Chapter 4*

The empirical portion of this thesis began by claiming that, within the psy-disciplines, a contemporary foundation for autism was laid down during the 1980s within the laboratories of cognitive psychologists. Three theories of autism were examined in detail within this chapter. The first of these was the metarepresentations hypothesis (Baron-Cohen et al. 1985; Leslie 1987) which claimed that autism was a specific deficit in a proposed decoupling mechanism allowing for a cognitive separation between representations of the world and knowledge of the world itself. It was proposed that, if a faulty decoupling mechanism were the primary deficit in autism, pretend play and knowledge of other minds might be areas of particular difficulty. The theory of Executive Dysfunction (Rumsey 1985) suggested that individuals with autism had a particular difficulty in integrating top-down existing knowledge about the world with bottom-up incoming sensory information and that this difficulty was manifest in particular experimental paradigms such as the Tower of Hanoi task. Finally, the theory of Weak Central Coherence (Frith 1989) argued that

the primary difference in autism was a ‘detail focus’, an overemphasis on parts of a system to the detriment of the whole. This particular cognitive processing style was demonstrated in weaknesses on tasks which involved reading for meaning (Frith & Snowling 1983), but also strengths in tasks such as the embedded figures paradigm (Shah & Frith 1983; Happé 1994b).

The central claim of this first analysis chapter was that these three hypotheses of autistic aetiology share a particular *savoir* (Foucault 1972: 15), a particular depth knowledge (Hacking 1995a: 198), of the social taken from cognitivist models of social psychology dominant since the mid-twentieth century (Danziger 1992; Danziger 2000; Greenwood 2004a). The construction of the social deployed within these models conceptualised social cognition as being entirely concerned with interpersonal engagement and differing from nonsocial cognition by degree rather than by kind. The social as an autonomous sphere does not exist within this framework. Considering the social in such a way allowed behaviours such as restricted interests and repetitive behaviours (RRBIs) and sensory abnormalities to become, in some sense, social deficits and at the very core of the autistic condition, a trend which has continued through to the 5<sup>th</sup> edition of the American Psychiatric Association’s Diagnostic and Statistical Manual of Mental Disorders (DSM-5; American Psychiatric Association 2013). The radical break from existing conceptualisations of autism that this *savoir* of the social ushered in was demonstrated by considering the work of Peter Hobson, and the disputes between Hobson and the proponents of the cognitivist models. While still considering ‘the social’ to be tied to interpersonal understanding, Hobson’s affective theory of autism (Hobson 1993a) relied upon the I-Thou/I-It distinction of Martin Buber (Buber 1987) which posits a qualitative divide between social and nonsocial acts. The chapter concluded by showing the apparent incommensurability of Hobson and the cognitivists’ positions by discussing the failure of all parties to find any empirical evidence to separate their theories.

There are two noticeable differences between the analysis presented here and existing histories of autism. The first is that this analysis centralises constructions of ‘the social’, something almost entirely neglected within the

existing social science autism literature. The second key novelty, which is not unrelated, is that the focus is upon the 1980s when previously the focus has been upon the 1960s and the effects of deinstitutionalisation (see, for instance, Evans' (2013) analysis of the UK context, and Eyal et al.'s (2010) version within a US context). This difference can, in part, be explained by the decision to focus upon cognitive psychology rather than upon child psychiatry. Within the UK context at least, such a decision seems justified. A recent study examining research trends showed that 56 per cent of all autism research in the United Kingdom (and around 44 per cent in the United States) concerns 'biology, brain, and cognition' (Pellicano et al. 2013: 26) and this is to a significant degree the legacy of Uta Frith and the research conducted during the 1980s. Nonetheless, and without unproblematically accepting the link between autism and deinstitutionalisation (see chapter 2: 44-45), there is no fundamental incompatibility between those works focusing upon the 1960s and the emphasis that is placed here upon the 1980s. The metaphor of archaeology in Foucault's work (e.g. Foucault 1972) alludes not only to excavation but also to addition of layer upon layer upon layer in the continual process of subjectification. Important work, for example, remains to be done surrounding the first decades of the twentieth century and the matrices that allowed both Kanner and Asperger to identify the first individuals diagnosed with autism as experiencing some form of impaired affective engagement. The key points raised within this chapter were, firstly, that the 1980s are a crucial and neglected decade in the concept formation of contemporary autism and, secondly, that constructions of the social have been key to understandings of autism.

### *Chapter 5*

Just as a focus upon the 1980s does not preclude the importance of the 1960s, neither does it exclude the possibility of subsequent changes in the concept of autism. The final analytic chapters of the thesis consider the possibilities of changes since the 1980s. The first change documented concerns the notion of autistic heterogeneity at the level of cognition, a concept which did not emerge until the 1990s (e.g. Happé 1991). This concept of heterogeneity underpins the oft-quoted maxim that 'if you know one person

with autism, you know one person with autism'. Examining discussions of autistic heterogeneity within the research interviews it was concluded that the term is itself heterogeneous. Firstly, the heterogeneity of autism was located within scientific knowledge production (*epistemic heterogeneity*), seen as an artefact of attempts to unify the findings of diverse methods and theoretical positions. Secondly, heterogeneity was positioned within the autistic condition itself (*aleatoric heterogeneity*), as a key part of the constitution of autism. Further, aleatoric heterogeneity could be further sub-divided into intra-personal heterogeneity and inter-personal heterogeneity. Inter-personal heterogeneity refers to the claim that there may be no one feature (at the biological, psychological, or behavioural level) which is shared by all individuals with autism. For example, during the mid-1990s (e.g. Frith & Happé 1994) it was suggested that while some individuals with autism had an impaired theory of mind others' abilities were intact in this respect. Thus, a different causative factor was deemed responsible for different instances of autism. Intra-personal heterogeneity refers to the claim that one cognitive deficit cannot be responsible for all autistic symptomology within a single individual. Within the contemporary literature, intra-personal heterogeneity is most readily applied to the concept of the fractionable triad (e.g. Happé et al. 2006) which claims that the social impairments, the communication impairments, and the RRBI's typical of autism do not emerge from a single cognitive deficit. While other pieces (e.g. Verhoeff 2012) have begun to consider the importance of heterogeneity within the contemporary autism concept, this thesis represents the first detailed consideration of the nature of autistic heterogeneity both historically and within interviews.

Following this consideration of autistic heterogeneity, the question was asked; how is autism research able to progress with such a disordered, incoherent conceptualisation of the object of study? Further analysis of the interviews seemed to show that order and coherence was found within a qualitatively distinct *experience* of autism which, for the researcher, was immediate, self-evident, and yet resistant to the scientific project. Autism was thus constructed in two quite different ways within the interview data; as a messy, disordered, difference of degree, and as an easily recognisable,

qualitatively distinct way of being in the world. It was argued that, to a significant extent, it was this qualitatively distinct experience of autism which allowed research to continue despite the disorder in laboratory settings. The work presented here is in agreement with both Silverman (Silverman 2004; Silverman 2012) and Fitzgerald (Fitzgerald 2012; Fitzgerald 2013), as well as the broader science studies literature on tacit knowledges (e.g. Collins 1974), in arguing that knowledge production in autism research is dependent upon a particularly broad range of activities, many far divorced from traditional conceptualisations of the scientific method. The recalled emotional reactions of researchers to early encounters with individuals with autism, a topic focused upon by both Fitzgerald and Silverman, was regarded as central to a continuing narrative constructing autism as a thing-in-the-world. Without these experiences, it seems far less likely that autism researchers would be able to continue unproblematically with their project.

### *Chapter 6*

Chapter 6 continued to interrogate this experience of autism as the socially abnormal and sought to show that such experiences were only ever intelligible within particular societal frameworks. Firstly, the chapter considers autism in relation to the requirements of DSM-5 and it is shown that researchers consider the social as an ‘ethical substance’. There is an ethical requirement to consider autism as a ‘social disorder’ because it is in interactions with society that autism most clearly shows itself. It follows that there is a moral project; DSM-5 *should* construct autism as a ‘social disorder’ in order to ensure service provision for individuals with autism. This explicit consideration of autism within a value-based framework is taken to show that the ‘objective social’ of the laboratory is indeed normative and best understood within the contexts provided by a particular society.

The chapter next considers the Autism Diagnostic Observation Schedule (ADOS) and it is again shown that normative frameworks are required in order to observe autism. What is more, researchers discuss how they must ‘elicit’ or ‘provoke’ social disorder into making an appearance. Similar claims are made of cognitive experimentation, with the suggestion that

social disorder is hard if not impossible to observe within an objective scientific framework. Finally, the chapter considers discourses of neuroplasticity and the notion that the normative somehow becomes the objective, that society is manifest within the brains of individuals with autism.

Drawing upon Latour's notion of 'sublata' this chapter suggests that the appearance of autism at a given moment is a multi-faceted 'achievement' (Latour 1999: 47). The achievement of autism relies upon a local encounter between the researcher and the participant, but this encounter itself is situated within a particular societal formation and with the availability of various methodologies and technologies; all of these are essential for autism to appear as it does. In a very real sense this chapter takes up Rosenberg's claim that "disease does not exist until we have agreed that it does, by perceiving, naming, and responding to it" (Rosenberg & Golden 1992: xiii) by suggesting that knowledge of autism, indeed autism itself, is always relational, always situated.

### *Chapter 7*

Finally, chapter 7 directly considers changing notions of autism within an era of cognitive neuroscience. It is claimed within this chapter that the autism spectrum is being stretched at both ends. It is first shown that three separate research programmes are extending the concept of autism towards the normal population. Firstly, there is now a significant amount of research conducted into the Broader Autism Phenotype (BAP; e.g. Sucksmith et al. 2011) which examines autistic-like traits within a sub-clinical population. Indeed, an increasingly significant amount of research into autism concerns entirely non-clinical populations (e.g. Ronald et al. 2005). Second is research into autistic savants who have exceptional talents in particular areas (e.g. Treffert 2009). For example, interviewees considered artists, poets, and musicians with autism. Finally, there is research into individuals with autism who are of generally high intelligence and who have no co-morbid conditions (for example, intellectual disability). It is argued that, despite working with populations that differ in many ways, these three research programmes all breach the divide between normal and pathological, quantifying the difference

between social disorder and ‘normal’ human social behaviour. In a sense, these research strands are making it possible to interpret ‘normal’ behaviour through the lens of autism. This is a novel suggestion in relation to autism, although the current project supports recent articles broadly centred around the larger notion of biomedicalization (Clarke et al. 2003; Clarke et al. 2010) which have argued that a quantification in construction of disease has led to the pathologicalisation of normal human behaviours (Dumit 2004; Shostak 2010; Williams et al. 2011).

If the aforementioned research streams are stretching the spectrum into the normal population, a range of emerging technologies are facilitating a quite different expansion. It is claimed that emerging neurotechnologies such as eye-tracking, electroencephalography (EEG), and resting state functional Magnetic Resonance Imaging (rsfMRI) are allowing autism to be studied in populations who previously stood outside of the experimental regime of cognitive psychology. What is novel about these new technologies is that they require no significant degree of comprehension on behalf of the participant; this is markedly different to the experiments of the 1980s (for example) which required participants to follow complex instructions and be able to report upon their thought processes. The analysis presented here agrees with Callard and Margulies (2011) that these methods are becoming key sites of interest in contemporary productions of the self.

The two most significant of the populations to come under the gaze of these new methods are, firstly, individuals with ‘classic’ autism or with severe intellectual disabilities and, secondly, pre-verbal infants. In both cases the researchers interviewed claimed that studies using these novel methods got ‘behind’ method to reveal the ‘real’ autism. Chapter seven argues that, on the contrary, these methods require a great deal to be ‘othered’ in order to assume that they examine social behaviour. It is claimed in particular that within these research streams the social is made ‘somatic’; this entails language, meaning, and intentionality to be divested of their importance and a focus upon the body given primacy. An example of the ‘somaticisation of the social’ may be found in research concerning mimicry (e.g. Marsh et al. 2013). In such experiments participants watch an actor perform an act and the response is deemed to be

‘social’ if that act is mimicked. Here ‘social input’ refers to a bodily state (an action, for example), social processing refers to bodily states (eye movements, activation of the social brain network, etc), and ‘social response’ refers to a bodily state (mimicry of the initial action). It is argued here that this description of the social is novel and deviates markedly from existing articulations that have thus far been described (e.g. by Danziger (2000), Greenwood (2004a), Matusall (2013), or Young (2012a)).

Finally, chapter 7 argues that, as studies seek to make comparisons between the two ends of the autism spectrum, the inability to make the intentionality, meaning, and language of the normal population intelligible within the context of, for instance, pre-verbal infants, ensures that ‘social behaviour’ in the normal population is likewise being made somatic. Once again the novel suggestion here is that bodily action is being made visible through the language of autism.

This relationship between normalcy and pathology in an age of neurotechnology has a distinctly Canguilhemian appearance (Canguilhem 1991). Firstly, the distinction between normal and pathological is quantitative rather than qualitative. Secondly, ‘normal’ social behaviour is being given voice through pathological behaviour. And finally, and as discussed in chapter six, biological norms are shown to be normative. Returning to evidence presented in chapter four, however, a Canguilhemian framework is rejected as a general rule. The model of the social applied to autism in the 1980s emerged within the psy-disciplines for many reasons but primary among them was to seek knowledge about two social environments in particular; the classroom and the battlefield (Danziger 1992: 325). In the first instance it was a model of *normal* social behaviour, alongside various institutions with the aim of surveillance over childhood (Armstrong 1983: 27; Danziger 1990: 79), which facilitated the disease classification of autism and it was only later that the autism became a possible model through which to articulate general social functioning. The relationship between the normal and the pathological, science and society, autism and the social is dialectal and reciprocal; attempting to understand one in the absence of the other is futile.

## Overarching claims

### *Constructing the social*

Excluding that literature which has rather uncritically welcomed the neurosciences into the canon of the social sciences (e.g. Camerer et al. 2005; Glimcher 2009; see Papoulias & Callard (2010) for an analysis), the existing literature has generally taken one of two approaches when considering cognitive/neuroscientific approaches to studying the social. The first approach is perhaps best captured by Michel Callon when he claims that when:

“...the society described by sociologists confronts nature (no matter which description they give), society always has the last word. If the norms are removed, the science collapses.” (Callon 1986: 2)

Shostak and Waggoner (2011), for example, entitle an article *Narration and neuroscience: Encountering the social on the “last frontier of medicine”*. Such approaches, which state that science ‘encounters’ the social, clearly position the social as exterior and in some sense prior to neuroscience, and ‘the social’ as something which invades the laboratory at every turn and that can be used by science studies scholars to undermine the objectivity of the (neuro)sciences.

The work of Joyce (Joyce 2005; Joyce 2006; Joyce 2008; Joyce 2010; Joyce 2011) and the ongoing project entitled *critical neuroscience* (Choudhury et al. 2009; Choudhury & Slaby 2011; Slaby 2010) perhaps typify this type of analysis. Both the critical neuroscience network and Joyce continue to provide key insights into the ways in which neuroscience and the human sciences are embedded within society, but this thesis has suggested limitations to this approach. Most simply, this thesis has sought to reconfirm that the social is an explicit object of concern that comes under scientific investigation. It is not always ‘encountered’ in the laboratory; it is studied and created there. This position has been repeatedly stressed throughout the thesis. Secondly, and perhaps more interestingly, this thesis has sought to show that ‘the social’ within the laboratory is continually walking with, informing, and being informed by broader society. In chapter four it was shown that constructions

of the social formed within the psy-disciplines have been core to the concept of autism. Chapter six sought to show that the social was constructed as an ethical substance by researchers, and that they stated that societal norms should be incorporated within research agendas and diagnostic criteria. Chapter seven examined, in part, the capacity of the concept of ‘autistic sociality’ to inform broader identities and societal configurations far beyond the laboratory through the concepts of the Broader Autism Phenotype and research into autistic savants. Others have noted that there is a:

“...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.” (Fitzgerald 2012: 46)

This thesis here sides here with Fitzgerald, as well as with those who, like Callon, see the practices of the neurosciences as having a constitutive and reciprocal relationship with society. When one views the cognitive sciences only as cultural products, a great deal of the productive work that they engage in goes unexamined.

The second approach towards the social in science is found within the small literature which has considered the construction of the social within the laboratories of social psychology (Danziger 1992; Danziger 2000; Greenwood 2004a; Stam 2006) and social neuroscience (Matusall et al. 2011; Matusall 2013; Young 2012a; Young 2012b). The current analysis can certainly be made to fit within this literature. Two aspects mark the current project out from the existing literature however. Firstly, previous studies have relied almost exclusively upon published sources meaning that the current project is amongst the first studies to utilise, in part, an interview methodology to examine scientists’ constructions of the social. Secondly, the existing literature considering constructions of the social within the psy- and neuro-disciplines have all addressed the issue directly, concerning themselves with research endeavours entirely concerned with the social. This project, by comparison, has sought to examine the social as it is constructed within the context of a disease classification, autism. In this sense the project has

mimicked the processes of the neurosciences which so often use atypicality – from Phineas Gage, to individuals with acquired brain damage, and indeed individuals with autism – as a way into typicality. It is also an approach which has been used productively in the examination of other psychological constructs, most notably memory (Hacking 1995a; Hacking 2002; Roth 1989; Young 1995).

Utilising these novel approaches, the current project has sought to complement the existing literature into the nature of the social by making two arguments. The first regards the multiplicity of socials swirling within the human sciences. The key works of Greenwood and Danziger have, perhaps because of their historical scope, argued that there have been relatively few variations in the way in which the social has been thought of within the psychological sciences. Greenwood (2004a), for example, sees only two models of sociality across twentieth century psychology. The current analysis comes far closer to Mol's hospital ethnography (2002) in its conclusions. Mol contends that, not only has the birth of the clinic ensured that diseases are different in the twenty-first century to the eighteenth, but also that diseases are diverse within different rooms of the hospital as various practices, technologies, and so forth constitute different objects. This claim resonates with the current thesis. In chapter four it was argued that there were two competing versions of the social being enacted within the 1980s autism literature. In chapter seven it was further argued that a specific branch of the neurosciences – those examining the subject at rest – were conceptualising the social in a novel way which, once again, looked quite different. More broadly, it has been argued across this work that constructions of the social are being partially shaped by a consideration of autism itself.

It is crucial to remember just how disparate the psy-disciplines are. The focus, in chapter five, on heterogeneity, draws attention to the differences within the cognitive sciences. The focus upon resting state technologies in chapter seven has drawn attention to the differences within the social neurosciences. It is an achievement of these disciplines that they appear to cohere, but it remains important to focus upon the disorder, as well as the order, found within these disciplines.

Finally, the existing literature has frequently appealed to a perspectivalist (Law 2004: 25) notion of the social whereby it pre-exists investigation of it. This is particularly evident in texts which speak of ‘the *mark of the social*’ (Greenwood 1997), ‘the *disappearance of the social*’ (Greenwood 2004a), and the need to ‘*reclaim the social*’ (Stam 2006). This thesis has once again (Callon 1986; Latour 1987; Latour 2005; Law & Urry 2004) questioned that assumption. The social that comes under the study of science is always historical and stands in relational to broader conceptualisations of society.

### *The place of autism in history*

In his earlier writings on the human subject, Ian Hacking goes to great lengths to avoid questions of the reality or otherwise of Multiple Personality Disorder (MPD; Hacking 1995a: 16; Hacking 2003: 122). By 2007, however, Hacking is prepared to consider the reality or otherwise of human kinds and, of particular interest here, autism. It is particularly noticeable that in writings on autism Hacking declares that it is “absolutely false” and “absurd” to say “infantile autism did not exist before 1943” (Hacking 2007: 303. See also Hacking 2009a: 500; Madsen et al. 2013: 39). In a recently published interview, Hacking says in relation to his belief in the ‘reality’ of autism, that it:

“...is not a matter of personal conviction when I state that it is not transient, and when I say that something answering to the current descriptions has been with human beings forever, but has only been separated out rather recently. This is the conviction of nearly everyone who works in the field.” (Madsen et al., 2013: 41)

Of course Hacking is not a naive realist, and nuances this claim with reference to the following distinction:

“(A) There were no high-functioning autists in 1950; there were many in 2000.

(B) In 1950 this was not a way to be a person, people did not experience themselves in this way, they did not interact with their

friends, their families, their employers, their counsellors, in this way; but in 2000 this was a way to be a person, to experience oneself, to live in society.” (Hacking 2007: 303)<sup>24</sup>

With the relevant labels and dates altered accordingly, Hacking accepts both (A) and (B) as being true of MPD. However, while Hacking concedes the possible veracity of (B) with regards to autism he, as quoted above, believes (A) to be ‘absurd’ (although little explanation is given for that position beyond quotes such as that provided in the interview, above).

Hacking is joined in this position by other notable scholars from within the medical humanities. Stuart Murray, for example, states that that:

“...autism is both timeless and totally contemporary. It is part of human life, as it always has been, and yet today it is represented in certain formations that exist only because of the peculiar contemporary inflections that discussions of the condition have been given.” (Murray 2008: 11)

Murray’s summary maps more-or-less perfectly with Hacking’s differentiation between (A) and (B), above, and allows Murray to state that Herman Melville’s story *Bartleby the Scrivener*, published in 1853, is one of the greatest depictions of autism in fiction (Murray 2008: 50-60).

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<sup>24</sup>The emphasis within (B) of Hacking’s taxonomy seems to be firmly upon *self*-construction and *self*-experience and this seems to stand in contrast to the structuralist approach which flavours much of the work of Hacking and Foucault (and, indeed, this thesis). For example, and with relation to the concept of probability, Hacking wrote in 1975:

“The probability to be described is autonomous, with a life of its own. It exists in discourse and not in the minds of speakers. We are concerned not with the authors but with the sentences they have uttered and left for us to read. We do of course tag sentences with the names of authors, but this is largely a matter of convenience. This shall be particularly so in prehistory. We are not concerned with who wrote, but with what is said.” (Hacking 1975: 16)

It is hard to see how the self-construction of (B) can be reconciled with the kind of structuralist claim made here. This is not the place to ask whether the importance ascribed to self-construction, above, marks a definitive change in Hacking’s work, although Hacking himself has begun to consider such questions and appears to believe the position advocated in (B) is indeed consistent with his *oeuvre* (e.g. Hacking 2004).

There are several good reasons to problematise the conclusion reached by both Murray and Hacking. Perhaps the most obvious would come from the perspective of Actor-Network Theory (ANT) and dispute (or collapse) the division between (A) and (B); if the metaphysical assumption (what Law (2004: 25) has called ‘perspectivalism’) that (A) is prior and independent from (B) is rejected, as it is in ANT, then Hacking’s argument is weak. This weakness is apparent because, unless it is assumed that autism somehow exists prior to its enactment in particular individuals, then the division between (A) and (B) is meaningless (see Law (2004: chapter 2) for a discussion of ANT’s rejection of perspectivalist approaches and for an example see Latour (1999: 145-146) on the historicity of microbes). However Hacking has stated that, while fascinated by Latour’s work, he is not convinced by the conclusions (Hacking 1992: 512) and, thus, a critique from ANT is perhaps not that damaging with regards to autism as a particular case. Of greater relevance is the fact that Hacking’s conclusions are not consistent with the insights of Foucault.

Consider the issue of sexuality in Ancient Greece with which Foucault concerned himself (e.g. Foucault 1984b). It is not the goal here to defend the theses of Dover, Foucault, Veyne et al. who have concerned themselves with sexual conduct in Greece, only to outline the logic behind their claim, a logic which is not particular to sexuality and which holds particular relevance for autism.

Scholarship in the final decades of the twentieth century has revolutionised the study of sex in Ancient Greece to the extent that Davidson summarises current consensus in the following manner:

“[The] sexualization of Greek homosexuality was impelled above all by two powerful tendencies: a desire to uncover more and more of the truth of sex that modern historians and ancient sources were thought to be covering up, and a desire to demonstrate the spuriousness of Greek homosexuality (Devereux), of homosexuality (Dover), of 80 per cent of sexuality (Veyne), of sexuality *in toto* (Foucault). Inasmuch as Greek (homo)sexuality was said to be concerned with roles in sexual acts

rather than gender-orientation it was demonstrably different from modern (homo)sexuality, thus proving that (Greek) (homo)sexuality was a sexuality of roles and was ‘social’, it demonstrated, in itself, a non-essential, gestural, social performativity in the field of (Greek) (homo)sexual identity.” (Davidson 2001: 46)

Within his work on Greek sexuality, Foucault is not denying, of course, that individuals have engaged in particular physical acts since the beginning of time (this is an evolutionary truism) or that, when acts across a lifespan are examined from a contemporary perspective individuals appear to be (exhaustively) non-, hetero-, homo-, or bi- sexual. Foucault’s claim instead rests on two points. Firstly, Foucault claims that sexuality is not about (or is certainly about *more* than) acts; sexuality is an experience, a mode of being, a stable state that subjects are believed to carry with them throughout their lives. This is a compelling argument; how else does it make sense for someone to ‘know’ their sexuality prior to engaging in any acts, or to ‘come out’ as being gay within an existing heterosexual relationship? If sexuality is about more than acts then the fact that acts that *look* to be demonstrative of particular forms of sexuality in ancient Greece is largely irrelevant to the question at hand. That acts *look* to be demonstrative of particular forms of sexuality in ancient Greece is as much evidence for the pervasiveness of the contemporary experience of sexuality as it is evidence for particular types of subject during that ancient time period. This is Foucault’s general point. His second, specific, point is that evidence of contemporary experiences of sexuality are not to be found in Greece. The reason that Veyne and Davidson talk of “Foucault’s positivism” (Davidson 2001: 41) is that there is an attempt within the genealogical endeavour to examine historical periods without a teleological search for origins (Foucault 1977: 140), without the metaphysical assumption that the same acts are demonstrative of the same subjects or the same experiences.

It is curious that Hacking in particular should so radically depart from Foucault on this matter in the particular instance of autism for, in relation to other objects, his writings have been entirely consistent with the viewpoint outlined above. Michel Foucault famously opens *The Birth of the Clinic* with

two descriptions of the arachnoid mater, the protective membrane of the brain and spinal cord (Foucault 2003: ix-x). Pomme, speaking in 1769, sees the tissue as “pieces of damp parchment”, that peel away and are excreted by the patient. Bayle, speaking in 1825, provides an acutely observed visual description of the arachnoid and the membranes, their colour, and their thickness. The visual descriptions differ so greatly that, without the knowledge that it is indeed the brain under discussion, it would be hard to ascertain that a supposedly unitary object lay beneath. Considering these descriptions, Hacking states:

“The kinds of things to be said about the brain in 1780 are not the kinds of things to be said a quarter-century later. This is not because we have different beliefs about brains, but because ‘brain’ *denotes a new kind of object* in the later discourse, and occurs in different sorts of sentences.” (Hacking 1986b: 30-31, emphasis added).

It is difficult to see why Hacking should reach the conclusion he does about brains and yet be so reluctant to extend that position to autism. This project has repeatedly shown that cultural experience of autism as social disorder is produced – could *only* have been produced and understood - within the context of particular forms of knowledge; it has been shown that the experience of autism rests upon particular constructions of ‘the social’ from within academic psychology and that, within interview, scientists are unable to make autism stand alone and instead only consider autism within a societal context. This point does not deny that autism has some ‘prehistory’ (in much the same way that sex acts in Ancient Greece are in some sense a ‘prehistory’ to sexuality) but it does suggest that, firstly, autism is about far more than any straightforward biological reality and, secondly, that without a teleological search for its origins autism would not be identified in any individual prior to 1943. This is the move from passive observation to active encounter, and the conclusions from this thesis are that autism was formed *within* an encounter and did not, in any particularly meaningful sense, pre-exist it. Thus, pro-Foucault (and Young, among others) but contra-Hacking, this thesis advocates a position of historical nominalism and claims that, to all intents and purposes, autism came into existence in living memory. Just as with sexuality, it may

convincingly be argued that the fact that we can see autism everywhere in history is evidence of the pervasiveness of the contemporary experience.

*The birth of socio-emotive politics?*

Chapter 2 (pp.20-22, 26-27) contained a discussion of various forms of knowledge, and associated mechanisms of surveillance, which have been hypothesised to have systematically formed various types of human subject over the centuries. Foucault claimed, for example, that across the 17<sup>th</sup> and 18<sup>th</sup> centuries a form of knowledge came about which was centred upon the individual(ised) body (anatamo-politics; Foucault 1997b: 241-242). This pole of knowledge was complemented at some point around 1820 by knowledges concerning populations, now conceived of as aleatoric entities with their own properties (biopolitics; Foucault 1997b: 242-243; Hacking 1983: 292). Finally, between 1875 and 1925 (Rose 1985: 3) an ‘event’ occurred whereby the governance of the human soul became a third pole against which “we can triangulate recent knowledge” (Hacking 1994: 35).

Ian Hacking refers to this new form of knowledge over the soul, which emerged between 1875 and 1925 and is most readily applied to the psych-disciplines, as ‘memoro-politics’. Hence, in Hacking’s writing, ‘memoro-politics’ refers to forms of knowledge beyond those explicitly concerned with memory, instead stating that “...what is memoro-politics a politics of? Of the human mind, the self, the ‘subject’? I prefer to say a memoro-politics of the human soul...” (Hacking 1994: 35). Such a definition positions memoro-politics as synonymous with Nikolas Rose’s ‘governance of the soul’ (Rose 1999). For the moment, however, it is useful to rein in Hacking’s usage. Reserving Rose’s ‘governance of the soul’ to refer to the extreme pole of knowledge-power positioned against bio- and anatamo-politics, ‘memoro-politics’ can be usefully deployed in a slightly more precise manner, referring to forms of knowledge-power production relating specifically to memory and which nonetheless had far reaching and profound consequences for the human subject from the twentieth century onwards.

Re-positioning memoro-politics in the aforementioned manner performs two important services. Firstly, memoro-politics is moved towards

the “clustering of intermediary relations” (Foucault 1978: 139) at the centre of the bio-/anatamo-/soul- nexus rather than at one of the poles for, as Allan Young has said, the sciences of memory emerging in the late 19<sup>th</sup> century were “born at the intersection of two streams of scientific inquiry: somatic and psychological” (Young 1995: 11), that is, between anatamo- and soul-politics. Secondly, focusing upon memory itself and the central claim that “what has been forgotten is what forms [part of] our character, our personality, our soul” (Hacking 1994: 33) brings into relief how important knowledges of memory *per se* have been for recent projects of subjectification.

The new forms of knowledge production associated with the birth of memoro-politics at the end of the 19<sup>th</sup> century, and the mechanisms of surveillance intended to observe and surveil this space, had several specific consequences. Firstly, various forms of disordered subject were ‘discovered’ within the newly mapped territory; the classifications of amnesia (Roth 1989), MPD (Hacking 1995a), and post-traumatic stress disorder (PTSD; Young 1995) may all be seen to have emerged within this new knowledge-power nexus. Secondly, there was a comprehensive process of subjectification extending to the normal population. Just as it would not only have been the prisoner in Bentham’s proposed panopticon who was altered in the wake of anatamo-politics, it was not just the soldier with their traumatic memories who was affected by memoro-politics; we all understand ourselves differently in the wake of these processes.

It is these two factors – that more than one type of disordered subject emerged in the wake of a particular knowledge-power nexus *and* the fact that these knowledges affected general processes of subjectification – which lift the study of memoro-politics, as divorced from the study of MPD or PTSD, above mere tautology. There seems little to gain by studying a condition that is known *a priori* to be novel (as was the case with these disorders of memory and, indeed, autism), hypothesising that these disorders are evidence of a new form of generalised knowledge, and finally claiming that the newly hypothesised form of knowledge can be empirically demonstrated only in those who come under the label which initiated the original investigation. The

ability of empirical investigation to reveal these broader effects of memoro-politics are what continues to give the concept its utility and its validity.

A question which thus emerges from the current study into autism, especially given recent research noting a ‘turn to empathy’ within the biosciences (Meloni 2013), is this: is the emergence of autism in the late-twentieth century demonstrative of a broader process of knowledge production, a *socio-emotive politics* akin to memoro-politics? Have emerging processes of subjectification, centred around the notion of the individual as a particular type of ‘social subject’ with empathy at its core, changed our very nature? As stated above, evidence of a socio-emotive politics would need to be demonstrated through both an analysis of various forms of disorder and an examination of subjectification within the non-clinical population. An analysis of the requisite institutional frameworks would also be required.

The current project, focusing as it does solely upon autism, cannot begin to answer questions regarding socio-emotive politics. There are, however, certainly hints that suggest there is significant value in future research. The first question to ask is whether, besides autism, there are any other disorders which may have emerged within the knowledge-power nexus of a socio-emotive politics. Two such conditions are obvious candidates: alexithymia and psychopathy. Alexithymia is a condition, first described in the 1970s (Nemiah & Sifneos 1970; Nemiah et al. 1976), which garnered increased interest during the late 1980s and early 1990s (Bagby et al. 1994: 23). Alexithymia is believed to affect approximately ten per cent of the population and has been described as “a subclinical phenomena marked by difficulties in identifying and describing feelings and difficulties in distinguishing feelings from the bodily sensations of emotional arousal” (Bird et al. 2010: 1516). In short, alexithymia can be described as difficulty in ascribing socio-empathetic states to the self. Psychopaths, by comparison, have been described as “superficially charming, often intelligent individuals, who nevertheless had shallow emotional depth and engaged in antisocial, sometimes violent, behaviour” (Pickersgill 2012a: 546). While the construct of psychopathy has a long history (see Pickersgill (2012a) for an overview) it, like autism and alexithymia, has undergone a “rapid elaboration” since around

1980 (Manning 2000: 621). Also like alexithymia and autism, psychopathy is constructed as an inherent characteristic of an individual, rather than as an illness or disease which may pass, or be cured.

Within the research interviews conducted for this thesis, both alexithymia and psychopathy came under discussion:

GH: Erm, could you speak a little bit about how, erm, maybe your general theories of empathy have helped you understand autism and perhaps vice versa as well?

BG: Right, I mean the::, the idea that I have about empathy is very simple actually so it's more like a lenses through which you view emotions so, if you have a clouded lenses you can't see emotions clearly and, er, and if you have a clear lenses you can see them very well. So, and it does not have to do with autism, it's autism is just one kind of clouding of the lenses, er, there may be other kinds of clouding of the lenses as in:: psychopathy. (BG06: 458-468)

In this extract BG, an Associate Professor, positions psychopathy very closely to autism as a similar 'clouding of the empathetic lenses'. This is a theme that BG would return to repeatedly, mulling over the differences and similarities between psychopathy and autism:

BG: ...if you think about psychopathy the person might be perfectly okay at understanding your mental states, erm, and making you believe that he or she is, er, he's a, is a, a, postman or something, and then chopping your head off really. So:: now, here is a problem again if you think of it, it's not just a problem of (.) deficit of empathy. It's not just that I don't feel your pain. I may not feel your pain but that does not give me the::, the impetus to chop your head off, that's a separate thing. (BG06: 496-505)

Psychopathy is 'a problem again' of empathy, but quite a different problem, 'it's not just a problem of a deficit in empathy' that makes you want to 'chop your head off'.

MN, a Research Fellow, again draws comparisons between autism and psychopathy but also introduces alexithymia:

MN: And there are lots and lots of areas of social processing that are totally intact in autism but which, they're not given that credit because it's just assumed that anything social will be impaired in autism.

GH: So do you think the definition of autism as a social disorder then is misleading?

MN: Hmm that's an interesting point. (.) That's an interesting point. (.) ((laughs)). Possibly, I, I don't really have a problem with that though because it's certainly the case that, er::, (.). Hmm, let me think about that. ((laughs)). I don't know whether I have thought about that specifically before. (.) Possibly. Possibly, possibly. There's, well, I mean, so let's take psychopathy for a moment, where (.) I'm going to use the term we loosely, where we think that, erm, that really is empathetic problems.

GH: Mm hm.

MN: Erm, and that certainly leads to problems in social interaction but they're very different to the autistic problems. And equally, this other term that people are bandying around with autism at the moment, alexithymia have you come across it?

GH: Uh huh, uh huh.

MN: Yeah. Erm:: again (.) we think that they're totally dissociable, that's totally dissociable from autism, erm, but again it's a, it has effects on social interaction. Er, so yeah, possibly. (MN05: 516-541)

Psychopathy is based around 'empathetic problems' and alexithymia, too, has 'effects on social interaction' despite being 'totally dissociable'.

Similar links between autism, psychopathy, and alexithymia have also been made within the literature. In Simon Baron-Cohen's recent book, *Zero Degrees of Empathy*, autism, alexithymia (Baron-Cohen 2011: 69), and psychopathy (Baron-Cohen 2011: 54) are all described as conditions arising from atypicalities in the empathetic brain. With his usual eye for a headline, Baron-Cohen would make the same argument within the pages of *The Guardian*, suggesting that Lionel Shriver's bestselling book concerning a high school massacre (Shriver 2005) would be better titled 'we need to talk about Kevin's lack of empathy' (Baron-Cohen 2011a).

The purpose of introducing these extracts and this literature regarding psychopathy and alexithymia at this point in the thesis is not to analyse them as such, but rather to suggest that there may well be a school of disorders emerging from a shared basis in a socio-emotive politics centred around the empathic, social subject. The possible relationships between these disorders, the institutional frameworks designed to monitor them, and the knowledge bases from which these classifications arise are all in need of significant research.

The second question to ask in relation to socio-emotive politics is whether the language of autism is used by members of the non-clinical population to articulate their own sense of self. Some of the processes of normalisation referred to in chapter 7 (pp.153-160) do indeed hint that the language of autism is increasingly being used to understand those at quite some distance from the condition. Similarly, the description of alexithymia as a 'subclinical phenomena' (Bird et al. 2010: 1516) positions it within the realms of 'normal' human behaviour. A number of interviewees also interpreted their own behaviour through the lens of autism. For example AO, a Research Fellow, says that they 'remember doing that' when they read a check-list of autism symptoms and that 'we've all got aspects of it':

AO: ...I mean behaviours that you see in autism aren't really specific to autism in that we've all got aspects of it. I mean the more you look at the diagnostic, erm, criteria the more you think hang on a minute I remember doing that ((laughs))

GH: ((Laughs))

AO: Erm, which is slightly worrying. (AO04: 146-152)

As with the references to psychopathy and alexithymia however, these are no more than hints and robust empirical work would be required to examine the veracity of claims towards socio-emotive politics as a general framework of subjectification. A productive area of investigation may be to work with the families of children diagnosed with autism in order to examine if parental (for example) self-constructions have altered in the wake of encounters with autism. The work is yet to be done, but one of the potentially most important, and entirely novel, suggestions to come out of this thesis is that there is an emerging general framework of human behaviour, a socio-emotive politics, which may have a profound impact upon what it means to be human in the twenty-first century.

It is in the uniting of these overarching themes that the central tenet of this thesis is found. The contemporary vision of autism emerged within a particular power-knowledge nexus of the social, a power-knowledge nexus which may extend into a broader notion socio-emotive politics. However, conceptions of the social are more malleable than existing analyses have assumed and autism is itself becoming fundamental to visions of the social

within the psy-disciplines. In autism, the social is finding a new language and taking on a new appearance as a result. This looping process between autism and the social is becoming increasingly key to the ontology of our social selves.

### Limits

#### *Who is asked?*

If this project is to take seriously claims of situating knowledge (as discussed in chapter 3), it is crucial to situate not only the knowledge under analysis but also the knowledge that has been, hopefully, produced here. The story told within this thesis is partial at best; there are numerous aspects of autism which have not been considered. Perhaps most obviously, only individuals associated with academic psychology, and holding academic posts, were interviewed for this project. It is evidently the case that there are a great number of (neuro)scientists and professional groups, therefore, who are excluded from this analysis. Previous work has left us in little doubt that, if autism is to be understood in anything like a comprehensive manner, it is important to consider autism as it is understood from within education (Fein 2011), genetics (Navon 2011), psychiatry (C. Silverman 2010), and so on. It is also essential to consider autism within clinical settings, and consider the back and forth between clinic and laboratory. None of these areas come under empirical investigation within this piece and that is worthy of consideration when considering the claims made.

Similarly, an exceptionally powerful group of actors in autism are those policymakers and advocates who have, since at least the 1960s, informed, disputed and indeed merged with scientific thought. Further, at least since Jim Sinclair wrote *Don't Mourn for Us* in 1993 (Sinclair 1993), those advocates have been joined by the powerful new voice of the self-advocate, and autistic people themselves are now contributing to the discourses constituting autism in an increasingly important fashion. Once more, these voices are absent and, thus, the story is incomplete.

If important voices are missing from this work then it is undoubtedly the case that important places are also missing. Interviewees repeatedly stressed just how different they believed the UK context to be from the US context; how the ties between the clinical and the research spheres were much closer in America, how different the research experience would be in the absence of universal healthcare and the relationship with the National Health Service, and how much more divisive the (self-)advocacy groups were in America compared to the generally well-liked National Autistic Society in the UK. It might, however, be argued that the repeated mentioning of the United States demonstrated that, despite the perceived differences, research in the US is in some senses quite close to the UK; what autism looks like in, for instance, Francophone territories where the psychoanalytic tradition continues to play a key role, or in the Middle Eastern clinic mentioned by one interviewee is completely unknown here. There is no suggestion that autism in these places is even intelligible to the researchers interviewed for this project.

If the absence of these voices is worrying, or in the case of self-advocates perhaps distasteful, it is perhaps just as troublesome that the manner in which the work of (self-) advocates intersects with the discourses of the psych-disciplines and informs what *is* written here is also absent. By considering only work emanating from within the British cognitive sciences there is the possibility of treating these knowledges as hermetically sealed, as if all work is carried out under the Haldane Principle. This is patently false and a project, surely to be completed, which considers the construction of DSM-5 in relation to autism will reveal it to be so. Reading chapter six it may be easy to conclude that what British psychologists thought about the social in DSM-5 directly determined the nature of the final document. Yet evidently policy-makers, psychiatrists, advocates, self-advocates, American conceptions of autism, and interested publics shaped both the psychiatrists' bible *and* the discourse of those psychologists interviewed for this project. The inability of this project to trace that full web of associations ensures that not only is it impossible for autism to be fully understood, it is impossible for the interview data to be fully understood. And of course by interview *data*, what is meant is interview *sublata*, for the notion that data are achievements applies as readily

to the data of science studies as it does to cognitive science. (See chapter 6 (p.143) on the data/sublata distinction, chapter 3 (pp.58-60) for the interview as a situated encounter).

*What is considered?*

If one limitation of the study concerns who was asked for answers, a second is surely the manner in which those questions were asked. The methods utilised here focus upon the published literature and qualitative interview data. For the reasons given in chapter three there is every reason to suppose that these were suitable methods for the questions under investigation here. Nonetheless, there is an irony in this choice of method which has to be noted. In chapter 7 (pp.163-167) it is claimed that rsfMRI and associated technologies were broadly turning away from a model of the social which incorporated meaning, language, and intentionality and turning instead towards a somatic version of the social which focuses upon unconscious understanding of bodily states and that, in turn, manifests itself within the body through acts such as unconscious mimicry and gaze following. The irony, of course, is that in focusing upon methods which *only* approach the world through discourse any examination of how the social is constructed bodily within the cognitive sciences becomes impossible. Watching ADOS examinations, or even the interactions between different members of a laboratory, it is clear that bodies and the interactions between them are crucial to constructing social abnormality and yet, because a broader ethnography is not considered here, analysis of those bodily states is not possible. Bodies have become:

“background *plasma*, namely that which is not yet formatted, not yet measured, not yet socialized, not yet engaged in metrological chains, and not yet covered, surveyed, mobilized, or subjectified.” (Latour 2005: 244, italics in original)

Transforming the bodies of scientists, and individuals with autism, from background plasma and into something more solid remains one of the most important tasks in the study of autism. If the methods considered in chapter 7,

those transforming the social into an ever more bodily phenomenon, become truly dominant then it will be absolutely crucial for future projects.

*What is not claimed?*

It is worth considering three things which are not absent in this analysis as such, but which are actively opposed. Firstly, the current project is *not* intended to demean or undermine the intensely troubling experiences that, at least some, individuals with autism endure. Allan Young said of his analysis concerning the contingent nature of PTSD:

“To say that traumatic memory and PTSD are constituted through a researcher’s techno-phenomena and styles of scientific reasoning does not deny the pain that is suffered by people who are diagnosed or diagnosable with PTSD. Nothing that I have written in this book should be construed as trivializing the acts of violence and terrible personal losses that stand behind many traumatic memories.” (Young 1995: 10)

That point deserves to be re-emphasised; no claim of malingering, self-interest or ‘choice’ is made here. The needs of individuals with autism, and their families, remain as important within this framework as they do within any other.

Secondly, any account concerning autism which shifts attention towards ‘the environment’ runs the risk of being accused of ‘blaming the parents’. Once again it should be stressed that parents are *not* being blamed here. Indeed, in constructing ‘the social environment’ as being “local, proximal, short term, and decomposable” (Danziger 2000: 334) it is the psychodisciplines who arguably centralise the parent within the environment. This thesis, which has focused upon structure, history, and relationality has said little about parents and that is broadly appropriate given the goals here. As Silverman has demonstrated (Silverman & Brosco 2007; Silverman 2004; Silverman 2012), parents can contribute a great deal to our understanding of autism, but they are not to blame for it.

Finally, in suggesting that there are alternative visions of autism available, the thesis is *not* taking a relativist position which states that the social world, the social brain, or social disorder can be any way we choose it to be. As Haraway (2004: 589) has said, the body is *both structured and structuring*. The possibility for change is not infinite but it is there, there are alternatives.

### *The shape of things to come*

If the previous sections of this conclusion have considered the experience of autism in the first Foucauldian sense, as “something to be investigated to determine its emergence and its conditions of existence” (Lemke 2011: 32), then it is right to finish by considering experience in the second sense, as an ‘objective’, an attempt to move beyond the limits that have been imposed (see chapter 3: 72).

This study has argued that autism cannot be understood outside of a historical and sociological context, that the condition emerges as a result of work done both by individual researchers and clinicians at particular moments, for instance in diagnosis, and as a result of larger historical trends that have led to a ‘social disorder’ being constructed in a particular manner. It seems easier to affect the course of local interactions than it does broader historical trends. Psychologists and clinicians could more critically examine their own conduct by considering diagnosis as an encounter between two individuals, each of whom are worthy of respect, and each of whom contribute to the diagnosis. Similarly, if those coming under diagnosis constitute themselves as a subject in a relational encounter rather than an object under observation, then there is a realisation that there is:

“...necessarily the possibility of resistance because if there were no possibility of resistance (of violent resistance, flight, deception, strategies capable of reversing the situation), there would be no power relations at all.” (Foucault 1997c: 292)

The present study cannot present evidence to suggest that such treatment would have positive consequences clinically, but if psychologists are also

serious about ethical and societal issues in autism then this would surely be a positive step.

Further, and in relation to the social, this thesis has reiterated the claim, neatly captured by Danziger, that methodology is not ontologically neutral (Danziger 2000: 332). When psychologists take the social to the scanner, different realities are produced (Law 2004) and giving some thought to those social realities is a worthwhile exercise. This is perhaps an even more important exercise for qualitative social scientists who must carefully consider if these *new* socials emerging from within the neurosciences are ones that they wish to engage with, and if so, how.

Finally, more fundamental changes into the nature of the social and the nature of autism are harder to imagine and are certainly not for the researcher to prescribe. What is more, opening spaces to think differently is an inherently *risky* activity and any change would require extreme bravery from those affected. This is especially the case because, in many senses, the constructions of autism which emerge out of the neurosciences appear to be significantly less problematic, less violent, than those of psychoanalysis – particularly when they take the form advocated by Bettelheim (or Kanner, for that matter). It should be recalled, however, that contemporary constructions of autism can also be far more insidious, and certainly far more invasive, than previous descriptions, as infants and previously-non-clinical populations fall under the gaze and direction of scientific, juridical, pharmaceutical, and educational authorities like never before. There remains a need for change. In continuing a project that is beginning to piece together the historical ontology of autism, this project is also concerned with the historical ontology of the twenty-first century subject. It is only once this contemporary experience has been mapped that the edges are revealed and it becomes possible to think of ways to exceed our experience's limits.

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## Appendices

### Appendix A: Interview Schedule

#### **Personal Background**

1) Could you tell me a little about your career history? How did you come to be interested in your projects on autism and neuroscience?

#### **What is autism?**

##### *General*

1) Why do you think it is important to study autism?

2) To start of very broadly, there seems to be a good deal of debate about what exactly autism is and what causes it. Do you have a particular view of about what autism is?

3) How do we go about diagnosing autism and how *should* we aim to in the future?

- DSM V

- International prevalence

4) And what are the interventions?

5) How do you see psychology contributing to these issues?

6) How do you see neuroscience contributing to these issues?

7) On quite a specific level, psychologists too seem to have quite different theories about autism. What do you think the main areas of dispute, the main questions, are within psychology?

8) And are there any areas which you think psychologists generally agree upon?

##### *Personal*

9) Focusing more upon your work, which goals would you like your research to achieve?

10) Do you prefer one theory of autism in particular? Could you explain this theory to me?

10) What makes you think the theoretical framework you work within provides the best model for thinking about autism? What is the evidence for this theory and against the others?

11) And what about method, why do you choose the methods that you do?

11) Is there anything about the neurosciences in particular that has moved our understanding of autism beyond that of an era based entirely upon cognitive psychology?

12) Do you think that autistic advocates, or the voluntary sector, are an important factor in how your research is conducted?

### **The contribution of social neuroscience**

1) Could you tell me a little bit about social neuroscience? What its aims and underlying philosophy are? Is it a label you identify with?

2) What are the outstanding questions in autism which you see neuroscience tackling in the future?

3) Beyond the study of autism in particular, what do you think the impact of social neuroscience has been upon psychology as an academic discipline?

### **Conclusion**

1) Is there anything you'd like to add? Anything important that you think I've missed or that you'd like to elaborate upon?

2) Are there any other researchers you think I should talk to?

Appendix B: Information for Participants

**Information for Participants**

**Gregory Hollin**

Institute for Science and Society

School of Sociology and Social Policy

University of Nottingham

University Park

Nottingham

NG7 2RD

gregory.hollin@nottingham.ac.uk

*What is this study is about?*

This interview is part of a PhD project considering models of autistic spectrum disorders (ASD) within psychology and social neuroscience, how these models are taken up by advocacy groups, and what these changes might tell us about our knowledge of ASD and the social world more generally.

*What will the participant have to do?*

The participant is being asked to consent to a semi-structured interview in which the topics of ASD, psychology, neuroscience, and advocacy groups will be discussed. Interviews are expected to last roughly an hour, although length may vary considerably. If the participant wishes to limit the length of the interview they are able to do so. Participants may request the questions in advance, although novel questions may arise during the course of the interview.

*What are the benefits of participating in the study?*

The participant cannot expect any immediate, tangible benefit.

*Are there any foreseeable risks to the individual if they participate in the research?*

There are no foreseeable risks in taking part in this research.

*Are there any costs or inducements to taking part in the research?*

Other than the time taken to complete the interview, there are no costs associated with this research.

*Is participation voluntary? What should I do if I not want to participate?*

Your participation in this research study is completely voluntary and you are free to withdraw at any point up until the project completion in the Summer of 2013. Whether or not you provide your consent for participation in this research study will have no effect on your current or future relationship with the University of Nottingham.

*What happens to the collected information?"*

The interview will be recorded by the principle investigator and some notes may be made both during and after the interview. The recording of the interview will then be transcribed for subsequent analysis. Both the audio file and the transcript will be saved as encrypted files to ensure the security of the participant's data.

*Is the collected data confidential?*

The researcher will employ pseudonyms for the individual/institution in all research output.

*What are the research outputs?*

Interview transcripts will be analysed and this analysis will be included in a forthcoming PhD thesis and may be included in subsequent projects (e.g. peer review publications).

*What other sorts of people are being asked to take part, and how are they being identified/selected?*

Interviews have targeted two particular groups. Firstly, academics within the UK who have experience with both neuroscience and ASD. Secondly, representatives for various ASD advocacy groups.

### **Contact details**

The researcher can be contacted via the means detailed above. If you wish to contact the researcher's supervisors, please contact;

Prof. Alison Pilnick  
School of Sociology and Social Policy  
University of Nottingham  
University Park  
Nottingham  
NG7 2RD  
alison.pilnick@nottingham.ac.uk

### **Complaint procedure**

If you wish to complain about the way in which the research is being conducted or have any concerns about the research then in the first instance please contact the Alison Pilnick. If this does not resolve the matter to your satisfaction then please contact the School's Research Ethics Officer, Professor Brigitte Nerlich (tel. 0115 846 7065, email [Brigitte.Nerlich@nottingham.ac.uk](mailto:Brigitte.Nerlich@nottingham.ac.uk)).

Appendix C: Participant Consent Form

**Institute for Science and Society  
School of Sociology and Social Policy  
University of Nottingham**

**Participant Consent Form**

**‘Neuroscience, patient groups and the construction of new autistic identities’**

In signing this consent form I confirm that:

I have read the Participant Information Sheet and the nature and purpose of the research project has been explained to me. Yes  No

I have had the opportunity to ask questions. Yes  No

I understand the purpose of the research project and my involvement in it. Yes  No

I understand that my participation is voluntary and I may withdraw from the research project at any stage, without having to give any reason and withdrawing will not penalize or disadvantage me in any way. Yes  No

I understand that the researcher may be required to report to the authorities any significant harm to a child/young person (up to the age of 18 years) that he/she becomes aware of during the research. I agree that such harm may violate the principle of confidentiality. Yes  No

I agree that extracts from the interview may be quoted in any report or publication arising from the research. Yes  No

I understand that the interview will be recorded using electronic voice recorder. Yes  No

I understand that data will be securely stored. Yes  No

I understand that I may contact the researcher if I require further information about the research, and that Yes  No

I may contact the Research Ethics Officer of the School of Sociology and Social Policy, University of Nottingham, if I wish to make a complaint relating to my involvement in the research.

I am aware that I will, if referred to at all, be referred to by pseudonym in any research output. Yes  No

I am aware that my institution will, if referred to at all, be referred to by pseudonym in any research output. Yes  No

I agree to take part in the above research project. Yes  No

\_\_\_\_\_  
Participant's name  
(BLOCK CAPITAL)

\_\_\_\_\_  
Participant's signature

\_\_\_\_\_  
Date

\_\_\_\_\_  
Researcher's name  
(BLOCK CAPITAL)

\_\_\_\_\_  
Researcher's signature

\_\_\_\_\_  
Date