ACCOMPLISHING SEQUENCING THE HUMAN GENOME

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SUMMARY

Modern biotechnology has been transformed from a largely academic pursuit to a multi billion-dollar commercial bio-industry that is seen as one of the foundations of the knowledge economy. The sequencing of the human genome is seen as one of the great achievements of contemporary science. Though narratives of the sequencing of the human genome concentrate on the leading figures, the Human Genome Project was the achievement of big science. Big science represents the transformation of scientific work from a craft-based adhocracy into a form of work conducted within bureaucratic organisations that employ huge teams of scientists and technicians with a proliferation of specialised roles. This 'industrialisation' of science led many to describe the Human Genome Project as involving 'production line' efforts, 'sequencing mills' and an 'Industrial Revolution' for biology.

This thesis investigates the experience of work at the Institute, a large-scale sequencing centre. Entering the 'hidden abode' of production, the study examines the sequence of the human genome as an achievement of labour, rather than the product of 'great men'. Interviews were conducted with a range of people across the 'sequencing chain of production'. The study finds that work at the Institute was quite unlike the dehumanising, alienating work that might be expected as a result of the 'industrialisation' of science. Rather, the work of sequencing genomes recruited the sentiments of those working at the Institute, producing committed workers. This thesis examines the generation of commitment at the Institute in comparison to 'high road' models of work organisation. Given the central role of the sequence of the human genome in the future of biotechnology as a key sector in the knowledge economy, the Institute is considered with regard to debates around the future of work in technologically advanced economies.

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PART ONE: PRIMERS AND PRESENTIMENTS

Part One of the thesis, *Primers and Presentiments*, provides the exploration of the Institute with context. What is the history of the Human Genome Project? What analogues for the Human Genome Project and the Institute can we can we find in the literature on big science? What are the effects on science and on work of organising in these kinds of ways?

Chapter A, *Primers*, provides the layer upon which the rest of the thesis is written. The chapter describes the mythic language, the big historical narrative, and large-scale economic story of the Human Genome Project. This chapter acknowledges these as a foundation from which to explore what is missing from these stories; the everyday descriptions of work, the history of the Institute from the view of those who sequenced the human genome, and the economic story of labour, not stock markets.

Chapter B, *Anatomies of Work in Big Science*, explores the descriptions of work in big science that are found in the sociological literature. The different levels of 'bigness' are considered; that of science as a whole, that of national science, that of a scientific discipline, and that of a scientific institution. The Human Genome Project was big science in a number of these ways, but the Institute is a big science institution. The possible consequences that result from organising work in such a way are considered in Chapter C, *Pathologies of Work in Big Science*. The chapter draws on sociological concepts such as rationalisation and alienation to suggest the 'pathologies' that might mark the experience of work in big science.

[A] PRIMERS

Primers are the short pieces of DNA that are used as the initial building blocks for synthetic DNA replication. From these starting points strands of DNA are generated. In that sense, this chapter contains the initial building blocks upon which this thesis is set down. From these foundations spiral threads of illustration and argument. A primer is also the initial coating that is used to prepare a surface for painting. Considered in this way, this chapter is a primer in that it serves as a layer on to which adheres the elaborations that are explored later in this thesis; the discussions of big science and big work, the illustrations drawn from the interview evidence to account for the subjective experience of work at the Institute, and the examination of the way in which these accounts fit into discussions of economies.

A1 INTRODUCTION

This chapter contains three distinct sections which, together, introduce a key idea of the thesis. Section A2, *Sequences of Metaphors*, discusses the way in which the Human Genome Project, and the human genome sequence itself, has been discussed. These discussions have been rich in metaphor and allusion, tending towards a poetic romanticisation of the human genome, and towards casting the Human Genome Project as a history of great men. This thesis is a counterbalance for these kinds of understandings of the Human Genome Project; it was an achievement of the everyday labours of hundreds upon hundreds of 'ordinary' men and women. This thesis, therefore, finds little room for an account that is framed in terms of questing knights and Holy Grails, but nevertheless it is important to discuss these kinds of understandings of the Human Genome Project. The men and women interviewed in the production of this thesis did not live outside these metaphors and allusions. As we see in Chapter G, *The Recruitment of Sentiment*, the way in which the story of sequencing the human genome was told as being one of great men did not leave the imaginations of the research participants untouched. For the men and women interviewed in this thesis, there really were heroes and villains.

The subject of section A3 is clear from the title; *A History of the Human Genome Project*. This section of the chapter draws primarily on the wide range of secondary sources that are available; journalistic, historical and sociological, coupled with accounts from some of the leading figures, to produce a potted chronological narrative of the Human Genome Project. This serves the purpose not only of putting this thesis in the appropriate historical context, but also works as the basis for drawing out the interconnections between the biographical accounts provided by the interview evidence and the larger events of history.

The final section of this chapter foreshadows the discussion of the concluding chapter of this thesis, Chapter J, *The Institute and the Knowledge Economy.* Section A4, *Genomes and Economies* describes the ways in which both the science and the knowledge of the human genome has never been 'pure'. Doing science is always an economic activity, and scientific knowledge is always an economic object. In this, the Human Genome Project and the sequence of the human genome are not unique. A4, *Genomes and Economies* provides examples of the various ways in which the economic nature of the Human Genome Project has been written in to the existing accounts. But it also argues that a key part of the economic existence of the human genome sequence has been written out of these accounts. The Human Genome Project has an economic life lived through the preoccupations of the business pages; speculation, share prices, and state funding. Through the evidence gathered during interviews with the men and women who worked to sequence the human genome, some parts of the essential aspect of economic life of the human genome sequence is revealed; the experience of work itself.

A2 Sequences of Metaphors

Genetics, and the Human Genome Project, are metaphor rich. Genetics has become the culturally big science, replacing the similarly culturally big sciences of atomic physics and information technology. These had occupied the status of the big science in the culture of most of the latter half of the twentieth century. The science fiction cinema of the time is a testimony to the hold these sciences had over the imagination. Cinema, of course, is not the limit of culture, and as with the big sciences that had preceded it, the rise of genetics to being the image of science in the public mind has led to imaginative and creative ways of speaking and writing about this science. As the organisationally and politically big science manifestation of genetics, the Human Genome Project has been the focus of these metaphors and allusions.

The metaphors at work in genetics and genomics have been explored by others. Even biological scientists such as Avise (2001) have realised the power of the language, not only *of* DNA but *about* DNA. Terms such as 'selfish' and 'parasitic' DNA, notions of genes as being 'beads-on-a-string', or of intergenic regions as 'genomic deserts' has shaped the imagination of scientists. But as interesting as these metaphors are, as much as they serve to shape scientific knowledges, it is the metaphors and allusions that are deployed to colour the narrative of *doing* science which are interesting for the purposes of this thesis.

Both the Human Genome Project and the human genome sequence that it produced find themselves spoken of and written about in language that is rich in metaphor, simile and allusion. The Human Genome Project is, according to various accounts, a quest for the Holy Grail (Gilbert, 1992), a search for the Code of Codes (Kelves and Hood, 1992), the writing of the Book of Man (Bodmer and McKie, 1995), the drawing of the Human Blueprint (Shapiro, 1991), and a mission for the Knights of the Double Helix (Davies, 2001). Indeed, Shreeve (2004) described Celera Genomics, the home of the private project to sequence the human genome, as a 'scientific Camelot'. Nelkin (1994) argues that these are promotional metaphors, full of promises, but also attractive looking traps towards which scientists might be lured. It should be noted that Richard Lewontin (2001) thought it noteworthy that a community made up of largely atheist scientists, many of whom had some Jewish background, should choose for itself metaphors that referenced Medieval Christianity. The curious importation of religious themes into a community that often prides itself on scepticism towards the appeal of the spiritual can be said to have reached its peak when we find that, in the year that the draft of the human genome was published, Mauron (2001) wrote an essay for *Science* that asked if the genome was the secular soul.

As Anderson (2002) points out, not only does describing the Human Genome Project as a quest for the Holy Grail lend "the scientific process an air of mystique and authority" (p. 329), but that metaphors such as code, book, and blueprint "lend support to the idea that the scientific enterprise will reveal the ultimate, objective 'truth' about the secrets of life" (p. 330). By evoking imaginings of science such as these, scientists are able to mobilise resources, political and financial, in support of their research. The idea that any scientific endeavour might lead to the ultimate truth is epistemologically flawed, as Glasner and Rothman (2004) point out. Such an assumption is built on extreme flavours of reductionism and biological determinism. Glasner and Rothman argue that the metaphors that might be said to more accurately capture the position of the genome in contemporary science are not the those that allude to the revelation of religious secrets, those that imagine a comprehensive parts list and a definitive set of assembly instructions, or even those that paint the Human Genome Project as a 'spectacular'; biology's 'moon shot'[1]. More appropriate, though much more prosaic, are the metaphors of tool (for example, Sydney Brenner, quoted in Davies, 2001, or Charles Cantor, of the DoE, quoted in Glasner and Rothman, 2004) and infrastructure (Hood and Smith, 1987). In less excited language, the Human Genome Project is thus imagined as a construction project to create the infrastructure of twenty-first century life science. Not a war, which is a concept that we consider shortly, not a quest for enlightenment, nor even heroic exploration, but the collaborative labour of hundreds upon hundreds of people. If

[1] Though the high-profile nature of the Human Genome Project as a 'spectacular' cannot have harmed the relative status of the life sciences in the public and, more importantly, the political minds.

we must borrow any of the existing metaphors, the metaphor of the human genome sequence as an infrastructure for science is the one that best complements the flavour of this thesis.

The feminist writers Rosner and Johnson (1995) note the centrality of 'male' heroes that the narratives and images present in these metaphors prompt; these are men as questing knights, as engineers working on a blueprint, or as explorers mapping an unexplored wilderness. And this is before we consider the ways in which the Human Genome Project has been described as a 'war'; a hyper-masculinalist understanding of the conduct of science. *The Genome War* (Shreeve, 2004) and *The Gene Wars* (Cook-Deegan, 1994), are the titles of two relatively popular audience books on the Human Genome Project. It should be noted that the Cook-Deegan book was published in 1994, several years before Celera Genomics turned sequencing the human genome into a race for priority and property. War, in this case, is therefore used as a metaphor for scientific endeavour, which can only be amplified when the sequencing of the human genome became a competition with the advent of the race in 1998. With a race begun, the war metaphor was provided with justification for some in the scientific community; Wade (2001) relays the story of a Nobel Prize winner comparing Celera Genomic's privatised attempt to sequence the human genome with the annexation of the Czechoslovakia by Nazi Germany. *Casus belli.* This sort of language provided the backdrop for a demand that members of the public project to sequence the human genome should identify themselves with either Chamberlain or Churchill – to appease the private project or to relentlessly oppose it. These metaphors of war, and the imaginings that they stimulate, stress the notion that science is, in its essence, combative and adversarial. These imaginations are stimulated and sustained in the face of the naked fact that the Human Genome Project was the largest collaborative project, by some margin, that there has ever been in the life sciences. Nevertheless, as we shall see in Chapter G *The Recruitment of Big Science*, the image of Celera Genomics as a mortal enemy, particularly as personified by its founder Craig Venter, finds purchase in the imaginations of the men and women interviewed during the production of this thesis.

It was Walter Gilbert who pushed the idea that the sequence of the human genome was the Holy Grail of human genetics. Walter Gilbert was, together with Fred Sanger and Paul Berg, the winner of the 1980 Nobel Prize for Chemistry for work on sequencing DNA. Gilbert began his academic life as a physicist but in the 1960s, after contact with James Watson, he moved into the field of molecular biology (Gilbert, 1980). In a 1985 letter, Gilbert writes; "The total human sequence is the grail of human genetics – all possible information about the human structure is revealed (but not understood)" (quoted in Cook-Deegan, 1994, p. 88). Gilbert maintained this as his metaphor of choice, even titling his contribution to an edited collection on the Human Genome Project *A Vision of the Grail* (Gilbert, 1992). Cook-Deegan (1994) writes of the myth-making metaphor adopted by Gilbert; "The Grail myth conjured an apt image; each of the Knights of the Round Table set off in quest of an object whose shape was indeterminate, whose history was obscure, and whose function was controversial – except that it related somehow to restoring health and virility to the Fisher King, and hence to his kingdom. Each knight took a different path and found a different adventure" (p. 88). Cook-Deegan was suggesting parallels between the Grail mythos and the sequencing of the human genome. This was before the advent of the private project in the form of Celera Genomics. Then, the sequencing the human genome became a race with apparently much more than mere scientific priority at issue. Scientific priority is usually only of concern to the scientists involved and their associates and patrons. When sequencing the human genome became a race between groups representing public and private interests, the ante on the table was raised, with high stakes now measured in morality and politics as well as in repute. This only enriches the mythic tone of the story. Craig Venter, the founder of Celera Genomics, a formerly publicly-funded genome scientist, can then be cast as a brash, impatient Gawain. Or perhaps as Lancelot; a brilliant knight whose story is dominated by a betrayal. He is certainly represented as having heroic qualities. In Chapter G *The Recruitment of Sentiment* we see how the people working at the Institute who were studied in the production of this thesis also imbued their own leader with heroic virtues. His virtues were described as qualities of a type diametrically opposed to those ascribed to Venter.

Against the background of these metaphors and allusions, the story of the Human Genome Project is written as a story of great men. Troubadours sang tales of gallant knights, and the questing scientists found contemporary writers able to produce ballads of heroic science. Some of the lyrics the scientists themselves wrote. It did not matter whether the virtues of the great men in question were their humility and lack of worldly ambition, as articulated in Sulston and Ferry's (2002) accounts of the publicly-funded effort, or were the counter-virtues of ambition and energetic strategising on political and economic planes, as in Shreeve's (2004) and Wade's (2001) sympathetic accounts of Celera Genomics' privatised approach to genome sequencing.

A central idea of this thesis is that the accomplishment of sequencing the human genome was not the result of the quests of knights errant. The story written by this thesis is not that of a noble Galahad and a brash but flawed Gawain competing in their Grail[2] quests, though this is how the Human Genome Project might seem when written through with hyperbole, moralising, improbable promises and a 'great man' view of human achievement. This is the case no matter how journalistic accounts are written, no matter who we choose to cast in these roles or how many knights our story will set on the path to enlightenment. The sequencing of the human genome was not big science because it involved a Round Table of knights who captured the cup of truth and the public imagination in the bargain. Rather, it was big science because, if we must use the metaphor of combat and war, it was achieved by the armies of science. And in this, the story of this war should not be of great generals opposing each other across a map, with individual soldiers aggregated into regimental and divisional symbols to be pushed across that map. Rather, it is the story of those regiments and divisions, which are, when we get inside, characterised as much by cooperation and community as they are by their opposition to the enemy. The argument can be made that, in dealing with the infantry of an army of science, this thesis is not about science simply writ larger, but science that, through its bigness, was qualitatively different to many of the stories of individualised scientific discovery

[2] The Grail metaphor found expression in a number of ways, even in the acronymised name given to the first gene-finding computer programme; GRAIL – Gene Recognition and Assembly Internet Link – which was developed at Oak Ridge National Laboratory (Glasner and Rothman, 2004).

by which it is preceded. This is certainly the case for the life sciences, but other branches of the natural sciences have had their big science manifestations. Some of these are explored in Chapter B, *Anatomies of Big Science*.

A3 A HISTORY OF THE HUMAN GENOME PROJECT

The Human Genome Project was the beneficiary of fortunate symbolic timing, at least when considered from the point of view of a culture that sees significance in years grouped into decades and centuries. The 'rediscovery' of Mendel's work in 1900 provided a kick-start to twentieth century genetics. The structure of DNA, the molecular carrier of genetic information, was discovered and described in 1953. The successful sequencing of the human genome was announced in 2000 (at a press conference) in 2001 (as a draft sequence) and in 2003 (as a finished sequence). Some of the key moments in the history of the science of genetics appear to have been planned, not only to occur at half-century intervals, but at half-century intervals that coincide with the arbitrary, but highly symbolic, division of time into centuries. Keller (2000) noted this when she called the twentieth century the *Century of the Gene*. Indeed, Keller draws a direct comparison between the re-discovery of Mendel's experimental results in three papers published in the *Proceedings of the German Botanical Society* in 1900, with the announcement of the completion of the first draft of the sequence of the human genome at the White House on 26 June 2000. These very different forms of scientific communication are the bookends of the Century of the Gene.

However, despite a near century of development in genetics and molecular biology, which can all be written into the history of genome science, the Human Genome Project began properly in 1990. This section provides a short history of the Human Genome Project, drawn largely from secondary sources such as Davies' *The Sequence* (2001), Cook-Deegan's *The Gene Wars* (1994), Shreeve's *The Genome War* (2004); first-hand accounts such as Sulston and Ferry's *The Common Thread* (2002); as well academic sources such as Balmer (1993), Glasner and Rothman (2004), Clarke and Ticehurst (2006), and Kelves and Hood (1992). Given the fact that the history of the Human Genome Project has enough pieces, each connected by several different threads, to, evidently, fill many books, these few thousand words are a limited account. This section divides the history of the project to sequence the human genome into four parts; pre-1985, which provides a prehistory of projects to sequence the human genome; 1985-1990, which covers the time period from the Santa Cruz meeting organised by Robert Sinsheimer in 1985, to the establishment of the Human Genome Project in 1990; 1990-1998, which accounts for the period during which the public project developed as a big science project without a competitor; and 1998-2003, which covers the founding of Celera Genomics and the beginning of the race, to the announcement of the complete human genome sequence in 2003.

Prehistory (pre-1985)

While the Human Genome Project might have begun in 1990, it did not spring unbidden from the pork barrels of the US Congress, nor was it unanticipated in the scientific community. It was in the late 1970s when some of the principle scientific connections were made that created the possibility of sequencing the human genome. Balmer (1993) argues that key in this was the increasing cross-over between genetic research and studies in molecular biology. "One significant point of contact... was the discovery in the late 1970s of... restriction fragment length polymorphisms (RFLPs)" (p. 6). This discovery suggested that the mapping – note, not the sequencing – of the human genome was a feasible goal. This potential, of using RFLPs as an instrument to systematically map the human genome, was brought to the attention of the wider scientific community in a letter by Solomon and Bodmer published in 1979. Walter Bodmer is a British geneticist who was president of the Human Genome Organisation (HUGO) between 1990 and 1992. HUGO was the co-ordinating body for the international human genome sequencing collaboration. He also co-authored *The Book of Man* (Bodmer and McKie, 1995), a book about the Human Genome Project aimed at a popular audience. The idea of using RFLPs to map the human genome was reinforced when it was stressed in a paper by David Botstein in 1980 (Balmer, 1993). David Botstein was a key figure in the mapping and sequencing of the yeast genome and also served on the National Research Council (NRC) committee that recommended that the Human Genome Project be publicly funded.

A second point of contact was the development of DNA sequencing techniques in the mid-1970s, with Fred Sanger's group at Cambridge (Sanger, Nicklen and Coulson, 1977) and Maxam and Gilbert (1977) at Harvard both publishing their methods in 1977. With genome mapping and DNA sequencing now a technological possibility, the scientific groundwork required in order to build the life science infrastructure that is the sequenced human genome was well along the process of being laid down. Other terrains, particularly those of the internal and external politics of science, were still to be prepared. The ability of some of the key scientists to step outside their sheltered laboratories and move with confidence through the more worldly domains of hard politics and big money was as important in the established of the Human Genome Project as were the technological advances.

During the 1980s these political foundations were laid. Meetings, predictions and propaganda transformed the scientific and political landscape, ploughing a fertile field in which a big science project to sequence the human genome could take root. Two important events took place independently in 1984 that put the idea of sequencing the human genome firmly into the mind of US science politics (Balmer, 1993). First, the University of California Santa Cruz (UCSC) failed in an attempt to set up a genome sequencing institute. Robert Sinsheimer, who was then chancellor of UCSC and a biologist by training, attempted to redirect a donation from the Hoffman Foundation into providing the capital for the first big science biology project. The donation was intended for a big science project, building an optical telescope, but the money was not needed when the telescope was funded in its entirety by the Keck Foundation. Sinsheimer managed to win support from the US National Institute of Health (NIH), but in the end the Hoffman donation was never awarded (Cook-Deegan, 1994). Sinsheimer would still play a key role in launching the Human Genome Project, acting as the catalyst to bring together many scientists who would become leading figures in the sequencing of the human genome.

The second event took place at the very end of 1984. A meeting, sponsored by the US Department of Energy (DoE), took place in December at Alta in Utah. The Alta Summit placed the idea of sequencing the human genome into the imagination of US Federal agencies. Cook-Deegan (1989), writing at the very dawn of the formal, publicly funded project to sequence the human genome, argues that through this meeting wind many historical threads which would be later woven into the fabric of the Human Genome Project. At first glance, it might seem curious that the DoE would be interested in a life science project such as the large-scale sequencing of the human genome. The meeting, however, was held to discuss the detection of mutations in the DNA of the survivors of Hiroshima and Nagasaki. The involvement of the DoE connects the Human Genome Project to a historical antecedent in the Manhattan Project. Both are institutionally big science projects that arise from, are sustained by, and in turn sustain, the culturally big sciences of their time; molecular genetics and atomic physics respectively. Robert Sinsheimer (1992) suggests, in interview, that the involvement of the DoE was one of the factors that pushed the human genome project into being 'big science'. "[T]he DOE is much more hierarchical [than the NIH]. Their programs are from the top down. And they're used to running collaborative big science, with accelerators and nuclear reactors" (p. 69).

That the big science of biology might have become a heavily resourced project controlled by the physicists of the DoE led to unease among biologists. The molecular biologist David Botstein suggested that the plan to sequence the human genome was developed to provide employment for 'unemployed bombmakers' (quoted in Roberts, 2001). The potential cost of a project to sequence the human genome worried biologists; at $3 billion it was a vast amount of money for the life sciences, leading to fears that if the Human Genome Project were to go ahead, funds would be drained from creative small science to feed the demands of monotonous big science. However, as Glasner and Rothman (2004) point out, and as was raised by Human Genome Project advocates such as Walter Bodmer, $3 billion for a 15 year project that would provide the infrastructure for future life sciences was, if not small change, a reasonable sum when compared to the contemporaneous proposals for the Superconducting Supercollider, an $8 billion project, or the International Space Station, a $40 billion project.

A gathering momentum (1985-1990)

So, unemployed bombmakers were seeking a rescue from redundancy by proposing to examine the fine detail of the damage caused by the weapons that they had built. But as we have seen, these were not the only people pushing for the establishment of a big science project to sequence the human genome. Robert Sinsheimer remained an influential proponent of a big science project in biology to rival the big sciences of physics (Roberts, 2001). In 1985, Sinsheimer convened a scientific meeting at UCSC which, according to Cook-Deegan (1994), was "the first meeting focused specifically on sequencing the human genome" (p. 79). Present at that meeting were a significant part of the cast list of the drama of the Human Genome Project. The roll call included Bart Barrell and John Sulston (who later ran the only significant Human Genome Project sequencing centre outside the US; the Sanger Centre), Walter Gilbert (who later attempted to set up a private project to sequence the human genome, and became one the leading promoters of large-scale sequencing), Leroy Hood (who developed automated DNA sequencing), David Botstein, and Robert Waterston (who ran the genome sequencing project at the University of Washington). This meeting is, in many accounts, accorded the status of the origin of the Human Genome Project, which was to officially begin five years later. However, the meeting concluded that sequencing complete genomes of organisms such as humans was not yet feasible. To make projects such as these possible would require significant leaps in technology. These leaps, in 1985, were not yet on the horizon (Cook-Deegan, 1994). Even in 1990, when the Human Genome Project began, there were years of technology development before the sequencing of the human genome could begin at any speed. The length of time required to develop the technologies required for genome sequencing can be seen by the progress that had been made in 1998, eight years after the Human Genome Project had officially begun. Over eight years, about $2 billion had been spent on genome sequencing and 97-98 per cent of the sequence of the human genome still remained to be sequenced (Davies, 2001). That a draft was completed in 2001 and the finished sequence was announced in 2003 tells us of the importance of technological development in the accomplishment of sequencing of the human genome.

The idea of a Human Genome Project was big, politically and scientifically. Objections were not limited to concerns over the effect that a human genome project would have on the funding landscape, as we have already seen. Davies records that Bernard D. Davis and his colleagues at Harvard Medical School wrote a letter of objection, even though they saw the Human Genome Project as 'politically unstoppable'. They wrote; "The magnification is wrong [...] like viewing a painting through a microscope [...] Our fundamental goal is to understand the human genome and its products, and not to sequence the genome because it is there" (quoted in Davies, 2001, p. 30). The Human Genome Project would not just drain the resources of biology, but it would also be bad biology. We revisit and expand on some of these objections in Chapter C, *Pathologies of Work in Big Science.*

Other objections to the Human Genome Project acknowledged the potential power of genomic knowledge, but, pointing to historical precedent, questioned the use to which this power will be put. Among these was Salvador Luria, who was James Watson's PhD supervisor. Luria suggested that a human genome project might be the infrastructure by which a eugenic programme of a 'softer' kind than history's examples would be accomplished (Davies, 2001). Concerns of this kind are among the prompts that resulted in between three and five per cent of funds that were committed to the public genome project being spent on Ethical, Legal and Social Issues (ELSI) programmes (Watson, 2000).

1986 was an important year in the development of the HGP. In March the DoE held a meeting in Santa Fe. By May 1986 Charles DeLisi, who was in charge of Health and Environmental Research at the DoE, had submitted a request for a genome project, to spend up to $4.5 million, to be included in the next year's budget. This request was approved. The genome project was encouraged in Congress as a means to reemploy laboratories in new work that was not linked to the Cold War. The DoE plan was to produce a physical map of the genome and improve the technology before sustained sequencing was to be attempted (Balmer, 1993). This plan, of having a first stage of extensive technological development and refinement, was the essence of the plan used by the Human Genome Project.

Also in 1986, away from the unemployed bombmakers, Renato Dulbecco used an article in *Science* to set out the potential benefits that a human genome map could offer cancer research. In June, a Cold Spring Harbour symposium discussed the feasibility of a Human Genome Project. In his keynote speech Walter Bodmer, who was then head of the Imperial Cancer Research Fund, put forward a string arguments in favour of a project (Balmer, 1993). At this meeting, Walter Gilbert estimated that sequencing the human genome would cost $3 billion. He is reported as theatrically writing the number across the board of the lecture hall; $3,000,000,000 (Davies, 2001). Later that summer, in July, Bodmer chaired a meeting sponsored by the Howard Hughes Medical Institute at the NIH campus in Bethesda, Maryland. This meeting agreed that the first stage of any project to sequence the human genome must be one of technological development. A contest over scientific turf began that would not be resolved until late 1988. James Watson argued for an increased role for the NIH and the National Academy of Sciences in a project. During November and December an NIH working group met to discuss the role of the NIH in a human genome project (Balmer, 1993).

Perhaps most importantly, 1986 was the year in which Leroy Hood and Lloyd Smith (Smith *et al.*, 1986), working at Caltech, developed the first automated DNA sequencer. The following year, in 1987, Applied Biosystems Inc. (ABI) produced the first commercial DNA sequencer, based on the technology developed by Hood and Smith (Glasner and Rothman, 2004).

In 1987, Gilbert attempted to establish a private company to sequence the human genome; Genome Corp. The project never got off the ground. Not because it was 'obscene', though that is how it was described in some quarters of the community of biological science, but because the stock market crash dried up the sources of investment capital that Gilbert had been chasing (Cook-Deegan, 1994; Davies, 2001). In addition, the widely known push for a public human genome project made investors wary of committing capital to a project that was in danger of being made redundant (Cook-Deegan, 1994). This is, ironically, a reverse of the position faced by the public project when Celera Genomics entered the race in 1998. Once Celera Genomics was founded, US legislators questioned the wisdom of continuing funding a project that the private sector had volunteered to take on.

1987 was the first year that big funding was committed specifically to genome research. Early funding for genome sequencing research came from the NRC, who established a $200 million per year genome science programme in 1987, and a more modest $12 million programme established in 1988 by the unemployed bombmakers of the DoE (Davies, 2001). The DoE, though, had committed itself to a $1 billion, seven year project to map and sequence the human genome; it was an attempt to establish the DoE as the leaders of human genome sequencing in the US (Roberts, *et al.*, 2001). In February and March of 1987 James B. Wyngaarden, director of the NIH, put the proposal to fund genome research before Congress, winning approval to allocate $17.2 million of the 1988 budget to a new Office of Human Genome Research, which was established October 1988, to be headed by James Watson. It was in this position that Watson made the decision that three to five per cent of funding would be set aside for ELSI work (Roberts et al. 2001). October 1988 also saw the beginning of collaboration between the DoE and the NIH, after pressure from members of Congress to consolidate their efforts.

In late-1989 the National Center for Human Genome Research was founded under the leadership of James Watson. The Human Genome Project officially began the following year, with a budget of $60 million per year (Davies, 2001). A genome database was established at Johns Hopkins University (Ferry, 2001). The target was to sequence the human genome in 15 years; by 2005.

The Human Genome Project (1990-1998)

By 1991 eight countries had established national programmes (Denmark, France, Germany, Italy, Japan, UK, USA and USSR) and seven were moving towards establishing such a programme (Australia, Canada, Chile, Korea, Netherlands, New Zealand and Sweden). The EC, UNESCO and others proposed international programmes (Balmer, 1993). The Human Genome Organization (HUGO), which had been established in 1988, attempted to manage the international effort. It viewed itself as organising a collaborative 'confederacy' of research programmes, with HUGO acting as the hub, rather than as an overarching, hierarchical centralised controller (Balmer, 1993). In total, twenty laboratories were involved in sequencing the human genome as part of the International Human Genome Sequencing Consortium, with centres in the US, the UK, France, Germany, Japan and China (Newton, 2004). That, in time, the genome project became more centralised and hierarchical on the macro-scale is a development which is mirrored at the micro-level of the genome sequencing institute that is studied in this thesis.

James Watson resigned as head of the National Centre of Human Genome Research in 1992. In a precursor to the some of the most politicised and moralised disputes of human genome science, the cause for Watson's resignation was a dispute over the patenting of genes (Roberts *et al.* 2001); over the status of scientific knowledge and the abstract human body as a commodity. Watson had argued that the US Government should not apply for patents on every gene the project discovered. However, Watson's resignation was also prompted by allegations of a conflict of interest; the New York Times reported that he or his family held shares in companies with a stake in the future of biotechnology, including Amgen, Glaxo, Eli Lilly, Oncogen and Merck (Hilts, 1992). In 1993, Francis Collins replaced James Watson as the director of the National Centre for Human Genome Research. At the press conference, he explicitly compared the project to sequence the human genome to the Apollo missions and the Manhattan Project (Davies, 2001).

The Sanger Centre, the only one of the G5 sequencing laboratories to be located outside the US, was founded in 1992 with support from the Wellcome Trust and the UK Medical Research Council (MRC). Based on former industrial land near Cambridge, it opened in 1993 (Wellcome Trust, 2000). It was named after, and ceremonially opened by, Fred Sanger, the double Nobel Prize winner. Sanger's second Nobel Prize, in 1980, was for developing the techniques for sequencing DNA. John Sulston was the first director of the Sanger Centre. Over the next few years, much of UK and European genome science was attracted to the Sanger Centre, including the establishment of the European Bioinformatics Institute (EBI) at the site and the relocation of the MRC's Human Genome Mapping Project Resource Centre from London (Wellcome Trust, 2000).

Throughout the nineties, the technologies and expertise required to sequence genomes was developed. In 1995, Craig Venter announced that his team at The Institute of Genome Research (TIGR), in collaboration with Hamilton Smith of Johns Hopkins University Medical School in Baltimore, had sequenced the genome of *Hemophilus influenza* (Fleischmann, *et al.*, 1995). This was the first time that the whole genome of an organism other than a virus had been sequenced. Wade (1995) reports that Francis Collins described the sequencing of the

1,830,121 base pair *H. influenza* genome as a 'significant milestone' on the road to sequencing the human genome. The paper in *Science* that announced the sequencing of *H. influenza* had forty[3] named authors, which pointed the way towards the kind of collaborative science by teamwork that was to be genome sequencing.

In February 1996, a meeting was held in Bermuda to establish a protocol for the release of genome data. The 'Bermuda Principles' included the idea that genome sequence data should be made freely available, that sequence assemblies should be released as soon as possible and that finished annotated sequences should be submitted to the database immediately, and that these principles should apply to all public large-scale sequencing centres. The Bermuda Principles were established to encourage research, to coordinate sequencing efforts and to maximise the benefit to society (Glasner and Rothman, 2004).

In 1996, the 12.1 million base pair sequence of the genome of baker's yeast, *Saccharomyces cerevisiae*, was announced. This was the first genome of a eukaryotic organism to be sequenced. In 1998, the 97 million base pair sequence of the nematode worm, *Caenorhabditis elegans*, was completed by a collaboration between the Washington University sequencing centre and the Sanger Centre in the UK (The *C. elegans* Sequencing Consortium, 1998). The named author of the paper that announced this achievement was the collective collaboration; to find the names of the scientist involved one had to go to the internet to web-based lists of individual authors. Nevertheless, despite this lack of individualised acclaim through authorship, thought to be a key component of the reward structure of science, the image of science dramatised in the questing knight narratives is reproduced in scientific journals. In the same issue of *Science* in which The *C. elegans* Sequencing Consortium announced the sequence of the worm, a brief article by a group of scientists singles out the leaders of the Washington University group and the Sanger Centre, Waterston and Sulston, and describes them as 'visionaries' (Hodgkin, Horvitz, Jasny and Kimble, 1998). The

[3] It is interesting to note that of the forty authors named on this paper, eighteen of them had female forenames. It is an open question whether the increasingly collaborative scientific work that characterises big(er) science generates greater gender parity than is achieved in the traditional, individualised mode of scientific labour.

individualised system of reward through esteem is reconstituted outside the authorship of journals. *C. elegans* was the first multicellular eukaryote to be sequenced; next step, the human genome.

It is estimated that, by 1998, $2 billion had been invested in the public Human Genome Project (Glasner and Rothman, 2004). Yet only two to three per cent of the genome had been sequenced. The costs of sequencing remained high in some centres, up to $10 per base (Davies, 2001). For comparison, by 2002, as the human genome sequence moved from draft status to finished status, the cost of sequencing at the Sanger Centre was 2p per base (Powell, 2006). In 1998, the founding of Celera Genomics, and the cost of the public project, led some to believe that the public effort was devoting resources in the pursuit of winning a race that was not worth winning.

Finishing the race (1998-2003)

The race to sequence the human genome began in 1998. Craig Venter left TIGR, which at one point was the largest DNA sequencing institute in the world, to head a company bankrolled, with $300 million, by Perkin Elmer (Davies, 2001). Perkin Elmer is the parent company of Applied Biosystems (ABI), which is, in turn, the leading manufacturer of automated sequencing machines. Venter was not a stranger to research with a commercial backing. While TIGR, established in 1992, was a non-profit research centre, it was privately funded. A sister company, Human Genome Sciences (HGS) was established to exploit the sequence data and discoveries that would flow from the research of TIGR (Davies, 2001). TIGR straddled the public-private divide, or, as a commentator quoted by Fortun has it, "Venter had one foot in the world of pure science and one foot in a bucket of money" (Fortun, 2006, p. 29).

Celera Genomics takes its name from the Latin *celer*, meaning swift, as found in words such as ac*celer*ate. At its launch in 1998 Venter boasted that Celera would be sequencing as many bases each month as there were currently stored in Genbank, the public genome database (Shreeve, 2004). According to Shreeve, Venter had agreed to lead this privately-funded human genome sequencing project with the belief that the business model would function, not on the privatisation of the genome sequence itself, which would be available to all, but on the principle of patenting key genes for subsequent exploitation. This did not seem to be a recipe for financial success. "The academic were afraid Venter was hiding something. The market analysts were *hoping* he was" (p. 119).

The establishment of the race and the end of the race threatened to be the same moment for the public project. When placed in the political landscape of the US, the very presence of a private project to sequence the human genome was an argument to end the flow of funding to the public project. Celera Genomics would win the race, not by producing the genome any quicker, but by draining the political will of the US Congress. Countering this threat was one of the motives behind the push to publish a draft sequence of the human genome by 2001, with the gaps to be filled in later (Shreeve, 2004). Great efforts were made by Francis Collins and James Watson in the US, and John Sulston in the UK, to secure the financial future of the Human Genome Project.

As Shreeve (2004) writes; "If something wasn't done, the human code of life would be subject to the control of a single corporation. God's language, in the mouth of Mammon" (p. 124). Of course, the interests of capital are not homogenous. The conduct of a fiercely fought race could be as profitable as actually winning the race. As long as the race was being run, Perkin Elmer, who provided the capital for Celera Genomics, would find that the demand for their ABI sequencing machines would remain high, with their customers being the most creditworthy of clients; state-funded organisations with a moral mission. The *Chicago Tribune* remarked on this strategy, describing Celera Genomics as "a sales story for the ages" (quoted in Fortun, 2006).

The public project to sequence the human genome responded to the challenge of Celera Genomics. According to Fortun (2006), John Sulston, head of the Sanger Centre in the UK, persuaded the Wellcome Trust to 'dramatically' raise its funding for human genome sequencing to £110 million over seven years. This represented a doubling of the financial commitment to the Sanger Centre, and a doubling of the Sanger Centre's commitment to sequencing. It would now sequence one-third of the human genome rather than one-sixth (Wellcome Trust, 2000). Francis Collins, the director of the National Human Genome Research Institute in the US, and James Watson, the previous director, secured increased funding for their genome sequencing centres (Fortun, 2006). The advent of the race prompted the rationalisation of the public project, with funding and responsibility for genome sequencing concentrated at a few large-scale laboratories. The task of sequencing the human genome was increasingly concentrated in the G5 group of laboratories; the Washington University group led by Bob Waterston, the Baylor College of Medicine group led by Richard Gibbs, the DoE Joint Genome Institute, led by Elbert Branscombe, the Whitehead Institute at MIT, led by Eric Lander, and, outside the US, the Wellcome Trust funded Sanger Centre led by John Sulston (Glasner and Rothman, 2004). This was not necessarily a welcome move, even on the part of some of those who benefited from this centralisation (Sulston and Ferry, 2002). The idea of international cooperation, of a universal community of science, were dented as the public project explicitly reorganised itself and concentrated its activities within the national borders of the two main funders, and even here the degree of 'internationalism' was limited to just one of the five sequencing centres being located outside the US. As we know, this was in the UK, a state that is politically closely aligned to, and shares a common language with, the US.

Fortun (2006) suggests that the images of the race as "public *versus* private science, sharing *versus* patenting genomic data, smart science *versus* dumb technology, British noblesse oblige *versus* ruthless American capitalism" (p. 27) can be turned on their heads, particularly the final two. This was not the mighty, corporately-resourced Celera Genomics racing against a small, under-funded academic laboratory, but rather Celera Genomics was, for all its corporate might, racing against "the entire state-supported genomics communities of the Western industrialized nations" (p. 27).

The image of Celera Genomics as the plucky underdogs might seem strange to British imaginations, who are accustomed to state-funded pursuit of public goods. Here, we are more ready to accept the image of Craig Venter as, put mildly, a practitioner of the enclosure of the genetic common wealth. In the US, the ideological climate is more likely to see such activities as the laudable taming of nature; the term 'gold rush', for example, was often used with little regard for the negative connotations. This is explored in section A4, *Genomes and Economies.* As such, that it might be possible for Celera Genomics to 'win' the human genome sequencing race without actually sequencing the human genome was not an unimaginable possibility. With a promise to sequence the human genome for $300 million, and with the public project having consumed $2 billion for a return of just two to three per cent of the human genome sequence, a case could be made that there was no further need for government funding. Glasner and Rothman (2004) report that the public project was being presented "as clumsy and bureaucratic, and no match for fleet footed, efficient, entrepreneurial private sector" (p. 40). If this line of thinking had won out, Celera Genomics would have won the race by default, though, it must be said, the sales of ABI sequencers would not have been so high.

But if there were differences in the public representation of the organisation of the public and private projects to sequence the human genome, there were also differences in the scientific techniques used by the two competitors. The public project used a technique called hierarchical shotgun sequencing, while Celera Genomics used a technique called whole genome shotgun sequencing. The *Harvard Business Review* published one of the most concise, and concomitantly illuminating, comparisons between the different genome sequencing methods employed by the public project and Celera's effort. The public project "is akin to having several teams laying bricks until various walls come together in a coherent structure." The whole genome shotgun method of sequencing employed by Celera is, by comparison, "like using a computer to assemble a 70-million-piece 3-D jigsaw puzzle" (Enriquez and Goldberg, 2000, p. 98). While organisationally Celera Genomics might have been the swift and efficient model of private enterprise, the whole genome shotgun strategy is a brute force approach. Simply put, whole genome shotgun sequencing involves decomposing the entire genome of an organism into fragments which are then sequenced and, using a supercomputer, the sequence of the genome is assembled. Hierarchical shotgun sequencing involves breaking the genome into parts which the researchers know will overlap and cover the genome. These large fragments of the genome are then cloned; in the public project artificial bacterial chromosomes (BACs) were used.

These relatively large parts are then broken up into smaller parts that can be sequenced. The sequence of each BAC is then assembled, which can then be combined to provide the sequence of each human chromosome and, eventually, the whole genome. Immediately prior to the launch of Celera Genomics and the beginning of the race there was an extensive debate in the scientific press (Waterston, Lander and Sulston, 2002). Hierarchical shotgun sequencing requires a greater amount of preparatory work; preparing a library of BACs that is spread across each chromosome. However, while whole genome shotgun sequencing avoids this preparatory work, questions were asked about the ability of this technique to achieve coverage of the full genome and to assemble the whole genome correctly.

The race reached a suitably symbolic end on 26th June 2000, when Francis Collins of the NHGRI and Craig Venter of Celera Genomics flanked President Clinton at a press conference, which Prime Minister Blair joined via a videolink, to announce the sequencing of the human genome (Fortun, 2006). Publication, the customary form of scientific communication, did not occur until February 2001, when the public project (International Human Genome Sequencing Consortium, 2001) and the private project (Venter, et al., 2001) simultaneously published their reports of the draft sequence in the two leading journals, *Nature* and *Science*. After the publication of the draft sequences there was a debate in the scientific press over the merits of the two sequences (see, for example, Waterston, Lander and Sulston, 2002; a reply by Myers *et al.*, 2002; and a commentary supporting the Waterston paper by Green, 2002). A principle point of dispute revolved around the fact that the paper published by the team at Celera Genomics was able to incorporate the data released into the public domain by the International Human Genome Sequencing Consortium, as required by the Bermuda Principles. As the Celera Genomics sequence data was not released to the public domain before publication, the public project was always at a disadvantage. The more that they sequenced, the more sequence data Celera Genomics had to use. That the race finished in a draw arranged by agreement did not settle the waters of scientific and political acrimony.

The finished sequence was not announced until 2003, with a publication in *Nature* timed to coincide with the fiftieth anniversary of the discovery of the structure of DNA. 99 per cent of the gene-containing areas of the genome were sequenced, with an accuracy of 99.99 per cent claimed. Some areas of the genome remain difficult to sequence, such as areas of high repeats, such as telomeres, or areas that are structurally difficult to sequence, such as centromeres. The press releases that accompanied the announcement of the finished sequence celebrated the fact that the project was finished ahead of schedule, taking thirteen rather than fifteen years, and within budget (National Human Genome Research Institute, 2003; Wellcome Trust, 2003).

This thesis, though, is not about the 'big' history of the Human Genome Project. It is not about negotiations at which vast resources were allocated; in the US Congress, in corporate boardroom meetings, and at meetings of the trustees of charitable organisations. To some degree, those histories have already been told. This thesis is an attempt to do what Mills (1959 [1970]) described as the task of sociology. Mills argued that sociology is the means by which people are able to "grasp what is going on in the world, and to understand what is happening in themselves as minute points in the intersections of biography and history" (p. 14). The history provided in this section provides a primer for this thesis, not because this thesis is a continuation of the discussion of the sorts of power and politics that are written through existing historical accounts, but because these accounts provide a background against which to understand the experiences of those who worked to sequence the human genome.

A4 Genomes and Economies

The Human Genome Project and the sequence of the human genome were never objects without an economic life. Consider, first, the economic input required to sequence a genome – inputs that pushed biology into the category of big science. Glasner and Rothman (2004) produce tables that show that the funding from the US government amounted to almost $3.4 billion over the period 1988-2002, while the second largest individual funder of the public project, the Wellcome Trust, which supported the Sanger Centre in the UK, contributed just over $286 million dollars in the budgets of 1998, 1999 and 2000. Over the same three year period total global funding for public genome research was nearly $3.7 billion, though it is not clear what proportion of this was devoted to sequencing the human genome. It is clear, though, that the Human Genome Project was a significant economic object, and big science, by virtue of the sheer scale of resources that it was allocated.

Shreeve (2004) illustrates the economic scale of the projects to sequence the human genome with an anecdote from a meeting to discuss Celera Genomics' budget. At this meeting, it was pointed out that the cost of pipette tips alone was expected to be $14,000 each day. Scale such as this was one of the pushes towards the development of robotic systems, in this case a system that did not use the disposable pipette tips that are a ubiquitous feature of the contemporary life science laboratory. Unlike previous big science projects such as particle accelerators, gravity wave detectors, or missions to the moon, the bigness of sequencing the human genome did not manifest itself in correspondingly spectacular buildings or vehicles. Rather, its bigness was a result of the mundane expenses of life science laboratories magnified many fold.

Despite this lack of the spectacular, the economic life of the human genome is not lived covertly. The economic life of the human genome is explicit. Among the frequently asked questions addressed by the website of the National Human Genome Research Institute is that of the cost of the Project to 'US taxpayers'. The hypothetical questioner is reassured; the Project came in under budget, at less than $3 billion, but this is not the limit of the economic life of the human genome. The rest of the answer reads; 'It is also important to consider that the Human Genome Project will likely pay for itself many times over on an economic basis - if one considers that genome-based research will play an important role in seeding biotechnology and drug development industries, not to mention improvements in human health" (National Human Genome Research Institute, 2005).

The Human Genome Project did not transform genetics from a science touched only lightly by economics, cloistered in ivory towers. Indeed, even studies of genetics that were conducted by literally cloistered scientists have had profound connections to economies. In their short note on the economic implications of the Human Genome Project, Elrod-Erickson and Ford (2000) write that the work of the monk-scientist Mendel "did not receive much commercial recognition" at the time, but that "[s]ince then, its impact has spread throughout the horticultural and agribusiness sectors of the world economy; and virtually all major commercial food crops and other agricultural and forestry products have either been genetically modified or selected to enhance their yields, resistance to pests and disease, [or] consumer acceptance" (p. 57).

Elrod-Erickson and Ford (2000) and other authors predict that the Human Genome Project, and the studies in genetics that it inspires, will have profound economic effects. It will have an "impact on the medical and pharmaceutical sectors, the agribusiness sector, defense industries, the insurance industry, the behavioural sciences, the labour force, and on the structure of public and private welfare and retirement provisions" (p. 60). This is not an unusual opinion (see the FAQ statement from the National Human Genome Research Institute above), and, indeed, there are many livelihoods built on predictions of this kind. The prediction does not need to come true, it merely needs to continue to be believed for these livelihoods to be secure. Others are not so sure. Hopkins *et al.* (2007) and Nightingale and Martin (2004), for example, have argued that 'the biotech revolution' is a myth.

The economic life of genome sequencing was also one that had an effect on the surrounding terrain of the economics of biology. Many scientists worried that the big science of genome sequencing would draw all the resources allocated to biology to itself, starving small science of its lifeblood. But no science is an island, and the economic life of the human genome had other dimensions.

If we return, for a moment, to the metaphors that are deployed to explain and dramatise genetics and the Human Genome Project, we need go no further than that of a biotechnology 'gold rush'. The idea of a 'gold rush' is found in publications from *Newsweek* (Bryant and Beals, 2000) to *Nature Neuroscience* (Mombaerts, 1999). This imagining of the economy of genomes is one of masculine, pioneering spirit, of lone fortune-seekers expanding the boundaries of 'civilisation' deeper into the wilderness. It is not one that accords with the reality of multi-national corporations, marketing departments, and multi-disciplinary research teams. The metaphor was used, unprompted, by the most senior scientist interviewed in the production of this thesis:

> [...] biology has very quickly moved into a Klondike period, a gold rush.
> [Professor Ingham]

It has to be noted that Professor Ingham was aware that, as with examples from history, gold rushes are events that ruin as many as, or more than, they make wealthy, with the value of plots, mining equipment and expertise rising far above the returns that can be expected in all but the most fortunate of cases. The fortunes made in gold rushes were often not made in mining gold, but rather in selling the equipment and supplies for mining gold. In this, perhaps 'gold rush' can help us to understand some of the most successful biotech companies. Applied Biosystems (ABI) supplied sequencing machines both to the public Human Genome Project and to the private project which was bankrolled by their parent company. The shovels and picks used when prospecting and mining for gold in DNA are far more sophisticated, and correspondingly more expensive.

Gold rushes also involve bubbles. The NASDAQ slump of 2000, an event Professor Ingham noted in the interview, demonstrated the unintended value of this metaphor in helping us understand biotechnology. 'Gold rush', then, might be a metaphor with some use, if we forget for a moment the image of the mythologised, pioneering '49er. Instead, we should remember the less laudable, but far more real, aspects of gold rushes. Price bubbles, fortunes spent on worthless plots and the latest equipment, and battles for the formalisation of property rights, including the enclosure of property to which others have strong claims. This interpretation did not pass unseen, despite the efforts of the biotechnology boosters to write 'gold rush' to read 'excitement and easy riches'. As Fortun (2006) noted, terms such as 'gold rush' and 'land grab' were also used by the scientific critics of Celera Genomics' attempt to commodify the human genome sequence.

The Human Genome Project was expressly intended to provide impetus to this gold rush. Fortun (2006) quotes Jack McConnell of Johnson & Johnson as arguing that the Human Genome Project was a fundamental and necessary part of maintaining US dominance of the pharmaceutical and biotechnology industries. It is important, perhaps, to note here that national boundaries do not lose their significance either in the 'universal' community of science or in the world of a 'weightless' knowledge economy.

Many (Bodmer and McKie, 1995; Russo, 2003) trace the advent of the biotechnology industry to the founding of Genentech by the geneticist Herbert Boyer and the venture capitalist (with a background in biochemistry) Robert Swanson in 1976. Boyer, with Stanley Cohen, invented recombinant DNA technology. Thus, one of the men who was a key figure in the development of the new genetics was also, within just a few years of his groundbreaking scientific work, leading the industrialisation and commercialisation of the knowledge and technologies that he made possible. At the birth of the Human Genome Project, in 1990, Genentech was taken over by Hoffman-La Roche, who acquired 60 per cent of the company for $2.1 billion. Boyer's initial stake was funded by a $500 loan (Bodmer and McKie, 1995).

Richard Lewontin (2001) described contemporary molecular biology as being a discipline in which all the prominent scientists were invested in the success of biotechnology businesses. An example of this played a part in precipitating the controversial resignation of James Watson from the position as director of the

National Center for Human Genome Research. His large private stock portfolio included investments in a variety of biotechnology and pharmaceutical companies, creating, for some, the impression of a conflict of interest (Davies, 2001). Walter Gilbert, the scientist who theatrically wrote the estimated $3 billion cost of a human genome project on the board during the 1986 Cold Spring Harbour meeting, was a founder of Biogen[4] in 1978, and was appointed CEO in 1982. The new genetics, and the Human Genome Project, have always been economic entities.

The failure of Genome Corp. to achieve the necessary capitalisation over 1987-1988 as a consequence, in a large part, of the stock market crash of October 1987 (Cook-Deegan, 1994; Davies, 2001) was an early demonstration that genome science was not only an economic entity tied into the largely unobserved circuits of the academic economy, but one tied into international flows of capital. Genome Corp., it should be noted, was also to be an entity with avowedly economic consequences. Not merely in that it was to be a profit-making organisation, a near-ubiquitous feature of economic life in a capitalist economy, but in that it promised to increase the efficiency of the life sciences by introducing a new and superior division of labour. Walter Gilbert is quoted as arguing that a private genome sequencing project would be the same kind of change in the life sciences as the moves from scientists making their own restriction enzymes or blowing their own glassware to routinely buying these items from a catalogue (Cook-Deegan, 1994). A genome project, developed as a resource for the academic community would, in this way, transform the essential economics, the division of labour, of the life sciences. To the extent that the success of Venter was seen as the triumph of economics and management, and not that of science and discovery, we can look to Leroy Hood's opinion; "He has never invented anything [...] The only thing he deserves credit for is scaling up the process" (quoted in Davies, 2004, p. 66).

[4] Biogen was a financial failure. This may have been a factor, along with the stock market crash of 1988, in Gilbert's failure to raise venture capital for a private effort to sequence the human genome.

We have seen how private capital saw possibilities for profit in genome sequences. This interest, of course, predates Perkin Elmer's $300 investment in Celera Genomics. Craig Venter, as we have noted, was already a scientist with close ties to the commercial exploitation of genome sequence, with the non-profit TIGR inextricably tied to a commercial sister company, HGS. To understand the scale of economic interest in the genome sequence, we need only point to the $125 million that SmithKline-Beecham paid for seven per cent of HGS in 1993, less than a year after its establishment (Davies, 2001). That price valued HGS at just under $1.8 billion. At the time, scientific opinion was that we were decades from a complete sequence of the human genome. With this investment, SmithKline-Beecham was buying a share of the exploitation of the future genome sequence produced by TIGR. For capital, the very promise of the genome sequences as property far outweighed the deterrent of a lack of existing assets, either material or informational.

As Fortun (2006) notes, one of the explicit rationales for establishing a public Human Genome Project was to create the infrastructure for the growth of the biotechnology and pharmaceutical industries and, inevitably, the commodification of various aspects of living things. Craig Venter and Celera Genomics were building a biotechnology corporation that would profit from the building of this infrastructure. It follows, therefore, that "[i]n a very real sense… Venter's history is not so much of competing with the HGP, but of extending and intensifying some of the original rationales of the HGP" (Fortun, 2006, p. 28). The centrality of the human genome to the developing 'knowledge economies' of the industrialised world can be seen by the effect of the creation of particular forms of property. When President Clinton announced that the human genome sequence could not be patented – a move that supported the public project and, at least in the perceptions of investors, damaged the business model of Celera Genomics – the NASDAQ, on which many biotechnology shares are traded, lost approximately $50 billion in market capitalisation in two days. Celera Genomics lost nearly 20 per cent of its value in this time (Fortun, 2006).

This was an overreaction. The patenting of the human genome might not have been possible, but it remained possible to transform human genes into private property. The *Harvard Business Review* reports that, by 2000, the year that Clinton made his announcement, Human Genome Sciences had been granted patents on 106 human genes, and was in the process of acquiring patents on thousands more (Enriquez and Goldberg, 2000).

Nonetheless, the sequence of the human genome is a commodity, even if it is not a commodity enclosed by property law in the service of particular owners. Commodities "are themselves the products of social activity, the result of expended human energy, *materialized labour.* As objectification of social labour, all commodities are crystallisations of the same substance" (Marx, 1859 [1999]). By contrast to the scientific paper – the output of small science – in which the product of labour bares the specific social imprint of its production, sequence data is universalised, standardised and abstracted.

But all this economic life is life as observed by the business pages of the major newspapers, by the *Financial Times*, characterised by stocks and shares, flows of state funding, mergers and acquisitions. The essence of economic life remains largely invisible; rather, the products of this life are represented as the entirety. Work and labour are invisible. This thesis considers the economic life of the sequence and sequencing of the human genome at its most fundamental level, at the level of the labour necessary to produce it. As Marx argued in *A Contribution to the Critique of Political Economy* (1859 [1999]), it is labour, and the social relations of production, that underpin everything else. This thesis addresses the questions; How was the industrialisation of the practice of biology experienced at the level of the bench? To what degree does this accord with other examples of big science? To what degree does this accord with other examples of industrialisation?

Through Chapter B, *Anatomies of Big Science*, and Chapter C, *Pathologies of Work in Big Science*, we explore the examples offered to us by other studies of big science. Chapter C, *Pathologies of Work in Big Science*, necessarily touches on sociological concerns with industrialisation of work on general, not just the industrialisation of science. Chapter F, through to Chapter I, address the bulk of the fieldwork evidence. While Chapter F, *The Factory and the University*, Chapter G, *The Recruitment of Sentiment*, and Chapter I, *Interlude: Dis-Engagement and Dis-Integration*, describe the way in which work at the Institute was experienced by those who produced the human genome sequence, Chapter H, *The High Road to the Human Genome*, connects these experiences to some of the current debates regarding work in the economies of the early twenty-first century. Chapter J, *The Institute and the Knowledge Economy*, places this fieldwork in a wider context, discussing the place of the Institute as a de-monstration in discussions about work in 'post-industrial' economies.

We return to questions of economy as seen by the *Financial Times* in Chapter J, *The Institute and the Knowledge Economy*. For now, we turn our attention to the question of big science itself; what are the precedents for big science such as the Human Genome Project, and how can these help us to understand the accomplishment of sequencing the human genome?

[B] Anatomies of Big Science

Anatomy is the science of the structure of living things. The word derives from the Ancient Greek *temnein*, which means to cut, or to divide. Using the analogy of a microscope, we consider the 'bigness' of science at varying degrees of resolution, slicing the term 'big science' into a collection of distinct but interrelated parts. Where student anatomists would have studied the illustrated plates of the classic texts to inform their own studies, here we take as our illustrations the studies of other big sciences. As such, as a selective review of the literature, this chapter is also in the tradition of anatomists; it is working with the bodies that have provided to us by others. Before we turn our attention to the body of the Institute, we anticipate the features that we might find.

B1 Introduction

This chapter considers the meaning and appropriate use of the term 'big science'. We visit the work of sociologists, such as Collins (2003) and philosophers, in particular Ravetz (1971 [1973]), and the reflective accounts produced by those within the scientific culture, especially Weinberg (1967) and Ziman (1984). From these sources, section B2, *A Drama of Scale*, describes the distinctive features of big science, considered at various levels of resolution. Section B3, *A Case Study in Big Science: LIGO*, makes use of an existing qualitative case study (Collins, 2003) to explore some of the features of big science organisations. This brings the narrowing of focus, begun in section B2, *A Drama of Scale*, down to the level of the laboratory. Adopting this level of resolution for the study of big science is discussed in section B4, *Big Science as a Quality of Laboratory Life.* The necessary contrast of big science with the concept of small science is examined in section B5, *Small Science*.

In arriving at the suitable level of resolution for an exploration of the experience of working at the Institute on the sequencing of the human genome we are reminded that science is a form of work. Section B6, *Understanding Work in Big Science Projects*, explores the distinctive features of big science as work. This is necessary before we can understand the intersection of the experiences of the research participants with the wider features of history and economy. Big science is discussed with reference to the Human Genome Project and the Institute in section B7, *Big Science and Biology*. This section draws on the accounts of leading participants in the Human Genome Project such as John Sulston (Sulston and Ferry, 2002) and Francis Collins, Michael Morgan and Aristides Patrinos (2003). Throughout this chapter, objections to and anxieties with the development of big science are touched upon as they are raised by the literature. However, this chapter avoids detailed discussion of what we might call the 'pathologies' of big science. The pathologies of work in big science that are particularly relevant to the level of sociological resolution that this thesis adopts are discussed in greater depth in Chapter C, *Pathologies of Work in Big Science*.

However, we should bear in mind the passage that Hevly (1992) wrote in the afterword to a collection of papers and essays on the subject of big science. "[E]ven after hundreds of pages of text, "big science" itself remains an elusive term" (p. 355). Indeed, as science can be big in a number of senses; geographic, economic, multi-disciplinary and multinational (Galison, 1992), or; large in scale, broad in scope or great in significance (Capshew and Rader, 1992), these few thousand words can only claim to offer sociological orientation, not unambiguous definition. One of the principle aims of this chapter is to disentangle the different aspects by which science can be described as being big, and determine which of these understandings best fits the example of the Human Genome Project and thus suits the focus of this thesis.

B2 A Drama of Scale

Capshew and Rader (1992) suggest that 'the drama of scale' is at the heart of science. They quote Latour; "The small and invisible are made large, the large and unencompassable are made small. The fast are made slow and the slow are speeded up" (p. 18). They argue that, as students of science, we recreate this drama of scale anew in our research. The idea of the dramas of scale is one that we can actively engage with as we anatomise big science. We can use a scientific metaphor that perfectly illustrates science as the human manipulation of the scale of nature; the microscope, an instrument that is used in the study of anatomy. If we imagine that it is possible to examine big science through a microscope, one of our choices is the degree of magnification to select. When we study big science through the least powerful lens, we see can examine the features of big science that are those of science as a whole.

Big science as a quality of the community of science

Indeed, big science first enters the literature of science studies as a characteristic of science as a whole. The term big science was brought into the common usage by Price in his book *Little Science, Big Science* (1963). Price approached the concept of big science from a scientometric perspective, arguing that the scientific literature, the number of scientific workers and the amount of money allocated to science funding grew at an exponential rate. For Price, the 'first law' of scientific growth is that science has grown exponentially for the past three hundred years, with a doubling in size every fifteen years. Caphew and Rader (1992) write that for Price, "while this growth might be correlated with the appearance of bigger equipment and larger research groups, these were incidental rather than defining characteristics of Big Science" (p. 7).

For Price (1963), it is the community of science, rather than any particular science, project or laboratory, which is the social object that is big. We might rephrase this to say that the cultural form of life that is science is the entity that has been, and is, expanding. This is a definition of big science which has sociological utility, both within the culture of science and beyond. Price suggested several consequences that might arise from the exponential growth of science. Given that Price adopted a scientometric focus, it is unsurprising that at the heart of these concerns are questions of effective scientific communication. Are the methods and processes developed in an age of small science still effective after the total mass of scientific information being produced has grown to the levels seen in this age of big science? If the answer to this question is 'no', we might find in the development of the internet the means of scientific communication that are appropriate to a big science culture. The internet facilitates big science as a quality of the culture of science as a whole by allowing near instantaneous communication via e-mail, especially e-mail lists. The internet allows a scientist to negotiate the ever-proliferating collection of journals by providing sophisticated search tools and electronic versions of print publications. And the internet allows the sharing of raw data and unpublished studies; in genomics databases such as Genbank are essential parts of the structure of scientific communication.

Complementing the scientometric analysis of the development of big science produced by Price (1963), Alvin Weinberg (1967) offered a reflective, philosophical account of a life in science as it grows. A leading physicist and director of Oak Ridge National Laboratory, Weinberg also considered the problems that resulted from the growth of the whole community of science to the proportions of big science. Perhaps naturally enough, given his position in the management of a large research laboratory, he also focused much of his concern on issues of scientific communication. Some of these, such as the problem of document retrieval and the development of indices, are problems that have been largely dealt with – or at least have been transformed into unrecognisably different problems – through the developments in information technology described above. Other problems of communication in big science that were identified by Weinberg have found no such technological fix. Weinberg argues that as science grows it fragments into increasingly differentiated fields of knowledge and practice. However, these fields of knowledge still draw on, and even depend on, the knowledges of 'neighbouring' fields. For this reason, the danger of a breakdown in communication between fields is a "cause of deep concern" (p. 43). Furthermore, as science fragments there is the danger that fields that do not communicate may become inconsistent with each other. Where fields do reintegrate, he argues, it tends to be at a higher level of abstraction. In doing so we lose resolution, we omit something, argues Weinberg. He writes that "knowing *in principle* is not the same as knowing" (p. 44).

The value of the reflective account of big science offered by Weinberg (1967) does not stop at the scale of the community of science. His understanding of the practice of science within disciplines, institutions and laboratories continue to be of service as we further our discussion of big science. Though this chapter considers big science at different levels of resolution, these different viewpoints are not independent of each other; the structures visible at one level of magnification are also visible at others, though at these other levels of magnification the appearance of those structures might be very different.

Big science, where the bigness is understood as a quality of the community of science as a whole, also has effects beyond the boundaries of science. The transformation of contemporary societies into ones in which questions of science are active in decisions that were once the preserve of politicians, is a challenge to the scope of democracy. The problems of decision-making and democracy in a society dominated by science have been discussed by writers such as Shackley, Wynne and Waterton (1996) and Funtowicz and Ravetz (1993). These problems can be understood as a consequence of the general growth of science, both in terms of its power to transform the world and its cultural dominance. Of course, questions regarding the governance of science and technology, and the incorporation into contemporary democracies of these products of human labour, which often seem to have an autonomous, independent existence, are questions that are resolved largely at national levels of resolution.

Big science as a quality of national science

The community of science prides itself on the degree to which it, of all human endeavours, is international and universal. Universalism is one of Merton's (1942 [1973]) norms of science. But science is tied into the fabric of economics and politics. Never more so than in the case of big science, as we have seen in the case of the Human Genome Project in Chapter A, *Primers*. If we increase the level of magnification of our social microscope, we move to a level of scale smaller than that of the global community of science. We find that we are looking at big science as a characteristic of national science.

Sharon Traweek (1992) summarises the literature examining the development of national scientific communities. She argues that most analyses of big science ignore the necessity of a national infrastructure. This infrastructure, she writes, consists of: "(1) sustained funding for education and research at all levels, from elementary schools to national laboratories, (2) a certain proportion of the country's gross national project (GNP) allocated regularly for scientific work, (3) a certain proportion of the country's population engaged in scientific work, and (4) scientists engaging in a high level of information exchange and documentation about their work" (p. 104). We might add to this list a public imagination for [big] science. Galison (1992) identifies the "cultural fascination of Americans in general for the large" and "depression-era delight in gigantism" (p. 3) as an important source of support for big science in the case of the United States. Without the national infrastructure to support it, big science is an impossibility. Investment in such as infrastructure can be justified on bases beyond a public delight in gigantism. In questions of the development of a 'knowledge economy', a concept not unconnected to the project to sequence the human genome, the development and expansion of an educational and research infrastructure is seen as a prerequisite for economic success. The development of a knowledge economy, and its relationship to work at the Institute is explored in Chapter J, *The Institute and the Knowledge Economy.* The echoes of a linear model connecting science to national growth, as set out by Vannevar Bush (1945 [2007]) are clear to see. A study of big science at the national level would, therefore, focus on the development and maintenance, materially, socially and politically, of scientific infrastructure.

Big science as a quality of a scientific discipline

At a similar level of metaphorical magnification, but with the application of different interpretative filters, we find that we can look at big science as being a characteristic of a scientific discipline. This takes the form of a public and political fascination with a discipline. An example might be drawn from a short story by Kafka that we return to when discussing the Human Genome Project. In *The Great Wall of China* (1931 [2005]), Kafka writes that; "Fifty years before the building [of the Great Wall] was begun... architecture, and masonry in particular, had been declared the most important branch of knowledge, all others being recognized only in so far as they had some connection with it" (p. 3). In Kafka's fictional account of the construction of the Great Wall of China, without the nurturing of the public fascination with a particular branch of knowledge, such an enormous technological and engineering project would have been impossible. And the majority of big science projects are, simultaneously, big technology and engineering projects. Consider the building of the Superconducting Supercollider, the Hubble Space Telescope or, in the example of this thesis, the necessary developments in sequencing automation and analysis technologies that allowed the Human Genome Project to be completed within a politically feasible timeframe.

Of course, not all examples are fictional. During the 1950s, when, in the blast wave of the atom bomb, physics was the big science discipline, visions of the future, whether nightmare or utopia, were often atomic dreams. In our current age we can see a similar public and political fascination with genetics and the fruits of the Human Genome Project. In Chapter A, *Primers*, we described some of the economic excitement surrounding the promises of biotechnology. In the 1950s, atomic power promised similar transformations[5]. Traweek (1992) argues that "big science requires a big audience". "The role of this awed audience for science is not to judge the value of the projects of scientists and engineers; its functions are to approve, fund, and to provide recruits" (p. 102). Traweek cites Lew Kowarski (1977), a key figure in the development of CERN[6], the largest particle accelerator in the world, as arguing that the cultivation of these big publics is essential to big science. A study of big science at a disciplinary level

[5] The canary in the coal mine that indicates the development of a culturally big science discipline is science fiction, particularly pulp science fiction. In this light we should note the transformation of the Incredible Hulk from the product of a gamma bomb in the comic books of the 1960s into the product of genetic experimentation in the Ang Lee movie of 2003.
[6] CERN stands for *Conseil Européen pour la Recherche Nucléaire* (European Council for Nuclear Research. The acronym was retained even after the name was changed to *Organisation Européene pour la Recherche Nucléaire* (European Organisation for Nuclear Research) in 1954.

would focus on the capture of public and political support, in terms of both sponsorship and recruitment, and the preservation of these resources once secured.

At the level of resolution that lets us focus on the scientific discipline as the aspect of science that is big, we can reintroduce the reflections of Weinberg (1967). As science fragments and is reunified at higher levels of abstraction, Weinberg suggests that these changes will be reified in a social reorganisation of science. He suggests that a hierarchy will develop. "At the first level are the bench scientists... [who] are kept under surveillance by the next group of scientists, the group leaders or bosses" (p. 47). The bench scientists work in narrow fields, communicating only with closely related bench scientists. It is the group leaders who communicate on a wider basis, in effect maintaining "contact between different groups of bench scientists" (p. 47). This hierarchy might extend upwards, with each superior 'boss' caste having knowledge of a wider area but at increasing levels of abstraction. If Weinberg is correct, a study of big science that takes the discipline as the object of enquiry will be interested not only in how a discipline recruits resources to feed and maintain its growth, but how, as it grows, it manages the problem of scientific communication.

Big science as a quality of a scientific institution

Increasing the level of magnification once again, we find that big science is a property of a scientific project, or a scientific institution. It is at this level that we find most contemporary accounts of big science. Reflective accounts of a life in big science are produced by major scientific actors, who, from their positions as leaders offer an institutional perspective, however myopic, on big science. Examples from the Human Genome Project include books such as *The Common Thread* (Sulston and Ferry, 2002), and ranks of papers, such as the reflections of Collins, Morgan and Patrinos on managing big science (2003). The picture from big physics is similar, with accounts by scientists such as Weinberg (1967) and Kowarski (1977). Popular accounts of big science by third parties are often set at the institutional level (the wide range of books about the Human Genome Project, such as Davis, 1990; Shapiro, 1991; Davies, 2001, are an example of this), and are often journalistic, sometimes in the best sense, stories of management, competition, obstructions, politics, money and breakthroughs.

Hevly (1992) calls for an increase in the number of studies using the 'institution' as a unit of analysis. He writes; "[b]ig science is, after all, institutionalized science" (p. 361). However, he argues that such studies "often fail to take the final step in their analyses to show how institutional context affects the intellectual content of science" (p. 360). One recent analysis that does make this leap is the Collins' study of the Laser Interferometer Gravitational-wave Observatory (LIGO) (2003), which is part of his exhaustive research on gravity wave science (see Collins, 2005). As such, we proceed through a summary of Collins' analysis, using it as a case study in research on big science.

B3 A Case Study in Big Science: LIGO

Collins (2003) describes LIGO as "centralized big science turning itself into mixed big science" (p. 262). From some perspectives, such as in comparison to the Human Genome Project, LIGO is a *relatively* small scientific undertaking, employing less than 50 scientists. Of course, such a team cannot be considered absolutely small, as if it was small science, and, with a cost of between $300-400 million, we can place it squarely in the category of big science[7]. To illustrate what it means for gravity wave science to become big science, Collins quotes a representative of the National Science Foundation; "This community is making a transition from individual entrepreneurial science to big science... they're giving up their individual control and identity... It's just that the high cost of equipment forces communities to move up this learning curve" (p. 267). The shift to big science, he says, will be 'wrenching'. Despite the leaders of gravity wave physics in the US expressing a preferences for small science, they saw a big science programme as an unfortunate necessity. 'Necessity' is a widely used justification for big science projects. We see this in discussions of the Human Genome Project.

As LIGO grew to become a big science project, opinion settled on the idea that a single authoritative director was required. Management by 'steering group' was no longer seen as an appropriate regime. This was a change in the management model of science and was resisted by the scientists involved. The first project manager is quoted as saying; "this was a different sort of activity than simply a continuation of past research activities... they [the scientists] were intellectually unprepared and emotionally... resistant" (Collins, 2003, p. 271). It took a second, more authoritative project manager to make this new management style a success. He is described as having "sliced through the indecision... cutting off options that otherwise would have survived as... pet projects". With this regime now in place, "the project leapt forward" (p. 274). The project manager, with no research experience in the core science of the project, gravity wave interferometry, "was

[7] Just what is big science? How big does science have to be, and in what terms, to be considered big? This chapter attempts to answer these questions, at least to some extent, but this leads us to endless agonising over categorising individual projects. For the purpose of progress in work, the boundaries must remain questions of judgement.

now making the basic scientific decisions" (p. 275). His institutional role was to close, by bureaucratic rather than scientific means, 'controversy' at an early date.

The term 'Skunk Works' was applied retrospectively to this regime of institutional management. Skunk Works are characterised by a strong leader, a highly motivated group of employees with a strong collective identity, a reward structure based on innovative talent rather than bureaucratic position or metrics of productivity, and an absence of outside scrutiny and accountability. Collins (2003) suggests that these kinds of institutional regimes can perhaps have unavoidable pathologies. Tremendous stress is placed on the scientists, and those who refused to 'declare loyalty' to the leader and team found themselves on the outside of the group. Secrecy, which is at least on the surface at odds with the ethos of science (Merton, 1942 [1973]), was also a problem under this regime. However, it was not as a result of the contradiction of the ethos of science, neither was it the significant discontent produced among the scientists by the 'premature' closure of scientific debate, that brought the fall of this regime. Secrecy proved to be the weakness of the Skunk Works management as it refused to accept the kind of oversight and monitoring of outputs that the National Science Foundation felt was commensurate with their capital investment.

As big science, LIGO is a client of the National Science Foundation, which in turn is a client of the US Congress. The NSF had traditionally been a funder of small science. Small science can proceed largely on the basis of self-governance. In big science though, "good work *needs to be seen to be done*" (Collins, 2003, p. 280). It is interesting to note that the pressure for accountability came not only from politicians and civil servants, the interested funders of the project, but also from within science; from actors who might be characterised as the interested competitors. As with many big science projects, as we see with the Human Genome Project, LIGO was opposed by those who feared "that it would suck funds from their own enterprises" (p. 274).

The regime that replaced the Skunk Works attempted to manage the institution by rationalising the activities of the scientists. This type of big science management system is discussed in depth in the next chapter. In retrospect, even the leaders of the new regime admit that the system "didn't take into account the peculiarities of the way physicists like to live or researchers like to think about problems" (Collins, 2003, p. 283). They illustrate their management philosophy, saying; "it's exactly the way a construction company would go out and put together a complicated construction project – somebody on top – some people at the next level that report to them – people at the next level below that report to them… [at] each of those levels the people have specific tasks responsibilities, budgets, schedules… we pretty well shut that [flexibility] out… we just put our blinders on and moved ahead" (p. 283). This, though, led to scientists feeling like that they were being pushed down the organisational order, leading to dissatisfaction, with one scientist saying that there was, among the scientists; "a feeling that they're cogs" (p. 285). Collins cites a scientist as being of the opinion that "scientists can stand low pay and status as long as they are having fun" (p. 276-277). But, according to another scientist, under this regime "[t]here is no attempt to allow scientists to have fun" (p. 285).

This new management, it seems, as opposed to the previous regime, became enamoured of bureaucratic measures of success, and importantly and distinctively, work. These were privileged over the assessments of scientists, the traditional way of judging scientific work and the products thereof. Collins (2003) reports that little time was set aside for serious thinking about what was being done, and that the management appeared to regard the deep thinkers being both the converse of, and less desirable than, those who were the 'doers'. Shifts and schedules came to be seen as means of increasing the utilisation and efficiency of the facility. One of these leaders suggested that this organisational model was a 'sandstorm', a phase that scientists have to ride out until "the sun comes out and we act like scientists [again]" (p. 284).

The routinisation of LIGO was bolstered intellectually by a belief in the reproducibility of the world that is strongly held in high-energy physics; the disciplinary background of the managers. A similar process can be observed in the Human Genome Project, and in biological sciences in general. This is a development that can be attributed to the migration of scientists from fields such as physics and computer science to the life science disciplines. Rose and Rose

(1972) speculated that this was because physicists were repulsed by the atom bomb, and sought a new intellectual home in the life sciences. Whether the movement of individual scientists such as Francis Crick and Walter Gilbert along this cross-disciplinary path can be attributed to that is questionable, but it is the case that, just as scientists with an education in physics were instrumental in the development of contemporary molecular biology, so too does the new genetics owe its existence to the infusion of scientific minds educated other disciplines, especially computer science. However, these imported or incorporated scientists did not come to the Human Genome Project as managers, as was the case with LIGO. In Chapter H, *The High Road to the Human Genome*, the influence of the scientific background of the managers on the organisation of the Institute is discussed. The small science backgrounds of the managers, especially that of the founding director, shape the work culture of the organisation.

In the case of LIGO, Collins (2003) suggests that large scale interferometry was not mature enough to be routinised; there was too much debate as to what counts as good experimental design and what counts as a genuine experimental result. At the laboratory level, the gravitational wave scientists regretted the deliberate reduction in the level of artisanry involved.

In the case study that Collins (2003) presents, we see that the institutional structure of big science is variable. We should avoid inadvertently buying into a determinism of scale. Traweek (1992) presents work in comparative anthropology that strengthens this rejection of simple determinism. Her comparative anthropology of high-energy physics demonstrated differences in the way that Japanese and American scientists organised their big physics projects. The American system was hierarchical, with authoritative leaders, in contrast to the Japanese decision-making system which was dependent on consensus. Of course, each group of scientists saw their own method of social organisation as superior, with stress placed on different features. While the 'bigness' of big science may make the adoption of certain management structures more or less likely, the form implemented depends on more than simple measures of scale.

B4 BIG SCIENCE AS A QUALITY OF LABORATORY LIFE

The case study of LIGO (Collins, 2003) incorporates sociological detail from the level of the laboratory. This, apart from demonstrating the weakness of the microscope metaphor and the 'bleed' between different levels of magnification, allows the study to suggest ways in which the institutional model adopted for the management of big science can colour the character of the knowledge produced. The bureaucratic closure of scientific controversies by actors outside the core-set (Collins, 1981) provides rails along which the knowledge produced must proceed. Given the upset that these closures caused among the scientists, we can assume that it is possible that, had the controversies in question been settled by the community of science alone, the science might have developed quite differently from the path along which it was driven. When the explicitly industrial model was adopted, it could be argued that the attempt to routinise the science could have led to the science being fixed in organisational amber. We explore this argument in the next chapter. Thus, the production of scientific knowledge might be said to be a victim of the contingency of management. Or, more importantly, the contingency of work practices, however arrived at. It is for this reason that we must increase the level of magnification once more.

When we increase the magnification for a final time, we arrive at the point at which our gaze is able to penetrate the walls of the institution, to see into the laboratory and witness science, and scientists, in action. By investigating big science as a process of work, our studies can begin to answer Hevly's (1992) complaint that little attention is paid to the question of how the bigness of big science influences the character of the knowledge produced. As Hevly warns us, urging that we do not become too enamoured of quantitative, scientometric analyses ofbig science; "Big science is not simply science carried out with big or expensive instruments... such instruments, despite their size, may be used in a manner consistent with traditional, little science" (p.356). Indeed, Hevly argues that big science involves more than a change in scale, but also "new forms of institutional, political, and social organization" and "new procedures for the conduct of scientific work". "Big budgets and big instruments are only part of the story; they represent indicators, which themselves should not be mistaken for the substantial changes they signal" (p. 356). The changes of prime importance, we can argue, are the changes in the nature of the work of science. While Hevly might write, as quoted above, that "[b]ig science is, after all, institutionalized science" (p. 361), we can follow that statement with the simple truism that all science, big or otherwise, is the product of human labour. The question that is at the centre of this thesis is; how was the work of science experienced by those who worked to sequence the human genome, i.e. in the big science of genetics, on the big science programme of the Human Genome Project and at the big science institution of the Institute? Modifying the argument of Ravetz (1971 [1973]) slightly, as Ravetz was discussing the craft character of scientific practice, we can say that, without an understanding of science as work we cannot begin to understand how "the subjective, intensely personal activity of creative science" results in "objective, impersonal knowledge" (p. 75), and how this work shapes the resultant knowledge. After all, as Galison (1992) writes; "Seen from the inside – from the scientists' perspective – big science entails a change in the very nature of a life in science" (p. 1). This, therefore, is where the microscope reaches its optimum magnification; the lens of choice for this thesis.

B5 SMALL SCIENCE?

Before we continue with this discussion of big science, using the anatomies drawn from others (Ravtez, 1971 [1973]; Ziman, 1984; Yearley, 1988; Collins, 2003) and begin our discussion of big science in biology, we engage in the production of an essential contrast. A discussion of big science must ask; big science is big in comparison to what? What is the small science implied in any discussion of big science? This section is a brief answer to this question, as much of what is small science is implied in our descriptions of big science. In our understanding of what is small science, a notional 'original' state of science, we begin to understand what is meant when it is argued that science changes as it becomes big science. In doing so, we prefigure the next chapter, as the changes to the distinctive features of small science imply possible pathologies of science that is big.

The leader of the DoE arm of the Human Genome Project, Ari Patrinos, is quoted describing the kind of biology that existed before the sequencing of the human genome.

> A few years ago biological research was really a small science operation. And I don't mean that in any derogatory way. But that was the approach. It was usually small labs with one or two principle scientists and a few post-docs and a bunch of graduate students and technicians and that constituted a fairly good-sized laboratory. Of course science flourished wonderfully that way because it was a different approach.
> (Patrinos, quoted in Lasker Foundation, 1998)

Small science corresponds, to some degree, to the popular image of science. It is, we might argue, often taken to be the ideal of scientific practice. In the popular imagination, scientists work in an autonomous, voluntaristic fashion. To do this small science generally involves a lesser degree of capitalisation and smaller research teams (Yearley, 1988). These are the differences in scale that are patent in the straightforwardly descriptive language. Big science, with its large teams and expensive equipment, is additionally often 'mission orientated' (Galison, 1992). In setting out the type of science that is not big science, Collins (2003) argues that small science is under little pressure to succeed quickly. Small science "can work toward a goal of excellence, defined by... [its own practitioners], without interference... the creators of an idea can be left to decide whether it is succeeding or failing" (p. 261). This thesis stresses these superficially scale-independent characteristics. The work of small science, even that conducted within institutions of large size, such as a university department, is distinguished from the work involved in a large-scale project in these important regards. The large-scale project is big science, organised to proceed towards a defined scientific goal, the pursuit of which is supervised by actors outside the science. Small science, even when gathered together, is a more heterogeneous collection of scientists and objectives, largely organised and scrutinised from within.

In all this though, and throughout this thesis, we should bear in mind Merton (1968 [1973]), who, when discussing the apparent change in the behaviour patterns of scientists, cautioned that we ought not get too nostalgic and build a false picture of past environments and practices of science. Indeed, the birth of big science is constantly being revised and pushed further into the past. Capshew and Rader (1992), for example, categorise the large observatories of pre-Modern astronomers – it would be anachronistic to call these men scientists – as big science. In terms of the kinds of big science under examination in this thesis, we need not only to stress the 'bigness' of the project in which work takes place, but the fact that science is, in the Modern era, a mass occupation. In this we see many parallels with other kinds of work in Modern society, not least when we consider the Institute as a 'factory'.

B6 UNDERSTANDING WORK IN BIG SCIENCE PROJECTS

In section B7, *Big Science and Big Biology*, we explore the idea of the Human Genome Project as big science. Before we do this we summarise previous attempts to understand for work in big science. Not all of these use the phrase 'big science', and the alternative titles carry descriptive weight and value; they communicate more to the reader than simply a hazy sense of scale.

Ravetz (1971 [1973]) describes a mode of social organisation in science that is recognisably 'big science' in nature, labelling it as 'industrialised science'. Ravetz argues that modes of organising scientific work are increasingly drawing their character from industry, a process of asymmetrical interpenetration between science and industry. Kleinman and Vallas (2001) present a picture where the process that Ravetz (1971 [1973]) identified is still underway. While Ravetz argues that the industrialisation of science is taking place across the community of science, it is patent that the most manifest examples of industrialised basic science are also big science projects. Ravetz uses the term industrialised science to mean, in the first place, the dominance of capital-intensive research, and the social consequences of the concentration of power in a small section of the community. Collins (2003) writes that the risk involved in the capital (and political) commitment to the scientific project is borne not only by the scientists, but by wider society; in particular the civil servants and politicians responsible for the grant of funding. The leadership of the project also tends to move away from the site of science, up the chain of risk. Pressure to adhere to a timetable means that "it can be more efficient to give decision-making responsibility to team leaders who do not have the same emotional commitments as inventors" (p. 261). Second, as science is penetrated by industry there is the dissolution of the boundaries that enabled different styles of work, with their appropriate codes of behaviour and ideals, to co-exist. Finally, the industrialisation of science involves the growth of science, both in particular units and in the aggregate, with the consequent loss of networks of informal, personal contacts binding a community.

We have seen in the example of LIGO as big science (Collins, 2003) how industrial models of work organisation are self-consciously adopted by the managers of big science projects, even in the most basic of sciences. This was to some degree found in the Skunk Works model borrowed from the military research division of Lockheed, but importantly, the subsequent regime evoked archetypal images of industrial labour. Collins argues that whereas previously 'craft work' was the normal manner of scientific work, as science becomes bigger it is often the case that industrial contractors bring to the project their expertise of working to "well-specified schedules and performance targets" (p. 262). The regimentation of scientists into shift patterns, the routinisation of the work and the measurement of work done were all aspects of the regime installed to fulfil the requirement of accountability. As Yearley (1988) argues, bureaucratic supervision can lead to scientific work being performed to meet targets rather than to pursue any scientific end. Thus, even in basic research "scientific work has come to resemble industrial scientific labour" (p. 76).

The physicist John Ziman (1984) described large-scale science as being collectivised science. He argues that the collectivisation of science is a process driven by forces external to science. This takes the form of state or corporate control of funding. As the apparatus required to work in science becomes more sophisticated as science advances, this collectivisation is reinforced by a demand internal to science for expensive apparatus and other material facilities. It would be incredibly costly to provide each researcher with these instruments, so to satisfy this demand there is an aggregation and sharing of research facilities. At the same time, as the sophistication of research apparatus increases the complexity can exceed the capabilities of a single individual. The variety of specialised instrumentation used in a single project in advanced science also demands a wider range of expertise than a single scientist can master. Research projects can therefore only be undertaken by the active collaboration of a number of researchers. Thus, a characteristic of collectivised science facilities is not only that they are immensely costly, but also that they require the cooperative work of large numbers of fully qualified scientists. Each scientist takes on a narrowly specialised role within the project. The results of this project are published as a single primary paper with hundreds of co-authors seeking some degree of recognition for their contribution to knowledge.

This notion that big science inherently involves a division of labour is an argument that crops up throughout the literature. Weinberg (1967) writes; "A division of labor between those who create or discover the facts and those who sift, absorb, and correlate the facts seems to be inevitable" (p. 51). He argues that it is only by this method that the community of science can 'systematise' the process of induction and thus contend with the problems posed by the mass of data, information and knowledge that accompany the growth of science. Beaver (2001) describes this as the process of creating 'fractional scientists'. Galison (1992) sums up the change in the nature of a life at work in science that he sees as resulting from big science; "Teamwork and hierarchy increasingly characterize daily work" (p. 1).

When discussing the notion of an increasingly acute division of labour in big science projects and laboratories, and specifically at the Institute, it is worth reflecting on the different ways in which the labour of scientific work can be divided. This thesis suggests that there are four kinds of division of labour, each of which is distinct in nature, which travel under the same name.

First, there is the multiplicative division of labour; the combination of the 'identical' efforts of many people to accomplish a goal larger than possible by individual labour. This is not a separation of skills or the subdivision of a job, and this is not a production line, merely the accumulation of outputs. Remove any worker from the combination and the task will proceed, only with a slower rate of accumulation.

Second, there is the division of labour according to rank or seniority. This is the kind of division of labour that occurs in workshops, or, as we are interested, laboratories. The highest ranking member of the workshop or laboratory delegates the dirty, the laborious, and the tiresome tasks – the tasks of low esteem – to more junior members. This is an intimate and personalised division of labour, based on power and preferences rather than resulting from the imperatives derived from the pursuit of efficiency or the differentiation in skills. In principle, all the members of the workshop are united in their craft but divided by their rank.

Third, there is the organic division of labour. This is the division of labour according to craft of profession, or, as in science, by discipline. These divisions between arrays of expertises and knowledges appear to exist as 'organic' entities; their boundaries are 'natural', at least so much as they are not the overt product of a bureaucratic division of labour. The organic division of labour can be seen when different crafts or professions combine their expertise and knowledge in order to accomplish a goal.

Fourth, we have the bureaucratic, or industrial, division of labour. This is where the tasks of the labour process have been broken down, perhaps through the mechanisation of the process, perhaps through the application of Taylorist principles. This is done for the purposes of efficiency of, or control over, production, and is the division of labour which is characteristic of a production line.

All these divisions of labour can be found in the work of science. Science as a community, or as communities, is reliant on something like the first division of labour, if we consider the path of normal science to be the accumulation of knowledge. In the Human Genome Project we find a more concrete expression of this division of labour. In order to produce the sequence of the human genome a great number of people performing the same task were required. Almost all scientific laboratories contain the second division of labour, in the form of the relationship between principle investigators, post-doctoral researchers, doctoral researchers and research assistants. Collaborative, inter-disciplinary science, which is seen by some as 'big science', involves the division of labour of the third kind. As does, considering this division of labour in a more abstract sense, the progress of science as a whole. However, it is only big science with an industrial character which involves the fourth kind of division of labour. It is this which makes the Institute distinct from superficially similar descriptions of a division of labour in science.

Both 'industrialised science' and 'collectivised science' are more evocative terms than 'big science'. Both 'industrialised' and 'collectivised' carry more descriptive content, informing readers of the mode of social organisation involved, than the nebulous, relative term 'big' is able to do. But they both also have the potential to mislead. 'Industrialised' is capable of implying private ownership and applied, economically exploitable goals. 'Collectivised', on the other hand, suggests that work of science is the collaboration of equals. It is important that we keep both of these extremes open as possibilities, even as we acknowledge their theoretical value. 'Big science' carries far less political luggage, capable of being both basic, public science, and private, industrial research, and we can allow our imaginations to conceive of big science as not only a collaboration of equal scientists, but also as a social system with a highly rationalised division of labour integrated according to a hierarchical structure.

B7 BIG SCIENCE AND BIG BIOLOGY

The Human Genome Project is considered to be the first big science project in biology. As was described in Chapter A, *Primers*, the Human Genome Project cost more than $3 billion[8], with 20 major research sites spread across 6 countries (China, France, Germany, UK and USA), though the bulk of the sequencing was conducted by the G5 centres: the Sanger Institute in Cambridge, UK (funded by the Wellcome Trust), the Joint Genome Institute in Walnut Creek in California (funded by the US Department of Energy), Baylor College of Medicine in Houston, Texas, Washington University School of Medicine in St. Louis, Missouri and the Whitehead Institute, Cambridge, Massachusetts (the latter three funded by the National Institutes of Health) (Collins, Morgan and Patrinos, 2003). According to Collins' (2003) typology of big science, which divides big science projects into; "centralized big science, such as the Manhattan Project and the Apollo Program; federal big science which collects and organizes data from dispersed sites; and mixed big science, which offers a big, centrally organized facility, for the use of dispersed teams" (p. 262), we would classify this effort as an example of 'federal big science'.

This was a multi-national, multi-disciplinary project that took over a decade from the official announcement of the Project to the announcement of the draft human genome sequence (International Human Genome Sequencing Consortium, 2003 and Venter, *et al.*, 2003). As Ziman (1984) expected, these kinds of large scale projects would produce publications with many authors. In the case of the publication of the draft human genome sequence, there were over 500 scientists listed as being responsible for the research and new knowledge contained in these papers. These 500-plus names are just the tip of the iceberg. One early interviewee from outside the Institute told the reseacher that, despite working on the Human Genome Project, his name had not been printed on the hard copy

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[8] Collins, Morgan and Patrinos (2003) point out that $3 billion is a modest amount in comparison to the $11 billion projected cost of the Superconducting Supercollider (SSC). In order to make the HGP seem ever better value, the authors argue that while the SSC will only have a lifespan of 30 years, the HGP will have a 'perpetual' lifespan. Of course the data, knowledge and technology generated by the HGP will be available in perpetuity, but this cannot be claimed for the HGP while being denied as a future 'lifespan' of the SSC.

editions, but was relegated, alongside the names of more than 2000 other scientists, to an appendix to the electronic version. This expanded list of authors of the 'knowledge' does not include the technical, support and supervisory staff who play key roles in the existence of large-scale scientific projects.

Francis Collins, Michael Morgan and Aristides Patrinos occupied key positions in the US National Institutes of Health, the Wellcome Trust and the US Department of Energy (DoE) respectively from the mid 1990s, a key period in the big biology phase of the Human Genome Project. In a retrospective paper discussing the lessons that could be learned from their management of the Human Genome Project, they argue that the management of the Human Genome Project was intentionally 'bottom-up' (Collins, Morgan and Patrinos, 2003). Arguing that this was a decision taken to maintain a grounding in 'solid scientific foundations', this strategy can be seen as an attempt, by drawing input from scientists, to maintain the 'democracy' of science. In Chapter II, *The High Road to the Human Genome*, we see that while a description of the Human Genome Project as being 'bottom up' might jar with the disciplined and goal-orientated nature of the project, in the case of the Institute there were important hangovers from the culture of small science. These made themselves felt in the experiences of those who worked to sequence the human genome.

Collins, Morgan and Patrinos (2003) argue that peer-review in particular proved to be both a means to legitimate decisions taken in the big biology approach and as a management tool. The Human Genome Project was, however, an attempt to produce scientific knowledge on an industrial scale. In discussing the management of the Human Genome Project, the authors make regular concessions to this conception of the sequencing effort, for example they write that centres that failed "to reach the most ambitious levels of production and cost efficiency" (p. 287) were phased out. It is important to remember that the DoE had experience in managing large-scale science prior to collaborating in the Human Genome Project, albeit not in biology.

Of course, big biology did not emerge from within an undisturbed unfolding of nature. Factors external to the interactions between scientists and nature, between scientists and the social sphere of science, and even between scientists and the immediate sponsors of research are paramount in establishing any big science project. John Sulston (Sulston and Ferry, 2002), in his memoir of his scientific, management and political role in the Human Genome Project, reports that the US Congress was attracted to the idea of sequencing the human genome at a very early stage. He suggests that this support sprang from more than a desire to expand the boundaries of knowledge, citing the expected boost that a complete human genome sequence would provide to the US pharmaceutical industry, thus countering the threat presented by Japanese plans in the early 1980s to occupy this field of commercial science.

Collins, Morgan and Patrinos (2003), writing after the publication of the draft human genome sequence, echo Hevly's (1992) description of the 'essential quality' of big science, that it touches many areas "beyond the boundaries of science narrowly defined" (p. 356). They write that the future 'Herculean challenges' of big biology are "not limited to biologists", and call on "leaders across science and society, across academia and industry, and across political and geographic boundaries" to back future big biology projects (Collins, Morgan and Patrinos, 2003, p. 290).

Service (2001) recounts many of the objections of the scientific community to the Human Genome Project. There were fears that the project would divert funding "away from investigator-initiated research, destroying the cottage industry culture of biology in the process" (p. 1182). Perhaps worse, it was suggested that a project to sequence the human genome was not science at all, but "a mindless factory project that no scientists in their right minds would join" (p. 1182). Taking the objection that the Human Genome Project was not hypothesis-driven science, proponents of the project such as Leroy Hood described it not as a 'fishing expedition' but as 'discovery science', providing "new tools for doing hypothesis-driven research" (quoted in Service, 2001, p. 1182). John Sulston describes the approach taken by the Human Genome Project as "'ignorance-driven' or, more grandly, 'Baconian science'" (Sulston and Ferry, 2002, p. 46).

Service (2001) argues that the objections to the Human Genome Project that centred on the notion that it would be a 'factory project' were allayed by the development of highly automated genome sequencing machines. It must be pointed out that the development of scientific facilities that are filled with rank upon rank of machines in no way dispels analogies with factory organisation. However, even if a 'worker bee' vision of biology was avoided by technological developments, both philosophical and sociological problems remain in the concept of automating scientific practice. Proponents of a big science vision of the Human Genome Project claim to be justified by the way in which events, that could not have been foreseen, unfolded. The unexpectedly effective techno-fix amelioration of the extremes of assembly-line biology was coupled with the expansion of the funding pot of public support for biology. This, concedes Maynard Olson (quoted in Service, 2001), was a gamble.

The industrialised nature of the project recurs in accounts of the Human Genome Project. In 1998, as the public project to sequence the human genome was scaled up, the Lasker Foundation interviewed many of the key scientists. Bob Waterston described how the Human Genome Project was a quite different kind of activity to small science biology.

> The other challenge... is this is not a typical biological activity. This is different from the way most biology labs operate. Organizing ourselves and disciplining ourselves and ensuring that the quality of the product remains high in the face of all these demands for productivity, these are the real challenges.
>
> ...Most of the work is being done by machines and technicians. So a lot of it is trying to set up a process pipeline that can turn out the data day-after-day-after-day. And that is a different kind of management task... This is not so much hypothesis driven. This is data driven.... We know what we're after. We don't know exactly how to get there. It does involve some significant unknowns in terms of getting all the pieces of DNA put together in the right order and making sure there aren't any holes and so forth. But a lot of it is process engineering and making the processes as effective as you can.
> (Waterston, quoted in Lasker Foundation, 1998).

The industrialised (Ravetz, 1971 [1973]) nature of the Human Genome Project cannot be ignored. John Sulston describes the Sanger Centre recruitment policy, at least for one rank of scientific labour. "We would recruit unskilled people... This group would have no need of academic qualifications. We judged them on school achievements, interview and something by which I set great store: the pipetting test" (Sulston and Ferry, 2002, p. 75). This is essentially a test of manual dexterity, and it would not be unfair to describe the people recruited in this way as sharing more in common in terms of knowledges and skills with a machinist rather than with a scientist. The important difference is highlighted by Ravetz (1971 [1973]) in his discussions of the craft nature of science; without an intimate craft knowledge of the materials that he or she is working with, a scientific worker cannot perform the kind of expert judgements that allow them to assess results and produce data, information and ultimately, knowledge. These scientific workers may wear white coats and work in a laboratory, but their skills lie in the ability to perform delicate manual operations accurately and repeatedly. This labour can be reduced to measurements of manpower.

It is this 'flushing out' of tacit knowledge (Keating, Limoges, and Cambrosio, 1999) from the process of science that enables scientific work to be automated. As with the example of LIGO provided by Collins (2003), biology, specifically molecular biology and the Human Genome Project, was the recipient of an influx of physicists and computational scientists. This might be the source of a belief that scientific labour can be reduced successfully to a catalogue of defined and repeatable actions, capable of being made mechanistic, which need not involve non-human machines (Collins and Kusch, 1998), and automated. Keating, Limoges and Cambrosio (1999) describe the rhetoric of the Human Genome Project with regard to this process. They quote Walter Gilbert: "the benefits of the Human Genome Project are the benefits of organization and scale" (p. 125). "[I]f large scale sequencing is to work, it must be treated not as a science but as a production job," a "pure technological problem quite apart from interpreting sequence" (p. 126). Likening the process to building an automobile, Gilbert is reported as saying that the work is to be done by "production workers... It is not done by research scientists" (quoted in Keating, Limoges and Cambrosio, 1999, p. 126). Daniel Cohen of Généthon is cited as speaking of "gene factories" and "robot biologists" that will render obsolete the "stage of craftsman laboratory work" (quoted in Keating, Limoges and Cambrosio, 1999, p. 127).

As for the supervisory aspect of anatomies of industrialisation, even John Sulston (Sulston and Ferry, 2002), who strikes a public pose as a traditional idealistic scientist, returns to the notion of measurable inputs and outputs. The scientific work of people recruited in the manner described above can be described in terms of productivity. Efficiency can be measured. The dollar cost per base pair of sequence is a calculation that recurs throughout Sulston's autobiographic account. This is to be expected; the Human Genome Project is big science.

The science journalist Joel Davis (1990), writing before the Human Genome Project began proper, argued that "a Big Science Genome Project" might change "*the way biology is done as a science*" (p. 143). It would, he argued, change the scale of biological research in general and make computers a central part of biological practice. "In years to come, more and more biology papers will have as many coauthors as do physics report". "[B]iology and genetics will become small Big Science. The Genome Project is a major contributor to that change. // And there is nothing anyone can do about it" (p. 144). This view, or at least the rhetorical positioning of big science as the inevitable future of biology was also taken by key actors in the drama of the Human Genome Project. Leroy Hood and Lloyd Smith (1987), arguing for a big science approach to sequencing the human genome, write, "biology has progressed to a point where many of its most exciting frontiers will require expensive and complex new instrumentation" (p. 46). Big science is presented here as a necessary concomitant of scientific progress in biology. Indeed, looking at biology after the Human Genome Project, it might be argued that these commentators were right. Big biology is here to stay. At least, that is the vision of major players in big biological science, such as Collins, Morgan and Patrinos (2003), who argue that the big science mode of doing science ought to now be put to use to increase our biological knowledge of other aspects of life.

Before we move on to the discussions of the consequences of big science modes of organisation that occupy Chapter C, *Pathologies of Work in Big Science*, this chapter closes by quoting from the National Human Genome Research Institute (2005) website. In their list of frequently asked questions, there is the question: "How has the human genome project affected biological research?" They answer by saying that the development of interdisciplinary teams, "[t]he era of team-orientated research in biology", is a break with the individualistic history of the discipline. The development of automated laboratory procedures has been coupled with the concentration of "research in major centers to maximize economies of scale".

Collins (2003) suggests that there are two kinds of science. There is 'developing science' which is best conducted as small science, and 'mature science' that is best organised as big science and routinised. Functowicz and Ravetz (1993), drawing on the ideas of Kuhn (1970), describe normal science as "the unexciting, indeed anti-intellectual routine puzzle solving by which science advances steadily between its conceptual revolutions" (Functowicz and Ravetz, 1993, p. 740). In the next chapter the Human Genome Project is described as being 'extraordinarily normal science'. The question that faces this thesis, as a sociological study of work in science is; what does it mean to work in 'extraordinarily normal science', a process that key figures in the Human Genome Project likened to a construction effort rather than 'real' science? In the next chapter we explore the possible consequences for the community of science, and for organisations such as the Institute, of being organised in a big science, industrialised manner, and the way in which this organisation might be experienced by scientists and scientific workers.

[C] PATHOLOGIES OF WORK IN BIG SCIENCE

The 'pathologies' of work in big science that are considered in this chapter should not to be imagined only as a disease of the 'body social'. Across this thesis, pathology, despite its prior use in sociology, should first be read down to its etymological roots, as the study of suffering. Only secondarily, do we consider the concerns with 'disorganisation' that are the preoccupations of most sociologies to which the word pathology is attached. This chapter is not about the normal and the abnormal. While this chapter does examine the ideas of function and dysfunction, as it touches upon a consideration of the sorts of social arrangements that might be best suited to the creation of scientific knowledge, this is not the focus of the thesis as a whole. Rather, as the thesis concentrates on the subjective experience of work in big science, the understanding of 'pathology' that should be taken into the exploration of the Institute should be that of the experiential kind. In light of the suggestion that big science represents the application of the principles of Modernity to scientific work, we should realise that we do not need to be a classical sociologist to see that 'progress' can be accompanied by sufferings.

C1 INTRODUCTION

This chapter serves two purposes, with each purpose drawing on a different understanding of the word 'pathology'. The first is a discussion of the effects, both potential and evident, that big science modes of organising scientific work have on the ability of the cognitive community of science to produce scientific knowledge. This is pathology considered as dysfunction. The society of science, in this analysis, is assigned a sense of purpose that exists outside and irrespective of a description of the actually existing society of science. This chapter discusses the rationalisation, routinisation and automation of scientific practices, and considers the effects these developments might have on first, the ethos of science, and second, the room for creative work within the social structures of science. While this discussion takes up the better part of this chapter, the empirical investigation is an exploration of the subjective experience of work at the Institute. As such, where it touches on the pathologies of work in big science it is in the sense of pathology as the study of 'suffering'. The second purpose of this chapter, therefore, is to foreshadow the empirical explorations of the subjective experiences of work in a big science organisation. Section C2, *Anatomies? Pathologies?*, further considers the biological metaphors at use in this chapter.

Building on the previous chapter, Chapter B, *Anatomies of Big Science*, this chapter discusses how the development of big science can be described as a process of rationalising the work of science. In section C3, *Big Science Structures and Rationalisation*, the concept of rationality and rationalisation are considered, drawing on the work of Mannheim (1940). Mannheim argued that there are two significantly different definitions of rationality; 'substantial' and 'functional'. The increase of functional, i.e. organisational, rationality is often accompanied by a decrease in substantial, i.e. individual, rationality. The increase in functional rationality, or rationalisation, can be thought of as a process of 'mechanisation'. This mechanisation need not necessarily be literal. This mechanisation can be metaphorical, as in the Weberian (Weber, 1922 [1948]) description of bureaucracy as a machine within which people perform as if they are cogs and gears. It can also be understood in terms of our understanding of human action, as when Collins and Kusch (1998) describe human actions as becoming machine-like. Or, of course, it can be a process of literal, material mechanisation. This is a process that Keating, Limoges and Cambrosio (1999), in their description of the contemporary molecular genetics laboratory, describe as demanding the 'flushing out' of tacit knowledge from the process being mechanised. Section C4, *The Automation of Scientific Work*, considers mechanisation and automation as the reification of rationalisation and touches on some of the ways in which the automation of work has been addressed. Despite the promise that automation will liberate workers from drudgery, this section points out that, very often, only certain classes of workers are liberated. Other kinds of work must still be done, but these workers are either displaced or their work is hidden in these narratives.

This chapter then considers the relationship between increasing rationalisation and various 'pathologies'. Increases in functional rationality and its material counterpart, automation, have been widely considered to be drivers towards the alienation of people, leaving them 'lost' (Aron, 1965 [1968]) in their social systems (Seeman, 1959 [1970]; Blauner 1964 [1970]b). Reflexive scientists, such as Levin and Lewontin (1985) have raised their concern at the potential of contemporary modes of organising the work of science to alienate scientists, and the dangers this poses for the maintenance of the cognitive community of science. Regardless of the functional integrity of the society of science, alienation, considered broadly, is a key concept in understanding the Modernisation of work. One of the characteristics of big science is that it represents the application of the kinds of changes that characterise the Modernisation of work outside of science, massification, routinisation, and bureaucratisation, for example, to the work of the laboratory. In section C5, *Meanings of Alienation*, in order to examine the effects of these changes on both the society of science and the experiences of people working in science, this chapter discusses the meanings of alienation (Seeman, 1959 [1970]; Blauner, 1964 [1970]a). It continues to consider the ways in which these meanings might be relevant in efforts to understand the effects, in particular the increase in functional rationality, of big science modes of social organisation. Three particular meanings of alienation are considered; alienation from the products of work, alienation from the norms of the community, in this case the ethos of science, and alienation from the meaning of the work.

In an attempt to understand the pathologies of work in big science of the first kind, those of the community, section C6, *The Ethos of Science*, discusses the idealised ethos of science. To examine these ideals, we draw on Merton (1942 [1973]) and those engaged in dialogue with his description of this ethos (Ellis, 1972; Rothman, 1972). The ideal of the ethos of science, this chapter argues, is a part of the special character of scientific methods of producing knowledge that work to justify the claim that scientific work produces reliable, valid and progressive knowledge. This can be illustrated by considering the effect of the alienation of scientists from the product of their labour, which, by eroding the justification for moral and intellectual 'ownership' of the knowledge produced, would leave many of the imperatives of the ethos of science without the force of normative weight. Bound up in this notion of alienation, big, rationalised science risks making the whole ethos of science irrelevant to contemporary scientific workers. This chapter argues that the individualised ethos of science, with its claims to being central to the special nature of scientific knowledge production, will struggle to maintain a normative hold over a social environment in which work in increasingly subdivided and communitarian. As the empirical investigations of the Institute demonstrate, particularly Chapter G, *The Recruitment of Sentiment*, the moral and sentimental orientation of the scientific workers that sequenced the human genome is based around the collective. Not in the generalised, abstracted sense, as a small science scientific worker's commitment to the discipline or, even more abstractly, the community of science, but, in the case of those working at the Institute, a commitment towards the local, concrete institution in which they work.

To close the discussion of the pathologies of the first kind, this chapter argues that the functional rationalisation of science, as manifested in routinisation, is only possible in disciplines that have moved, or are moved, into a fully mature phase (Collins, 2003). In section C7, *The Parable of the Needle and the Haystack*, this chapter suggests that the resulting science is not merely the normal science of a mature discipline as described by Kuhn (1970), but extra-ordinarily normal. In extra-ordinarily normal science, the paradigm is embodied in the largely inaccessible workings of automated machinery, and/or the similarly inaccessible workings of a functionally rationalised work procedure. This chapter argues that this represents a sclerotic pathology of science, restricting the possibility of the rationalised science to develop by means of human creativity.

This chapter then turns from the examination of work in science as a special kind of work in a special kind of community, to an examination of work in big science as being of the same kind of thing as all other kinds of work. In section C8, *Work, Modernity, and Big Science*, the chapter compares the development of big science organisations from small science laboratories to the development of industrialised forms of work more generally. This provides the thesis with a foreshadowing of the kinds of issues raised in the empirical exploration of the Institute, and suggests a humanistic grounding for the thesis, concerned as it is with the subjective experience of work, irrespective of claims of 'progress'.

Though both this chapter and Chapter B, *Anatomies of Big Science*, are concerned explicitly with the development and consequences of 'big science', this thesis claims that cases such as the exploration of the Institute have value beyond the intrinsic interest possessed by grotesques. While we might be unable to magnify the social world, except in our imaginations, we do find that in some cases the social world presents us with exaggerated examples of developments and structures that are typical of a more widely found social setting. The drivers towards rationalisation that are patent in big science modes of organisation are not absent in contemporary small(er) science. Big science modes of organisation set an example of a successful way to manage the search for knowledge and account for the funding allocated for research. As we saw in Chapter B, *Anatomies of Big Science*, the big science mode of organisation is presented by some as an inevitable model for the future of biology (Davis, 1990; Collins, Morgan, and Patrinos, 2003). Furthermore, outside the particular and unusual domain of science, technologically advanced workplaces are thought to be a key feature of an emerging knowledge economy. The extent to which a study of the big science organisation of the Institute can illuminate the social domains of science and/or work in a contemporary economy is considered in greater depth in Chapter D, *Case*.

C2 ANATOMIES? PATHOLOGIES?

There is a danger in using organic metaphors in sociology, even, or perhaps particularly, in the sociology of biological science. They arrive with us having travelled from a historical hinterland that is stalked by conservative, even reactionary, politics. This thesis, particularly Chapter B, *Anatomies of Big Science*, and Chapter C, *Pathologies of Work in Big Science*, does not aim to find itself in such territory. For Durkheim, pathologies are the components and consequences of social organisation that do not serve a function in the stability or survival of the social organism. Thus, they are a statement of his functionalist philosophy of society and societal change (Cheal, 2005). But the organic metaphors used by Durkheim, outside of their role as illustrators of the philosophical position taken, also serve as rhetorical devices. The metaphor of society as organism is used as a means of convincing the audience of the importance of the moral propositions made within the sociological work (Crow, 2005). The rhetorical value of these words is the purpose, aside from the obvious nods to the intellectual preoccupations of the scientific workers under study, for their adoption in these chapters.

In so much that we employ the term pathology in a Durkheimian sense, the pathologies that are the focus of this chapter are, despite the focus arrived at when considering the various anatomies of big science in the previous chapter, not those of big science institutions. These might survive, as imagined Durkheimian social organisms (Durkheim, 1895 [1938]), in a perfectly healthy, stable form, with, for example, an alienated scientific labour force. These changes might have an effect on the subjective experience of a working life lived within science, but this in itself would not be a pathology of an institution imagined as an organism of itself. But, as Collins (1985) writes; "Science policy ought not to be about maintaining efficient institutions, but about maintaining the sort of *cognitive* community that will produce the desired scientific products" (Collins, 1985, p. 554). The Durkheimian pathologies that might arise from big science modes of social organisation are pathologies that affect not individual institutions but the cognitive community of which they are a part. The pathologies exist as obstacles to the ability of this community to produce knowledge in such a way that it can claim to possess the virtues, such as reliability and novelty, that are characteristic of the special kind of knowledge that is science.

Pathology, though, primarily serves this thesis as a rhetorical tool, used to foreshadow the kinds of issues that are explored in the empirical investigation of work at the Institute. It is a device that evocatively flags the potential sufferings that are present in the Modern modes of social organisation of scientific work that characterise big science projects. In this usage, this purpose is served regardless of the position taken by this thesis, or a reader, with regard to organic concepts of societies.

A Rejection of Simple Determinism

This chapter ought not be taken to be an appeal to simple determinism. If, for nothing else, for the fact that the empirical investigation of the Institute reveals the way in which contingencies shaped working life during and after the Human Genome Project. The pathologies that we describe are not the necessary result of either scale or technology. As Gallie (1978) shows, the social response to technological developments in the workplace, including automation, is dependent not just on the shape of the technology, but also on the shape of the social system into which these technologies are introduced. Science is a social system of peculiar shape. In Chapter B, *Anatomies of Big Science*, we cite Traweek (1992), who demonstrates that the social organisation of work within big science projects varies even within a single scientific discipline, that of high-energy physics, as a result of national cultural differences. Further, we can look at the work of Stinchcombe (1959 [1970]). He investigated the construction of large scale tracts of housing, the closest thing that exists within the construction industry to repeatable, standardised mass production. He argued that the social organisation of this work remained, contrary to the expectations of theorists of bureaucratisation, craft controlled. He argued that "the professionalization of the labour force in the construction industry serves the same functions as bureaucratic administration in mass production industries and is more rational than bureaucratic administration in the face of economic and technical constraints on construction projects" (1959 [1970], p. 261). Not all jobs are amenable to the rationality of the 'assembly line'; the contingencies of the task at hand shape how the organisation of work responds to the imperatives of Modernity. While the experience of scientists (see, for example, Kowarski, 1977; Levin and Lewontin, 1985; Sulston and Ferry, 2002; Collins, Morgan, and Patrinos, 2003) and students of science (see, for example, Ravtez, 1971 [1973]; Ziman, 1984; Yearley, 1988; Collins, 2003) supports the thesis of rationalisation, routinisation, and automation in big science, this is not enough to argue that it is a necessary aspect of big science organisation. Indeed, in Chapter F, *The Factory and the University*, we see that while these scientists see a world of work that is rationalised and routinised, their descriptions cannot be separated from their perspective, the hinterland of work from which they approach big science.

C3 BIG SCIENCE STRUCTURES AND RATIONALISATION

Before we discuss the connection between the theoretical consequences for the cognitive community of science and the structures of big science, it is appropriate for us to enjoy a diversion to the literary example that was cited in the Chapter B, *Anatomies of Big Science*. The description below is of the construction of the Great Wall of China, as offered by Kafka (1931 [2005]), the creator of bureaucratic nightmares. It evokes, at least to a gaze that shares the preoccupations of this thesis, the story of the sequencing of the human genome.

> The Great Wall of China has been completed at its most northerly point. From the south-east and the south-west it came up in two sections that were united here. This system of piecemeal construction was also followed within each of the two great armies of labour, the eastern army and the western army. It was done by forming gangs of about a score of labourers, whose task was to erect a section of wall about five hundred yards long, while the adjoining gang built a stretch of similar length to meet it. But after the junction had been effected the work was not then continued, as one might have expected, where the thousand yards ended; instead the labour-gangs were sent off to continue their work on the wall in some quite different region. This meant of course that many great gaps were left, which were only filled in by slow and gradual stages, and some indeed not until after the completion of the wall had actually been announced. It is even said that there are gaps which have never been filled in at all, and according to some people they are far larger than the completed sections, but this assertion may admittedly be no more than one of the many legends that have grown up round the wall, and which no single person can verify, at least not with his own eyes and his own judgement, owing to the great extent of the structure.
> (Kafka, 1931 [2005], p. 1).

The story of sequencing the human genome can be, and is, told in very similar language. It was sequenced piecemeal, though not without design, with many gaps in the sequence remaining after the announcement of finished sequence. It was completed by the recruitment of great teams of scientists and scientific workers, labouring at different sites and on the sequencing of different chromosomes. If we return to the summary of the public project as described by *The Harvard Business Review*, we read it "is akin to having several teams laying bricks until various walls come together in a coherent structure" (Enriquez and Goldberg, 2000, p. 98). As for the suggestion that the project is so large that its completion cannot be verified by a person's own judgement, the sheer size of big science projects leaves little possibility of independent replication or verification.

The process described by Kafka, which can be seen as a mirror of the organisation of the work of the Human Genome Project, is an example of where the division of the work of a large project involves the functional rationalisation of the organisation. Karl Mannheim (1940) discussed the meanings of the term 'rationality' in industrial societies. He argued that there are two significantly different definitions of rationality; 'substantial' and 'functional'. What is to be understood as a 'substantially rational' act is one that "reveals intelligent insight into the inter-relations of events in a given situation" (p. 53). 'Functional rationality', by contrast, does not refer to the knowledge and understanding expressed by an individual in his or her actions. Rather, it is used to describe actions that are "organized in such a way that it leads to a previously defined goal, every element in this series of actions receiving a functional position and role" (p. 53). These two definitions of rationality are not, at first glance, logically incongruent. A thought experiment, however, can reveal that it would be reasonable to conclude that, in many cases, working within a functionally rational system reduces the capacity of an individual to act with substantial rationality. Mannheim asks us to consider the Army. "The common soldier... [can carry] out an entire series of functionally rational actions accurately without having any idea as to the ultimate end of his actions or the functional role of each individual act within the framework of the whole" (p. 54). Weber (1922 [1948]) describes bureaucracy as a machine, in which people perform as if they are cogs. Perhaps, as our understanding of machines has changed, we could now use a new, electronic metaphor for the collectively orientated and meaningful component individual of a 'machine'. In the light of the rhetoric of a new economy that generates wealth without reliance on old-fashioned notions of capital and labour, and their attendant visions (see Chapter J, *The Institute and the Knowledge Economy*), we should instead imagine a dematerialised update suitable for an 'informational age'. Rather than human beings as cogs or gears, perhaps human beings as coding scripts or logic gates? What we should note from the example offered by Mannheim (1940) is that the soldier remains, or can remain, a highly skilled 'worker', as, after all, a cog can be a piece of precision engineering. An increase in functional rationality does not, necessarily, result in deskilling. It does, it seems, lead to reductions in substantial rationality.

The definition of substantially rational actions as being those acts that involve "intelligent insight into the inter-relations of events in a given situation" (Mannheim, 1940, p. 53), sounds very similar to any non-mystical definition of scientific creativity. We return to the notion of creativity at a later point in this chapter. Demands for creativity, and the fact that the expertise of science, with its emphasis on novelty and progress prevents codification of work processes, have shaped the organisation of work in small science. As Kinsella (1999) writes; "scientific work is especially characterized by conditions of high task uncertainty, complexity, and relatively loose coupling among researchers – rendering overt and monolithic forms of control ineffective. Simultaneously, scientific cultures are characterized by a strong ethos of autonomy that encourages and enables scientists to resist classical forms of direct or bureaucratic control" (p. 177). The observation made by Stinchcombe (1959 [1970]), that certain jobs are not amenable to 'bureaucratic administration', might be at least as true in science as it is in construction.

While the Human Genome Project, and the Institute in particular, is the empirical focus of this thesis, we can return to Collins (2003) and the case study of LIGO. The regime at LIGO was described by its instigators as being the way that a "complicated construction project" would be run, a hierarchical pyramid with people at each of those levels having "specific tasks, responsibilities, budgets, schedules..." (p. 264). This is a description of functional rationalisation. Collins finds that scientists subject to this regime described themselves as 'cogs', though the significance of this should not be overestimated as this is, at best, insight through cliché. The description of the regime offered, not only by its detractors but also by its champions, accords very well with the definition of mass production offered by Stinchcombe (1959 [1970]). He wrote that mass production is defined "by the criterion that *both* the product *and* the work process are planned in advance *by persons not on the work crew*" (p. 262). This quote does not come from the sociology of science, but from the sociology of work. It is untainted by an attempt to force its description of the work of mass production into a box containing the special field of activity that is science, yet it appears to fit the organisation of LIGO, and, indeed, much of the work of the Human Genome Project, particularly the work of the people at the Institute interviewed for the production of this thesis.

The Human Genome Project, including work at the Institute, involved the development of shiftwork and production targets. In moves that demonstrate a degree of functional rationalisation exceeding that found at LIGO, the Human Genome Project involved a distinct increase in the division of labour, recruiting accredited scientists in addition to large numbers of untrained technical labourers to perform the bench work of sequencing (Sulston and Ferry, 2002). In this task they were assisted by the development of automated sequencing (and sequence assemblage) technologies (Collins, Morgan and Patrinos, 2003). This chapter argues that automation is the material reification of rationalisation.

The irony of the functional rationalisation of scientific work is that, outside of the community of science, the functional rationalisation of work procedures are often described as 'scientific management' (Cooley, 1981). As Ravetz (1971 [1973]) argues, just as science has penetrated industry, reshaping its organisation and social processes, so too does industry penetrate science. Kleinman and Vallas (2001) argue that the exchange between the work world of science and the work world of industry is not symmetrical; that work in science is increasingly taking on the characteristics of work in the commercially-orientated world. As a lament in *The Scientist*, a magazine for life scientists, has it;

> Newton had it easy [...] For Darwin, science was a country gentleman's pastime [...] The Industrial Revolution changed all this. For the first time in human history, it became possible to produce goods and services on a mass scale. Industrialization proved so successful that science also fell victim to it, and academic research now resembles a commercial operation.
> (Aszódi, 2007)

But this is not the full story. This is not simply an exchange between two social worlds, each passing to the other some of aspects of itself. Attitudes and technologies derived from science were a feature of the social changes in the world of work during the 20^{th} century. The rationalisation, routinisation and automation of science are therefore not strictly the results of an exchange, but rather an example of the influence of science coming full circle to 'scientise' the social world of science. The laboratory, the very engine of Modernity, has now been Modernised.

Keating. Limoges and Cambrosio (1999) suggest that molecular biology involves 'green-fingered' techniques. However, they reach what is an intuitively paradoxical conclusion, that "molecular biology as a whole has been singled out as ripe for automation partly because of the lack of routine" (p. 125). The automation of other areas of work has been seen not only as a means to increase economic efficiency, but also to move control from the shop-floor, or the laboratory bench, and to the management office. Watson (2003) describes how Noble (1984) demonstrated that computer numerically controlled (CNC) machines were not so much a technology shaped by the cost-conscious imperative of capital, but rather CNC was a product of the interests of the military customers of the Massachusetts Institute of Technology to have technologies that took control of the methods of production away from the skilled shop-floor workforce and transferred it to management. In a knowledge economy, where ideas are capital, this transformation of tacit knowledge into objects that cannot walk out through the factory gates is a process the logic of which is patent.

The process of automation necessarily involves "[f]lushing out tacit knowledge" (Keating, Limoges and Cambrosio, 1999, p. 126) from the laboratory practice being automated. All the knowledge and expertise must be made explicit. An automated process is an example of a supremely rationalised process; each part and practice of the procedure is defined and described, in advance, according to the, at least apparently, most effective pursuit of the predetermined goals. It is possible to view the Human Genome Project, as the organisational embodiment of these programmes. As we discussed in Chapter B, *Anatomies of Big Science*, Keating, Limoges and Cambrosio (1999) report Walter Gilbert as being of the opinion that the work of sequencing the human genome is to be done by "production workers... It is not done by research scientists" (quoted in Keating, Limoges and Cambrosio, 1999, p. 126). In the same chapter, Daniel Cohen, of Généthon, is quoted as speaking of 'gene factories' and 'robot biologists', developments that he believes will render obsolete the "stage of craftsman laboratory work" (quoted in Keating, Limoges and Cambrosio, 1999, p. 127).

This discourse that speaks of the 'end of the craftsman' echoes that of previous technological developments that affected the organisation of work.

The automation of specific laboratory protocols is just one part in the process of rationalising scientific practice as a whole. Keating, Limoges and Cambrosio (1999) write that "the 1980s saw a shift in rhetoric from automation *in* the laboratory to automation *of* the laboratory" (p. 127). We can take their analysis further. The 'flushing out of tacit knowledge' involves not just the installation of machines, but the mechanisation of human action. Human action is made machine-like (see Collins and Kusch, 1998) when tacit knowledge is removed by codifying and standardising protocols of action, in this case by routinising laboratory procedures. The installation of a new machine, and the way that this changes the shape of the social environment, is an interesting point for sociological research. But we should be wary of seeing the automation of laboratories, or indeed other workplaces, as merely this. Rather, the installation of a new machine is just a materialised component of a wider programme of rationalisation.

Early visions of the Human Genome Project presented a landscape of dark, Satanic sequencing mills. Zweiger (2001) describes how, when he was a young scientist, plans to sequence the human genome were presented to him. He was faced with a vision of technicians sequencing genomes on an 'assembly line'. Their work would be assessed on the bases of cost and efficiency. He writes; "It was as bad as the rumours we had heard of factory-like sequencing operations in Japan. It all seemed so inelegant, even mindless" (p. ix-x). However, later accounts of the Human Genome Project in the scientific press describe the way that these nightmares of a factory-like genome project never saw waking life as rapid improvements in automation prevented the recruitment of hundreds of accredited scientists into drudgery (Service, 2001).

This appears to be an example of Blauner's (1964) 'inverted U curve', the representation of the relationship between technology and alienation. However, the empirical investigation of the Institute reminds us that, while there might not have been a demoralised army of accredited scientists working on the benches of the Human Genome Project, there were hundreds upon hundreds of technicians. Some of these, as we have read, were recruited on their performance at school and their manual dexterity (Sulston and Ferry, 2002). This should remind us of the observations made by some of the critics of Blauner. Watson (2003) quotes Nichols and Beynon (1977) and their study of the chemical industry; "for every man who watched dials [the non-alienated process worker of Blauner (1964)] another maintained the plants, another was a lorry driver and another two humped bags or shovelled muck" (Watson, 2003, p. 182-183; quoting Nichols and Beynon, 1977). This insight not only enlightens our understanding of the process of the automation of science, but also reminds us that claims that a turn to a knowledge economy will improve all our working lives should be read with scepticism. Not all the material necessities of work are transferred onto the pistons of robots or into the circuits of computers; some are displaced and, to some, hidden.

This process can be seen in an interview the researcher conducted, before beginning substantial fieldwork, with a scientist working on the automation of science. He was not automating processes in the laboratory, and he was not, as Keating, Limoges and Cambrosio (1999) write about, attempting to automate the laboratory itself. Rather, he was attempting to automate the discovery process of 'normal' (Kuhn, 1970) molecular biology. When the scientist described his discovery robots, it became clear that the ideal was a lab emptied of scientists. This would be a progressive development, he argued, as scientists are now free from the manual labours of the lab, and even the routine intellectual processes of 'normal science', and are able to use their creative intelligence to advance science. However, as the interview progressed, it became clear that these laboratories would not be empty of human beings; maintenance workers would need to attend the machine, cleaners would be required, manual labour would be needed to deliver and load supplies, and so on. There would be a process of upskilling and a decrease in alienation, so long as the majority of the people whose labours are necessary for the production of scientific knowledge are written from the scene.

Rationalisation and automation are intimately connected with notions of alienation. It is, after all, the very definition of the instrumentalisation of the activity of work. Previous advances of rationality across the world of work have come in such forms as scientific management, eponymised as Taylorism. Watson (2003) writes that Weber saw Taylorism as a 'triumph' of rationality fulfilling its dehumanising potential. Braverman (1974) described the general logic of industrialism as being one that routinises, mechanises and deskills; the transformation of labour into a homogenous commodity. Again, the contingency of these connections must be stressed. Rationalisation, automation and the instrumentalisation of work do not necessarily result in increased alienation (Cotgrove, 1972). Such an outcome depends on the shape of the work culture that is subject to these developments (Edwards, 1979; Thompson, 1989). In some cases there is a counter logic, that leads to upskilling, rather than deskilling (Senker, 1992), and arguments exist for the increasing automation of work as leading to a decrease in alienation (Blauner, 1964). This chapter argues that the work culture of science is an integral component of what it is that makes science a special way of knowing the world that provides reliable, valid, progressive knowledge. In the empirical investigation of the Institute, particularly in Chapter H, *The High Road to the Human Genome*, we see the importance of the peculiar character of science in producing an organisation with a particular culture.

C5 Meanings of Alienation

Before we can begin discussing the possible alienation of scientists, we must clarify what we mean when we use the term alienation. Watson (2003) describes alienation at its most simple, as separation. However, Aron (1965 [1968]) provides us with a succinct definition of alienation as "a sociological process by means of which men or societies construct collective organizations in which they become lost" (p. 148). Despite the elegance of the definition offered by Aron, we still need something a little more comprehensive if we are to effectively use this concept.

Seeman (1959 [1970]) is determined to treat alienation as a social-psychological experience focused on expectations, values and behaviour, rather than an assessment of the objective conditions. These are important in gauging the realism of the individual's alienated response. However, to divorce alienation from the subjective experience of those described as alienated would be neither the means to robust argument, nor the intellectual basis for an effective strategy for research. This thesis proposes to use the term alienation to describe the subjective experience of people in relation to their work, and the products of their work.

There are dangers in allowing the concept of alienation to become too loosely used a term. Alienation is not the same as dissatisfaction, though defining it as a subjectively experienced phenomenon does push it close to synonymity. It is important to keep these two categories of experience distinct. We could say that one can be satisfied with work thanks to, say, an extremely comfortable working environment and a generous package of pay and benefits. But one could still be alienated from the work if it is undertaken solely as a means to this physical comfort (Mills, 1962 [1963]) and no meaning is derived from the work itself and the worker has no connection with the products of their work. In investigating the pathologies of big science modes of social organisation, the question of the alienation of scientific workers requires us not to ask; 'are scientific workers satisfied with their jobs?', but to ask the subtly different question; 'are scientific workers satisfied as scientific workers?' These two questions capture the differences between the extrinsic satisfactions of work, in which, in *extremis*, work is a means to an end and possesses only instrumental meaning, and the intrinsic satisfactions of work – whether it is enriching, challenging, fulfilling, for example – which carries with it expressive meaning (Watson, 2003). This, as we shall see, connects the maintenance of the cognitive community of science, which we discuss in this chapter, with the subjective experience of scientific workers, the focus of the empirical explorations of this thesis. The maintenance of this community is important for the production of scientific knowledge. As Ravetz (1971 [1973]) argues, the self-discipline that scientists require to produce quality work is dependent on "the strength, health and integrity of the community involved" (p. 82).

This chapter concentrates on three related meanings of the word alienation. The first of these is the notion of becoming alienated from the products of labour, a concept that traces its descent through Marx to Hegel (Jordan, 1971). It should be noted that, as Watson (2003) describes, this definition of alienation has been criticised on the basis that it connects human fulfilment entirely with work, and thus it shares a view of the world with those who use the term 'work ethic' in the most unreflective manner, or, more recently, those who argued for 'self actualisation' through work. Seeman (1959 [1970]) identifies five further meanings of alienation which relate to the subjective lives of those who are alienated. In so much that this chapter is concerned with the society of science, we concentrate on two of these, 'meaninglessness' and 'normlessness' in addition to the classical notion of alienation from the product of labour. These are summarised below. Understanding alienation as 'powerlessness', 'isolation' and 'self-estrangement', which are, for Seeman, the further three meanings of alienation, might have little utility for understanding the pathologies of big science *qua* the society of science, but they can be of use in helping us understand the pathologies of working lives. Consideration of these meanings of alienation echoes through the discussions of the empirical investigation of the Institute.

The notion of alienation from the product of labour can be illustrated by a literary example. The human genome sequence is often referred to as 'the book of life' (see, for example, Pennisi, 2001). Considering the role of creativity in science, we can enjoy some play with this metaphor. Whether fiction or non-fiction, books are commonly held to involve a degree of creative and imaginative input on the part of the author. How can this process be rationalised, mechanised, automated? In *Nineteen Eighty-Four*, George Orwell (1949 [1989]), provides us with a fictional example[9]. Julia works "on the novel-writing machines in the Fiction Department. She enjoyed her work, which consisted chiefly in running and serving a powerful but tricky electric motor. She was 'not clever', but was fond of using her hands and felt at home with machinery" (p. 136). Consider this in comparison to the description of recruitment to the work of sequencing at the Sanger Centre "We would recruit unskilled people... This group would have no need of academic qualifications. We judged them on school achievements, interview and something by which I set great store: the pipetting test" (Sulston and Ferry, 2003, p. 75). In *Nineteen Eighty-Four*, Orwell (1949 [1989]) describes the relationship that Julia had with the products of her labour; "She could describe the whole process of composing a novel, from the general directive issued by the Planning Committee down to the final touches by the Rewrite Squad. But she was not interested in the finished product. She 'didn't much care for reading', she said. Books were just a commodity that had to be produced like jam or bootlaces" (p. 136). Julia was alienated from the product of her labour; the process of production of the novel rendered her distant from the content of the novels and allowed them to become 'mere' commodities. In the empirical investigation of the Institute, particularly in Chapter G, *The Recruitment of Sentiment*, we see that the people working at the Institute, even those recruited by the 'pipetting test', had a much greater connection with the products of their labour.

But perhaps we ought to offer ourselves an enlightening corrective. When using the metaphor of 'the book of life', scientists, commentators and politicians do not imagine that they are talking of scientists writing the book of life. Rather, they are taking a view that science unveils pre-existing natural facts, and thus, that the act of producing the book of life is one of transcription. But here, for all its

[9] The common caveat for the allegories that are drawn from fiction applies here; while this is a literary illustration, it is not intended to be taken literally.

eminently sensible realism, we are confronted with the rationale for the conduct of science stripped of creative imagination; an anti-science vision.

Returning to Seeman (1959 [1970]), the first meaning of alienation that we use in this thesis is 'meaninglessness'. Seeman cites Mannheim (1940), using his "description of the increase of "functional rationality" and the concomitant decline of "substantial rationality." Mannheim argues that as society increasingly organises its members with reference to the most efficient realisation of ends (that is, as functional rationality increases), there is a parallel decline in the ability of individuals to act with intelligence in response to a given situation. Seeman (1959 [1970]) writes; "One might operationalize this aspect of alienation by focusing upon the fact that it is characterized by a *low expectancy that satisfactory predictions about future behavior can be made*" (p. 386). The second meaning of alienation that Seeman provides to this thesis is 'normlessness'. He likens this meaning of alienation to 'anomie'. "[A]nomie denotes a situation in which the social norms regulating individual conduct are no longer effective rules for behavior" (p. 387). When the culturally prescribed goals are no longer attainable, normlessness leads to the development of 'adaptations' – technically effective, if not culturally legitimate, procedures. "[T]he anomic situation… may be defined as one in which there is a *high expectancy that socially unapproved behaviors are required to achieve given goals*" (p. 389).

C6 THE ETHOS OF SCIENCE

One of the special characteristics of science as both a way of working and a way of knowing is that the community of science can be seen as holding to a set of norms and values that make it distinct. The community of science is able to produce knowledge that is both valid and novel as a result of "an institutional framework which involves a complex of values and norms which elaborate the appropriate approach and methods to be employed in the quest for knowledge, and which also define standards for the acceptance and certification of additions to the body of scientific knowledge" (Rothman, 1972, p. 102). Some of these norms and values are technical, defining the accepted community boundaries of the 'scientific method'. Rothman suggests that these include; empirical validity, logical clarity, logical consistency of propositions and generality of principles. Adherence to technical norms is necessary for the generation of valid scientific knowledge by a community, but they are not sufficient. Merton (1942 [1973]) offered the initial formulation of the value system of science; "The ethos of science is that affectively toned complex of values and norms which is held to be binding on the man of science. These are prescriptions, proscriptions, preferences, and permissions... legitimised in institutional values" (p 268-269). This 'affectively toned complex' can be understood as part of the sentimental orientation of a scientist. In Chapter G, *The Recruitment of Sentiment*, we consider the role of the affective in the accomplishment of sequencing the human genome at the Institute. Merton identifies "[f]our sets of institutional imperatives – universalism, communism, disinterestedness, organized scepticism – ... [that] comprise the ethos of modern science" (p. 270). Each of these imperatives is summarised in turn.

For Merton (1942 [1973]), universalism refers to the idea that "the acceptance or rejection of claims entering the lists of science is not to depend on the personal or social attributes of their protagonist" (p. 270). Science is international and impersonal, and the career of the 'scientist' is open to anyone of talent. In this, science can be seen as bearing one of the hallmarks of Modernity, being without regard to the characteristics of the individual that are irrelevant to his or her role as, in this case, a scientific worker. Merton argues that this aspect of the ethos of science is democratic (p. 273). Communism (sometimes tamed by being re-labelled as 'communalism') refers to the common ownership of goods. The goods in this case are the product of science; scientific knowledge. This knowledge is the product of social collaboration, both directly and indirectly, and the ownership of product is assigned to the community. Rather, then, than material ownership of knowledge, scientists are concerned with claims of priority. These are assertions of the right to the recognition and esteem that are the rewards of discovery. Merton argues that this drive for recognition and esteem pushes scientists into publishing their discoveries and thus sharing the knowledge that they have produced. Rose and Rose (1969) describe the cognitive community of science as being "maintained by a value system which emphasises universality and disciplinary communism and a reward system whereby the scientist, in return for the gift of knowledge to his readers, is accorded status and recognition" (p. 8). Merton describes the drive to share knowledge as operating as a 'moral compulsive'. This reward system, however, is dependent on visibility and recognition, as can be seen by the fact that the greatest reward in science, beyond any prize, is being eponymised, and immortalised, in scientific fact, technique or law. If, in big science, scientists become 'invisible', as suggested by Beaver (2001), then an important connection with the products of their labour has been broken. Scientists do not own the knowledge that they create. Instead, their 'property' is found in priority; being recognised, by name, as the first discoverer of a fact, as the first to develop a technique, as the first to devise an explanatory theory. This impossible for the great majority of scientists in the event that science becomes not only a mass profession, but further, a job conducted in industrial laboratories of mass employment. This alienation from the products of their work is a challenge to the maintenance of the ethos of science.

Suggestions that the communism of knowledge are being undermined are not new. Today they are characterised by debates over commercial research and the attendant secrecy, over patents and licensing, and over the ownership of journals. Merton (1942 [1973]) noted that advanced capitalist conceptions of knowledge as property were a challenge to this plank of the ethos of science. For Merton, "[t]he demand for disinterestedness has a firm basis in the public and testable character of science" (p. 276). Merton argues that the fact that the 'public' of science are, in effect, fellow scientists, is an institutional contribution to the high integrity of members of the community of science. Organised scepticism, the final of Merton's institutional imperatives, is interrelated with the other three elements. Science asks questions, holding no uncritical reverence for any aspects of nature and society.

The behaviour of people who cannot be classed as anything other than 'scientists' has often deviated from the ideals of the ethos. That individual members of any community will contradict the community norms is a fact that should be expected. This fact does not render these norms, and the ethos that binds them, invalid. How else would the rules of a society be tested, displayed, and demonstrated? It is the institutional chastisement of those who display this deviant behaviour that demonstrates the functional value of the ethos to the cognitive community of science[10].

However, in some cases the behaviour of the community of science, not simply individuals or small sub-groups of scientists, appears to violate the norms that are said to represent the scientific ethos. Rothman (1972) presented 'a dissenting view on the scientific ethos'. He reminds us that we ought not uncritically take the ethos articulated by Merton (1942 [1973]) as being identical to the actually existing ethos and ideology of members of the community of science. There is evidence that the community of science behaves contrary to these norms. Particularism, to be imagined as the opposing state to universalism, is a common aspect of the assessment of the quality of scientific knowledge. Traweek (1992) discusses the way in which science from outside the West is overlooked. Rothman (1972) suggests that particularistic criteria are applied within national science cultures, distinguishing work produced on the basis of institution, for instance. Rothman argues that the norm of communalism is also infringed by the development of 'invisible colleges'. Weinberg (1967) suggests that these kinds of structures are reactions to the problem of communication arising from the growth

[10] See, for example, commentary of the recent case of Dr Woo Suk Hwang, the South Korean cloning scientist who was exposed as a fraud. In *The Guardian*, Richard Horton (2006), editor of *The Lancet*, wrote an article titled 'The cloning fraud case is a scientific success story'.

of science. Further, we could suggest that a drive towards greater secrecy might flow from increased competition for funding, and attempts to commodify and profit from scientific knowledge.

Rothman (1972) argues that the forces that are reducing the strength of the hold that the ethos of science articulated by Merton (1942 [1973]) exerts over the behaviour of scientists are external. He suggests that the intrusion of contemporary 'middle class culture', with its "dominant value themes of materialism and success, conformity and morality" (Rothman, 1972, p. 107) is an expected result of the recruitment patterns of science. But more, Rothman suggests that the apparent weakening of the scientific ethos is the result of the growth of 'big science', in particular "its resultant dependence upon external money" (p. 107). What effect does the development of big science projects have on the norms and values of scientists? What consequences do these changes have for the cognitive community of science? We suggest that the loss of the special ethos of science leaves it without both the established means to maintain standards of knowledge, through social and psychological checks, and the traditional drivers to 'progress', both through the sharing of knowledge and the promotion of novelty.

Merton (1968 [1974]) wrote of possible changes, or adaptations, in the ethos of science as a result of science becoming a mass occupation[11]. He quotes Zuckerman; "as the social organization of scientific work becomes more complex... the visibility of *individual* role-performance is reduced" (p. 332). As Beaver (2001) argues, in the 'new paradigm' of teamwork in science, of which high-energy physics is described as the exemplar; "most participants are invisible, in a formal sense, to the larger research community. They are just "names" on a paper, "fractional" scientists, essentially anonymous" (p. 370). This chimes with the phrasing that Aron (1965 [1968]) uses to describe the notion of alienation, writing that it is a process by which people become 'lost' in organisations. Merton (1968 [1974]), however, suggests that the late 1960s belief that the ethos of science is in a process of change "is the result of parochial perception" (p. 334).

[11] In the same paper, Merton (1968 [1974]) warned that we ought not simply replace the old, outdated mythology of science with by mythologising the modern organisation of science.

Even if historical parochialism was responsible for concerns for the ethos of science in the 1960s, these concerns deserve re-examination in light of further developments in the social organisation of science. Sulston and Ferry (2002) write; "Insidiously, over the past few decades, the prevailing ethos of the world of science has shifted. What was once a collective enterprise, in which discoverers were acknowledged but their results freely shared, is now frequently constrained by the demands of commercial competition. Motivated by financial gain, hamstrung by sponsorship deals, or simply out of self-defence, many researchers trade their discoveries with the rest of the community only under the protection of patent law or commercial secrecy" (p. ix). This reads as though this were a response to the same events that prompted Hans Gaffron to lament; "Not only has the ruthlessness of accomplished business techniques invaded the areas where industrial exploitation overlaps research, but this kind of behavior is no longer considered alien to science" (quoted by Merton, 1968 [1974], p. 327). Gaffron, though, was speaking in the 1960s, mourning the apparent passing of his cherished ethos of science in 1920s. Sulston and Ferry (2002) are recording the concerns of one of the leading figures of early twenty-first century science as he looks back to the scientific ethos of the 1970s. It is in the Human Genome Project and the Institute that the processes of biological science were rationalised, automated and bureaucratised. While Sulston's professional biography (Sulston and Ferry, 2002) tells a story of a man tied to a Mertonian ideology of science, the organisational arrangements that were resorted to in a defence of these values may have played a part in laying the material foundations for the dissolution of the norms of the cognitive community of science.

The bureaucratic organisation of big science is at odds with what is often imagined as the ideal method of organising scientific work; the adhocracy. Kinsella (1999) writes; "Although traditional and popular views of science emphasize the autonomy of scientists, the contemporary social practice known as big science [...] foregrounds the constraints that are also implicit in scientific work and its management" (p. 174). Kinsella goes on to suggest that the culture of science, in which an ethos of autonomy is particularly valued, lends scientists the means with which to resist bureaucratic management. In the empirical investigation of the Institute we find narratives of autonomy, particularly as articulated by research participants in Chapter F, *The Factory and the University.* However, here we should note that the autonomy of technical workers is not the autonomy that is idealised in the ethos of science. It is not a state in which "the choice of subjects and the actual conduct of research is entirely the responsibility of the individual scientist" (Polanyi, 1962, p. 53). Indeed, in a big science project such as the Human Genome Project there are no *individual* scientists.

We should also pay attention to the ethos that develops among scientific workers in settings outside academia. As my investigation of the Institute revealed, and is highlighted by the fieldwork anecdote described in Chapter E, *Conduct*, the Institute has absorbed industrial values beyond simply the adoption of an 'assembly line'. Ellis (1972) investigated scientists working in an industrial setting and found that in such a setting the idealised image of scientific work, and the accompanying ethos of science, was dissolved by exposure to a world of commercialised science. The scientists who participated in the research demonstrated little attachment to the apparent ideal and appeared to feel little dissatisfaction as a result of their working experience. Rather, as Ellis notes, these industrial scientists would often adopt an ethos developed for a commercially orientated social world. This suggests that an industrial setting for scientific work can break scientists' adherence to an ethos that is peculiar to science. We might say that as a consequence of the social organisation that these scientists inhabit and work within they have been alienated from the scientific ethos. But they have not been rendered normless. The adoption of an ethos rooted in commercial values is an 'adaptation' to this situation. However, while scientists might adapt to the inappropriateness of the scientific ethos to their experience of work in industrialised science, this should not be taken as a replacement of equivalencies. The adapted scientists might identify with commercial values and priorities and draw satisfaction from the 'applied' nature of their work. But with little opportunity to work on research of their own choosing, and with "the increased subdivision of complex scientific tasks… [that] takes away from them the opportunity to understand and appreciate their work as a whole" (Yearley, 1988, p. 73), these scientists might have adapted in ways that counter the normlessness of the loss of the ethos of science, but they cannot avoid alienation in the senses of powerlessness and meaninglessness, and, more fundamentally, from the products, in this case scientific knowledge, of their work.

The dissolution and replacement of the ethos of science may involve factors that we might not expect to find in big, but basic, science, for example in the Human Genome Project and at the Institute. Examples of these would include commercial pressures to arrive at profitable research findings and the imperative for industrial levels of secrecy. But in other aspects of its character, industrial science, with hierarchical control over large, multi-disciplinary research and development teams, can be seen as a close counterpart of big basic science. And if these developments lead to the loss of the scientific ethos, then one of the normative bases by which science ensures that knowledge that is both novel and valid is lost.

C7 THE PARABLE OF THE NEEDLE AND THE HAYSTACK

In a diversion from considering the experiences, and pathologies, of working lives in big science, let us consider the philosophy of science that underpins the rationalisation of science as it reaches its extreme end-point, the automation of discovery. Glymour (2004) discusses the various methods that might be employed in the search for knowledge. He offers a metaphor to illustrate his call for the automation of, not just laboratory procedures, but, as was the goal of the scientist who was discussed in section C4, *The Automation of Scientific Work*, the very process of discovery; "When scientists seek to learn new, interesting truths, to find important patterns hiding in vast arrays of data, they are often trying to do something like searching for a needle in a really huge haystack of falsehoods, for a correct network among many possible networks, for a robust pattern among many apparent but unreal patterns" (p. 76).

First, we ought to recognise that in this introduction the scientific method is described without reservation as the process of uncovering of truths already existent in nature. Truth, Glymour (2004) assures us, is out there, but it is hidden and need to be revealed. It must be uncovered, not from a mass of data, but a mass of 'falsehoods'. There is no 'construction' at work at all, and so, despite Glymour referring to 'creativity' on the part of scientists, we might begin to doubt where this particular vision of the scientific method has room for it.

Glymour (2004) asks; "So how does one find a needle in a haystack?" (p. 76). He offers us six options[12], two of which he suggests are akin to automated discovery. These are; "5. Set the haystack on fire and blow away the ashes to find the needle", and "6. Run a magnet through the haystack" (p. 76).

[12] Option two is; "Pick something you like out of the haystack. Subject it to a test. If it doesn't pass the test, find a weaker test (e.g. is the thing long and narrow?) that it can pass" (Glymour, 2004, p. 76). Glymour suggests that this "is practiced and effectively advocated by many social scientists (you need only replace 'something you like' in 2 with 'theory')" (p. 76). His methodological digs at social science continues with remarks about the social studies of science (which he labels as 'postmodernist'), which betray a lack of engagement with even intellectually conservative accounts of how science, as a social process, is actually accomplished.

The destructive and mechanistic attitude of these last two methods ought to reveal to us that there is a significant weakness in this conception of the automation of discovery. It is bound to work entirely within the bounds of the existing ontology and understanding to collect new data of the same sort. It can only do 'extraordinarily normal science'. It can tell you nothing more about the haystack except how many needles are, or were, inside it. For that, you must know that there are needles inside, and that these needles are the objects of prime concern. The answers to these questions might be interesting, but creativity does not merely serve as a recreational outlet that keeps scientific researchers satisfied with their work[13]. It serves an important purpose in the maintenance of the cognitive community of science, organised, in the loosest sense, in the pursuit of both valid and novel knowledge. It is by this creativity and the consequent generation of novelty by which science develops new categories, new ways of looking at objects, new ontologies.

If we return to Mannheim (1940), and to the notions of substantial and functional modes of rationality, we can ask; rather than the Army, how could the creativity of scientific work be made subject to a process of functional rationalisation? We might suggest that the overall experiment is broken down into a series of procedures in a pathway. Scientists would then be trained to become experts in a single procedure. Feeding the results of these procedures – which could be data or material products – through the rationalised experiment pathway this collective of scientists produces an experimental result. This result can then be published. Throughout Part Three, *Illustrating and Accounting*, we see discussions of such a 'chain of production' in genome sequencing. In examples such as this, no single scientist need understand the whole experiment, nor does he or she necessarily understand the position of his or her results in the knowledge produced. Indeed, if the process of producing the published paper is standardised, or done away with altogether, this decrease in substantial rationality can run deep. Note that in this example we did not discuss the deskilling of scientists, though that may be an associated consequence of increasing functional rationality. Rather, we have

[13] This, we must stress, is the reason why the alienation of scientific workers can be considered a special problem, and ought to be a concern of those who are nevertheless indifferent to the alienation of workers in general.

concentrated on way in which an increase in functional rationality might reduce the space in which individual scientists are able to respond with substantial rationality, with intelligence and with insight (Mannheim, 1940), in other words creatively, towards the work that they perform and the data they produce. Without the scope to work creatively, in a system in which functional rationalisation is maximised, scientific workers are alienated from the meaning of their work.

The functional rationalisation of science, manifested in routinisation, is only possible in disciplines that have moved, or are moved, into a fully mature phase (Collins, 2003). The resulting routinised science is not merely the normal science of a mature discipline as described by Kuhn (1970), but extra-ordinarily normal. This might be an apt description for what the proponents of the Human Genome Project have called 'discovery science' (Collins, Morgan and Patrinos, 2003). Bob Waterston described the Human Genome Project as being a 'data driven' scientific project, unlike traditional 'hypothesis driven' science.

> ...Most of the work is being done by machines and technicians. So a lot of it is trying to set up a process pipeline that can turn out the data day-after-day-after-day. And that is a different kind of management task... This is not so much hypothesis driven. This is data driven.... We know what we're after. We don't know exactly how to get there. It does involve some significant unknowns in terms of getting all the pieces of DNA put together in the right order and making sure there aren't any holes and so forth. But a lot of it is process engineering and making the processes as effective as you can.
> (Waterston, quoted in Lasker Foundation, 1998)

Normal science, according to Kuhn (1970), is "an attempt to force nature into the preformed and relatively inflexible box that the paradigm supplies. No part of the aim of normal science is to call forth new sorts of phenomena; indeed, those that will not fit the box are often not seen at all. Nor do scientists normally aim to invent new theories, and they are often intolerant of those invented by others. Instead, normal-scientific research is directed to the articulation of those phenomena and theories that the paradigm already supplies" (p. 24). Extra-ordinarily normal science involves the hardening of the box that bounds the paradigm. How could this be done? One suggestion is that it might result from the embodiment of the paradigm in the inaccessible workings of automated machinery, and/or the similarly inaccessible workings of a functionally rationalised work procedure. The scientific paradigm, and the knowledge that it represents, is black-boxed. A hardening of the boxes of this kind would be a sclerotic pathology of science. In normal science, the box that represents the boundary of the paradigm can be broken open by human insight and creativity. Black boxes are built to be indestructible.

C8 Work, Modernity, and Big Science

Doing and Hilgartner (2006), in the abstract for their session at the 2006 *Society for the Social Studies of Science* conference, state that "[t]he laboratory and the factory are two of the central symbols of modernity, and they stand in a dialectical relationship to one another in a way that broadly parallels the dialectics of fact/technique, designer/operator, worker/knower, and creative/routine" (p. 208). This thesis is, in part, the result of the realisation that the work of big science, and of the Institute, can be seen as embodying a combination of these contradictory forms of social organisation. They are laboratory *and* factory, and in being so, big science institutions can be seen as thoroughly Modern workplaces.

Grint (1998) provides us with a 'history helix' to help us to understand the qualities of work. This charts the differences between 'pre-industrial', 'industrial' and 'post-industrial' modes of work. The changes that this text-book summation takes to characterise the transition from pre-industrial to industrial forms of work can be seen as capturing, to an informative degree, the changes that mark the move from small science to big science. The moves from patriarchy to hierarchy, from craft production to mass production, from autonomy to heteronomy; all these are features not only of the industrialisation of economic activities outside the special society that is science, but also of the industrialisation of science. Taken in this way, it should be natural that a similar sort of gaze be cast on the Institute as has been cast on the industrial workplaces of the past.

However, as we see in Chapter J, *The Institute and the Knowledge Economy*, for some time now debates have centred on moves away from industrialised forms of work. This move has been labelled post-Fordism, post-industrialism, a facet of post-Modernism, and the turn to a knowledge economy. However, as Grint (1999) writes, these new ways of working embody "shadows of the pre-industrial past" (p. 320). However, just as science is 'industrialising', so, the narrative goes, much of the economy is turning to knowledge work, the post-industrial character of which involves the adoption of characteristics of small science (Kleinman and Vallas, 2001). In this, autonomy finds a post-industrial shadow in 'self-discipline', craft production in 'niche production', patriarchy in 'trust' (Grint, 1999). In Chapter H, *The High Road to the Human Genome*, we see just how these apparently paradigmatic features of contemporary knowledge economy work can be seen, in so much that they are manifested in the Institute, as the hangovers of the culture of small science. It is not so much that Institute is post-industrial, but, to take for a moment a vision of development as linear and progressive, that the Institute was never thoroughly industrialised. When Kinsella (1999) writes that "the characteristics attributed to contemporary knowledge-intensive organisations are exemplified, perhaps most clearly, in the work of organized, large scale science" (p. 177), he is suggesting that a form of work that proved more resistant than most to the triumphs of Modernity should be taken as a model of the future of work.

Indeed, big science, in the form of the Human Genome Project and the Institute, seems to be resistant to many of the contemporary trends that we diagnose. For Watson (2003), post-Modernity involves 'the fragmentation of existing patterns'. Thinking in terms of work, the post-Modern condition of "heterogeneity, plurality and constant innovation" (Watson, 2003, p. 70; quoting Best and Kellner, 1991) can be seen another description of the post-Fordist model of organisation in a 'knowledge economy'. However, while the project to sequence the human genome relied on ever improving automation, this is not the 'constant innovation' of post-Modernity, but the simple, linear progress of Modernity. And far from an expression of homogeneity and plurality, the Human Genome Project involved the concentration of resources in a single, standardised task, which was itself increasingly geographically and organisationally rationalised. The Human Genome Project was a thoroughly Modern endeavour.

As much as the character of the Human Genome Project swims against the tide of post-Modernity, so it resists the wave of globalisation. Even as it accommodates the global in so much that it is a collaborative project, and as much as it is a work of science, which, as we have seen, imagines itself as a field of human labour that effortlessly crosses borders and cultures, the Project was a triumph of state funding and national glorification. While Watson (2003) references Lash and Urry (1987) to discuss the way in which capitalism is increasingly disorganised by the waning powers of the nation state and organised labour, the Human Genome Project was the state-funded organisation of biology, bringing many aspects of the disorganised diversity of small science into concert. And, once the Project was 'complete', its accomplishment was appropriated by heads of government to bolster the narratives of national success. There are few other scientific projects that find their accomplishment announced not by journal publication, not even by press release, but by presidents and prime ministers. The Human Genome Project may not have been as centralised as the Apollo Program, but that is not the appropriate comparison. The historical movement was not one of decentralisation and increasing heterogeneity, as would mark a move from the Apollo Program to the Human Genome Project. Rather, it is a move from disorganised small science biology to the big biology of the Human Genome Project. In this, it is the similarities with the Apollo Program, as an ideographic reference, and the discontinuities from small science biology, as a historical ancestor, that are of interest.

The Human Genome Project appears, therefore, to be an island of Modernity in a perceived sea of post-Fordism, post-industrialism, and post-Modernism. Perhaps this view is the result of an error scale in our cartographies; perhaps the sea is not so big as it seems, perhaps there is more land, sub-continents, continents even, of Fordism, industrialism and Modernity. This would certainly be the response of many who see the mapping of these trends as exaggerations (Callinicos, 1989). Or perhaps the Human Genome Project is a Surtsey in this sea, a spectacular and captivating eruption of Modernity, but one that results in only a small, steadily eroding outcrop nonetheless.

The following chapter, Chapter D, *Case*, considers the ways in which a case study such as this exploration of work at the Institute can find application and value in other settings. Knorr-Cetina asks us to "consider the possibility that laboratories exemplify features also present in organized settings such as the clinic, the factory, the garden, the government agency" (quoted in Kinsella, 1999, p. 174-175). And as Kinsella noted, work in the knowledge economy is perhaps exemplified by work in 'organised, large-scale science'.

PART TWO: STRATEGY AND METHOD

The second part of this thesis addresses questions of strategy and method. What kind of evidence is used in this thesis? What are the particulars of the case, and how was the research conducted? Part Two is divided into two chapters, Chapter D, *Case* and Chapter E, *Conduct*, and addresses both the theoretical and practical aspects of gathering and using evidence. Chapter D, *Case* discusses the strategy employed. The empirical exploration in this thesis is a case study. After addressing the particulars of the Institute, this chapter considers the ways in which this example can serve as a scaffold for more general understanding. Questions addressed include; What kind of case study is that of the Institute? What sort of understanding of the social world can be gained through a case study? Can this case study have generalisable or transferable qualities?

Chapter E, *Conduct* discusses the use of interviews as a research method and the practice of the research in this instance. Questions that are addressed include; Why use interviews to conduct social research? How were the interviews developed and conducted? What sort of evidence do interviews gather, and how is this evidence used? Considering the fieldwork experiences, this chapter reflects on the effects of location on the interview, on the method of recruitment, and on an uncomfortable challenge to the anonymity of the research participants.

These two chapters bridge the gap between discussions of the history of the Human Genome Project and the sociological literature on big work and big science, and the contextual, living meaning derived from the evidence gathered. Throughout, this section steers a path mapped so as to avoid falling into 'methods talk' (Gubrium and Holstein, 1997), concentrating on connecting the discussions of strategy and method to the study of the Institute as case and in wider context, and the actual practice of research.

[D] CASE

The word 'case' suggests the particular. When used in phrases such as case study, the word case is used to denote an instance. The connotations of this meaning of case suggest that we ought to be pessimistic when considering the ability of case studies to provide knowledge and understanding of the world outside the particulars of the case in question. However, case can also be used to as a synonym for box or frame.

A case study that produces only a statement of the particulars of an instance has only a weak claim to be sociologically interesting. But a case study can also produce the outline of a general social setting, providing us with an example, an ideal type perhaps. Seen in this way, a case study can be used to build a theoretical and/or archetypal frame which we can then use as a scaffold. This scaffold supports our attempts to build an understanding of the social world beyond the particulars of the study in question. A case study that embraces this second meaning of the word case has the potential for transferable sociological value.

D1 INTRODUCTION

This chapter considers what it means when we describe this study of the Institute as a case study. Section D2, *The Institute*, provides sets the scene for the case. This section presents some of the particulars of the setting explored in this thesis. We are presented with a short history of the development of the Institute and a description of the Institute as it exists. These descriptions provide a material context in which to position the accounts of the research participants that are used to illustrate the empirical analysis of Part Three. Section D3, *A Typology of Case Studies*, considers the ways in which case studies have been differentiated into categories. The section asks how the research that makes up this thesis could be described according to these types. Section D4, *Case Studies of Work and Science*, is a short tour of the way in which the case study has a rich history in providing insights into the worlds of work and science. Qualitative case studies are the pre-eminent method for gathering rich, contextual evidence. Finally, in section D5, *Monsters and De-Monstrations*, the chapter closes by suggesting several ways in which this research might serve as a scaffold for wider understanding. This case study has the potential to illuminate settings beyond the instance of the Institute. Given the setting, the evidence gathered, and the focus of the analysis, light may be cast upon the workings of the social world of science, upon a subset of that social world in the form of peculiar world of big science, and upon the much broader social domain of work in a technologically advanced economy.

D2 THE INSTITUTE

The Institute was founded in the early 1990s, jointly funded by a major medical research charity and the Medical Research Council. When the Institute was founded there was just 7 staff, rising to 34 by the end of the first year. By the end of 1999 there were over 500 people working at the Institute. By the time the research fieldwork was conducted in 2006, the Institute employed over 800 people (Powell, 2006). Purpose-built laboratories were opened in 1996, and a major expansion to the Institute having been built between 2001 and 2006. In time, the Institute was joined at its site in the English countryside by other molecular biology organisations to form a 'campus'. In particular, a world-leading bioinformatics institute was relocated to the site from continental Europe over several years during the mid-1990s.

The Institute was one of the G5 laboratories, and played a leading role in the publicly-funded Human Genome Project. It sequenced approximately one-third of the human genome and, in the process, helped to develop the techniques, technologies and protocols for genome sequencing. Politically, the Institute was an important site in the Human Genome Project, not only because it internationalised what was otherwise an overwhelmingly an American project, realising at least part of the global aspirations of the public project, but also because those at the Institute, including the founding director, played a key role in arguing that the human genome sequence should be public property.

In 2000, the founding director handed over the leadership of the Institute to an American scientist from outside the Institute. As a specialist in animal models of genetics, not, notably, in the sequencing of genomes, the new director was a key part of the re-orientation of the Institute from genome sequencing goals towards an emphasis on translating sequence information into biomedical benefit. To fulfil this vision, the Institute recruited scientists to establish laboratories to conduct relatively small science projects. These laboratories benefit from the genomic resources, the funding, and the prestige of the Institute. This re-orientation can be seen in the way that the balance of employment at the Institute changed between 1999, the height of the race to sequence the human genome, and 2006, when the fieldwork visits were conducted. In 1999, over 60 per cent of those employed at the Institute were employed sequencing genomes. This workforce of over to 300 people produced approximately 25 to 30 million bases of sequence each day. By 2006, the same amount of sequence was being produced by approximately 130 people. This is fewer than half the number of people employed in equivalent roles in 1999, but the most important change is the one of balance. By 2006, well over 80 per cent of the people working at the Institute were working on a range of smaller scale research, building on the genomic information produced over the previous decade. The sequence has become a platform (Powell, 2006), a piece of the infrastructure of science. The major medical charity that had backed the Institute from its inception confirmed this re-orientation with a commitment of several hundred million pounds of funding in 2001. This funding also brought with it a change in name, a symbolic mark to the end of a phase in the history of the Institute.

The changes in sequencing productivity over the history of the Institute are startling. Between 1995 and 1999, there was a six-fold increase in output, with a similar increase between 1999 and 2001. Since the end of the race, the increase in output has slowed, but not stopped; in 2005, the amount of sequence being produced had increased by one-third on the figures from 2001. Similar development can be seen in the decreasing cost of sequencing each base. In 1998, it cost around 16 pence to sequence each base. By shortly after the end of the race, in 2002, this had fallen to two pence. Unlike output, this development did not slow after the end of the race; by 2005, the cost of sequencing a base had fallen to just 0.07 pence (Powell, 2006). With the end of the race there was far less demand to sequence a lot of DNA, but there is always pressure to sequence more cheaply, especially when the goal cannot be described as a mission to safeguard common human heritage.

The Institute is set in the English countryside, a few miles from a historic university town. A security building controls access to the Institute, but there are no forbidding fences or conspicuous patrols. The campus bears few marks that provide any clue to its previous incarnation as a site for industrial research dating back to the 1950s. The buildings are clean and clear-lined; frontages of glass and light brickwork indicate that the Institute was built in the period that straddled the Millennium. Invention and originality in the design and layout of the buildings differentiates them from the office buildings that are thrown up on commercial estates, though they clearly belong to the same family, the architecture of the Institute creates a cutting-edge atmosphere. The Institute is given an open feel by the pedestrianisation of the areas between the buildings, with gentle slopes and curves characterising the space. Set away from the laboratories is a listed country house that provides comfortable conference facilities. Public artworks bring a distinctive character to the Institute. These artworks include a spiralling wooden sculpture representing the DNA of the genomes that have been sequenced at the Institute, and, interestingly, a stained glass window using the symbols of contemporary genetics to ape religious iconography. The community pavilion is one of the more distinctive buildings; a glass-walled building with a curved shell of a roof, from some angles it appears as a concrete wave.

The interiors of the buildings are determined by their functions. Once we are beyond the foyer, the coffee shop and snack bar, and the reception with an LCD display that tracks the number of bases sequenced, the main building is a series of plain white-walled labs, indistinguishable from their counterparts in academia. It is in this building that the laboratories of the Pre-Finishing labs are based, as are many of the small-science faculty[14]. Other buildings have a distinctly different feel. The building in which the Finishing teams work houses open-plan offices that are similar in layout and feel to the workplaces found in many contemporary settings. Members of the Finishing teams work at individual workstations that can be observed from without, through the windowed walls, and within, from the offices of the team leaders that are sectioned off from the central workplace. Though obvious care has been taken over the styling of these interiors, this does not set these spaces apart from a recently built workplace of the administrative division of an anonymous company.

[14] Before visiting the Institute the researcher would have described these as post-genomic projects. However, it was forcefully pointed out by one of the research participants that these projects were not post-genomic. Sequencing the human genome, or any other genome, does not propel biology into an age after genomes, but an age during which genomes are a central feature of the work. The research participant insisted that these small-science projects were the epitome of biology in the genomic age.

The exception to this clean contemporary architecture, a small city of science, is the building that was the temporary home to the Production teams; the Sequencing Centre. This was not a building of light brickwork and glass, but a one storey building of prefabricated metal walls and small windows. This building housed the laboratories used for preparing the DNA for sequencing, the offices of the team leaders and co-ordinators, and the row upon row of sequencing machines. One of the research participants referred to this building as 'the cowshed'. This was an incongruous addition to the landscape of the Institute, which was, in the main, and especially the 2001-2005 expansion, designed to be accommodated into the landscape of the surrounding countryside. This building was dismantled when the Production teams were able to move back into a permanent home in 2007.

This, though, should not be taken as the picture of the Institute throughout its history. Major changes were made to the Institute between the sequencing of the human genome and the fieldwork visits. The most significant changes were the expansion of the Institute, which was begun in 2001 and completed in 2005. These changes reflected the re-orientation of the scientific mission that animated the Institute, with a move from large-scale genome sequencing to a diversity of smaller scale laboratory projects. This did not go unnoticed by the new director, who remarked when the Institute won a 2006 architectural award; "It's not only the physical structure of the campus that has been extended and remodeled, but our science also. We have added new programs that will bring biological discovery and medical understanding from our work on genomes" (quoted in Higginbotham, 2008). The end of the Human Genome Project, and the changing emphasis of the Institute, is therefore written in the steel and glass of the new developments.

As was noted in Chapter A, *Primers*, molecular biologists are inextricably bound to the business of biotechnology (Lewontin, 2001). Indeed, when the new director of the Institute was appointed a Fellow of the Royal Society, the press release boasted of his role in founding several biotech start-up companies (Powell, 2002). But this entanglement of the commercial and the academic is not simply found in the threads of individuals, but in the spaces of science. Universities often have space dedicated to start-up and spin-off companies that will commercialise the academic work being conducted. These spaces range from a collection of prefabricated cabins to dedicated, permanent science parks. Despite the seeming transmissibility of knowledge in an Information Age, proximity[15] of the sites of commercialisation to the sites of discovery and invention remains important. With this in mind, it should not be a surprise to find that the Institute has set aside land for an 'innovation centre' to incubate genomics-related start-up companies, seen as an essential part of translating basic research into medical systems and products (Higginbotham, 2008).

The chain of production

The sequencing of genomes at the Institute is conducted by in a manner that, in some respects, resembles a production line. Since the founding of the Institute, the technology of DNA sequencing has become increasingly automated. As one of the major customers of DNA sequencing equipment, the Human Genome Project, and, as part of that, the Institute, has not simply been a recipient of increasing automation, but also an instigator. Driving the development of automation were issues of "cost, space, flexibility and the requirement for high quality data" (West, Clee and Rogers, 2006, p. 169). By 1998, 250,000 reads were generated a month using Applied Biosystems 377 and 373 slab gel DNA sequencers. These sequencing machines were labour intensive, requiring technicians to pour and load more than 300 gels each day. By 2000, advances in the techniques and technologies, including the automation of pipetting through the use of robots, had quadrupled the number of reads to 1,000,000 each month. By 2006, this level of output was being achieved by just 50 people in a space half that of 2000; a reduction from 5,000 square metres to 2,500 square metres. Significant cost reductions are achieved through the use of automation systems that reduce the use of plasticware and reagents (West, Clee and Rogers, 2006). The 'big' of big science in the Human Genome Project was not produced simply

[15] The Institute itself illustrates the value accorded to proximity in science. While it is found in a fairly rural setting, it is around 10 miles from a major university town with a reputation and history in genetic science. Further, as its development as a campus demonstrates, the clustering of scientific work in one location is a persuasive model of organisation.

by the increase in the cost of large pieces of apparatus[16], as is the model supplied by social studies of big physics, but by the increase in the consumption of labour and materials. It is not a case of conducting the equivalent of one enormous experiment, as is the case with a supercollider, but of conducting a previously unimaginable number of small procedures.

In the course of this research, representatives from three parts of the 'chain of production' were interviewed; Production, Pre-Finishing and Finishing. As Alice, a manager in the Finishing team described, within this chain of production there is a hierarchy.

> So production is, as you say, the first step. And then finishing is a little bit, you have to be a little more intelligent, I guess, to do the finishing. Then from finishing you've got your annotation, which again you have to be more competent and they look for people that have got PhDs to do. So yes, there is that sort of hierarchy.
> [Alice]

Brian, a manager in the Pre-Finishing team offered a critical view of the development of such a hierarchy. He made sure that the researcher was aware that the task of sequencing genomes has not always been divided into distinct and separate jobs; there is nothing 'natural' about the division.

> As soon as that [the separation of the production and the finishing of sequence into separate teams] happened it was always seen that production were lower than finishers. Even if one couldn't survive without the other, it was always seen that finishing was a higher job than production. Whereas personally I don't think that it was.
> [Brian]

Finishing and Production became separate teams in late 1998, though the actual task of producing sequence was divided into the jobs of production and finishing

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[16] Indeed it is debatable whether automated sequencing machines are equivalent to supercolliders or radio telescopes. However, when we understand the equipment as a collective; an array of automated sequencers combined with the required supercomputing power, we produce a piece of apparatus that is perhaps comparable with the models of big physics. This understanding does reinforce the notion that the sequencing of the human genome a highly collective accomplishment; the combined effort of many nations, many centres, a great number of scientific and technical workers and an enormous (and continually updated array of machines).

well before this. In an e-mail, Peter, a Finishing Manager, attributes this to the 'managerial challenge' (Peter, 2006). This managerial decision physically separated the people producing genome sequence into separate teams. We discuss the development of an increasingly acute division of labour, including physical separation, and the attendant hierarchy, in Part Three, particularly Chapter F, *The Factory and the University.*

Figure D2.1 The Sequencing Chain of Production

Production is where the sequencing is carried out. Prior to the large-scale introduction of capillary sequencers at the turn of the century, sequencing was carried out using machines that used polyacrylamide gels, or slab gels as they are descriptively known. The workhorse machine of this technique was the ABI 377. Capillary sequencers have now replaced slab gel sequencers, and the standard model used in sequencing the human genome was the ABI 3700.

The terms 'slab gel' and 'capillary' refer to the technique by which DNA fragments are sorted by size. This sorting enables the products of chain terminator sequencing to be ordered into sequence. Compared to slab gel sequencing, in which the slab gels had to be prepared manually close to the time when they would be needed, capillary sequencing requires much less labour input. The fieldwork at the Institute included interviews with people who had spent their early days on the Human Genome Project making slab gels. According to Julia, who is now a Pre-Finishing Team member, during the late 1990s, as the Institute was scaling up to complete the sequencing of the human genome, the pouring of slab gels occupied about twenty people. Between them, working shifts, they made gels twenty-four hours a day, seven days a week, to ensure that that the sequencing machines never sat idle. The reduction in the amount of labour required to produce sequence does not stop with the elimination of the task of pouring slab gels. The integral sample loader and flowable gels of the ABI 3700 automated sequencer enables the machine to run technician-free. Michael Phillips of ABI describes this as 'walk-way automation' (Hodgson, 2000).

The move to capillary machines was also an improvement in quality in a number of ways, not least the removal of the 'green-fingered' process of producing slab gels. Hodgson (2000) quotes Chris Clee, of what was then the Sanger Centre, as saying:

> Gel quality varied from batch to batch and depended on who was preparing it [...] Even the time of year made a difference. Gels were better during the summer because of the lab temperature, although sometimes that affected the water quality.
> (Clee, quoted in Hodsgon, 2000, p. 40)

More than this, slab gel sequencing produced problems for the optical readers that transform the separation of fragments of DNA on a gel into data. With 96 lanes on a gel, the reader would sometimes pick up the fluorescent readings of adjacent lanes, which then required the finishing team to compare the reads from adjacent lanes (Hodgson, 2000).

Pre-Finishing involves a variety of laboratory procedures designed to streamline the finishing process. The laboratory procedures are designed to improve the sequencing of low quality regions and fill gaps. Directed sequencing reactions are conducted to resolve problem areas of sequence. This process can take several rounds of reactions.

Finishers use computer programmes such as Phred, Phrap and Consed to assemble the sequences. Phred 'calls' the bases, on the basis of the strength and spacing of the fluorescence reads, Phrap is a tool that helps a Finisher make judgements on assembly by comparing the overlap of sequences, while Consed helps Finishers examine the quality of sequences from overlapping regions and determine what new sequencing runs are required to improve data quality in particular regions or to fill sequence gaps. As we see in Chapter F, *The Factory and the University*, this job is seen as requiring a greater degree of formal education than the other two aspects of the chain of production considered in this study. Nevertheless, these tasks are also subject to automation. Hodgson claims that the ‘Autofinish’ module “can reduce the decision burden for the human finisher by 85 per cent” (2000, p. 42).

D3 A TYPOLOGY OF CASE STUDIES

Bryman (2004) distinguishes between several different kinds of case studies[17]. The ones that are relevant to a discussion of this case study are the 'unique' case, the 'revelatory' case, (both drawn from the classifications of Yin) and the most common form, the 'exemplifying' case. Into which of these categories does a qualitative investigation of work in the Institute fit most comfortably?

Bryman (2004) describes the unique case (or the extreme case) as those instances where interest is granted by exceptionality. Clearly, a case study of the Institute, as part of the Human Genome Project, could be categorised as a unique case. The Institute, particularly during the sequencing of the human genome, is unique historically and disciplinarily; a representative of a particular, now completed project that adopted a degree of industrialised organisation that is unlikely to be seen again in biological research. This is the category of case that best fits the view of the Institute as a 'monster', as considered in the next section of this chapter. Unique cases are not without sociological merit. Their position in the literature of sociology is certainly not marginal; Bryman suggests that Mead's study of childhood in Samoa and Fielding's study of the National Front are examples of cases that are unique. However, the derivation of transferable qualities is not so straightforward. A qualitative sociological investigation of the experience of work at the Institute has intrinsic interest on the basis of its existence as an extreme case, but if we can draw parallels, which begin to dissolve the sense of uniqueness, then there is much more that can be gained.

Bryman (2004) describes the revelatory case as one that permits research into phenomena that was previously inaccessible. We can see how the Institute can be seen in this way if we consider the quote from Bruno Latour which we visited in Chapter B, *Anatomies of Big Science*: "The small and invisible are made large, the large and unemcompassable are made small. The fast are made slow and the slow are speeded up" (quoted in Capshew and Rader, 1992, p. 18). As suggested when

[17] We should remember that "[c]ases are always hypotheses" (Walton, 1992, p. 122). The identification of a case *as* a case betrays our assumptions of the world; the kinds of social units that are intelligible, the borders of these social units, and, not least, their importance.

we first visited this quote, which is a description of the 'drama of scale' at the heart of science, sociology, unlike the natural sciences, does not possess the apparatus to magnify the world to peer into the smallest spaces, or alter the scale of the social in any way. But these apparatus, this ability to alter the scale of the world, allows scientists to gain knowledge of the previously inaccessible; they are revelatory. If we are to seek revelation of sociological knowledge by these principles, we cannot hope for the development of apparatus[18], but must rely on 'naturally' existing alterations in the scale of things. This is one way in which the Institute is a 'de-monstration'; by the rapidity of its development and its very size, it might allow us insights into process usually hidden by the scales of space and time.

Bryman (2004) describes the exemplifying case as being chosen "because they will provide a suitable context for certain research questions to be answered" (p. 51). In this sense, we might examine the notion, which is drawn from existing social theory and set out in the Chapter C, *Pathologies of Work in Big Science*, that big science organisations are driven towards rationalisation, bureaucratisation, and with this comes the concomitant alienation of those people whose work lives are conducted within these organisations. The Institute, as a particular instance of a big science organisation, would be an exemplifying case. However, as Bryman notes, "[o]ften, what a case study exemplifies will only become apparent *after* the case study has been carried out. It is only at a very late stage that the singularity and significance of the case becomes apparent" (p. 52). As the research progressed, it became apparent to the researcher that the Institute could serve as an exemplifying case for social processes beyond the limited domain of big science; it could shed light on the experience of work in contemporary science, and, even more generally, experiences of work in technologically advanced economies. Through this, a case study of the Institute might have something to say in debates about the 'knowledge economy' and the future of work. These are explored in Part Three, *Illustrating and Accounting*, and in the final chapter, Chapter J, *The Institute and the Knowledge Economy.*

[18] Some, such as Savage and Burrows (2007), do argue that empirical sociology is on the verge of radical transformation as a result of the development of new, largely IT-based methods. These arguments underplay the power of existing empirical methods of sociology.

Hartley (1994) has written on the use of case studies in organisational research. Hartley identifies the strength of case study research is the maintenance of context. Conclusions are reached inductively from the data collected during the detailed investigation of the case. Hartley argues that, as a result of the maintenance of context in case study research, "there will always be too many 'variables' for the number of observations made" (p. 209). This goes some way to ameliorating concerns that pervade this research that simple, smooth sense cannot be made of the Institute. Law (2004) urges that care be taken when attempting to make simple, smooth sense of our research, and the world. Not only is the world patently not shaped or textured so smoothly, but in producing such stories we are in danger of simply reproducing the opportunistic (Myrdal, 1970) beliefs about the world held by dominant groups.

Criticisms of case study research include the argument that case studies lack "rigour and reliability and that they do not address issues of generalizability" (Hartley, 1994, p. 208). The simplistic inversion of these arguments stresses the power of case study research to provide 'rich' and 'meaningful' data, "shed[ding] light on the fine-grain detail of social processes in their appropriate context" (p. 208). Hartley offers a more sophisticated elaboration on the strengths of case study research, arguing that "case study methods [...] are more likely to be better able to adapt to and probe areas of original but also emergent theory" (p. 210).

D4 Case Studies in Work and Science

Case studies have formed the backbone of studies of both the workplace and the laboratory. Many of these studies are ethnographies, of one kind or other. This thesis relied on evidence gathered through interviews. In Chapter E, *Conduct*, the choice of interviews as a research tool, and the sort of evidence that interviews produce, are discussed. In the studies of science, ethnographic explorations such as Latour and Woolgar's *Laboratory Life* (1986), a first edition of which was published in 1979, is the archetypical text. Latour spent time in a neuroendocrinology laboratory, and was granted remarkable access and his own office space as he engaged in an observational study of the social process by which scientific laboratories produce knowledge. Latour and Woolgar note that the laboratory is, in some ways, unlike a factory. Bench spaces and the office spaces combine to produce papers in a process of 'literary inscription'. Science is primarily a literary activity, a process of reading and writing. Latour and Woolgar note that much of the informal communication taking place in the laboratory was concerned with the substance of formal communication. This even applies in the laboratory itself through the use of inscription devices, 'which transform pieces of matter into written documents' (p. 51). The Human Genome Project, for all its industrial trappings, was a process of mass inscription; the transformation of a three-dimensional molecule into a string of letters. The length of the inscription was such that the production of the results on paper would be a pointless, wasteful exercise. When science is engaged in mass inscription, the devices of the 'information age' are required.

Studies of big science, of the modes of work that the Human Genome Project resembles to a greater degree than it does the small(er) science laboratories that Latour studied, have also tended to be built around the case study. Galison and Hevly's (1992) *Big Science*, is a collection of case studies, some historical, some ethnographic, such as the chapter by Traweek (1992). A more recent addition to the literature has been Collins' (2004) *Gravity's Shadow*, which built on decades of observational research and interviews with researchers attempting to detect gravity waves. Some of this work is incorporated into Chapter B, *Anatomies of*

Big Science, as Collin's (2003; 2004) study of LIGO demonstrates one trajectory that the management and organisation of a big science institution might take.

Case studies have also been one of the standard research strategies of the sociology of work. Many of these case studies concentrate on the collection and analysis of qualitative data. Examples include Beynon's *Working for Ford* (1973 [1984]) and Burawoy's (1979), in *Manufacturing Consent*, in which the researcher spent ten months as a machinist at a factory in Chicago. The great regret contained in this thesis is the fact that the evidence for this research does not emerge from such in-depth ethnographic observation and evidence gathering; compared to the studies of the sociologists of work in the examples of Beynon (1973 [1984]) and Burawoy (1979), and the sociologists of science in the examples of Latour and Woolgar (1986) and Collins (2003; 2004). Nevertheless, for all that this thesis is missing in comparison to these classic case studies, some of the strengths of those studies can be replicated. Despite the limits of interviews as a means of gathering sociological evidence, as discussed in Chapter E, *Conduct*, this thesis can marry the richness of the contextual description of the setting, its lived experience, to the wider currents of history and economy. As was highlighted in Chapter A, *Primers*, Mills (1959 [1970]) described the task of sociology as being the method by which people are given the tools to "grasp what is going on in the world, and to understand what is happening in themselves as minute points in the intersections of biography and history" (p. 14). This thesis is an attempt to connect the specifics of the local, the peculiar even, to wider changes in the conduct of science and work in the early twenty-first century.

D4 Monsters and De-Monstrations

Walton (1992), in his account of conducting a case study of 'California's little civil war', describes a process by which a case study, initially framed as the study of one thing, becomes, as the researcher moves back and forth between the case, history, and theory, a study that is appropriately framed in a different manner. Hartley (1994) argues that as "case studies may begin with (in some situations) only rudimentary theory or a primitive framework, they need to develop theoretical frameworks by the end which inform and enrich the data and provide not only a sense of the uniqueness of the case but also what is of more general relevance and interest" (p. 210). It is through theoretical connection of the instance of the case with the wider currents of history and economy that the case study finds the mean to fulfil the purpose that Mills' (1959 [1970]) assigned sociology. Case study research can thus marry the richness of the storytelling afforded by the narrative and description of the instance with meaning over and above that of 'the story' alone. The popular accessibility of books such as *Working for Ford* (Beynon, 1973 [1984]) as provides a model in which the poverty of meaning found in description, however richly detailed, is alleviated, and the work enriched through the intellectual labour of analysis and contextualisation.

This thesis was imagined, in the first instance, as having transferable value in a very limited sense, to the growing number of laboratories practising big(er) biology. In the fieldwork visits to the Institute, through the preliminary analyses of the interviews, and as a consequence of engagement with a wider range of theory, this study grew into being understood as a case of an event, or perhaps a location, within a technology advanced society with claims to be moving towards a knowledge economy. This iterative process provides this thesis with claims to be a scaffold with a wider set of uses than simply an aid to examination of big biology.

A question that surfaced time after time in the production of this thesis was that of 'monsters' and 'de-monstrations'. Should the case study of the Institute, as part of the Human Genome Project, be seen as a:

[1] Monster. A product of exceptional forces, and therefore is an example only of a limited category of its own?

Or a:

[2] De-monstration. A product of the same currents of history and economy that shape other aspects of the society, whether small science, technological advanced work, or more general features of society, which therefore possesses value beyond the investigation of this particular instance of big science?

The degree to which a case study is de-monstration or monster is not simply an essential product of the features of the case at hand, but is also dependent on the way in which the case is considered. This is not to say that any case can be an exemplar of any thing, irrespective of its actual features. But it is a recognition that a case, in so much as it exists as a sociological object, only exists once the study has been written. An approach that can see the case as nothing but a monster, produces a monster. An approach that attempts to draw connections between the 'monster' and the wider contexts of history and economy, produces a de-monstration.

In which domains can this study of the Institute find use as a scaffold? The first domain to turn to is that of the workplaces of science. What purpose do narratives of, for example, an increasing division of labour, increasing degrees of rationalisation, the development of laboratory automation, and the growth of research teams, serve when applied beyond the Institute, beyond even the 'grotesques' of other big science projects? If we revisit Bruno Latour's elegant description of the conduct of science, we find in it the basis for an argument that the case of the Institute ought to be of interest to those concerned with the world of small science. It should not only be of interest to those fascinated by rare instances of the massive. "The small and invisible are made large, the large and unemcompassable are made small. The fast are made slow and the slow are speeded up" (Latour, quoted in Capshew and Rader, 1992, p. 18). This 'the drama of scale', which we described in Chapter B, *Anatomies of Big Science*, is at the heart of the claim that the case of the Institute can enlighten understandings of small science. The argument runs; The physical sciences are able to use microscopes, mass spectrometers, indeed, even DNA sequencers, in order to make small objects appear to be big. The objects, enlarged, are then accessible to investigation and understanding. Sociology has no such apparatus to hand. If it is the case that big science is shaped by exceptional drivers, then it is a monster, to which our reaction must, at one level, be to marvel, rather than to use it as a measure of the social world of science. But this is not the case. Big science is subject to the same economic, political and social drivers that shape and reshape the work of small(er) science. Biological science, in general, is a world in which papers are 'authored' by an increasing number of contributors, where there is pressure to bring teams together in collaborations, and bring together different kinds of scientists in multidisciplinary work. In this, big science offers us a demonstration of the possible future shape of work in science.

This was once the limit to the imagined claim for de-monstration for this thesis. In the production of this thesis the case has acquired its theory and context, as Walton (1992) described. Even if the big science of the Institute is qualitatively different to the science found in other settings, in academia and in industry, rendering the case useless as a scaffold in these domains, this does not exhaust the uses to which the scaffold of this thesis can be put. The features that might make work at the Institute distinct from the emerging modes of practice in biology, such as the 'extra-ordinarily normal' nature of the science, the similarity of the Human Genome Project to a 'construction' or 'process engineering' project, and the overwhelming proportion of technical and scientific workers to professional, career scientists, work to strengthen comparisons with other forms of work in a technologically advanced economy. The form of organisation of this labour, and the subjective experience of this labour, is a question of significance, given the rhetoric that suggests that the development of a knowledge economy is a transformation of society and economy of the same scale as the Industrial Revolution. In this transformation to a knowledge economy, biotechnology, of which the Human Genome Project is imagined a fundamental infrastructural element, is considered an economic sector on which we will rely for our future wealth.

The Institute, which played a key role in building the foundations for future biotechnology work and, as a consequence, the development of a knowledge economy, can therefore serve as a site for exploration of the organisation and subjective experience of 'knowledge work'. In addressing the framing of the case study in this way, this thesis engages with contemporary ideas of the organisation of work, particularly, in Chapter H, *The High Road to the Human Genome*, with the 'high road', 'high commitment' organisations and cultures that are often associated with successful technologically advanced workplaces. Chapter J, *The Institute and the Knowledge Economy*, which concludes this thesis, draws connections between work at the Institute, and the Human Genome Project in general, with ideas about knowledge economies. For all that the Institute is a monster of big biology, alien to the social world of small science, and even if the Institute is the last example of centralised big biology, this case still finds use to which the scaffolds built here can be transferred.

[E] CONDUCT

In our role as social researchers we conduct interviews. What is signified by the word conduct? First, the imagination is drawn to an image of a conductor leading an orchestra. Do we, as social researchers, orchestrate our interviews? But this is not the only image evoked by the word conduct. The word conduct is also used in the physical sciences to describe the transmission of energy. Are social researchers conductors in this sense? Do we put one hand on the social world, the other on our word-processors and, through us, allow the truth to be transmitted? A naïve empiricist might hope that we are the latter. If researchers are passive transmitters of the facts of the world, then we do not need to reflect on the process. As social scientists we realise that this is not the case. We are, inevitably, the orchestrators of the evidence that we collect. We shape the pace, the rhythm, and the tone of a focused conversation in order to produce a product. Conducting an interview is a deliberate act of skill. This chapter reflects on this process.

E1 INTRODUCTION

Fontana and Frey (1994) argue that interviewing is interaction and sociology is the study of interaction. Silverman (2001) cites Denzin to describe the research interview as 'focused interaction'. The interview, therefore, is both the tool and the object of study. This chapter discusses the use of interviews in this research. Interviews are considered on both an abstracted, theoretical level, and in terms of the conduct of the interviews in the field.

Section E2, *Why Choose Interviews?*, examines the choice to use interviews to explore the subjective experience of work at the Institute. This section argues that there is a 'narrative gap' in the existing discussions of the Human Genome Project. This 'narrative gap' leaves the bench workers, the army of technical, scientific workers who did the day-to-day sequencing of the human genome, unrepresented. The 'virtues' of interviews are considered. The strengths of interviews, such as flexibility and the ability to reconstruct past events, and weighed against their weaknesses. Interviews are identified as being a social research tool particularly suited to exploring the subjective experience working in large-scale genome sequencing.

In section E3, *Structure: Form and Content*, the interview itself is examined. Considering, first, 'form', arguments relating to the concept of structure in interviews are rehearsed. The interviews used in this research are positioned within the categories that are found in the literature. Turning to 'content', the themes explored in the interviews conducted at the Institute are discussed. The context of the interviews is reflected upon, with the questions of place and the identity of the interviewer considered.

This chapter covers questions of access and sampling in section E4, *Access and Anonymity*. The process by which access to the Institute was gained, and maintained, is discussed, and the process of recruiting participants is considered. A table of interviewees is provided. A 'tale from the field' is recounted, in which the conduct of academic research at the Institute rubbed up against the interests of

Public Relations. As much as any evidence gathered through the use interviews, this event highlights ways in which the Institute is unlike an academic organisation, but rather shares features with bureaucratic organisations.

The question of evidence is discussed in section E5, *Evidence and Illustration*. The process of content analysis is summarily described, before the question of understanding the variety of accounts provided by research participants is addressed. Discussing both ideas of a 'Rashomon effect' and Gilbert and Mulkay's (1984) use of the idea of 'linguistic repertoires', this section suggests ways in which the evidence gathered in the course of the exploration of the Institute can be understood as sociologically valuable illustration. This discussion sets the ground for Chapter F, *The Factory and the University*, in which quite different ways of understanding the Institute as a workplace are analysed.

E2 Why Choose Interviews?

The Narrative Gap

The Human Genome Project and the new genetics are not orphan-subjects. Neither the Human Genome Project, nor the field in which it is historically and scientifically located, want for commentaries, analysis, histories, and, even, futurologies. Despite this, the choice to use interviews to gather the evidence used in this thesis is the product of the view that, though they are ever narrowing, there are gaps in the literature that seeks to describe and explain the Human Genome Project and its position in and significance for society.

The existing accounts of the Human Genome Project are numerous. As described in Chapter A, *Primers*, these accounts include the journalistic (Davies, 2001; Shreeve, 2004; Wickelgren, 2002), those that examine the sequencing of the human genome from ethical and sociological perspectives (Clarke and Ticehurst, 2006; Glasner and Rothman, 2004; Kelves and Hood, 1992), and the first-hand accounts of powerful insiders (Bodmer and McKie, 1995; Sulston and Ferry, 2002; Watson, 2000). Savage and Burrows (2007) suggest that the saturation of narrative has made the interview an outmoded tool of social research. They write; "[i]t is now not very clear what the significance of the in-depth interview is in an age of *knowing capitalism* [...] the world-views of diverse populations [are] now routinely presented to us in the popular and new media in such a manner that their summary characterization by sociologists is no longer as necessary (or as interesting) as it once was" (p. 894). Setting aside the questions of the role of the sociologist as analyst rather than simple re-presenter of narratives, the 'popular and new media' has left the story of the Human Genome Project with unrepresented world-views. There has not been a 'saturation'. The gap which remains, when these existing accounts of the Human Genome Project are collected, is that of the bench workers who, day by day, sequenced the human genome. Not the Lancelots and Gawains that drew to themselves the glories of the quest for the human genome, but massed ranks of unnamed foot-soldiers. As Edwards (1979) argues, quoting Marx, the social world of production is a 'hidden abode'.

This is not just a question of the identity of the narrator, but of the content of these accounts. As Atkinson and Delamont (2006) argue; "While the 'voices' of otherwise muted groups may be charged with political significance, we cannot proceed as if they were guaranteed authenticity simply by virtue of narrators' social positions" (p. 170). This thesis does not seek to collect the perspectives of those who worked on the day-to-day sequencing of genomes, merely as a consequence of their previously unheard voices. Absent from the existing accounts are narratives that provide us with the means to grasp what it was to have worked on this big science project; as a technical worker on one of the great foundations of the knowledge economy; on a project with claims to historical significance.

Given the historical significance of the sequencing of the human genome, the absence of these accounts is no surprise. Journalists, sociologists and prominent scientists are keen to address the big impacts of big science, and the specific impacts of the knowledge at that; what does sequencing the human genome mean for health care, for equality, for the surveillance of populations? As described in Chapter A, *Primers*, Mills (1970 [1959]) argued that sociology is the means by which people are to be able to "grasp what is going on in the world, and to understand what is happening in themselves as minute points in the intersections of biography and history" (p. 14). This thesis considers the apparently big stories of big science to be well rehearsed. These stories leave standing the question; what was it to have one of the hundreds of scientific workers who accomplished the sequencing of the human genome? And what do their stories have to tell us about work in science and in technologically advanced economies in general?

Interviews and their Virtues

The interview, in all its variation, is a standard component of the toolkit of social researchers (Fielding, 1993; Bryman, 2001). The interview can, though, range from pre-scripted and therefore highly structured questionnaires, to open-ended, explorative unstructured 'guided conversations' (Lofland and Lofland, 1984). The interviews to gather evidence for this thesis fall toward the unstructured end of any continuum of structure; the questions were open-ended, the order of the questions rearranged according to the shape that a particular interview was taking, though each interview was conducted with a strong thematic focus and with the benefit of a detailed question guide. Fielding (1993) reminds us that qualitative research involves strategies of discovery, and, in pursuit of discovery, interviews have versatility as one of their strengths. Through the adaptability of interviews, and the flexibility of face-to-face interaction, the researcher is able to respond in ways that allow us to get some hold of the concepts and phraseology of the interviewees. 'Focused interaction' is an apt term (Denzin, cited in Silverman, 2001). A discussion of the structure of the interviews used in the exploration of the Institute is provided in the next section, E3 *Structure: Form and Content*.

O'Brien (1993), re-introducing the reader to sociology, offers various characterisations of the boundaries the discipline. Echoing the debates in the sociology of science (cf. Gieyrn, 1999), O'Brien offers brief portraits of parallel sociologies, with one having its boundaries delineated by the limits of the professional sociological hierarchy, another is bounded by its theoretical positions, a third is defined in terms the 'social problems' that it concerns itself with. Pertinent here, in this discussion of the methods used in order to understand the social world, is his description of a sociology defined by the "methods for studying empirical questions." O'Brien describes the breadth of the methods that mark sociology. "These methods... range from the observational (watching what happens), through the interactional (asking about what happens), to the mathematical (predicting and modeling what happens)" (p. 3). Interviews fall into the category of interactional methods. In discussing the choice to use interviews, we should reflect on the relative strengths of the interview as a means to access the social. Why not use observational or mathematical methods to explore the social world of the Institute? What are the features of the interview that makes it an appropriate tool to help us to understand what it was to have worked, ignored by the gaze of journalism, and when it condenses and congeals, pop-history, as one of the scientific workers who accomplished the sequencing of the human genome?

Patently, a 'mathematical' approach, as O'Brien (1993) labels it, will not help us to capture a subjective, qualitative understanding of what it was to work in the big science of the Human Genome Project. It can provide us with plenty of information, a wealth of apparently unproblematic data. Section E5, *Evidence and Illustration*, discusses some of the ways in which the evidence produced by interviews can be problematic. Sociological research that requires the researcher to interact with inhabitants of the social world in question produces empirical material that is far more rich and vivid than the thin, though plentiful, data provided by questionnaires and surveys (Gillham, 2000).

So, if 'mathematical' methods would have collected evidence that would not have helped us to explore the subjective experience of work at the Institute, perhaps more relevant is the question; why not use observational methods? Observational techniques have a distinct and distinguished history in social studies of science. As described in Chapter D, *Case*, Latour and Woolgar (1986) provided the pioneering text for studies investigating life inside a laboratory. The decision to employ observational methods to study science was an attempt by Latour to "become part of a laboratory, to follow closely the intimate processes of scientific work, while at the same time to remain an 'inside' outside observer, a kind of anthropological probe to study a scientific 'culture' - to follow in every detail what the scientists do and how and what they think" (Latour and Woolgar, 1986, p. 12). Chapter D, *Case*, also provided examples of well-regarded studies of the workplace. These kinds of observational studies also involved interaction, a necessary product of the researcher's presence. Given such eminent antecedents, what argument could there be in opting not to use interviews in an exploration of the experience of work in the Human Genome Project?

As Bryman (2001) points out, interviews allow the reconstruction of events, something that few other tools of accessing the social world can achieve. The Human Genome Project was 'completed' in draft form in 2001, and in 'finished' form in 2003. The fieldwork for this thesis was conducted during 2006. As O'Brien (1993) tells us, interviews are interactional methods, and interactional methods involve asking about what happens. This includes the capability to ask about what *has* happened. Time can be explored subjectively. An observational study would not have been able to capture the story of the development, growth, and, once the human genome sequence was complete, re-orientation and diversification of the Institute. An observational study, even when we consider the interaction that is a necessary product of the researcher's presence, would focus on the Institute as it is at the moment of research. Conducted in 2006, this would produce a study of the workplace that is described in Chapter I, *Interlude: Dis-Engagement and Dis-Integration*. Such a study would have failed to capture the experience of working to sequence the human genome, and would have lacked the subjective sense of history, with which we are able to understand the negative aspects of accomplishment.

There is also a far more pragmatic reason for choosing interviews over observational methods. Interviews are less intrusive. The disruption caused by the researcher visiting the site to conduct interviews pales when compared to the accommodation that would be required in order to allow the researcher to have extended access to laboratories and offices, and to the activities that are conducted in these workspaces. This is a particularly important factor when negotiating access to many organisational settings (Bryman, 2001). This research did not have the luxury of many possible research sites. As the discussion of 'monsters' and 'de-monstrations' that closes Chapter D, *Case*, suggests, as the research developed, the wider historical and social context became apparent, and the significance of the exploration of the case of the Institute changed from the intrinsic interest of the big science grotesque, to a more generalised interest in work in an emerging 'knowledge economy'. On entering the Institute, it appeared that much of its interest lay in its unique qualities. With this view, fieldwork access had to be gained to this site, or to no site at all. A conservative approach to research, with as little disruption caused to, or accommodation required on the part of, the research participants and the organisation that employs them was, therefore, a suitably pragmatic choice.

A further advantage to conducting research using interviews lies in the interview as an event. Gillham (2000) notes that the 'special occasion' of the interview often elicits rich evidence. The interviewee works hard to make the interview work. This "reflects the fact that people are often not listened to; that their views are not treated as being of any account" (p. 7-8). Though we should be wary of the way in which this phenomenon might be a product of the 'interview society'

(Atkinson and Silverman, 1997; Atkinson and Delamont, 2006), the exploration of the Institute could have benefited from this dynamic. Not only because the majority of the people that were interviewed were technical workers, distant from the public glories of the Human Genome Project. But also because, as we see in Chapter I, *Interlude: Dis-Engagement and Dis-Integration*, the Institute has been re-orientated, from a focus on genome sequencing, in which the people working in Production, Pre-Finishing and Finishing, however junior, were the point and purpose of the Institute, to, after the accomplishment of the sequencing of the human genome, a diversification of goals, with the growing faculty of small(er) science 'post-genomic' research taking centre stage.

The undisputed 'artificiality' of the interview should not be read as an unalloyed negative. Formal interviews, even those which are conducted in the place of work, as was the case in the exploration of the Institute, place research participants in a situation that is outside that of their day-to-day setting. The fact that the interviews conducted during this research took place inside the Institute during working hours is deceptive; a pre-arranged interview, in which an outsider takes interviewees aside for one hour long structured discussion, all of which is recorded using a digital sound recorder, is indisputably artificial. The first response of a qualitative social researcher might be to see such artificiality as a factor that works to inhibit the ability of the researcher to grasp the social world in question. A different perspective can be taken, one from which the artificiality of the interview is lit by a more constructive light. For a student of biology, analogy can be drawn with the virtues of *in vitro*, as opposed to *in vivo*, research. The generation of a small degree of isolation of the research participants from their context allows for in-depth probing of their views, attitudes and experiences (Fielding, 1993).

Artificiality, though, is not the only drawback to the interview as a tool of social research. As Gillham (2000) reminds us, the 'organised interview' has a formal quality that, no matter how loosely structured, associates them with a variety of undesirable interactions with power. The interview is something that most people encounter only when applying for a job, when being assessed at work, when applying for welfare benefits, or, perhaps, when interacting with the police.

For all our efforts, the association between the interview as a form of interaction and existing relations of power cannot be broken[19]. Calling the interview a 'chat' for example, has the potential to present as an even more sinister experience. One does not need to be particularly cynical to know that when a person of power invites someone for a 'chat', the term is, at best, simply a euphemism for an interview, but, at worst, represents the transformation of informal space and time into an event controlled and put to the use of the powerful. In the explorations of the Institute the researcher was an outsider, and the appearance of the researcher, as a young, informally dressed sociologist, should have suggested that he was not an agent of supervision and evaluation. Nevertheless, the fact that the researcher had arranged interviews through gatekeepers who occupied management positions was not unproblematic. Section E4, *Access and Anonymity*, discusses the recruitment of research participants.

[19] Perhaps this association does not exist in the case of 'elite' interviewing, in which the power dynamic is so obviously reversed. Elites, however, have their own negative associations with interviews, in, for example, the form of critical journalism.

E3 The Interview: Form And Content

Structure?

As touched upon in the discussion of the 'virtues' of interviews, 'the interview' can mean any one of a set of quite different social research methods. The customary continuum, upon which these different methods are differentiated as if they were DNA fragments being sorted by electrophoresis, is that of structure (Fielding 1993). This continuum runs from highly structured questionnaires in which almost every utterance of the researcher, or perhaps, given the degree of standardisation, an agent of the researcher, is scripted, through to unstructured 'guided conversations' (Lofland and Lofland, 1984). A discussion of the degree of structure present in an interview should not obscure the fact that all these approaches to gathering evidence are *organised* approaches. The use of interviews as a method of social research involves a process of theorising about the social world, forethought and planning, and the component that is hidden, as it is inadequately translated from action to written account; skill.

As suggested by section E2, *Why Choose Interviews?,* the interviews that were used to explore the subjective experience of work at the Institute were of the sort to be found towards the unstructured end of the spectrum imagined above. Gaining an understanding of the subjective experience of work at the Institute was a process of discovery, despite the prior insights of sociological theory, so the adoption of an interview style that asked open-ended questions in a flexible environment was appropriate. This flexibility enabled the interaction of the interview to respond to the circumstances within each instance of the interview, to the differences between interviews, and to the developing understanding of the researcher. By eschewing too much structure, we also provide the flexibility to "allow respondents to use their own particular way of defining the world" (Fielding, 1993, p. 151). The nomenclature present in the social research literature provides us with many labels to attach to this approach. Are these events non-standardised interviews, unstructured interviews, or focused interviews? For Fielding (1993), these are interchangeable terms for the 'guided conversations' (Lofland and Lofland, 1984) that make up much qualitative social research. But terminology is important; the impressionistic implications of 'unstructured' are quite different to those evoked by 'focused'. The choice of label reveals the virtues of the technique that the researcher wishes to stress, and, indeed, the weaknesses that the researcher seeks to minimise.

To some audiences, the term 'non-standardised' could suggest that the interview is nothing more than a conversation. An audience approaching the term with such a perspective is unlikely to take the opinion that 'naturally' occurring conversations are appropriate tools of organised research. In such a reading of 'non-standardised', these conversations might be the inadvertent, ad-hoc flints that provide sparks of social knowledge, but they are not tools purposefully fashioned for the generation of empirical knowledge that help us to describe and understand social worlds. Non-standardised interviews are not, of course, nothing more than conversations. With a change in terminology, but with no change in practice, we can describe the interviews that were performed in order to gather empirical material for this research as 'focused'. This re-labelling can work to radically re-orientate perceptions of the semi-structured interview as a tool. Now it evokes deliberate shaping, organising and skill. To an audience with an education and training in the natural sciences and without a background in sociology, 'focused' stresses what these kinds of interviews *are* rather than what they *are not*[20]. Sensitive to the reading that might be brought to this thesis by the research participants, 'focused' stresses the deliberate fashioning and use of interviews as a tool of research. The question of the particular points of focus is considered in the second part of this section.

Questions and Schedules

The ability to conduct effective research is not straightforwardly associated with the ability to handle theory and concepts. One need only examine the attempt made by Marx (1880 [1961]), in his *Enquête Ouvrière*, to conduct a piece of original empirical research. The questionnaire Marx designed ran to 101

[20] To a different audience, these terminological associations can be stood on their heads. 'Non-standardised' can be taken to stress the freedom for both the interviewer and interviewee to interact, allowing for an understanding of the social world of the interviewee to be communicated in rich, contextual detail. 'Focused', by contrast, would evoke rigidity and restriction, of the closing down avenues of interaction and communication.

‘questions’ and involved asking late-nineteenth century British workers to write their answers to questions such as; “15. State the number of workshops in which the different branches of the industry are carried on. Describe the special branch in which you are employed, giving information not only about the technical aspects, but also about the muscular and nervous strain involved, and the general effects of the work on the health of the workers” (p. 212). Not all of the 101 ‘questions’ are as demanding as this, but it is not unrepresentative. The weakness of the self-complete questionnaire to capture the evidence that Marx sought to collect is another example of the virtues of the face-to-face qualitative interview; its ability to work as a tool of discovery, to uncover the phraseology and world-view of the interviewees, ensures that, even if the researcher enters the field with questions to which the researcher participants cannot relate, the question schedule is quickly re-designed and re-ordered.

The development of a question schedule was, therefore, an iterative process. By responding to the interviews as they were conducted, and to insight gained from a preliminary analysis of transcripts, the interview was improved as a research tool. As described above, the choice was made to opt for a style of interview that allowed for conversation rather than one that demanded rigid structure. The question schedule had key questions and prompts to ensure all the ground of interest was covered, and so that an interview might be completed if the research participant was unusually uncommunicative and the conversation was in danger of stalling. However, the schedule is not a map of the structure of any particular interview. Each interview was different. This was in line with the researcher’s prior knowledge of the interviewee, in response to the experience of previous interviews and the success or failure of certain lines of conversation, and in response to the changing themes and research questions of the overall project. Nevertheless, for every interview the following key conversation areas were worked into a series of appropriate questions.

Degree of Resolution	Principle Areas of Interest
Individual	The nature and development of the job
	Training and education
	Career
	Identity
Team	Supervision
	Reward and recognition
Institute	Growth and development
	Interaction with other teams
	Culture and character
Community of Science	The race to sequence the human genome
	Engagement with science

Table E3.1 Interview Themes

The interview, in sociological research is an im/personal tool. For the researcher, a qualitative interview may be one of many very similar interviews, with individual participants becoming, in time, anonymised voices, and even, collectively, archetypes. For the participant, the content of an interview is not a set of descriptions of generalisable or translatable social events, but the details of an individualised, personal life. This distinction holds true even if the communication between the researcher and the participant has achieved a one-to-one level of correspondence between their respective aims and understandings held in regard to the research project.

The degree to which interviews are acutely personal events reminds us of the statement made by a documentary director. He found that his subjects became suspicious, then hostile, and then withdrew their co-operation entirely. He wrote, "[f]or its subjects, at least, the camera is a surgical instrument rather than an artistic one" (Cumming, 2007). For the sociologist, an interview might be a means of collecting depersonalised evidence. For the subjects of the sociologist's research, the interview may well feel like an interrogative and invasive tool. It is here that the confidence developed during the face-to-face interview process is vital, as is the process of being vouched for by a colleague of the interviewee, both of which generate trust. The process of recruitment is discussed in the next section, E4, *Access and Anonymity.*

E4 Access and Anonymity

The interviews were conducted over a series of three visits to the Institute between spring and late autumn 2006. A table of interviewees is provided below.

Interviewee Pseudonym	**Role at the Institute**
Professor Ingham	Senior Genome Scientist
Nick	Research Scientist
Susan	Senior Sequencing Manager
Neneh	Administrator
Mark	Senior Production Manager
Megan	Production Manager
Pat	Production
Abi	Production
Sophie	Production
Louise	Production
Brian	Pre-Finishing Manager
Nicole	Pre-Finishing
Anna	Pre-Finishing
Julia	Pre-Finishing
Maggie	Pre-Finishing
Colin	Pre-Finishing
Claudia	Pre-Finishing
Peter	Senior Finishing Manager
Jill	Finishing Manager
Judith	Finishing Manager
Catherine	Finishing
Elizabeth	Finishing
Alice	Finishing
Helen	Finishing
Victoria	Finishing
Joe	Research Engineer
Alex	Research Engineer

Table E4.1 Interviewees at the Institute

During the period of funding for this thesis, but prior to negotiating access to the Institute, six other interviews were conducted. These were with; a laboratory technician would had briefly worked in a laboratory connected with the Human

Genome Project; a computational biologist who had worked on the Human Genome Project, a scientist working in the field of proteomics, and three members of a research group developing methods for automating scientific discovery.

Research participants at the Institute were recruited using a purposive 'snowball' method. Contact was made with several of the team managers, and, by e-mail and telephone, the purpose and nature of the research was described. Here, the personal biography of the researcher was invaluable in developing trust. With a BSc and an MSc in the life sciences, research participants had their suspicions of the sociology of science disarmed[21]. These managers acted as gatekeepers, inviting the researcher to visit the Institute and interview members of their teams, recommending other potential research participants, and vouching for the research that was being conducted. These new contacts then put the researcher in touch with other members of their, or other, research teams. A major limitation of this method is that it is not well suited to recruiting research participants who no longer work in the organisation under study. This process continued until, after three visits to the Institute, the twenty-seven people listed in table E4.1 had been interviewed.

Any suggestion of neutral interconnectivity between a flat network of potential participants in this 'snowball' method of recruiting interviewees is misleading. Research participants included some of the most senior members of the management of the Institute. Indeed, the very quality that marked a member of the Institute as a potential gatekeeper to access was their seniority. Team managers not only have the ability to grant access to the laboratories and teams under their supervision, but, pragmatically, they are also contactable, with publications, press releases, and web profiles. This raises questions as to the degree of voluntarism and anonymity involved in the participation of workers recruited in such a way. We can also ask questions of the image that the researcher presented when conducting interviews. Was it the image of the disinterested, and perhaps uninteresting, academic researcher asking obscure and

[21] This background in the life sciences did lead to some interviewees making the assumption that the researcher possessed a far higher degree of knowledge than he could claim.

irrelevant questions? Or was it, perhaps, the image of an informant to and ally of management? This, of course, does not exhaust all the possible roles into which the researcher might be imagined by the research participants, but it does run from pole-to-pole of an important spectrum to be considered when intruding into a place of work. Though the appearance of the researcher, who looked anything but the image of a professional assessor or consultant, should have placed fears of the researcher being an informant to management to rest, the fact remains that the interviews were conducted in the workplace itself.

That the interviews were conducted in the place of work of the research participant means different things for different participants. For some, such as managers and administrators, this means that they were interviewed in their own offices. In the case of one manager, a large part of the interview was conducted as he led the researcher on a guided tour of the laboratories over which he had responsibility. For the scientific workers who volunteered to take part in this research, working at a laboratory bench in the case of production and pre-finishing workers, or at a computer in an open-plan office, as in the case of finishing workers, there was no private or personal space in which interviews could be conducted. In these cases, the interviews took place in vacant offices, or, in the case of one day of interviewing, in a scarcely-used staff-room.

The location of the interviews should be considered. Gillham (2000) writes that "there is a common assumption that people talk more freely 'on their own ground'" (p. 8). But he cautions us; "Sometimes that is certainly true, but those familiar contexts can also be inhibiting" (p. 8). However, interviewing research participants at the Institute was the only sensible option. Pragmatism acts as the final arbiter of methodological choices. The difficulty of arranging interviews at a time and location that is outside of work would be compounded by the fact that conducting research in such a manner, when access to the Institute as a site has been granted, would suggest the research has some unsavoury, underhand qualities. Not only might this deter all but those most motivated to provide an outsider with their narratives, but it would have been a crippling mis-step, lending credence to the suspicions of Kurt, the representative of the Press and Public Relations office, who seemed to view the research as an undercover, conspiratorial attempt at exposé. This encounter is described in the second part of this section.

Anonymity Challenged

This research faces several problems when it comes to anonymising the identities of the research participants. One of these, clearly, comes from the unique qualities of the Institute. The very qualities of the Institute that lend it intrinsic interest are those that render it identifiable. Providing the research site with a generic name does not make it anonymous. However, perhaps more serious, if we take the principle of anonymity to be to prevent harm to research participants, as human beings, rather than institutions, is the method of recruitment. Managers know who in their team has been interviewed, and which of their colleagues has granted access to the researcher. This cannot be avoided, but the use of pseudonyms, and the provision of little personal detail, except in so much as it directly relates to, rather than merely decorates, the discussion at hand, is a means to limit the identifiability of participants. When the use of pseudonyms was explained to the research participants, many of the more senior members of the Institute regarded the process as unnecessary. The argument ran that they would not say in interview anything that they would not say in public. Nevertheless, despite their suggestions, all the interview extracts used in this thesis are identified only by the pseudonyms listed in table E4.1, *Interviewees at the Institute.*

However, the value of protecting the anonymity became clear in an incident that occurred shortly after completing the third visit to the Institute. This visit was three days of interviews with people working at the Institute, ranging from the most senior managers to technical workers, and included meeting prior interviewees for lunches and coffees. Not long after this visit, Kurt, a representative of the Press and Public Relations Office, contacted the researcher.

As described, the research approached accessing the Institute as if it was the same kind of organisation as, for example, a university. That the researcher had spent some time considering the potential anatomies and pathologies of big science organisation, which raised questions of bureaucratisation and industrialisation, ought to have served as forewarning that the Institute would be a place in which organisational procedures really do matter. The Institute is not a university.

The researcher had, during the summer of 2006, contacted Kurt to ask for details of the history of the Institute. Kurt supplied as much detail as was to hand, and offered to facilitate access to the Institute. The researcher replied, by e-mail, making it clear that he had already conducted laboratory visits, but that he would be in touch if assistance was needed. This e-mail was misread, it seems. As the purposive snowball recruitment process gathered new interviewees, some recommended research participants declined to take part. As the researcher tried to expand the research further along the sequencing chain of production a potential research participant, recommended to me by one of my senior management gatekeepers, did not reply but forwarded the initial contact e-mail to Kurt.

During the telephone conversations that followed, it became clear that Kurt had misread the e-mail that had made plain that research was being conducted. This left Kurt with the impression that the researcher had been conducting some kind of clandestine investigation. This left him 'disappointed' and called him to question the '*bona fides*' of the researcher. When the researcher suggested that he had been clear in his earlier e-mail, Kurt threatened that he would find the e-mail and read it back over the telephone. The e-mail had presented the research as had been suggested.

Nonetheless, this did not disarm Kurt. The research was still an underhand, secretive pursuit. He asked for the names of the research participants. When Kurt was told that this could not be done, his tone grew even more suspicious. The irony is that, had the researcher been able to draw on the identities of his research participants, the degree to which access to the Institute was approved by the highest levels of scientific management would have been evident. This irony is compounded by the fact that it was the researcher's refusal to compromise the anonymity of the research participants that strengthened Kurt's claim that the work was lacking in scruples. Kurt, of course, is a pseudonym.

Kurt informed the researcher that he would discover the identities of the research participants regardless. He would also, he said, contact the supervisor of this research. The researcher contacted key gatekeepers who informed the researcher they would be able to 'smooth over' any problems, and reminded the researcher that the research had been 'sanctioned' by senior managers, though perhaps not through 'official channels'. It does not seem that Kurt got his list of names, and he did not contact the supervisor of this research. Kurt did send a long e-mail that presented a narrative of the events quite unlike those understood by the researcher to have occurred. In particular the e-mail exchange of summer 2006, the misreading of which Kurt had based much of his 'disappointment' with the conduct of the researcher, was omitted. Presumably as, given that the researcher had approached Kurt to discuss the study of the Institute without guile or deception, this no longer suited the story that he wished to tell. This e-mail was copied to several key figures in the Institute, including some of the research participants. This potentially positioned the researcher as presented by Kurt; devious, underhand, and now publicly chastised. Despite the desire to correct this account, the researcher settled on assuming a contrite position. At this stage, the researcher had collected interviews with members of the Finishing team, the Pre-Finishing team and the Production team. No further interviews were sought, though e-mail contact was maintained with several key gatekeepers.

The purpose of this story is not simply to ensure that the researcher's version of this story is on paper, though that desire is present. Nor is it to warn other researchers that a press and public relations expert possesses the skills and knowledge required to win battles of presentation, though that advice might be useful. More, it is an illustration, uncomfortable as it is for the researcher, that even though senior members of the scientific management of the Institute behaved, it seems, as if they had the autonomy of action and communication that is afforded to academics, this autonomy led to a collision with the bureaucratic structures of big science. In the next chapter, F, *The Factory and the University*, we consider the research participants' own categorisation of the workplace of the Institute.

E5 Evidence and Illustration

All the interviews conducted at the Institute were transcribed in full. Transcription was conducted partly by the researcher, and partly by a professional transcription service. The use of the professional transcribers was problematic. In addition to a presumably standard error rate in transcription, technical terms were frequently mis-transcribed. The mis-transcription of one word transforms the sentence, acting as a mutatagen by encouraging the transcriber to mis-transcribe neighbouring words in order for the sentence to make sense. Transcription may appear to be the mere reproduction of an audio record in written form, but as this process involves translation it is, necessarily, interpretative (Poland, 1995). The interview transcripts were then subject to content analysis, a process that developed as the analysis of the evidence progressed.

While the researcher arrived at the interview transcripts with concepts such as 'alienation', 'engagement', 'automation' and 'division of labour', ready to begin the preliminary content coding of the evidence, as the process of analysis progressed, the evidence presented new points of exploration. Aspects of the transcripts that highlight the way in which the interviewees categorise the Institute as a workplace were coded, the results of which is explored in Chapter F, *The Factory and the University.* The transcripts were interrogated for descriptions of the way in which the interviewees subjectively engage with their work, which is explored in Chapter G, *The Recruitment of Sentiment*. This engagement leads to questions of commitment, which connects with contemporary ideas about the organisation of work in a knowledge economy, discussed in Chapter H, *The High Road to the Human Genome.* Finally, we close the empirical exploration of the subjective experience of work in Chapter I, *Interlude: Dis-Engagement and Dis-Integration*, with a discussion of the ways in which these accounts of sentimental engagement and commitment are not universal. Some of the research participants' accounts describe a resistance to the recruitment of their sentiments, while all note that the changes that have occurred after the accomplishment of sequencing the human genome have led to distinct changes.

What are we to make of the evidence provided by interviews? Sociologists of scientific social life who employ qualitative research methods are faced by the scepticism of the enumerating scientific imagination. The plural of anecdote is not data, we are reminded by our research participants. If social researchers are to call evidence that is collected during interviews data, then should we treat such evidence *as* data with all the demands and binds that this places on us? Poland (1995) argues that it may be more valuable to think of the transcript of an interview as a *telling* rather than a *finding*. Gilbert and Mulkay (1984) remind us that "participants' use of language can never be taken as literally descriptive" (p. 15), and Fielding (1993) argues that we cannot take language as a simple indicator of thought and action. As he writes, "expressed attitude is a problematic indicator of what people have done, or will do" (p. 148). However, in this study, we are concerned with experience and attitudes, not in predicting action, to which interviews are well suited.

However, Wooffitt (1993) cites Gilbert and Mulkay (1984) to argue that "social events are the 'repositories' of multiple meanings, by which they [Gilbert and Mulkay] mean that the 'same' circumstances can be described in a variety of ways to emphasise different features" (p. 303). In Chapter F, *The Factory and the University*, we see this process in action, as work at the Institute is described by research participants using diametrically opposed analogies. The question is then asked; how do we make sense of this variety of accounts?

One way of understanding the how a social researcher might handle the differences between the accounts of a social world provided by different participants draws on a much referenced fictional example. The 'Rashomon Effect' is named after Akira Kurosawa's 1950 film in which the 'same' story is told, quite differently, from the perspectives of four different people, a bandit, his two victims, one of whom provides his account from beyond the grave, and a passing woodcutter. A true version of the event is not portrayed, reminding us of Wooffitt (1993) when he writes "there is no privilege for the analysts' decision as to what constitutes an 'objective' or 'accurate' version of the world" (p. 304).

The concept of the Rashomon Effect has been used in discussions of qualitative social research. Most prominently, it was invoked by Heider (1988) in an attempt to provide some resolution to the fierce dispute over the authenticity of Mead's *Coming of Age in Samoa* (1943)[22]. However, as Rhoades (1989) argues in a short reply to the Heider (1988) article, the film *Rashomon* may not be the greatest assistance in helping us deal with disagreement between social researchers, which is what the Mead-Freeman debate was. It may, though, be a good method of accounting for the role of social researcher. Rhoades suggests that, in the film, the passer-by, a woodcutter, occupies an analogous position to the social researcher.

The accounts provided by both the social researcher and the woodcutter are to some extent 'self-serving'[23]. Nevertheless, Rhoades (1989) argues that "he nonetheless does a reasonably good job of describing and explaining what he, *as an observer* sees the participants doing even though they are incapable of providing such an account" (p. 171). In other words, writes Rhoades, an invocation of 'The Rashomon Effect' ought to be a defence of the possibility of "an ethnographic "truth" achieved in the face of informants' variant testimony" (p. 171).

But this does not help the social researcher who relies on interviews, who does not *see* the research participants do anything other than present their variant accounts. While 'Rashomon' is commonly used shorthand for a story in which there are variant accounts, it is too tied into the concept of 'witnessing' the event to be of use in a study in which variant accounts *are* the evidence. Here, perhaps, it is appropriate to turn to a study of discourse, rather than contested accounts of action.

Wooffitt (1993) describes how Gilbert and Mulkay's (1984) *Opening Pandora's Box* uses of the concept of linguistic repertoires. Woofitt (1993) describes a linguistic repertoire as "a set of descriptive and referential terms which portray

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[22]An attempt that was dismissively rejected by Freeman (1989) as 'constructivist' and 'quaint'.
[23]The woodcutter in *Rashomon* is also a coward. One imagines that their might be a productive discussion to be had on the virtue of cowardice when a social researcher is 'in the field'.

beliefs, actions and events in a specific way" (p. 292). Quoting from Potter and Wetherell (1987), Wooffitt (1993) reports that "a repertoire will be organised around specific metaphors and figures of speech" (p. 292). However, for this thesis, we should note that Gilbert and Mulkay (1984) were arguing that *a* scientist had the use of two repertoires; the empirical and the contingent. Chapter F, *The Factory and the University*, argues that the use of 'specific metaphors and figures of speech' that enable interviewees to describe the Institute as being like a factory or like a university are dependent on the 'hinterland of work' from which a person arrives at the workplace in question. In other words, we find that the linguistic repertoires used to categorise the Institute as a workplace that we present in Chapter F, *The Factory and the University*, are the products of backgrounds of the research participants. This, as we see throughout Part 3, *Illustrating and Accounting*, is not a statement that is wholly relativistic. Silverman (2001) asks, "[m]ust we choose between seeing interviews as *either* 'true' reports *or* situated narratives?" (p. 113). While the meaning of a workplace might be relative to the perspective of the viewer, this does not mean that the features that are being seen are not real and material.

PART THREE: ILLUSTRATING AND ACCOUNTING

Part Three, *Illustrating and Accounting*, is the exploration of the empirical evidence gathered during the study of the Institute. Chapter F, *The Factory and the University*, explores the ways in which those working at the Institute categorise the workplace. The chapter demonstrates that the Institute is seen as embodying the two symbols of Modern work, the 'factory' and the 'university'.

Chapter G, *The Recruitment of Sentiment*, is an examination of the most subjective of experiences of work. The chapter describes the ways in which the sentiments, as well as the hands and minds, of those working at the Institute were recruited. Some of this recruitment was the product of the exceptionalism of the workplace of the Institute; work in science, work on 'the human', and the moral dimension to the race, for example. Some, though, was the product of features of the Institute that could exist in any workplace; the charisma of the leader, the 'flat' workplace culture, the celebration of accomplishments, for example.

Chapter H, *The High Road to the Human Genome*, is a comparison of the Institute to the descriptions of the new forms of organising work that are described as characteristic of post-industrialist, post-Fordist, knowledge economies. As the recruitment of sentiment generates high commitment, the Institute appears to bear many of the hallmarks of the high road, high commitment, high performance models of workplace organisation, but this chapter argues that they owe more to PhD than MBA; to the small science hinterland of the founders of the Institute than the conscious management practice.

Chapter I, *Interlude: Dis-Engagement and Dis-Integration,* arranges and re-presents the accounts of the research participants that describe processes that run counter the recruitment of sentiment and the generation of commitment. These disengaging and dis-integrating trends are the product of growth, a change in leader, and 'triumph of accomplishment'. Completing the sequencing of the human genome has left many of the research participants without the feature of work that was central to the recruitment and animation of their sentiments.

[F] THE FACTORY AND THE UNIVERSITY

The 'factory' and the 'university' represent, in many ways, polar opposites in the organisation of work. Yet, as Doing and Hilgartner (2006) argue, these two kinds of workplaces, imagined as unlike, are among the 'central symbols' of Modernity. As Weber saw it, the Taylorist factory represents the 'triumph' of rationalisation. Control, acute divisions of labour, meaninglessness, alienation. The university, by contrast, is the site where reason triumphs. Autonomy, creativity, engagement. This chapter explores the ways in which these archetypes are used in narratives of work at the Institute. In demonstrating that both of these archetypes are drawn on by research participants, and in suggesting an explanation for this variety of accounts, this chapter presents a more complex picture of the workplace than the simple visions of 'sequencing mills' that fill the popular and scientific accounts of the Human Genome Project.

F1 INTRODUCTION

Becker (1992) asks us to consider what is "the underlying imagery with which we approach the phenomenon we study. What do we think we are looking at? What is its character? Most importantly, given what we think it is, is the way we study it and report our findings congruent with that character?" (p. 210). We arrive at the Institute, and the Human Genome Project, with a particular set of imagery. The language of production lines, construction projects, process engineering, automation, mindless, inelegant, and repetitive work, comes to us from the accounts of scientists. Together with our understandings of this kind of work drawn from sociological theory, we produce a particular image of the Institute, of work in the Human Genome Project. Of dark, satanic sequencing mills.

Becker (1992) writes that we construct our initial imagery of a case "imaginatively (or stereotypically) from a few facts" (p. 211). These images contain details that we 'know' about a social domain, in Becker's example of the style of décor and tone of discussion in a particular Chicago neighbourhood. But, though "[i]maginative, well-read social scientists can go a long way with a little fact[, s]ince, however, we claim to be social scientists, we don't stop with imagination and extrapolation, as a novelist or filmmaker might. We do checking to see if we're right. Research. We gather data" (p. 211).

This is the first chapter of Part Three, *Illustrating and Accounting*, which handles and analyzes the evidence drawn from the interviews at the Institute. The first task of the chapter, therefore, is to account for the highest level of category used by research participants when describing the Institute as a workplace. In 'account for' we find the second task of this chapter. The chapter 'accounts for' the ways that the research participants describe their workplace both by sorting and re-presenting these descriptions, but also by attempting to understand the ways by which these categories are arrived.

Section F2, *From the University to the Factory*, describes the images that these workplaces evoke. These workplaces are placed at the opposite ends of many of the oppositional relationships that are used to differentiate the varieties of contemporary work. As the extracts from the interview evidence demonstrate, the Institute is described using repertoires that draw on elements of these polar opposites.

Section F3, *The Institute as Factory*, opens with a recapitulation of the 'industrial' descriptions that marked the accounts of the Human Genome Project of leading scientists and journalists. These kind of descriptions are also found in the interview evidence, proving to be a popular image of work at the Institute not only with those observing from outside, or from above, but also from those at the level of bench-work.

The Institute as routinised and factory-like was not the only image of the workplace presented in the accounts of the research participants. Section F4, *The Institute as University*, collects and re-presents the ways in which people working at the Institute also draw on comparisons with the archetype of the university. Given the public presentation of work in the Human Genome Project, and at the Institute, these descriptions present a vision of the work of large-scale genome sequencing that is at odds with common understanding.

Section F5, *Hinterlands of Work*, is an attempt to explain the use of these two archetypal workplaces in the description of a single workplace. The section argues that the use of quite different descriptive repertoires to present an image of the workplace of the Institute is a product, not so much of the position of the worker within the Institute, but 'where' the worker has come from before they arrived at the Institute. The use of references to the similarity of the Institute with a factory does not appear to map on to the experience of routinisation and automation within the Institute. Members of the more heavily 'industrialised' Production team commonly use comparisons to archetypal features of the university in order to describe the Institute, while those working in Finishing, which involves comparatively more problem-solving and variety, are apt to use references to the factory-like features of work at the Institute. If the choice of descriptive comparison was determined by the objective reality of work at the Institute, we would expect to see the opposite set of associations. But the choice of descriptive repertoire is a product not only of current work, but of the 'hinterland of work' from which the worker emerges. This hinterland is not just experience, but also the expectations produced, and examples set, by, for example, educational background.

F2 FROM THE UNIVERSITY TO THE FACTORY

The archetypes of the 'university' and the 'factory' are workplaces that share little in common except their Modernity. Between them they demonstrate the contradictions within Modernity. Consider, apposite to the discussion at hand, the relationships that the university and the factory have with science. Universities are the workplaces within which the work of science is conducted; they are home to the expansion of knowledge and reason. Factories are the scientific workplace, the home to Taylorism and calculative, rational management practices, mechanisation and automation. The factory is the site of the application of reason and knowledge.

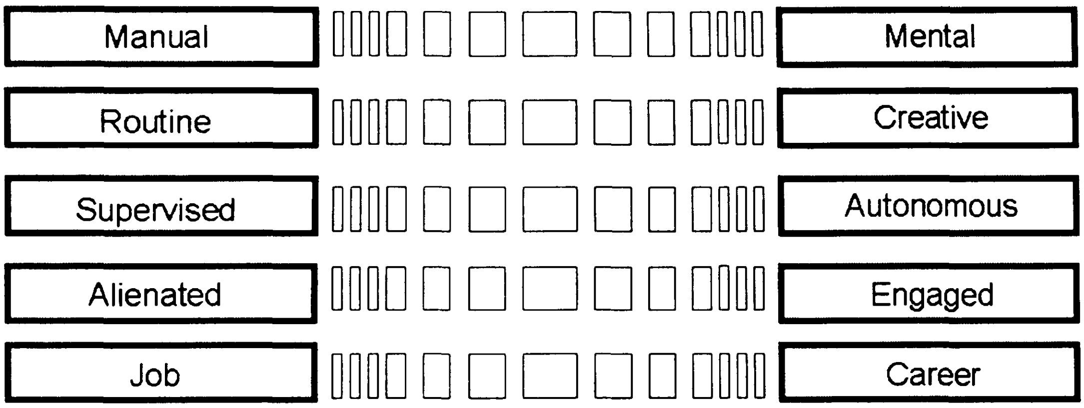

Figure F2.1 The Dichotomies of Work

These two archetypal workplaces occupy the opposite ends of many of the apparent dichotomies by which workplaces can be described. In the university, labour is conducted by the intellect. The factory is the home of manual labour. The university is a workplace of professionals. Work in the factory is a job. Work in the university is imagined as creative, self-directed and fulfilling. Work in the factory is routine, supervised and subjectively meaningless. Using 'laboratory', corresponding to where this chapter uses 'university', Doing and Hilgartner (2006), in the abstract for their session at the 2006 *Society for the Social Studies of Science* conference, state that "[t]he laboratory and the factory are two of the central symbols of modernity, and they stand in a dialectical relationship to one another in a way that broadly parallels the dialectics of fact/technique, designer/operator, worker/knower, and creative/routine" (p. 208).

As Kleinman and Vallas (2001) report, the rise of knowledge economies is marked by organisations that incorporate elements of these two archetypes.

It is in the literature on technical work that we find most starkly the play of these oppositional relationships. Discussing technical work, Nelsen (1997) argues that the work settings can be ordered either vertically or horizontally. A horizontally ordered work setting is, she argues, best characterised by that found in the crafts and the professions. Roles within these "work settings are neither sharply differentiated nor hierarchically structured" (p. 155). Each person in the work setting is a member of a 'community of practice'. We could suggest that the archetypal horizontally ordered work setting is that of academic science. Nelsen describes these settings as being ones in which each "full member is, in principle at least, just as qualified to wield authority as any other full member. As a result, actors enjoy a measure of technical and moral equality which renders coordination by command inappropriate" (p. 155-156). Rather, there is coordination by collegiality and persuasion. A vertically ordered work setting, on the other hand, is one of command, supervision, and hierarchy. Superiors have greater technical and moral authority, and as such are granted the right to command, which subordinates have a duty to obey. The archetype here is the Modern bureaucracy, or the factory. Both forms of work ordering encourage a form of social cohesion; the egalitarian collegiality of the horizontal ordering, and the subordination of the vertical ordering. In Chapter G, *The Recruitment of Sentiment*, we explore the character of social cohesion in the Institute, and examine the ways in which this cohesion is challenged in Chapter I, *Interlude: Dis-Engagement and Dis-Integration*.

There is a moral dimension to these oppositional relationships. Considering the two forms of social cohesion, Nelsen (1997) writes; "There is an obvious tension between these ideals; each reviles what the other demands. Yet, both moral visions are juxtaposed in the context of technical work" (p. 156). As we explore the way in which the research participants categorise the Institute in this chapter, we see the juxtaposition of horizontal ordering, i.e. the university, and vertical ordering, i.e. the factory. Further, we see in the accounts of the research participants laments for lost forms of work; this is not the exchange of neutral alternatives.

Throughout Chapter B, *Anatomies of Big Science*, and Chapter C, *Pathologies of Work in Big Science*, we see that big science appears to involve a move from one pole of these apparent dichotomies to the other. However, the movement away from the archetype of the university, towards the factory-like nature of big science, is movement along continuums, not a leap from pole to pole.

This study began with the suggestion that work at the Institute was like that of a factory, a state apparently antithetical to the ideals of the workplaces of science. Once the researcher visited the Institute, and interviewed those who worked there, it became apparent that the workplace of the Institute did not simply stand, as a factory, in opposition to the ideals of work in science. Rather, in the accounts of those working at the Institute, the work of sequencing genomes embodies elements of both of the contradictory forms of social organisation that are the university and the factory. Both of these workplaces are the products of a Modern, scientistic attitude, and in this the work of the Human Genome Project was the epitome of Modernity. This chapter demonstrates that the accounts of the Institute as a workplace presented by the research participants make use of both of these archetypes. In doing so, we must ask; how do we get from the university to the factory in a single workplace? Section F5, *Hinterlands of Work*, suggests that these variant accounts are not the product of a different experience of the Institute itself, but of a different engagement with the possibilities of Modern work.

F3 The Institute as a Factory

Even before the Human Genome Project was properly begun, the workplaces of genome sequencing were imagined as factory-like industrialised laboratories. As presented in Chapter B, *Anatomies of Big Science*, Walter Gilbert has been quoted as arguing that "the benefits of the Human Genome Project are the benefits of organization and scale" (quoted in Keating, Limoges and Cambrosio, 1999, p. 125). "[I]f large scale sequencing is to work, it must be treated not as a science but as a production job," as a "pure technological problem quite apart from interpreting sequence." Large-scale genome sequencing is like the task of producing cars; the work is to be done by "production workers... It is not done by research scientists" (Gilbert, quoted in Keating, Limoges and Cambrosio, 1999, p. 126).

Hood and Smith (1987) were quick to see the negative aspects of organising science in this way. "Sequencing currently requires the skill and judgement of professional scientists to be carried out effectively. Yet it is also repetitive, boring and labor-intensive. It would be a waste of intellectual talent to have many young scientists engaged in this task now" (p. 38-39). As with Gilbert, Hood and Smith argued that the answer to this problem lay in automation and the development of efficient "industrial laboratories". "Obtaining the complete sequence is a production-line effort" (p. 38)[24].

Hilgartner (1995) notes that objections to the Human Genome Project within science included a constriction of the pool of training opportunities available for young scientists and the degree of laboratory drudgery that would be involved. To return to Gary Zweiger's account, we read his distaste in his account of first exposure to the plans for 'industrial laboratories'. "[Maynard] Olsen used our

[24] Hood and Smith (1987) explicitly made the case that sequencing the human genome was not science but an "essentially nonresearch production task" (p. 46). They argue that, as sequencing would neither ask questions nor (therefore) formulate and conduct experiments that it would not be science. The authors draw parallels between the genome sequence and particle accelerators. "[D]eveloping the technologies requires science and engineering. Obtaining the complete sequence is a production-line effort, and using the resultant data is science" (p. 38). If this argument is correct, then the correct comparison cases for sociologists to draw on might not be the sociologies of big physics laboratories, but rather the sociologies of the construction of big physics apparatus.

attention-filled hour to drone on about a scheme to determine the nucleotide sequence of enormous segments of DNA (deoxyribonucleic acid). The speech was a bore because it had to do with various laboratory devices, automation, and costs. He described technicians (or even graduate students) working on what amounted to an assembly line. He analyzed the costs per technician, as well as costs per base pair of DNA. It was as bad as the rumours we had heard of factory-like sequencing operations in Japan. It all seemed so inelegant, even mindless" (p. ix-x).

This vision of a resource-appropriating sequencing mill, in which talented graduate students are subjected to daily drudgery, was one of the objections to the Project when it was proposed. Accounts of the Project in the scientific press describe how visions of a factory-like genome project were allayed by the rapid improvement in automation (Service, 2001), as well as the increasing routinisation of laboratory protocols. These developments removed the need for highly-skilled 'green-fingered' workers. But this "[f]lushing out [of] tacit knowledge" (Keating, Limoges and Cambrosio, 1999, p. 126) is a key component of the rationalisation of work. These technological and organisational developments might have 'rescued' several hundred graduate life-scientists from decades of uncreative but highly-skilled routine. Even this was imagined in 'industrial terms'; Cook-Deegan (1994) quotes Gilbert as writing that "[m]any of those who complain about the genome project are really manifesting fears of technological unemployment" (p. 91).

The development of routinised protocols and laboratory automation, which relieve the drudgery of work for accredited scientists, do not, in themselves, undo the development of factory-like organisations in the pursuit of the human genome sequence. As we were reminded by Nichols and Beynon in Chapter C, *Pathologies of Big Science*, "for every man who watched dials another maintained the plants, another was a lorry driver and another two humped bags or shovelled muck" (Watson, 2003, p. 182-183; quoting Nichols and Beynon, 1977). As Claudia said when I asked her about the development of automation in Pre-Finishing, the machines cannot do everything.

> Ah well, funny you should say that. Our boss has actually got a robot that hides away in a room somewhere and think only he knows how to work it. [...] But I think there is one- I think there's a few things which this machine can't do, which is why you need people in. [...] Because I reckon if it could, I don't think you'd have been interviewing us six either! [...] It'd just be this machine to talk to!
> [Claudia]

Through these developments, molecular biology moves from an era of craft work to a way of working that demonstrates characteristics of both the 'Machine Age' and the 'Information Age'. As Hodgson's (2000) article in the journal of the Institute of Electrical and Electronic Engineers (IEEE) had it, sequencing the human genome involved 'gene sequencing's Industrial Revolution'.

As we have described, commentators taking a retrospective view of the Human Genome Project often stress that improvements in the automation of sequencing ameliorated the industrial qualities of work in organisations such as the Institute (see Service, 2001; Collins, Morgan and Patrinos, 2003). But some accounts, perhaps inadvertently, remind us of the counsel that Nichols and Beynon offered to their contemporaries who saw technology as having the potential to be, independently, the route to fulfilling and engaging work for all. As quoted in Chapter A, *Anatomies of Big Science*, one of the leaders of the Human Genome Project described a recruitment process that was resolutely factory-like. "We would recruit unskilled people... This group would have no need of academic qualifications. We judged them on school achievements, interview and something by which I set great store: the pipetting test" (Sulston and Ferry, 2002, p. 75). In Chapter A, *Anatomies of Big Science*, this is described as being akin to the recruitment of a machinist. The skill of a worker recruited in this way lies in the ability to repeatedly and accurately perform delicate manual operations. It is labour of this kind that can be unproblematically[25] reduced to measurements of manpower. Indeed, the 'output' of these workers can be described in terms of productivity; by the production of bases of genome sequence. In Sulston and Ferry (2002), as in many histories of the project, one of the key themes is the continual reduction in the economic cost of each base pair of sequence.

[25] By those with the world-view of management and consultancy, at least.

To what degree do accounts of working lives within the Institute reproduce these descriptions of genome sequencing as factory work, as production lines, as, even, drudgery? The second part of this section collects and discusses accounts of work at the Institute that explicitly stress the 'industrial' nature of the Institute. These are the ways in which research participants use the archetype of the factory to describe work at the Institute; this is the Institute as factory.

The recruitment of untrained, or, as Sulston and Ferry (2002) put it, unskilled workers to work in genome sequencing means that many of the research participants had prior working lives outside of the peculiar environment of scientific laboratories. Several of the interviewees, therefore, were in a position to invoke the archetype of the factory informed by personal experience. Those that do have a background in unambiguously industrial work make comparative statements that do not so much play down the industrial nature of the Institute, but rather, that stress that the Institute is a 'good' example of such a workplace. Claudia, a member of the Pre-Finishing team, who had worked in manufacturing, describes the Institute in terms of a comparison to her previous place of work.

> It's like a glorified factory; a lot cleaner and a lot more perks
> than where I used to work.
> [Claudia]

As described in Chapter D, *Case*, there are differences between the sections involved in the 'sequencing chain of production'. Considering the features of most relevance to this discussion, Production is the most automated section and involves the closest degree of supervision, the work in Pre-Finishing consists of varied but routinised laboratory work, while Finishing involves comparatively skilled computer work. The sequencing chain of production, therefore, embodies an acknowledged hierarchy that separates the different sections in terms of the perceived degree of skill required and 'industrialisation' involved. Elizabeth is a member of a Finishing team who joined the Institute from school and has since gained a degree. She describes why she left her previous role in Production, invoking the undesirability of work in factory-like settings.

> [B]ecause Sequencing, it is kind of almost like being in a factory.
> [Elizabeth]

Colin, who works in Pre-Finishing, also looks down the sequencing chain of production to describe Production as being factory-like. For Colin, it is the routinisation of work in Production that makes it factory-like. The variety of work in Pre-Finishing is presented as part of what makes Pre-Finishing unlike working in a factory.

> I think that's a bit more sort of like working in a factory because they've just got the bog standard job that they probably do *en masse*, whereas ours is smaller and more varied.
> [Colin]

In accounts such as these, the factory-like nature of Production is contrasted to the work of Pre-Finishing and Finishing. Along the sequencing chain of production, a gradient of increasing likeness to the factory can be seen as we move from Finishing to Production. This is present in the accounts of the research participants, as we would expect from our knowledge of the kind of work performed by each team. However, this does not mean that the work of the Pre-Finishing and Finishing teams is not described by some interviewees as being factory-like. Alice, a Finisher who has achieved a degree of seniority in her time at the Institute, contrasts the difficult problem-solving work that she is involved in with the majority of work conducted by the Finishing team. The rest of Finishing is:

> [...] more of a factory production line I would say, in a way.
> [Alice]

Again, this is a downwards view, diagnosing undesirable 'industrialisation' from above. For Alice, her description of the work of Finishing, with involves skilled IT work, as being a 'factory production line' is justified on the basis that, from her perspective, the work of Finishing appears routine. The question of perspective is explored in section F5, *Hinterlands of Work*.

Many accounts offered by the research participants stress that the workplace of the Institute is a developing environment. Since its inception, through the race to sequence the human genome, and after the accomplishment of that goal, work at the Institute has not been a static arrangement of practices and technologies. Time, therefore, should be expected to be a factor in the way in which the Institute is described as a workplace.

The development of the Institute reminds us of Edwards (1979) description of the change in the organisation of the Modern firm. Edwards takes the reader from the late nineteenth-century, when "[T]he ownership and management [...] reflected a small-business character" (p. 25). From his description of the way in which both the rewards, in terms of profits, and the responsibility, in terms of management, were concentrated with the founding entrepreneurs and their families, we can draw parallels with the reward and control system of small science. These small firms were organised in ways in which "[t]he entire firm was, in a way, the capitalist's own workshop" (p. 25). They were "small enough for all, or nearly all, the workers to have some personal relationship with the capitalist" (p. 26). This form of organisation echoes through the organisation of the laboratories of small science. The degree to which the personal relationships that those working at the Institute enjoyed with the founding director are important in accounts of work in genome sequencing are explored in Chapter G, *The Recruitment of Sentiment*.

As Edwards (1979) continues his discussion of the development of the Modern firm, we find a useful scaffold to help us to describe the development of a big science organisation such as the Institute. "As... firms outgrew their entrepreneurial origins, direct personal control by the capitalist became increasingly difficult. [...] More and more managers had to be employed, but soon not even all the managers could be supervised directly" (p. 28). Edwards describes a process in which, accompanying the growth of the firm and an increasing division of labour, 'head workmen' became managers. We hear echoes of this kind of development in the organisation of production, which took place in many industries around the turn of the twentieth century, in accounts of work at the Institute at the turn of the twenty-first century. In the decade and a half that the Institute has existed it has grown from a group of green-fingered craft laboratories, into an increasingly capitalised, automated, and hierarchical organisation.

One of the changes that has accompanied this development is the introduction of new technologies. Tremendous improvements in the automation of genome sequencing have been complemented by, for example, the development of new Finishing tools. Helen, who works in a Finishing team, describes the impact of the improvement in the computer tools used to assemble genome sequence.

> [I]t is nice in a way. You feel like you're getting more done, but
> it does feel now like a ... kind of a factory kind of thing.
> [Helen]

In her account, Helen acknowledges the improved productivity that this new technology enabled. It is important to note that she does not, however, see this improvement in productivity in unproblematic terms. Unlike Mark, who, while he showed me round the laboratories that he was responsible for, spent a good deal of time describing his achievement in transforming Production into a workplace with an increasingly industrial character, Helen's likening of Finishing to a 'factory kind of thing' is a lament.

Helen implicitly recognises that technologies change the social situation into which they are embedded. Returning to Gallie (1978), we are reminded that the social response to technological developments in the workplace is dependent not just on the shape of the technology, but also on the shape of the social system into which these technologies are embedded. Helen accounts for this change in character by reference to organisational changes in addition to technological development. She argues that the increased factory-like character of the Institute is a product of the 'streamlining of jobs'. This 'streamlining' consists of a progressively more acute division of labour and the introduction of increasingly visible production targets.

Brian, a Pre-Finishing team leader, describes the way in which his experience of the Institute has changed over time. For Brian, it is the increasingly acute division of labour that marks the development of the Institute into something that is like a factory.

> It changed I guess when the teams started to get split up into more specialist groups because obviously the work was more defined. People started to feel like they were more in a factory and it was kind of then. You know when you didn't have that identity with that piece of work any more. It wasn't a personal thing any more. It was more of a, "Oh, I need to get this done". [Brian]

This extract from the interview with Brian is interesting because he does not limit his likening of the Institute to a factory to a story of the increasing division of labour. Brian also describes the development of a sense of alienation, a detachment from the products of his labour, as the division of labour grows more acute, as the Institute acquires the characteristics of a factory. This separation from the meaning and identity of the work is a classic form of alienation, as discussed in Chapter C, *Pathologies of Big Science*.

Brian, while he has no degree, joined the Institute in order to work in science. This makes him unlike many of the members of the Production and Pre-Finishing teams interviewed by the researcher. Characteristically, these do not have backgrounds in science, in terms of either work or education, and did not join the Institute in order to work in science. This interest in science, and the deliberate seeking of a scientific career, makes his account read similarly to that of those participants who arrived at the Institute possessing a scientific background. Brian's background in science is part of his 'hinterland of work', the concept of which is discussed in section F5 of this chapter.

Victoria, a member of the Finishing team, has a postgraduate-level scientific education. Like many of the research participants who have a background in science, she joined the Institute in its early years with little prior work experience. In the interview, she was asked how work at the Institute had changed since she had joined. She summed the changes up by describing a move towards a more industrial form of work.

> I think... more, more industrial, more industry-based I guess with the targets and [inaudible] and yeah...
> [Victoria]

Victoria sees the growing visibility of supervision, expressed here in the rationalising quality of performance targets, to be a key aspect of the industrialisation of the workplace. Rationalisation is used here in the strictest sense, that of the development of ratios. The rationalisation of the workplace requires the enumeration, or pseudo-enumeration, of all aspects of the work. The development of the Institute after the accomplishment of sequencing the human genome, which research participants identify as being the start of particular instances of rationalisation that have had the most deleterious effect on their experience of work, is discussed in Chapter I, *Interlude: Dis-Engagement and Dis-Integration*.

In contrast to Victoria, Joe, who worked as an engineer at the Institute, had enjoyed a varied work history. Despite this experience of work settings outside the limited world of genome sequencing, Joe describes a change in the way that the Institute can be described as a workplace. Rather than the opposition of the factory and the university, Joe uses a different formulation that captures some of the same relationships listed in *Figure F2.1, The Dichotomies of Work*; opposing the world of science to that of business. He describes a workplace that is increasingly organised according to the principles of business. In the interview, Joe was asked to describe the way in which the Institute had become more business-like.

> It's just that everything is more structured. And there are all limits on everything and there is a budget for this and you've got to make sure you're within that and you have to do it within that time. And there is more of that now I think, I feel.
> [Joe]

Once again, we see a description of increasing rationalisation featuring in an account of the changing workplace of the Institute. And, once again, this is a lament. Only Mark, a manager in Production, the most 'industrialised' section of the sequencing chain of production lauded the making factory-like of the Institute in unambiguous terms. These descriptions of rationalisation through enumeration are used to illustrate the development of the Institute from something with similarities to a university to something akin to a factory. Or, in the terminology used by Joe, the Institute has changed from something that operates on the rules of science, to something governed by the principles of business. Interestingly, Joe suggests a path of causation that explains these developments. He positions the origin of this increasing rationalisation in the person of the new director of the Institute. Joe describes the new director as being 'more work orientated'. According to Joe, the new director carries with him the values and principles of business.

> [...] more an actual businessman than an actual – I mean, I know he's a scientist but I think he's got more of a business side behind him.
> [Joe]

For Joe, then, the character of the new director explains why the Institute has become 'more regimented'. In Chapter G, *The Recruitment of Sentiment*, we explore the way in which the person of the founding director is taken as a keystone around which the sentimental commitment to the Institute of many research participants is built. This is labelled 'contingent charisma', and a similar process is at work here. This is not to deny the differences in character and outlook of the founding director and the new director, but is to suggest that other changes, beyond and greater than the personality of one man, are necessary to produce such a changed experience of work within an organisation of the size of the Institute. In Chapter I, *Interlude: Dis-Engagement and Dis-Integration*, the subjective effects of the development of the Institute after the accomplishment of the sequencing of the human genome are discussed in greater depth.

Jill, a manager of a Finishing team, provided another account of the increasingly industrial nature of the Institute. Jill has a degree in the sciences and, prior to joining the Institute, she had worked for a year doing routine laboratory work. She joined the Institute when it was still very small, when there were very few members of staff. In her account, Jill contrasts the contemporary atmosphere of the Institute with that of the 'academic way of working'. She characterises academic work as taking place in a sociable environment and involving a voluntaristic long-hours ethos. As the Institute has grown, things have changed.

> It's very much nine-to-five. You very much do the hours that you're paid for and no more. In a way I see that as a more sort of normal job than academic way of working.
> [Jill]

The academic workplace, or, in other words, the university archetype, is characterised, for Jill at least, by commitment and engagement. And it is these qualities that have been lost as the Institute has grown and developed into a sequencing factory capable of helping to win a race to keep the human genome sequence in public ownership. Jill has an explanation for this development. Unlike Joe, who suggested that these changes were the result of a radical change at the top, Jill explains the change in the character of the Institute terms of the organic maturation from below.

> We were all single, we were all just out of, more or less, straight out of university, we socialised a lot together, we went to the pub every weekend [...] as lives change and as we settle down and we have children, priorities change in terms of what we want to do with our spare time and our work time.
> [Jill]

The character of the Institute is merely the character of its staff, written organisationally. Where it once was young, eager, committed and single-minded, it has now grown older, developed a more sensible outlook, and has developed other interests.

Explanations of organisational change such at those offered by Joe and Jill should not simply be dismissed. They provide insight into the way that research participants see the changes that are occurring, how they put these changes into context and how they draw together the different strands of their working lives. These accounts also remind us of the problem of variant accounts discussed in Chapter E, *Conduct*. Both Joe and Jill describe a workplace that has become more industrial and less academic, having acquired characteristics of that are imagined as typifying business and having lost those that distinguish science.

Despite this, they each build narratives of causation that are quite different, locating the prime mover in these changes in the character of the Institute in quite different material changes.

Not all the descriptions of the industrial nature of the Institute rely on changes over time to give the characterisation context. As this chapter has shown, in some cases it is parts of the Institute that are factory-like, when compared to other sections of the sequencing chain of production. In other accounts, the industrial nature of the Institute was apparent at the first point of contact, before experiencing variation over time or within the Institute. Abi, who works in Production, describes the way in which she came to work at the Institute.

> [T]he advert for the production side of it was quite strange really. It said you work on your own. If you've got production skills – which is what I thought well, I've worked in a factory, I've got that. Production skills, work on your own.
> [Abi]

Abi arrived at the Institute with experience of workplaces that can be unambiguously and unproblematically described as factories. The production skills that Abi describes herself as having were developed during this previous employment. This background, indeed, this 'hinterland of work', gives Abi an interesting perspective on the industrial nature of the Institute, which are explored in the section F4, *The Institute as University.*

As the Institute has 'industrialised', the educational background of the kinds of people recruited to the Institute has changed. Megan, manager of a Production team, describes the reason why the profile of a new member of the Production teams has changed.

> It's also unlikely for us to take graduates on in the production area, because it's quite routine, especially now, because a lot of it is robotic. Whereas when I was doing it it was all by hand.
> [Megan]

This reminds us the study of a pharmaceutical firm by Keefe and Potosky (1997). They found that the recruitment of higher degree holders in Quality Control, the most factory-like of the sections, was 'disastrous'. The more highly qualified of the recruits "found their work boring, routine, and even demeaning" (p. 65). The firm therefore made it a policy not to recruit those with higher degrees to technical positions. However, the structure of the pharmaceutical firm was one in which those recruited to management positions were required to have a full degree. This meant that promotion was always from without. In the case of the Institute, while Production is no longer seen as a suitable job for graduates, this is in contrast to the recruitment practices of the Finishing teams, who almost exclusively recruit graduates. This differentiation of recruitment profiles has not always been so pronounced. New workers were expected to work on the sequencing machines, before graduating to finishing, learning the sequence chain of production as they went. Some of the current finishers passed along this route of progression, despite their lack of degrees. In Megan's account, it is technological developments that are responsible for the changing profile of the Production worker. When Megan began work at the Institute, the tasks now performed in Production were 'green-fingered'. Such highly-skilled laboratory work was suitable work for graduates, though degrees were not a necessary requirement. This produced, Megan suggests, a future-orientated recruitment strategy in the early days of the Institute.

> And I think the people that started then, the idea was these people would move up. And most of us have moved up, people who are now managers of teams.
> [Megan]

Megan's account presents a history of work in genome sequencing that is characterised by increasing automation and routinisation. The work has been de-skilled (Braverman, 1974). As the work of Production has been de-skilled, the possibilities have shrunk for someone entering the Institute as Production worker to make a career, rather than a job, at the Institute. Again, the impact of improving technologies upon the social system of the Institute has not simply been the increase in efficiency or productivity.

This section presents a range of narratives of 'industrialisation', of the Institute becoming more factory-like. The degree to which research participants describe the Institute as being like a factory differs between sections of the sequencing chain of production. According to the accounts of participants, Production is more factory-like than Pre-Finishing, which, in turn, is more factory-like than Finishing. All sections, however, are described by at least some participants as being in some way factory-like. The Institute has also become more industrialised over time. For the research participants, 'industrialisation' is the product of increasing automation, an increasing division of labour, and a growth in bureaucratic oversight. Causal narratives include the maturation of the workforce and the individual qualities of the new director. Subjectively, participants describe a reduction in the degree of engagement, in other words increasing alienation, as the workplace has become more factory-like. These ideas are explored in Chapter G, *The Recruitment of Sentiment*, and Chapter I, *Counter Narratives and Dis-Integration*. The next section of this chapter discusses the ways in which research participants draw on the archetype of the university to describe the Institute as a workplace.

F4 The Institute as a University

The university is the model of the workplace in which science is conducted. Public descriptions of the Human Genome Project tended to stress the way in which the workplaces of genome sequencing, such as the Institute, were quite unlike universities. Universities are the homes of small science. The Human Genome Project, accomplished by workplaces such as the Institute, was big science. Or, as Hood and Smith (1987) argue, not science at all. Section F3, *The Institute as a Factory*, shows how the archetype of the factory finds expression in the accounts of the research participants. However, as suggested by the re-presenting of interviewee accounts in that section, in the way that different sections of the sequencing chain of production were more or less industrial than others, and in the way that the Institute has grown more factory-like over time, there is another side to these descriptions. The accounts of working at the Institute provided by the research participants often use the archetype of the university as a way of illustrating aspects of the workplace that contrast with the industrialising narrative. This section sorts and re-presents accounts that explicitly stress the way in which the Institute is university-like.

Accounts of the Institute as being university-like draw on a range of aspects of the workplace. Some of these are the seemingly superficial, such as in this extract from the interview with Brian, a manager of a Pre-Finishing team, in which he discusses the aesthetics of the Institute.

> I mean even the way buildings have been made and the artwork created and put in place has all added to that ethos of a university.
> [Brian]

The Institute, as described in Chapter D, *Case*, certainly could be a recently built extension to an existing university, all glass, metal and light brickwork. The imagination of the architecture sets it apart from the business parks, and it does little to evoke the images of industrialism. Other accounts of work at the Institute, though, draw on more substantial archetypal characteristics of the university. If we return to Joe, for example, we find a stress on self-direction and the absence of supervision.

> [I]t was like a university in a way. It was quite free and easy. There was no – really no pressure. And it was just get on with it kind of like. And when you've done it come and see me kind of thing.
> [Joe]

In a similar way, Helen describes the Institute as "quite a non-pressured environment". Of course, a lack of pressure from supervisors is not unique to universities, and neither is it a necessary feature of actually existing universities. However, as displayed in Figure F2.1, *The Dichotomies of Work*, in our comparison of archetypal Modern workplaces, the university placed in opposition to the factory on the continuum of autonomy to control. The autonomy of academia contrasted with the supervision of Taylorism.

Brian describes the way in the subjective experience of work was vastly different at a time when the Institute felt more like a university. In section F3, *The Institute as a Factory*, we see how he feels that the increasing division of labour has alienated people from the products of their work. Referring back to his experience of the Institute during its early years, Brian describes a degree of engagement with work that is not present in the later incarnation of the Institute as it develops into an industrial workplace.

> [E]veryone was there because they liked science, they wanted to do a good job and see what's possible and get on with it. And everyone, not so much liked working with each other, but respected each other's work ethos in that everyone would work for as long as it took that day to get the job done. Whether that be working from 6 o'clock in the morning to midnight, 2 o'clock in the morning. Or if there was no work to get done the entire place would go down the pub for lunch.
> [Brian]

Not all the descriptions of the Institute as like a university are laments for times past. Abi adopts a more ambivalent view. If we return to her account, we see another description of the way that the Institute shares features with the archetype of the university. Again, this categorisation hinges on the lack of a visibly strong system of supervision.

> [B]ut it is like a university, that – that's what it seems like here to me. [...] You know, yeah. They wander in and then they have coffee and then they go and do a bit and then they have a bit more coffee! And I'm like – how can you do a day like that? That's just so unstructured. [...] But to them, I suppose it's structured to them because that's – you know, their wandering is structure for them, so!
> [Abi]

We should note that, in Abi's account, the Institute remains university-like. Given that Abi has, as noted in section F3, *The Institute as a Factory*, a background in factory work, the researcher prompted her to consider the comparisons that have been made between working in large-scale genome sequencing and work in a factory. Her immediate response to this suggestion is fairly unequivocal.

> Yeah. But it's not, ha!
> [Abi]

An attempt to phrase the question in a different way was cut off. Nonetheless, the attempt at rephrasing served as an effective prompt. Here, Abi reflects on the ways in which the workplace of the Institute could be considered factory-like, or university-like.

> No, it's nothing, I mean – no. [...] No. If it was – it – what I do, yeah. In the tiny bit of like putting things on the machine and having those bits, yes. They're very – but I mean I suppose, yeah. Over there, there's a lot more use of automation now than there was. And yeah. You have got to get a product out at the end of the day and – but I think because it was – because the product we were – my dad always used to say, because the products you're producing is free, like for one you know, you're not having to do deadlines and you know. [...] Oh well, this has got to be on the Net by ten o'clock tomorrow morning, or this has got to be at the customer for so and so. You haven't got that pressure; you're not producing a physical thing that somebody's then got to do something else with. And I don't – I think because they're all – a lot of them are academics, they just move in here as if they've moved out of the university and moved in here, and – you know, it's a campus.
> [Abi]

In this interview extract, Abi identifies automation as being the aspect of the work of genome sequencing that provides the material backdrop against which descriptions of the Institute as factory-like are made. Abi considers this to be a superficial similarity. The lack of economic pressures, which is the product of working in a non-commercial setting, outweighs the industrialising nature of automation. In addition, as is the case with Brian's comments on the aesthetics of the Institute, she comments on the *place* that is the Institute. The physical organisation of the Institute as a campus is important in her characterisation of the workplace as being quite unlike a factory.

The features of workplace that are highlighted in explicit descriptions of the Institute as being like a university are the absence of heavy-touch supervision and control, and the absence of a restrictive structure. These are placed at odds with the features highlighted in section F3, *The Institute as a Factory*; the industrialising nature of automation, an increasingly acute division of labour, and lately, increasing rationalisation. We should not imagine, though, that the kind of 'autonomy' described by the research participants is the kind of autonomy idealised by science. It is not professional autonomy (Varma, 1999). The autonomy of the technical workers is not the autonomy of science. It is not a state in which individual scientists choose their own research subjects and determine the actual conduct of research (Polanyi, 1962). Indeed, at the Institute, in the Human Genome Project, there is no *individual* scientist. As Kleinman and Vallas (2001) warn, while knowledge economies might be marked by organisations that bear the hallmarks of both the factory and the university, of the values of industry and of academia, this 'convergence' is not symmetrical. It is the norms of industry that hold the upper hand.

Watson (2003) describes how the label that might more properly characterise work in a post-industrial economy is Drucker's (1992) concept of a 'post-Modern factory'. This incorporates elements of the archetype of the Fordist factory with the more flexible forms of organisation that Kleinman and Vallas (2001) argue are being co-opted into commercial settings from academia. Drucker uses a naval analogy to explain the difference between the 'Modern' factory and the 'post-

Modern factory'. While the Modern factory is like a battleship, the post-Modern factory could be better imagined as a flotilla; "a set of modules centred around stages in the production process or a set of closely related operations" (Watson, 2003, p. 128; quoting Drucker, 1992). But the Institute, when it was sequencing the human genome, was no flotilla. It was a battleship of a scientific workplace, with a single goal, a line of production and a large collectivised crew. In so much that it was part of a fleet, it was part of a fleet of battleships, the G5 laboratories, with a great number of smaller ships providing auxiliary support. As we see in Chapter I: *Interlude: Dis-Engagement and Dis-Integration,* since the accomplishment of sequencing of the human genome, the subsequent diversification of the pursuits of the Institute renders the analogy of a flotilla more suitable. As the image of a flotilla might fit the kind of work conducted in a university[26]. If we expand the picture a little, the sequencing arm of the Institute is no longer a battleship, but something more akin to a supply ship or tanker; large, and necessary for the other ships to travel their routes, but subordinate to the goals of pursued by other ships.

To some extent, the way that the research participants draw on radically different workplace archetypes is the product of time, as the Institute appears to develop along the lines of the Modern firm described by Edwards (1979), only in a radically condensed time frame, a century after most industries went through these changes. In other cases, the characterisation of sections of the sequencing chain of production as being factory-like is the product of perspective within the Institute; it is a feature of looking down the hierarchy. The changes of time and perspective within the Institute explain the presence of the use of a both the descriptive repertoires that accompany the university and the factory within the accounts of research participants. In section F5, *Hinterlands of Work*, this chapter argues that, in some of these cases, different people are looking at the same workplace, with the account of one drawing on the archetype of factory, and the account of the other drawing on the archetype of the university. This is a product

[26] In fact, for any university department that takes the ideal of the autonomous direction of intellectual labour as a goal, flotilla would be too organised an analogy. Lots of tiny craft ships travelling their own routes, exchanging maps, sometimes sailing together, sometimes in opposite directions would be a more appropriate, if a lot less concise, analogy.

of the different 'hinterlands of work' from which these workers arrive at the Institute.

F5 Hinterlands of Work

This chapter opens with a quote from Becker (1992), who urges us to consider "the underlying imagery with which we approach the phenomenon we study" (p. 210). The social researcher, though, is not the only person who approaches the Institute with a set of images that shapes his perceptions of the kind of workplace that is the Institute. Each person recruited to work at the Institute arrives there from a 'hinterland of work'.

It would be gratifyingly simple if the accounts of the Institute that describe it as being like a factory, and those that describe it as being like a university, could be accounted by straightforward reference to the division of labour. Production is the most routinised and industrialised of the sections of the sequencing chain of production. We might expect those working in Production to have a greater sense of the Institute as being a like a factory. Finishing, by contrast, is the most skilled of the sections, recruiting graduates for problem-solving IT work. We might expect those working in Finishing to have a greater sense of the Institute being like a university. This would provide us with a situation where the imaginations of work in the Institute were determined by the immediate material conditions of the research participants. This, unfortunately for the agreeably neat explanation, does not match the interview evidence gathered. Indeed, as we see in section F4, *The Institute as a University*, as exemplified in the case of Abi, some of the accounts that most explicitly reject the description of the Institute as being factory-like are provided by those working in Production.

The realisation though, that workers in a routinised, automated laboratory reject the factory as a descriptive label for work at the Institute, should not lead us to reject a material explanation for the variation between the accounts of research participants. We can still claim that the immediate conditions of work are straightforwardly responsible for the use of radically different archetypes *within* accounts. Whether looking down the sequencing chain of production, or reflecting on the changes in work at the Institute over time, reference to the division of labour, and to the degree of bureaucratisation, routinsation, and automation found at the Institute can be called upon to explain the internal variation of accounts[27]. The use, by people working in these sections, of the archetype of the university to describe Production, and the use of archetype of the factory to describe Finishing, requires reference to the wider experiences of work of the research participants. The 'hinterland of work'.

As Colin, a member of the Pre-Finishing team points out, the prior experience of workplaces presents the individual with examples for comparison. Or, perhaps, in some cases, a lack of experience leaves individuals with a poverty of reference points.

> Well I think a lot of people up here have come straight from university and never worked anywhere else so they probably, you know, they probably think this is just what a normal job is like possibly.
> [Colin]

The extract from the interview with Colin helps us to articulate the point that it is not only where a person is now, in terms of immediate working environment, which establishes their view of what sort of workplace is the Institute. But that we must also take into account where a person has come from; their hinterland of work. This hinterland should be understood as including both direct work experience and socialised expectations of a working environment. Considered this way, and condensing the archetype of the factory to routinisation, we might expect those research participants working in the most routinised sections to see the Institute as less factory-like than those in the least routinised roles. This is no longer a counter-intuitive suggestion. The research participants working in the most routinised sections tend to not have a degree, and to have employment histories outside science. In some cases this includes having worked in factories, in manufacturing. The socialised expectation of routinisation in work will therefore be relatively high. On the other hand, research participants working in the least routinised sections tend to have degrees in science and, where they did not enter the Institute immediately after leaving education, have experience of

[27] There are, of course, real differences between the different sections. The accounts of those, such as Elizabeth, who have worked in more than one section testify to this; she stresses the factory-like qualities of Production.

work in science. The socialised expectation of routinisation held by research participants whose hinterland of work is dominated by an education for a career in science will likely be very low.

This idea can be illustrated in following series of diagrams. Figure F5.1, *A Continuum of Forms of Work*, suggests that the Institute is neither factory nor university, but something in between, of the kind of organisation that writers such as Kleinman and Vallas (2001) suggest will be the form of workplace that is emerging as knowledge economies develop.

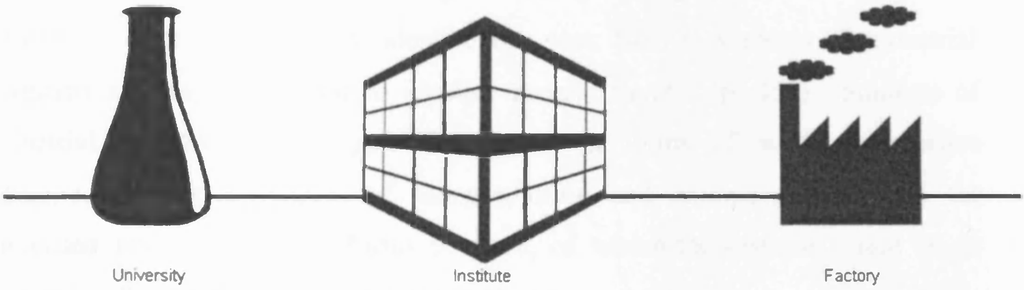

Figure F5.1 A Continuum of Forms of Work

On this continuum we can place all the apparent dichotomies of work described in section F2, *From the University to the Factory*. Wooffitt (1993) cites Gilbert and Mulkay (1984) to argue that "social events are the 'repositories' of multiple meanings, by which they [Gilbert and Mulkay] mean that the 'same' circumstances can be described in a variety of ways to emphasise different features" (p. 303). The diagrams below represent the 'view' of the Institute as it is approached from hinterlands of work dominated by the factory and by the university. The distance of the Institute from the hinterland is exaggerated, and the distance of the Institute from the oppositional archetype of that hinterland is minimised.

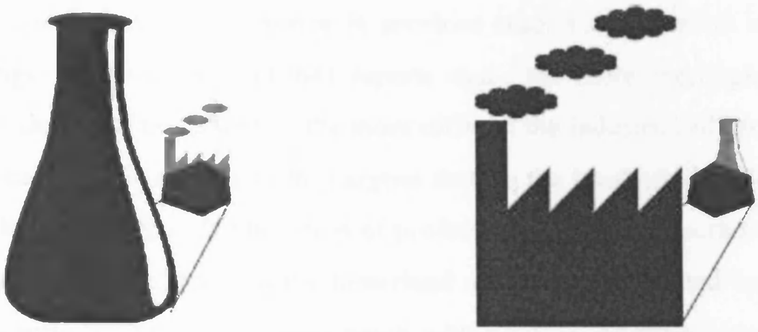

Figure F5.2 Views from the Hinterlands of the University and the Factory

Kleinman and Vallas (2001) suggest that this effect of perspective colours the academic literature of the knowledge economy. They write that "analysts of academic research commonly idealize the past, [while] students of industrial scientists and engineers tend to idealize present trends" (p. 452). Students of industrial scientists and engineers, steeped in forms of work organisation governed by the principles of manufacturing and commerce, celebrate the increased autonomy of new forms of work, of which the Institute might be an example. Researchers that have spent their careers examining academic work, much more strongly, see the erosion of autonomy in these new forms of work that are closer to industrial norms than is the academic ideal.

Wooffitt (1993) describes Gilbert and Mulkay's (1984) use of the concept of linguistic repertoires. He describes a linguistic repertoire as "a set of descriptive and referential terms which portray beliefs, actions and events in a specific way" (p. 292). Wooffitt reports that repertoires are organised around the use of specific metaphors and descriptive devices. We should note that Gilbert and Mulkay are arguing that *a* scientist has the use of two repertoires; the empirical and the contingent. And, indeed, we see that research participants working at the Institute are capable of drawing on two very different descriptive repertoires to describe work at the Institute. But more than that, the interview evidence suggests that the choice of descriptive repertoire is not determined by the immediate experience of work, but that metaphors and descriptive devices that are used are dependent on the 'hinterland of work' from which a person arrives at the workplace.

This conclusion finds a mirror in previous studies of scientists in non-academic settings. Abrahamson (1964) reports that "the more thorough the academic socialization of the scientist, the more difficult the industrial adjustment" (p. 208). In a similar vein, Miller (1967) argues that, as the level of education increases, so too does the degree of alienation of professionals in a bureaucracy. In both cases, the greater the degree that the hinterland of work is dominated by imagery of the university, the greater the mis-match with work in industrial science. Technical work occupies a curious middle ground, much like the Institute itself. Nelsen (1997) argues that technical workers are prone to suffering from a contradiction between their self-conception, in so much that they imagine that that they ought be part of a horizontally ordered work setting, and the ways in which they are perceived and treated, in that they are actually part of a vertically ordered work setting. Keefe and Potosky (1997) argue that for technical workers, "professional career expectations formed in educational programs have rarely conformed to their job experiences" (p. 55). For technicians with expectations of being accorded professional status, though, this mis-match leads to status incongruity, role ambiguity and feelings of deprivation. Keefe and Potosky write; "Technicians invidious comparisons with professionals [...] and high levels of status inconsistency were correlated with expressed dissatisfaction, increased levels of interest in unionization, heightened interest in occupational certification, and reduced occupational and organizational commitment" (p. 55). In the terms used by Flecker and Hofbauer (1998), the socialising process of an education in science "generates 'superfluous' subjectivity, that is, aspirations, desires for self-actualisation and so on, which exceed what is required" (p. 121). In Chapter G, *The Recruitment of Sentiment*, we see how, despite the kind of predictions that can be drawn from the work of Keefe and Potosky, at the Institute the sentiments of technicians was recruited into the task of sequencing genomes as much as was their mental and manual labour.

In Chapter J, *The Institute and the Knowledge Economy*, we explore the ways the Institute can be seen in the context of the development of knowledge economies. One aspect of the Institute that connects with debates about work in knowledge economies is the way that it was organised. Barton and Delbridge (2004) argue that the growth of 'progressive' human resource management (HRM) practices, such as those discussed in Chapter H, *The High Road to the Human Genome*, is driven, at least in part, by the desire for innovation to meet competitive demands. This is the 'new manufacturing', the 'learning factory'. "The emphasis on innovation in manufacturing has called into question the traditional division of labour under Taylorist approaches to the management of the labour process. [...] the need for workers who *think* as well as do requires a 'post-Fordist' management system [...] the reintegration of production and innovation and of intellectual and physical labour" (p. 333). The trend, Barton and Delbridge argue, is towards the 'upskilling' of manufacturing workers into 'knowledge workers', with responsibilities beyond physical work. A second way in which the exploration of the Institute can have value in discussions regarding the knowledge economy is as an exploration of the subjective experience of technical workers.

Keefe and Potosky (1997) write that "science technicians, as a distinct occupational group, appeared at the historical moment when science was bureaucratized and relocated into large public and corporate bodies" (p. 54). In this sense, technicians are substitutes for scientists, allowing them to dispense with the routine tasks and pursue research unencumbered by the repeated and repeatable processes that characterise science as a service. In the case of the Institute, scientists were able, through routinisation and automation, to 'hive off' the 'mindless' and 'robotic' work of genome sequencing and pursue the work that better suits their education, training and skills; scientific research. Here, 'scientific research' is imagined as opposed to the knowledge creating enterprise of genome sequencing.

As well as being skilled, working with complex technologies or technical processes, and manipulating symbols, technical "occupations are also characterized by the acquisition of a craft version of professional knowledge" (Keefe and Potosky, 1997, p. 53). So the workers at the Institute are different from other skilled workers, in that the knowledges that they use are derivative to a system of formalised knowledge, in this case that of molecular biology. In this sense, the construction of the occupation of technicians is 'passive'. Whereas the professions and the crafts are 'active' with regard to the boundaries of their occupations, set through struggles to control the system of knowledge, the processes of training and recruitment and so on, the occupation of technicians is shaped by powerful actors from outside. "The new occupation is assembled from tasks discarded by professionals through the "hiving off" process" (p. 54). Therefore, "most technicians work in complex organizations where they neither set the entry and performance standards of their occupation, nor control the educational process through which new recruits are trained. Most cannot formally self-regulate or self-govern their work practices. Most important, technicians often operate within an established profession's field of knowledge and competence" (p. 54). This results in a blurred identity, they are highly skilled but they are also within the orbit of the dominant parent profession. This sets claims that technical workers will be a new class, reshaping the social, in a rather more pessimistic light.

The idea that approaching the Institute from a particular hinterland of work provides speakers with a preference in the use of descriptive repertoires also helps to explain the fact that, even when genome sequencing was more a green-fingered craft, well before the Institute became as factory-like as it became, according to the accounts of the research participants, the archetype of the factory was the descriptive repertoire of choice for scientists and scientific commentators when writing about work in the Human Genome Project. But it is not so much a question of what work at the Institute actually is, but how it compares to where our commentators have come fom. In this example, we have people whose hinterland of work consists overwhelmingly of universities and small-science laboratories. From the perspective of a background in workplaces such as those, the Institute would look like nothing but a factory.

We should be careful. Gilbert and Mulkay (1984) write that "participants' use of language can never be taken as literally descriptive" (p. 15), and this is true in this case. The mistake of taking the accounts of research participants to be 'literally descriptive' is a two-part mistake. It does not refer simply to the mistake of taking the accounts of research participants as being in correspondence to the features of the external world. It can also refer to the mistake of taking the accounts of the research participants solely as attempts at correspondence with the features of the external world. As described in section F3, *The Institute as a*

Factory, when Hood and Smith (1987) described a future large-scale project to sequence the human genome, they were practising large-scale politics. Presenting genome sequencing as industrial presented genome sequencing as eminently doable, as a practical investment of the resources of the state. This suggests that we should be aware of the small-scale politics that make the accounts of our research participants something more than solely attempts at description.

As Wooffitt (1993) reminds us, "descriptions are designed not merely to *represent* the world, but to do specific tasks *in* the world" (p. 297). In this, the description of the Institute as being like a university serves to highlight the way in which the workplace of the Institute is unlike industrial and commercial settings. The comparison is used, with few exceptions, to stress the subjectively positive features of work at the Institute. Vice versa, the description of the Institute as a factory serves to stress the features of work at the Institute that make it unlike work in a university or research laboratory. The comparison is used to stress the negative features of work at the Institute.

[G] THE RECRUITMENT OF SENTIMENT

Work is imagined as existing somewhere on the spectrum from manual, in which case the workers are hands, to intellectual, in which case the workers are minds. Work, though, can also draw on the heart; or the sentiment. In some cases this is overt, as in the case of emotional labour (Hochschild, 1983). In other cases sentiment is recruited into manual and mental labour. This recruitment of sentiment is experienced as engagement, as intrinsic satisfaction. Work, through the recruitment of sentiment, itself has a value. But just as in the term 'recruitment of sentiment' we avoid using the word sentiment in the sense of mawkishness, we should avoid that kind of sweet sentimentalism in our exploration of the Institute. Sentiments do work other than to warm the subjective experience of work; the recruitment of sentiment involves the internalisation of the values of the organisation, it eases the extraction of discretionary effort, and it hides the nature of the relationship between employer and employee. Sentiments have value.

G1 INTRODUCTION

In the title of their own contribution to their edited collection *Workplaces of the Future* (Thompson and Warhurst, 1998), Warhurst and Thompson (1998) identify three aspects of the worker; hands, hearts, and minds. They write that "[t]he popular view is that organisations are opting, by choice or necessity, to engage with hearts and minds" (p. 1) in the face of changes that have, apparently, rendered the notion of workers as 'hands' obsolete. Hearts and minds, this thinking goes, are the parts of the worker from which value will be extracted in knowledge economies. For Warhurst and Thompson, this is a suggestion that 'sits oddly' with their view of the nature of contemporary workplace. As with much rhetoric of the development of knowledge economies, there is little attention paid to the continuities of work, or to the way in which these developments affect people whose work is outside the relatively privileged band of well credentialed workers that are unambiguously defined as 'knowledge workers' (Kleinman and Vallas, 2001). In Chapter J, *The Institute and the Knowledge Economy*, we explore the ways in which this study of large-scale genome sequencing can be used to illuminate aspects of the development of knowledge economies.

This chapter explores the ways in which work at the Institute, despite its label as a factory of genome sequencing, involves the recruitment of more than just 'hands'. Those employed at the Institute find that it is not merely the efforts of their bodies that are recruited into work. Workers find recruitment to the project is not, even, limited to their talents, skills, and knowledges. Rather, it is the case that, explicitly, implicitly, and/or contingently, the sentiments of those working at the Institute are recruited into the projects of sequencing genomes. With hearts so recruited, alongside hands and minds, those working at the Institute are *engaged* in their work. This recruitment of sentiment is a process that ameliorates the subjectively alienating tendencies of industrialised, factory-like work that are described in Chapter C, *Pathologies of Work in Big Science.* This chapter draws illustrations from the accounts of work at the Institute, categorising and re-presenting evidence extracted from interviews with research participants.

The accounts of the work at the Institute provide illustrations of several different ways in which their sentiment was recruited and their feelings were engaged. This chapter makes an attempt to tease these apart, to separate the strands and threads of the recruitment of sentiment present in the accounts of the research participants.

The chapter opens, in section G2, *Exceptionalism*, with a discussion of the ways in which work at the Institute was considered 'special'. This section discusses, in turn, the way in which the notion that science is a special form of work, that the human genome was a special project, and that the Institute itself is a special place of work. The idea that work at the Institute is different from other kinds of work shape the accounts of research participants.

In section G3, *The Spark of Competition*, this chapter discusses an aspect of work at the Institute that is particular to the case at hand; the race to sequence the human genome. To some extent this way in which the sentiments of research participants was recruited into the Institute can be understood as the negative counter-image of the exceptionalism of the human genome itself. Negative, due to the fact that sentiment was recruited as a consequence of the existence of a 'villainous' other. Mordred, perhaps?

The recruitment of sentiment discussed in sections G2, *Exceptionalism*, and G3, *The Spark of Competition*, can be considered particular to this case and a small number of other situations. Not all work can drape itself in the exceptionalism of science, or motivate through a moral narrative of competition. In sections G4, *Celebrating Work* and G5, *The Work is Flat*, the focus of the chapter shifts from the special to ways of recruiting sentiment that are more general. In section G4, *Celebrating Work*, this chapter explores the ways in which the accounts of research participants contain fond memories of events and parties at which being a part of the Institute was celebrated. Section G5, *The Work is Flat*, deals with the more day-to-day ways in which work at the Institute was experienced as being part of a collective, through the absence of overt symbolic markings of hierarchy. This aspect of work at the Institute is also discussed in Chapter H, *The High Road* *to the Human Genome*, in which the experience of work described by the research participants is connected to contemporary discussions on the way work is organised.

The final section that teases apart the different strands of the recruitment of sentiment is G6, *Personalising the Workplace*. This section discusses the ways in sentiment was recruited to the Institute through the proxy of the founding director. The person of the founding director is associated with the structure and organisation of the Institute, and the successes of the Project. In this, the founding director is described being 'contingently charismatic'. The theme of the personalisation of the subjective experience of work is returned to in Chapter I, *Interlude: Dis-Integration*, in which the character of the new director is understood as being responsible for the dis-integration of the collective community of the Institute.

This chapter concludes, in section G7, *Collectivism or Communalism*, with a discussion of a key difference between the orientation of the sentiments recruited in the case of the Institute, and those recruited in what we have considered to be the model of the *engaging* workplace, the university. The sentiments recruited in the archetypal of the university are, in the first instance, orientated around the individual or the small group; the scientist and the laboratory. This is exemplified in the pursuit of the individualised reward structure of science, built around names on papers. In the second instance we have the Mertonian notion that the sentiments of science are orientated towards the communality of the discipline, or of science in general. These are the questing knights and their squires of science, and their communality is not Medieval Christendom but the new genetics. But, as we noted in Chapter A, *Primers*, the human genome was not sequenced by a Gawain or a Lancelot, but by the regimented armies of Modern science. And, in line with this metaphor, the sentiments recruited to these armies are those orientated towards the concrete collective, rather than an abstract communal. In this case, towards the Institute, and the Project.

We describe how engagement can be considered the opposite of alienation, in terms of the subjective experience of work. However, this does not mean that the recruitment of sentiments at work is something that should be read as an unproblematic positive. While such recruitment of sentiment may engage, which ameliorates a subjective sense of alienation, it might also serve to co-opt workers in their own exploitation. Successful recruitment of sentiments into the service of a workplace can obscure the exploitative relationship between employer and employee. The idea that commitment is merely a new form of control, suitable for the new organisational forms of the knowledge economy is discussed in Chapter H, *The High Road to the Human Genome.* The recruitment of sentiment is not new. Though the management trends that produce the Taylorisation of work involved the construction of the "will-less and opinionless 'man-as-machine'" as the 'model worker' (Flecker and Hofbauer, 1998, p. 105), this extreme, this 'triumph' of rationality, does not describe work in general. Rather, work at a specific moment in history, when the factory most resembled the archetypal image. At other points, the recruitment of sentiment has been important in the organisation of work, whether it was the master-apprentice relationship or the 'civilising' efforts of Christian industrialists. Describing manufacturing in pre-Fordist environments, Edwards (1979) discusses how, in the entrepreneurial manufacturing firm, it was the 'personal touch' of the owner that provided the sentimental glue that bound the workplace and eased the extraction of labour. "Loyalty had a direct and personal meaning", he wrote, but warned that the romaticisation of this era involved ignoring what this recruitment of sentiment actually does. "The personal ties [...] tended to obscure the real class differences" (p. 27).

At the Institute, the recruitment of sentiment did produce real effects in the way that research participants worked. A particularly marked effect is the role of sentiment in eliciting 'discretionary effort'. Jill, a manager of a Finishing team, recalls the early years of the Institute. At this time the Institute was still small, and there was close personal contact between the laboratory bench workers and the most senior of scientists. The hierarchy, symbolically, at least, was at its flattest, and the celebrations held to acknowledge achievement were the least contrived. Jill describes the working patterns of the time.

> And a lot of us were prepared to stay till seven, eight. I'm never talking ten and eleven at night. But yeah, I did work longer hours back in those days.
> [Jill]
>
> Was that because-
> [Researcher]
>
> Because I wanted to. I don't think there was even a general expectation that people would do that, I think it was just a personal choice.
> [Jill]

The history of the Institute, though, is just that; history. The narrative of this history involves growth, change, accomplishment and end-points, and, lately, diversification. In Chapter I, *Interlude: Dis-Engagement and Dis-Integration*, we explore the role these developments play in the research participants' descriptions of the way in which their sentiments are no longer recruited into work at the Institute with the same intensity. Jill illustrates the way in which this change in subjective experience of work has effects beyond the imaginations of each worker. To revisit an extract from the interview with Jill that illustrates discussions of the factory-like nature of the Institute in Chapter F, *The Factory and The University*, we see one of the effects[28] of the decrease in the recruitment.

> It's very much nine-to-five. You very much do the hours that you're paid for and no more.
> [Jill]

Ideas of the discretionary effort are discussed, among other things, in Chapter H, *The High Road to the Human Genome,* in which the Institute is considered from the perspective of the contemporary categorisations of work. This chapter concentrates on the different ways in which the research participants account for the recruitment of their sentiment.

[28] Of course, it could very well be the case that a change to nine-to-five timekeeping would itself produce a decrease in the recruitment of sentiment. This, though, is not the way that these changes were experienced by the research participants.

G2 EXCEPTIONALISM

This section examines the ways in which research participants understand their work as being in some way 'unique'. This 'exceptionalism' produces a sentimental attachment; there is something special about working at the Institute. The exceptionalism of working at the Institute comes in three forms. The first of these is the idea that science is a special kind of work, the second that there is something special about working on the *human* genome, and the third is that the Institute is an intrinsically special workplace.

It is the case that some of the research participants described that they applied to work at the Institute because they were motivated by an ambition to work 'in science'. Most of the research participants had very little concept of the Human Genome Project when they arrived at the Institute. Julia and Maggie are an extreme case, but they illustrate the fact that, for all the cultural power of the Human Genome Project, the notion that is was something that a previously untrained worker could be a part of was far from the imagination. Julia and Maggie were asked if, when beginning work at the Institute, they knew anything about the Human Genome Project.

> No, and considering we both live about two miles away from it.
> [Julia]
>
> Yes, so-
> [Maggie]
>
> I think you live two miles one way, I live two miles the other way. And no-
> [Julia]
>
> No, didn't know anything.
> [Maggie]

Despite the presence of one of the large-scale sequencing centres within a short drive of their homes, the notion of the Human Genome Project as an accomplishment of work was hidden from Julia and Maggie.

While the Human Genome Project was quite often absent from the imaginations of the research participants when arriving at the Institute, it many cases it soon became part of what made their work distinctive. There are exceptions. In the cases of Elizabeth and Neneh, whose accounts we visit in greater depth during this section of the chapter, the attraction of the Human Genome Project was the main reason they came to work at the Institute.

The combination of the notion that science is a special form of work, and that the human genome is itself a special object, produces a vision of a special workplace. It is a workplace that conducts special work and, since the accomplishment of the sequencing of the human genome, has achieved special things. However, the Institute is itself exceptionalised in the imaginations of some research participants, becoming a special workplace in its own right, without overt and explicit reference to the exceptionalism of the scientific work being conducted.

Science is special

There are arguments for objectively considering science to be a special form of work. These arguments might rely on the idea that science is special as a consequence of it being a creative pursuit of the unknown, or the notion that science is an activity conducted for the benefit of all humankind. If arguments of these kinds are to be a factor in the subjective experience of work at the Institute, in the recruitment of sentiment, research participants would have to understand the work that they do as being part of science.

The image of science as an engine that benefits all humankind is an idea that engaged several of the research participants. Claudia, a member of the Pre-Finishing team, had worked in a factory before she arrived at the Institute. She was asked to describe the difference between her previous work and work at the Institute.

> Oh, fantastic. When I first got here it was really good. I felt like
> I was doing something worthwhile.
> [Claudia]
>
> [...]

> Because I always wanted to do something good but I never quite knew what I wanted to do [...] It's nice to do something that you feel benefiting.
> [Claudia]

Elizabeth, a member of a Finishing team, claimed that she was not particularly interested in the biological significance of the stretches of sequence on which she had worked. However, the more general idea, that she was a part of a scientific project that would help us to understand human biology did recruit her sentiments.

> I was always quite proud of being in the- doing the job anyway, because even though what you were doing didn't look much, you knew that it went off and then somebody found genes and cures for things and whatever.
> [Elizabeth]

Despite describing herself as being disengaged from the details of the scientific knowledge that her work produces, Elizabeth was nevertheless engaged by the idea of working in science, in which science is understood as a motor of progress. The fact that, in the interview extract used to illustrate this point, Elizabeth speaks of her pride in the job in the past tense is a result of the double-edged nature of the 'triumph of accomplishment', which is explored in Chapter I, *Interlude: Dis-Engagement and Dis-Integration.*

Several of the research participants do recall being engaged with the knowledge that was produced by the Institute. Catherine, a member of a Finishing team, recalls the way in which this engagement was not the result of 'curiosity' as a feature of the abstracted worker, but of Institute policy.

> At one stage in the human they would – well, through the human in fact – annotators would tell you if there were genes and stuff found within your clones and what the genes were. After the human that kind of stopped. They did revive it for a while during the zebra fish, I don't know if it's still going ahead. But that's quite cool to know. That whatever you finished contained X and Y.
> [Catherine]

This policy, of feeding back to members of the Finishing team the biological significance of the sequences of letters that they arranged as part of their day-to-day work, injected 'science' into the working life of Finishers. Several of the research participants describe how this kept them engaged, even after the sense of 'doing something worthwhile' had shrunk in the face of the mundanity of day-to-day work. Neneh, a scientific administrator who had worked in Finishing, describes how this feedback kept her connected with the product of her work.

> But to start with it was really interesting because then you felt like you were doing something a bit more worthwhile. Because in some respects finishing is just- Because you spend so much time in front of a computer, you feel a bit like you're in your own little world and you're not really aware of the sort of effects that it has. But when you get that back saying that this is for such and such a gene and you can see that someone, somewhere, is going to use that you feel a bit more, 'Oh, that's good, I'm quite impressed about that.' Yeah.
> [Neneh]

Catherine sums these attitudes up in a single sentence.

> It certainly makes it a bit more scientific than just looking at a bunch of letters.
> [Catherine]

Not all sections of the sequencing chain of production received this kind of feedback. Claudia, who had worked in Production at the height of the Human Genome Project, did not receive the kind of feedback described by Catherine and Neneh. The attractiveness of the idea of receiving some kind of feedback as to the meaning of the work being conducted was evident from Claudia's response when she was asked about the feedback that had been received by Finishers. Claudia described how this kind of feedback would change the connection that she had with her work.

> No, and I wish we did. I'd love that feedback. It'd be great. The other day, was it- we have to say what we've been sequencing and someone came out with some posh word. And it's like, well, what's that? And they went, 'it's a pea'. It's like, well, why are we sequencing peas? You know, it just- it would be interesting.
> [Claudia]

The desire to be engaged in the products of work, to understand the meaning of the day-to-day labours, is evident in the extract from the interview with Claudia. Julia, like Claudia, a member of the Pre-Finishing team, also says that she would like to know more about the scientific significance of the laboratory procedures that she carries out. Julia suggests that the reason that she is not engaged is a product of her unusual working hours, which puts her outside the same circuits in which she imagines that the rest of those working in Pre-Finishing are involved. However, the feedback, which is identified by Finishers as being their main route to any engagement in the meaning of their work, was not a feature of work in Production or Pre-Finishing. So, while members of Production teams and the Pre-Finishing team are separated, even alienated (Seeman, 1959 [1970]), from the meaning of their work, for a time at least, members of the Finishing teams were subjectively engaged in their work.

Helen, a member of a Finishing team, describes how she feels that the process of engaging workers in the meaning of their work, by providing feedback on the biological significance of the sequence that they finish, does more than provide people working in Finishing with satisfaction.

> Because otherwise you just... it'd just a whole bunch of letters that you put together and it just means nothing. So it was really nice to know, and then it reminds you that it's important that you get it right.
> [Helen]

Helen suggests that engagement with the meaning of work is an important means of maintaining high standards of quality. This argument, given Helen's description of finishing as being 'just a whole bunch of letters you put together', applies even when conducting tasks that might be considered routine compared to the working lives of research scientists who have the room to act independently and creatively. If Helen's observation is correct, this supports the arguments found in Chapter C, *Pathologies of Work in Big Science*. One of the 'pathologies' of big science is not simply the subjective alienation of scientific workers, but that, in being alienated from the products of their work, scientific workers can be separated from the moral imperatives that commit those working in science to produce reliable knowledge.

Helen expands on her description of the effects of separation from the meaning of work. While, subjectively, engagement in the meaning of work is a rescue from routine, it also produces objective effects.

> And maybe that's one of the problems, maybe if they still e-mailed that around then the finisher might be thinking, well maybe I should make sure, doubly make sure, this is right.
> [Helen]

It is not just through engagement in the meaning of work that one has a sense of 'working in science'. The special nature of science is able to recruit sentiment into the workplace through other means. Elizabeth, who described her awareness that the day-to-day details of her job were part of the production of progress, described how she was also linked into the unique reward and recognition structure of science. Even though this connection is largely passive, being accorded the authorship of a scientific paper is symbolic marker of being part of the special community of science. Elizabeth describes how she discovered that she was a co-author on a scientific paper.

> I tell you what was quite nice, that I found out completely by accident. I was bored one day and I Googled myself.
> [Elizabeth]
>
> AB: Right.
>
> And I found out I am a co-writer on a paper about genes.
> [Elizabeth]

Elizabeth describes how this process did not produce engagement with the meaning of her work.

> [laughing] And I was none the wiser about this entirely. I remember the project because it was a complete and utter pain in the arse, which often the ones with gene-heavy areas are [laughing], but I haven't got round to reading the paper yet.
> [Elizabeth]

This formal recognition of their labour as scientific work is a symbolic marker that they belong to the special community of science. This does not, in itself, produce engagement. It does not, as we can see from the experience of Elizabeth, necessitate a connection with the meaning of work. But being part of the reward and recognition system of science does allow some of the research participants, particularly those working in Finishing, to think of themselves as not only working in science, but as scientists.

For some though, this sense of being involved in science is ebbing. This is part of the changing experiences of work that are explored in Chapter I, *Interlude: Dis-Engagement and Dis-Integration*.

> To the outside world I'm a scientist. To any other public or private institute, yeah, we're scientists. It is science – there was science in there somewhere once.
> [Catherine]
>
> [...]
>
> I mean, it's the whole finishing, sequencing – it's more production that true science.
> [Catherine]

As is discussed in Chapter F, *The Factory and The University*, there is a sense that the Institute is becoming more factory-like. This factory cannot recruit the sentiments to the same degree as the scientific, university-like workplace that the Institute was. For some, of course, there is little sense in which they consider themselves scientists, even if they do work in science. When Julia was asked if she thought of herself as a scientist, she replied; "Only when we're using dry ice".

The human is special

The idea that it was not just science, but the project to sequence of the human genome that was 'special', is a feature of accounts of engagement with work at the Institute. Again, there are arguments that make the case that the human genome is objectively a special object, but this is a discussion of the subjective experience of work at the Institute. The Human Genome Project is special, in both the public imagination[29], and in the understandings of those working on the production of genome sequence. In the accounts of research participants, the special nature of the human genome is twofold. First, the research participants present a sense of working towards the achievement of a goal of historical importance. Second, the research participants display a sense of working on a project that possesses a degree of fame and celebrity, even if the majority of those working to achieve it do not. Even so, more than one of the research participants was invited to discuss the Human Genome Project in the popular media, and these invitations were not limited to platforms that had any claim to being educational or otherwise 'high-brow'.

Neneh, who arrived at the Institute with a postgraduate degree in the biological sciences, describes the way in which the human genome became a part of her immediate connection with her work at the Institute.

> When I first applied for it the advert did say 'finisher' and I had no idea what a finisher was. So it was kind of a blind interview. But then I found out what it was going to be, and mainly then it was sort of the human genome. I thought that was pretty exciting and it was new and it was- you know, it was something at that point, well, still is really, that nobody else in Britain was doing. So I thought that was pretty cool.
> [Neneh]

It was the idea of sequencing the human genome that recruited Neneh's sentiments into her work.

> [...] I did get quite excited and quite proud of what we did really.
> [Neneh]

Elizabeth, who was engaged by the idea of working in science, came to work at the Institute in order to take part in the Human Genome Project. The public profile of the Human Genome Project ensured that it stood out from the rest of the world of science.

[29] As noted in Chapter B, *Anatomies of Big Science*, genetics is not just big science in some specific cases by virtue of organisational size, it is generically big science. It was, through the 1990s and into the twenty-first century, the scientific genre of choice for pop-science and science fiction.

> Oh yeah. I mean, that was the main reason I came here actually. It was something I was quite excited about. I got very into genetics when I was doing my A-Levels, and I had read a lot about it in papers and news and whatever so, yeah. I know not everybody did.
> [Elizabeth]

The exceptionalism of the Human Genome Project, in Elizabeth's account, is not an essential feature of the scientific object, but a product of the cultural position of the Human Genome Project at the end of the twentieth century. The human genome is not inherently special, nor is it necessarily special when considered by reference to the internal society of science. But the human genome is certainly a special idea in the public, political and journalistic imaginations.

Brian, the manager of the Pre-Finishing team, recalls the motivation that the sequencing the human genome lent those working at the Institute. The extract from the interview used to illustrate the recruitment of sentiment that was achieved by the exceptionalism of the Human Genome Project also contains its counterpoint; the dis-engagement that comes from the 'triumph' of accomplishment. Ideas such as this are explored in Chapter I: *Interlude: Dis-Engagement and Dis-Integration.*

> Yeah, I remember the motivation of doing the Human Genome Project. I think that's one thing that's changed as well for the kind of detriment of the institute is that when doing the Human Genome Project it was all, 'Yes, come on we've got a real good goal, it doesn't matter how much money we spend. We just need to get it done. This is fantastic, think of the future. We're all going to make a huge difference'.
> [Brian]

Brian binds the recruitment of sentiment that results from the exceptionalism of the human genome to other features of work at the Institute. Engagement and dis-engagement, integration and dis-integration; these processes involve a complex tangle of factors. The special nature of the human genome is a factor that is external to, though influenced by, the Institute. Nevertheless, the experience of the exceptionalism of the human genome is not immune to the organisational forms adopted.

> And now that's kind of been lost in the way the institute's now run because there isn't that kind of morale boosting talk going on any more. And people can't see the bigger picture because we have been split up into these little teams.
> [Brian]

The extract from the interview with Brian suggests that the increasingly acute, and material, division of labour at the Institute has had an alienating effect. Experiencing working on the human genome as work of an exceptional nature is therefore, according to Brian, inextricably tied to the organisational choices made at the Institute, and the culture that these choices creates.

Part of the culture of work at the Institute during the sequencing of the human genome was regular celebrations. These are discussed in more detail section G4, *Celebrating Work*, but the following extract from the interview with Elizabeth illustrates the difficultly involved in pulling apart the different threads by with the sentiment of workers was recruited into the task of sequencing genomes at the Institute.

> I mean the Human Genome Project, I think because it was, I suppose, it was human, there was definitely, especially when [the race began and the Institute] had a big pulling together and everybody felt a really big part of it. And you felt very proud to be in that, and I think that the idea of all working together was very good. And we also had, you know, parties. The parties are definitely an important thing. Whereas I don't think there is the same feeling about it with the mouse and the zebra fish. It's not got that, partly, I suppose, because it's not us. It hasn't got that kind of- I used, you know, to say with pride I was working on the Human Genome Project. Working with zebra fish or the mouse project doesn't really engender the same thing
> [Elizabeth]

The extract begins and ends with the assertion that the human genome is special, exceptional when compared to the genomes of the zebra fish or the mouse. In working on a project which has an exceptional object as its focus, there is a resulting sense of pride; Elizabeth finds that her sentiments are recruited into her work. However, this extract also suggests that the race to sequence the human genome, which is discussed in section G3, *The Spark of Competition*, and the regular celebrations, were important in recruiting sentiments into the work at hand. Elizabeth cannot separate the special nature of the *human* genome from the race to keep the genome sequence public property, or from the celebrations held to reward and recognise the accomplishment of milestones in the sequencing of the human genome. For many of those working on genome sequencing at the Institute, though the human genome has an inherently exceptional status as a scientific object, the experience of this cannot be separated from the contingent features of work at the Institute and the politics of the commercialisation of science. In their combination, these features of the experience of work at the Institute recruit sentiment, build a sense of collectivity, and make the working on 'the human' special.

The Institute is special

Both the exceptional nature of science and the special character of the human genome are features of work that are resident in the Institute as a workplace. This makes the Institute itself a special place to work. For research participants this is not limited to an account of how the Institute is simply a good place to work, but that it is a workplace with unique qualities that work to recruit their sentiments.

Julia describes her pride in working for the Institute. However, given that when she arrived to work at the Institute she had very little idea of its existence, Julia tempers her description of the pride of working at the Institute. She remarks that working at the Institute, naturally enough, only impresses those who have some idea what has been achieved at the Institute.

> I mean it's- you feel sort of proud when you say, you know, 'I work for [the Institute]'. But the majority of people who are normal, everyday people won't know what that is or what it's to do with. I mean when you perhaps are somewhere like at the doctor's, for example, and they might say, 'What do you do?' Then they know, and they say, 'Oh wow, you know, that's a cool place to work. [...] I mean I live [nearby] and the majority of people there don't even know, apart from seeing it on signposts. [...] But if people do know, you're sort of, 'Oh yeah, that's me. I'm part of that'.
> [Julia]

Julia identifies herself as part of the Institute, part of the workplace as a collective. For Julia, there is a pride not only in the particulars of her job, or in the work that she did that contributed to the Human Genome Project, or in the details of the work that she does now, but in the membership of Institute. Her pride in her work, the recruitment of her sentiment, is orientated around a sense of collective identity.

Jill describes the features of work at the Institute that make it a good place to work. But her description is not limited to the features of the workplace that could be built into unexceptional workplaces.

> The benefits of working here are enormous. In terms of sporting activities, there's a gym, the hours are flexible, there are free buses to work from [town], there's a fantastic green transport policy. I think, on the whole, if you asked people, it's a really good place to work. And I think because of what has been achieved by [the Institute] I think there is a certain pride in belonging to that.
> [Jill]

Free buses, gym membership, flexible hours; all these are features of work at the Institute that work to recruit sentiment, but they are not what makes the Institute itself an exceptional workplace. The exceptionalism of the Institute is found in its history, in its accomplishments. Jill describes pride in being a member of the Institute as a result of its history. It is the membership of a collective with an identity that marks it quite different from other workplaces, as being exceptional rather than mundane, which recruits the sentiments of the research participants.

G3 The Spark of Competition

As described in Chapter A, *Primers*, in 1998 Celera, led by Craig Venter, announced that they would sequence the human genome quicker and for less expense than the existing publicly-funded Human Genome Project. Sequencing the human genome became a race. Competition for priority is part of the history in science (see, for example, Merton's classic discussion, 1957 [1973]). This race, though, was different in two ways. First, the race was publicly declared and had a celebrity life. The race for priority was a subject for polarised reporting in the popular media. Second, this public life was a result of the fact that the outcome of the race was understood as having consequences far beyond the boundaries of science. It was not just that the sequencing of the human genome would change the world, but that the question of *who* sequenced it would determine the character of these changes. The race was characterised as being between the public good and private enclosure. Or, conversely and less commonly, between free enterprise and socialistic communalism. The experience of being part of a race brought its own effects to the way in which sentiment was recruited into work at the Institute.

One of the research participants had been involved in a race for priority before. He likened the experience of racing against the privately-funded project to his experience of the search for a gene involved in breast cancer.

> But I mean previously there was another race I don't know if you know about. It was for the breast cancer gene which was back in '96 in the old building. [...] But we were specifically racing against time to find the breast cancer gene against the Americans. And that really, really inspired people back then.
> [Brian]

Speaking of the race to sequence the human genome, Brian says that the spark of competition reinvigorated people.

> Yes it did. Although that [the race] was coming more towards the end of the Human Genome Project so there was less of that sort of enthusiasm. That did kind of spark it up again.
> [Brian]

In this account, the race to sequence the human genome is placed firmly in the tradition of priority races in science. The advent of the race was a spark that rekindled the fire of engagement in work at the Institute, which had been fading as the Human Genome Project moved past the excitement of beginnings. But while the outcome of the search for breast cancer genes has the potential to have a tremendous impact on the world outside of science, the race itself is not conducted in the full view of the public. The race between the public project and Celera held public and political interest that is usually absent from these competitive expressions of scientific creativity.

The public and political interest in the outcome of the race to sequence the human genome was an indication that this race for priority did not motivate and engage solely through competition. The race to sequence the human genome contained a moral and political dimension. To those who worked to sequence the human genome at the Institute, it mattered who won, not for the sake of pride, but because they were the 'good guys'. Helen describes this moral vision of work.

> In the past I really enjoyed being part of the Human Genome Project. I liked the idea that the sequence that we were putting out, it was going- I liked the whole race thing, that we were the good guys trying to put it out there for free, and it was going to be used to help researchers, with disease and stuff.
> [Helen]

So, the race was not just between 'us' and 'them', between one academic laboratory and another. In the emergence of a rival that was not just a scientific competitor but that was also a political antithesis of the scientific values of the Institute, the work acquired a moral weight. As Professor Ingham explains, this was a win or lose situation for more than just the laboratories concerned. The identity of the victor would have consequences beyond the distribution of the rewards of science, beyond even the award of prizes, or the little immortality that accompanies the naming of memorial chairs of genetics.

> [It was a w]in or lose situation. As I say, in terms of ownership, not in terms of the quality of the science or the way it was done. All of that was very irrelevant, but it was definitely the question of public ownership or private ownership.
> [Professor Ingham]

Contrary to the intuitive expectations, the advent of the race is not remembered as manifesting itself materially in the form of an intensification of work. We might expect a win or lose race with moral and political implications to lead to increased pressures to meet targets, to cut costs, to improve efficiencies. But this was not the experience at the level of the laboratory bench. When asked if there was any pressure to be 'economical' during the race to sequence the human genome, Brian, who, during that time, was then working as a sequencer, explained how the experience of the race was quite different to what might be expected.

> Certainly not, certainly not. It was more like an open cheque book. We could hire as many people as needed, we could throw basically everything chemical-wise at the projects that we could and no-one really batted an eyelid.
> [Brian]

It was not merely, then, that the race brought a sense of moral purpose to those working at the Institute. It was also that, in this case, the sense of the race being an all or nothing struggle appropriated the resources that were needed to ensure victory. This provided the Institute with the means that were required to liberate itself from the constraints of efficiencies and the binds of tight structures.

In this race between scientific organisations with ideologically antithetical understandings of the ownership of knowledge, the idea of being a 'good guy' and that the opponents are the 'bad guys' found expression right through the Institute. Most of the research participants were far removed from the politics of international science funding and intellectual property. Nonetheless, those working at the day-to-day production of sequence found their sentiments were recruited and organised against the enemy. Elizabeth describes the reaction of those working in Production to the advent of the race.

> [W]e weren't going to let him [Venter] get away with that [laughing], you know? So lots of pictures of him appeared with devil horns and things around the place [laughing].
> [Elizabeth]

When the researcher expressed surprise that someone working in sequencing would be aware of the race in this fashion, Elizabeth expanded on her description.

> Oh yeah. Oh yeah, definitely. Everybody knew about it. It was kind of a personal challenge. Everybody, it really did. I don't know whether that would work now, but it really was kind of, we weren't, after all that, we weren't going to let him go patenting all our genes and all that. [...] [T]here was definitely a feeling that this was a challenge for everybody.
> [Elizabeth]

Claudia also worked in Production at the advent of the race. Describing how it felt to do something worthwhile, she asked the researcher if he had heard of Craig Venter, and then explained the level of antipathy.

> Well, he was trying to get out exactly the same [sequence], but he was going to try and privatise it. [...] Whereas we were doing it for the public, and so that made it even more like, 'Oh my God, yeah! We must beat him because we don't want him, like, playing God with everybody's lives. And you know, I was really up for it coming into the public [domain], so yeah. It was really great. Great atmosphere. Everyone seemed challenged [...]
> [Claudia]

The race was not a common, garden-variety race for priority of the kind that is found throughout the history of science. Rather, it was understood by those working at the Institute as a contest between public good and private selfishness. The challenge of the race recruited the sentiment of those working at the Institute not only through adding a sense of challenge, but through injecting a moral dimension to their labours. This moral dimension required the presence of a villain, Craig Venter, who was both cartooned into the role of minor devil and feared as scientist who wanted to play at being God. While the recruitment of sentiment produced by the exceptionalism of science, of the human genome, and of the Institute are the result of positive associations, for the race to be a moral mission required a negative against which the Institute could be contrasted. And it was the Institute that was contrasted, as this moral dimension was not understood as question of private virtue of those working on the public project. Rather, being the 'good guy' was the collective virtue of the Institute. The sentiments were organised around the collectivity. In so much as they extended beyond the concrete and the local, the interests that those working at the Institute imagined themselves to be defending through their labours were those of all humanity.

G4 Celebrating work

Accounts of working at the Institute are marked by reference to regular, collective, on-site parties. As Elizabeth noted in the extract used to illustrate G2, *Exceptionalism*, these were important in creating a sense of 'all working together'. Elizabeth describes the way in which collective celebrations were made part of work life at the Institute.

> I know it sounds sort of weird because it's, you know, but [the founding director], they would always find some excuse for a big party in about June/July time. It wouldn't matter what the excuse was. It would be- they would find a landmark that had been passed and you would have a 10 mega base party or a 100 mega base party or 200 mega base or whatever or finish of the Human Genome Project. They would find one. There was normally a landmark that could be found around July time and they would have a party. We don't seem to do that now.
> [Elizabeth]

We should note that Elizabeth makes reference to the founding director when accounting for the regular celebrations. The personalisation of the engagement in work at the Institute is discussed in section G6, *Personalising the Workplace*. Elizabeth displays noticeable cynicism with regard to whether these celebrations were a response to the achievements of any 'real' landmarks. They would always find some excuse, she says. However, this cynicism, unlike that the kind which is discussed in Chapter I: *Interlude: Dis-Engagement and Dis-Integration*, is coloured by an appreciation of the benefits that these collective social events bring. Again, this account of engagement is marked by a regret that these events are no longer a feature of working at the Institute.

The lament that these celebrations are no longer a central part of a working life at the Institute is a feature of the accounts of many of the research participants. Neneh, who has moved into working in administration as the Institute has developed, placed her description of the celebrations in the context of an understanding of the economic pressures that led to the end of regular celebrations.

> And then when we did achieve things we had those big celebrations which was great, we were lucky to have them. [...] And it's the one thing now that people who have been here a long time always say that they miss. Because it was nice, and we would get- you know, they would write off an afternoon basically and say, 'Well done, go and have a party'. So we'd all just go and sit outside, which was nice.
> [Neneh]

The role that the laments of the ending of regular celebrations play in the accounts of the present is discussed in Chapter I, *Interlude: Dis-Engagement and Dis-Integration.*

Celebrations order the recruitment of sentiments around the sense of belonging to the Institute. For Durkheim, the maintenance of collective sentiments requires symbols. Symbols of the collective ideals can take many forms (Lukes, 1973). The symbol can be a place, such as a site of pilgrimage[30]. Symbols can be human beings; we find them in the images of the great men of science conjured by their hagiographers. In emblems; at the Institute we find the symbolic use of the double helix of DNA in both sculpture and stained glass. And, not least, in events, in celebrations. The role of collective events in maintaining the orientation of sentiments around the collective, the Institute, can be seen not only in the positive accounts, but also in the laments. And, interestingly, in the example of resistance to the recruitment of sentiment that we examine in Chapter I, *Interlude: Dis-Engagement and Dis-Integration.* The celebrations, never mind the bouncy castle and the free alcohol, might not carry the research participants to the heights of religious ecstasy. Nevertheless, they serve to create "periods of creation and renewal" when "men are brought into more intimate relations with one another, when meetings and assemblies are more frequent, relationships more solid and the exchange of ideas more active". They bring "men together and [make] them communicate in the same intellectual and moral life' (Lukes, 1973, p. 422, quoting Durkheim).

[30] The Large Hadron Collider, a big science apparatus cum institution, has been described, with irony but not without meaning, as a cathedral of science.

The parties and celebrations that mark accounts of working at the Institute helped to produce sense of collective identity. They ensured that the achievements of the Institute were felt to be the achievements of all the people working there. But they also were an attempt to ensure that the achievements of the people working there were felt as the achievements of the Institute. The celebrations, as the most visible sign of reward and recognition, involved senior scientists, team managers, and workers from right down the length of the sequencing chain of production, to those who spent their shifts pouring the gels for the automated sequencers, drinking and bouncing on inflatable castles side by side.

G5 THE WORK IS FLAT

Many of the research participants' accounts of the experience of work at the Institute describe very little overt hierarchy. This is not just in 'social' areas such as the cafe, in which participants describe no distinction between the members of the board of management, senior scientists, and those working at the day-to-day task of genome sequencing. More than that, research participants recall that there were deliberate attempts to avoid any sense of overbearing hierarchy; in work spaces as well as spaces that might be designated 'social'. 'Flat' organisational structure is something that has swung into fashion in the management literature, as we explore in Chapter H, *The High Road to the Human Genome.* In the case of the Institute, the impetus for adopting this model can be traced back not to MBA, but to PhD.

Professor Ingham, a senior genome scientist, describes some of the motivation to build into the organisation of the Institute as flat a hierarchy as possible.

> [We were] keen and I think we were somewhat successful in running as flat a structure as possible. In other words, it wasn't tremendously hierarchical. In fact we interestingly, in a way driven by the staff, and with a tiny bit of management training, we were forced to make some structures, more than I would have liked. People like it, otherwise things get unfair. If you have a totally flat structure there are people who feel they aren't being treated right, they're not getting exactly the same treatment as someone else. So that means you have to make some sort of grades, promotion structure, and all the rest of it, and then everybody settles down. But I think in terms of the sense of teamwork I really wanted to have it as nearly as possible like [a small science, university-based laboratory].
> [Professor Ingham]

In this interview extract, Professor Ingham is explicit that the organisational character that senior scientific figures sought to create at the Institute was that of the small science model. The desire for a flat organisational structure did not come from management training, which, indeed, was a non-existent expertise until after the Institute was established. Rather, it emerges from the cultural origins, from the hinterland of work, of those with responsibility for creating the

Institute and leading the Human Genome Project; the world of work of small science.

The extract from the interview with Professor Ingham describes the way in which, as the Institute developed, the peculiarities of the organisational model of small science became apparent. These peculiarities rubbed against the nature of a workplace that did not adopt the scientific reward system of publication, esteem and autonomy. To resolve this, a greater degree of formal structure was required, but the cultural origins, indeed, the hinterland of work, from which those creating the Institute emerged, produced the argument that the model for successful scientific work was as flat a structure as possible. Susan, a member of the board of management, recalls the way that the work of sequencing genomes was originally imagined.

> And the way the original proposal was set out was that basically we would have seventeen teams of ten people doing the same thing in parallel. [...] And that team of ten was based on the lab structure that [the founding director] had built up at the [small science laboratory] to do the first worm sequence – cosmid sequencing. So we were just going to multiply it umpteen times.
> [Susan]

The original vision was for the Institute to be, in effect, a series of small science laboratories. This did not happen, as the difficulties of performing "sub-cloning, picking, sequencing, and then the finishing, all done within that little environment" [Susan] were evident before the Institute was established. Problems of expertise and technique were later coupled with the pressures of the race. As described in section G2, *Exceptionalism*, this race was understood as a 'win or lose situation'. Professor Ingham describes the 'irrationality' of the rationalisation that the race demanded of the Institute.

> So at that point it was beyond science, now we were talking politics and economics. [laughs] To simply keep the thing in the public domain. That's a whole different kind of pressure. There's absolutely no reason to do genome sequencing as fast as it was done. It would not have been much slower anyway to be honest. Saving a few months became very important at that point for very irrational reasons.
> [Professor Ingham]

The race was understood as one of the factors that denied the Institute the 'flat' organisational and cultural model chosen for it by senior scientists. This entailed the loss of the small science-style recruitment of sentiment, organised around a laboratory, around individual reward and recognition by scientific peers, and abstractly, around sets of ideas. However, this was off-set by the fact that the race itself worked to recruit sentiment, as described is section G3, *The Spark of Competition*. By producing a world in which there were 'good guys' and 'bad guys', the race to sequence the human genome engendered a different sort of recruitment of sentiment. This was organised around the larger collectives of the Institute and the Human Genome Project.

Despite the way in which the values produced by a hinterland of work in small science were forced to adapt to the demands of large-scale sequencing, the attempt to produce a flat culture was felt by the research participants. 'Flatness' was felt in the informality of the relationships of the research participants with their superiors. This is seen in both accounts of work, and accounts of the way in which members of the institute socialised together. Again, we find that the descriptions of the past arrangements of work at the Institute are contrasted with an undesirable present. Claudia, who had worked in a factory prior to joining the Institute, described the increasing formality of work at the Institute. This formality makes the structure and hierarchy of work plain.

> Yes, definitely. Whereas before I'd just go in the office and go, 'Oi! You', just face-to-face, talk to someone. But it it's like, well, 'mail the boss'.
> [Claudia]

This does not so much describe a change in the hierarchy of the Institute, but a change in the way the hierarchy of the Institute is experienced in day-to-day work. The effects of the increasing visibility of the hierarchical structure of the Institute on the experience of work are discussed in Chapter I, *Interlude: Dis-Engagement and Dis-Integration*.

Hierarchical relationships are not limited to differentiation by reference to formal rank. There are also hierarchies of esteem. A steep hierarchy of esteem would impress itself upon social relationships between people even if their formal ranks are relatively undifferentiated. This is the kind of hierarchical relationship that we see in academia, often despite the lack of great differences in formal rank and bureaucratic power. In the case of the Institute, the hierarchies of esteem were not experienced by the research participants as being particularly steep. Julia and Maggie, in the context of describing the way in which they could find out about the biological significance of their work, such as through the seminars that are available, describe the degree of social differentiation in the canteen.

> Oh yes, there's some very clever people.
> [Julia]
>
> Yes, very.
> [Maggie]
>
> Because you wouldn't know it really. You know, you wouldn't know it from sitting down in the canteen with them[31].
> [Julia]
>
> No.
> [Maggie]
>
> Or, whatever, you know.
> [Julia]
>
> So very open?
> [Researcher]
>
> Yes. You know it's- there's no sort of top people sitting down at one end and the other- you know, you wouldn't know whether you were talking to the cleaner or the-
> [Julia]
>
> No.
> [Maggie]
>
> The person in charge.
> [Julia]

[31] This might be a subtle reference to a lack of social skills among those people classed as 'very clever'.

This is almost certainly an idealised vision of the way in which the canteen operates as a social space. Nevertheless, this ideal is believed to be true. Section G4, *Celebrating Work*, describes how the 'social' aspects of work at the Institute are not limited to the canteen, the coffee shop and the laboratory. There are also celebrations. These celebrations break down the experience of a hierarchy of rank and esteem. With celebrations such a central aspect of work at the Institute during the Human Genome Project, a key aspect of the experience of work are occasions that demand that people behave as equals. Indeed, as people who are sometimes embarrassingly equal, as Claudia remembers the founding director at one of the celebrations.

> Running down the corridor at a party, booze, girl in arms, having a laugh [...]
> [Claudia]

This diminution of the experience of hierarchy recruits the sentiments of those working at the Institute. The director, the members of the board of management, the senior scientists, the team managers, all are 'equal' to the bench worker, all are members of the Institute, and, as so, share the same interests. Elizabeth describes how her friend recalls a particularly informal interaction with the founding director.

> Elizabeth: [...] I mean, a friend of mine is quite proud of the fact that he went to [the founding director's] leaving party and gave him the bumps [laughing].
> [Elizabeth]

These celebrations, as we discuss in section G4, *Celebrating Work*, not only rewarded people collectively and tied the sentiments of those working at the Institute with the successes and achievements of everyone working there, but they also demanded that people working at the Institute experience interaction with each other as equals. They worked to 'flatten' the experience of hierarchy of the Institute. That the Institute felt like a socially flat place of work was the product of the values derived from the small science hinterland of work of its leaders, and the deliberate fostering of a sense of collectivity.

G6 Personalising the Workplace

Sentiments might be collective, as Durkheim argued, but they are experienced as being acutely personal. Therefore, it is not surprising that the personalisation of the workplace is one of the most common ways in which the research participants illustrate the way in which their sentiment was recruited. In working at the Institute, their sentiment has been orientated towards the founding director, who is a *contingently charismatic* leader. As the founding director is a symbol of the values of the Institute, the sentiments are recruited into the service of the collective. The personalisation of the workplace mirrors Edwards (1979) description of entrepreneurial firm. But the power of contingently charismatic leadership also has its limits. "Naturally, control exercised personally by the capitalist could be no more effective than the force of the individual capitalist's personality" (p. 26-27).

Claudia describes how the founding director transformed the most individual of events into a collective accomplishment that would recruit sentiment to the collective. The founding director was awarded the prize in question for work that he had conducted before founding the Institute.

> I mean even when [the founding director] picked up the award, he was so humble about it. He was- you know, it's just like, you felt proud working for him because he was so nice and, like, 'I take this on behalf of everyone, not just myself'. [...] Even back to the people in the lab and that, you just think, 'Yeah! That's great, you know, he didn't forget anybody'. And then obviously we've got a new person in now who maybe ain't the same. You know, he's not the same.
> [Claudia]

This extract from the interview with Claudia demonstrates two aspects of the personalisation of the workplace. First, it describes one of the more overt ways that the charisma of the founding director recruited sentiment from those working from him. Second, it illustrates the way that research participants bcate many of the changes that have taken place at their workplace in the personalities of the directors. The recruitment of sentiment was not just organised around the founding director, but also around the Institute. By thanking 'the people in the lab' the founding director made a successful show of humility with regard to the individualised rewards and recognition that he was receiving, and included all the people of the Institute in the highest reaches of the reward system of science. They were included as members of a collective, rather than as individuals.

Catherine, a specialist member of the Finishing team, personalises the changes that have occurred in the organisation of work at the Institute. Catherine feels that the main change in the way that work at the Institute is arranged is in the increasing sense of structure. She explains the comparatively unstructured past of the Institute through reference to the personality of the founding director.

> I suppose it's the changeover from [the founding director] stepping down when the human was kind of finally complete to [the new director] taking over. I suppose [the founding director] essentially an old school scientist and a hippy. So everything was very laid back and it was kind of like, it's science it'll happen in its own time.
> [Catherine]

For Catherine, as for others working at the Institute, the personality of the founding director enjoyed a degree of correspondence with the arrangement of work. Under the leadership of the new director, Catherine describes the organisation of work and the definition of roles at the Institute as becoming increasingly defined.

> So [the new director] kind of coming in it's less laid back, it's a lot more work centric, a lot more- I think there's a lot more structure as to what's expected of you.
> [Catherine]

Catherine's experience of being engaged with the Institute and the Project is inextricably connected to her experience of the founding director as a person. This provides people working at the Institute with a way of personalising the end of the Human Genome Project and the post-human genome diversification of the Institute. The structure of work, and through this, the character of the Institute, is understood by reference to the personalities of the directors. Understood in this way, the celebrations, the sense of flatness, indeed, all the aspects of work at the Institute that were engaging and integrating, were the products of the personality of the founding director.

At the very end of her interview, Helen suggested that one aspect of a working life at the Institute that had been missed in the conduct of the interview was the importance of the person of the director.

> I think a big change was with the change of director. That... I think that made a big difference because they had different priorities; they came from different points of view. [...] But because [the founding director] had been there from the beginning and he always came across as being this really easy-going nice guy, but then [the new director] came over from America, and he was much more American in, like, I don't know, his style of going about things.
> [Helen]

This 'American' style is experienced as an increase in the visibility of management practices, of pushes for efficiency, of targets and monitoring. The ways that these changes in the subjective experience of work are accounted for by the research participants is discussed in Chapter I, *Interlude: Dis-Engagement and Dis-Integration.*

The person of the founding director is the object upon which many research participants locate their engagement and integration with the Institute. The way in which the person of the founding director recruited the sentiments of those working at the institute was by making them feel a part of the Institute. Even when personalised in this fashion, the sentiments that are recruited are organised around the collective. Julia suggests that making people feel part of the Institute was a distinctive feature of the founding director.

> Julia: And like [the founding director], I mean he was always for that, wasn't he, when he was here?
> [Julia]

It is possible to understand the founding director's personality, in terms of his attitudes towards work, as the product of enculturation in small science; his arrival at the Institute from a hinterland of work in academia. Indeed, in Chapter H, *The High Road to the Human Genome*, we suggest that the similarity of some features of the Institute to the kinds of practices that are advocated for managing knowledge work are a feature of convergent evolution rather than common descent. From PhD, not MBA. But the new director is also a scientist. Reflecting on Helen's observation on the origin of the directors, it certainly might be possible to say something about the academic cultures of Britain and of the US that produced the founding director and the new director respectively. The new director certainly was a far better fit for Lewontin's (2001) characterisation of academic molecular biologists as being thoroughly entangled in commercial biotechnology; the new director certainly had experience of the commercialisation of science. But it is not simply a question of the personality of a leader producing corresponding structures in the workplace. Even powerful characters, even the most charismatic, must bow to the fact that "[m]en make their own history, but they do not make it just as they please; they do not make it under circumstances chosen by themselves, but under circumstances directly encountered, given and transmitted from the past" (Marx, 1852 [2006]). As much as the accounts describe the shaping of the Institute according to the character of the directors, these accounts also personalise and individualise the organisational structures and changes that are the products of circumstances beyond even the will of accomplished scientists.

True, the founding director was charismatic; it was common for research participants to describe his easy-going, laid back, yet motivational nature. But he was contingently charismatic; the historically important project, the vast resources that accompany such a project, and the fact that the Institute began under his guidance as a handful of people all allow the demonstration of charisma. Conversely, the new director is, as a consequence of the accomplishments of the Institute, the leader of a workplace that is without a moral mission such as the Human Genome Project, that, in order to survive, needs to diversify, ending the position of large-scale sequencing as the purpose of the Institute, and which, in the face of funding agencies whose pockets no longer seem bottomless, needs to economise. These circumstances are not the product of the character of the directors, but the experience of their character is a consequence of these circumstances.

Mommsen (1989) describes Weber's definition of the charisma of a person as being "the strength of the belief of the masses in their capacity for leadership" (p. 13). In the circumstances of the Institute during the Human Genome Project, the research participants held full belief in the capacity of the founding director to lead. Charismatic leadership "motivates the leader's followers from the inside out. They are supposed not only to lend full support but also to rationalize their own conduct in accordance with the ideals spelt out by the leader" (p. 116). The contingent charisma of the founding director recruited the sentiments of the research participants into the Institute, into the ideals of a collective workplace with a mission to secure the sequence of the human genome as a public good.

We should reflect that in so much as charismatic leadership contains the potential to be creative and innovative, for Weber this can serve as a counter-balance to the sclerosis of bureaucratisation as described in Chapter C, *Pathologies of Work in Big Science*. As Mommsen (1989) describes the problems of rationalised, bureaucratic rule; "the impersonal nature of legal rule, associated with the progressive elimination of all forms of individual activity, creates conditions which will eventually precipitate its failure, or more often its petrification" (p. 116). Incentives for innovation are lost, as is the ability to react flexibly to meet challenges from within and without. Is the solution to the problems of the Modernisation of science a combination of charismatic rule and bureaucratic governance? Brown and Hesketh (2004) identify a change from bureaucratic management to charismatic leadership as being one of the images of the transition to a post-industrial future.

But charisma is not some essential feature of a human being. Not only is it social being that determines consciousness (Marx, 1859 [1999]), seen in this example in the way that the founding director's arrival at the Institute from hinterland of work in small science can be seen as shaping his ideal of the workplace, but that the contingent circumstances determine the range of charismatic leadership that can be displayed. If we are to avoid the pessimism of big science, we might then suggest that, in order to retain the dynamism and creativity of science in the face of scientific organisations that are Modernised, rationalised and bureaucratised, we cannot rely on the emergence of 'great men', but rather we need to defend, or create, systems that produce contingent charisma.

G7 COLLECTIVITY AND COMMUNITY

This chapter demonstrates that is difficult to tease apart the separate threads by which sentiment has been recruited to work at the Institute. The exceptionalism of work in science, of the human genome, of the Institute itself, coupled with the race to sequence the human genome, the celebrations to reward and recognise the achievements of the Institute, the culture that eschews overt hierarchy, and the charisma of the founding director; all are intertwined in the recruitment of sentiment. In this chapter, we comb out some of the tangles that are present in the accounts of the research participants. Nevertheless, while ideal types might have sharply defined edges, there is a reason why these only serve as scaffolds are not a descriptions of the real thing; messy interweaving *is* the description of the social shape of work at the Institute, not a failure of description.

The sentiments recruited from those working at the Institute are organised around the larger collective, the Institute itself. At Chapter I, *Interlude: Dis-Engagement and Dis-Integration* shows, some of this collectivity was a product of the small size of the workforce. This size was amenable to close social relationships between people working at the Institute, between people of different rank in the hierarchy. Size does its own work in generating a feeling that there was a 'flat' structure. Elizabeth describes the feeling of the Institute during the early years.

> Especially when I first started it was almost a sort of- there were so few people and your team was like a family and it was really close.
> [Elizabeth]

As the Institute grew, the degree to which the Institute could be experienced as a 'family', and could therefore lay claim to the sentiments of the research participants, was diminished. This, as we see in this chapter, was ameliorated by the conscious creation of a feeling of collectivity through events such as celebrations, as well as through the personality of the founding director. Circumstances allowed the founding director to write a laid-back feel through the structures of work at the Institute. The 'specialness' of various aspects of working at the Institute also engaged research participants, as did the firing of the competitive sparks as the race ignited. These worked not only to recruit the sentiments of those working at the Institute into their own work, but also into the Institute and the Human Genome Project.

The recruitment of sentiment is the product of a workplace in which there are intrinsic satisfactions. Watson (2003) cites Parker (1983) to list some features of work itself that are indicators of increased satisfaction and dissatisfaction. The experiences of creating something, of using skill, or working wholeheartedly and working with other competent people, all build satisfaction. The experiences of doing repetitive work, of only making a small part of something, of doing useless work, of being insecure, of close supervision, all generate dissatisfaction. Watson (2003) describes how work can have intrinsic satisfactions, if it is enriching, challenging and fulfilling, for example. These provide work with expressive meaning. Work, of course, can also provide extrinsic satisfaction, in which work is a means to an end. In these cases work has instrumental meaning. As we see in the case of Abi, described in Chapter I, *Dis-Engagement and Dis-Integration*, there are some at the Institute who resist the recruitment of sentiment and yet are not dissatisfied. Rather, they find their work extrinsically satisfying.

Watson (2003) cites Blauner (1960) to discuss satisfaction. Blauner suggested four main areas that govern the degree of satisfaction experienced. First, the relative prestige of the job. Second, the degree of independence and control that the worker has with regard to their working conditions. Third, the degree of social satisfactions enjoyed from working in a group. Fourth, the degree that the worker is involved in non-work socialisation with colleagues. Examining this list, we can see these fit onto the people working at the Institute, helping us to understand the recruitment of sentiment and the degree of commitment that they display. The work that they do was prestigious, in that it was part of the Human Genome Project, and it remains prestigious, though not to the same degree, as it is work in science. The work is not directed on a minute-by-minute basis, with responsibility devolved. Though this could be seen as an internalisation of supervision, it is experienced as some degree of autonomy. The work might not be obviously social, but it is not isolated either. This is most clearly demonstrated by the stories of the parties and celebrations enjoyed by those working at the

Institute. The Institute clearly was, though less so now, an occupational community.

Occupational communities can be the product of spatial or temporal isolation. However, Watson (2003) describes how other groups of workers, for example, the professions, can constitute occupational communities. In this vein, scientists, perhaps subdivided into particular fields, can be said to constitute an occupational community. The occupational group acts as their key reference point, in a similar way, though not geographically bound, to the role it plays in communities of, say, industrial workers. Watson cites Salaman (1974), who observed that there are occupational communities based on shared geography, but there are also occupational communities based on the occupation as a whole. Salaman described the similarities between architects as a profession, and a geographically delineated occupational community of railway workers. Work at the Institute falls into the middle ground. Just as in Chapter F, *The Factory and the University*, in which we described how technical work requires the acquisition of knowledges that are derivative to a system of formalised knowledge, the sense of community at the Institute is derivative of the occupational community of geneticists. But, given the industrialised nature of the Institute, the occupational community is also localised, on the workplace. The recruitment of sentiment to the work at the Institute is orientated around this localised community; the recruitment of sentiment at the Institute is orientated towards the collective.

The collectivisation of the recruitment of sentiment is one of the ways in which this 'big science' is discontinuous from the laboratories of 'small science'. It was envisioned that the Project could be completed by a series of small science laboratories working in parallel, a product of the small science hinterland of work from which the instigators of the project emerged. But for technical and political reasons a collectivisation of effort was required, with an increasingly acute and concrete division of labour. As the Institute is not a series of small science laboratories, it was not surprising that the sentiments recruited into the work were not organised around the same principles as those of the academic scientist. They were not individualised, but collectivised.

As described in section G1 of this chapter, *Introduction*, the recruitment of sentiment, though engaging, is not an unproblematic positive. It hides differences in interest between employee and employer, and facilitates greater exploitation. In the context of a post-Fordist economy the recruitment of sentiment presents further problems. Watson (2003) cites Gorz (2002) as arguing that post-Fordist employment produces pressures on the worker to commit their 'whole self' to the organisation. In an archetypical Fordist system, labour is understood by employers as a mere commodity, and thus work is conceived of the selling of labour-power. In a post-Fordist environment, argues Gorz, employers demand a higher level of commitment, to the degree that the autonomy, and autonomous identity, of the worker is threatened. In Chapter H, *The High Road to the Human Genome*, we explore one of the ways in which the response of work organisations to the challenge of post-Fordism, post-industrialism and the emergence of 'knowledge economies' finds analogy in the arrangement of work at the Institute.

[H] THE HIGH ROAD TO THE HUMAN GENOME

The term 'high road' carries moral cargo along its route. The Institute can be said to have taken the high road to the human genome in three ways. First, politically. The Institute was part of the public Human Genome Project, and sought to keep the human genome sequence as a public good rather than allow it to be enclosed as private property. Second, epistemologically. The public Human Genome Project adopted a method of sequencing the human genome that ensured the quality of sequence and coverage of the genome. This is unlike the private project, which, in addition to relying on brute computer power, relied on the public data in order to complete its draft of the human genome sequence. Third, organisationally, which is the 'high road' of interest in this chapter. Considering the ways in which the Institute differs from the archetype of the factory, and the way in which sentiment was recruited into work, it could be argued that it bears many features in common with the high-performance, high-commitment models that are described as being 'high road'.

H1 INTRODUCTION

Throughout Part Three, *Illustrating and Accounting*, we see the ways in which the Institute appears to incorporate features of the both the archetypes of the 'factory' and the 'university'. We know that when the Human Genome Project was proposed it was imagined as being quite unlike the work of science, instead being akin to the kind of work conducted in industrial settings. The Human Genome Project would require factory-like models of work organisation, with an industrial, production-line approach to the generation of sequence data. This claim was often coupled to, and in some cases the argument for a large-scale project to sequence the human genome was countered by, images of alienating, laborious and monotonous work. Some of those images are present in the accounts of those who work at the Institute, as illustrated in Chapter F, *The Factory and the University*, and Chapter G, *The Recruitment of Sentiment*. Nevertheless, in these accounts there also exists a second set of images; those suggesting that the Institute is university-like, something un-factory-like that carries with it characteristics of the academy. We find accounts of engagement and autonomy rather than alienation and control. Chapter G, *The Recruitment of Sentiment* closes with a reflection that the orientation of sentiments was quite unlike the individualised sentimental commitments elicited in a 'university' setting. Rather, sentiments were organised around the collective; the Institute. In this, we can say that those working at the Institute are committed to the organisation.

To move the exploration of the experience of work at the Institute from Chapter G, *The Recruitment of Sentiment*, we can refer to Etzioni (1961). Etzioni argued that moral involvement in the organisation was synonymous with high commitment. Using Etzioni's terminology, from the evidence of Chapter F, *The Factory and the University*, and Chapter G, *The Recruitment of Sentiment*, it appears that rather than the Institute possessing the coercive-alienative model of compliance relations, the type associated with the most negative visions of industrial work, or the remunerative-calculative model associated with instrumentalised visions of work, it instead demonstrates a normative-moral model of compliance relations. The question of sentiment is rephrased as a question of control.

Commitment, and organisational commitment, is not a new idea in sociology. Becker (1960) produced an early anatomy of the concept of commitment as used in sociology. In thinking about the meaning of commitment, we are reminded that it carries the meaning of being set on a course, of being bound by the decisions that we have made. If we are committed to an organisation, have we made a decision to join it on its route, to intertwine our interests with those of the organisation? Swailes (2002) reviewed the concept of organisational commitment. In this chapter we are concerned with attitudinal or affective commitment and normative commitment. The concept of behavioural commitment is beyond the scope of exploration by use of the kind of interview evidence that was gathered. Commitment considered attitudinal, affective or normative is considered by some to be 'real' commitment. Consider for example, 'loyalty' to an organisation, as measured by turnover rates. Loyalty resulting from an instrumental pursuit of the benefits of organisational membership, for example, a desire to retain a particular level salary or benefit, produces the same behavioural outcomes as loyalty resulting from "a strong belief in, and acceptance of, the organisation's goals and values, a willingness to exert considerable effort on behalf of the organisation, and a strong desire to retain membership in the organisation" (Swailes, 2002, p. 159). This kind of 'real' commitment demands the recruitment of sentiment.

This chapter examines some of the recent literature on the subject of high road, high commitment, high performance workplaces, and considers the degree to which the Institute can be considered as an example of this category of workplace. Section H2, *The High Commitment Workplace* explores the category of the 'high commitment' workplace, the criticisms, and the ways in which these ways of organising work are seen to 'pay off'. In section H3, *High Commitment at the Institute*, the descriptions of high commitment workplaces are compared to the experiences of working in the Institute. Section H4, *Convergent Evolution, Common Descent?*, places the experience of work at the Institute in the contexts of both its origins in small science and its position as a fundamental part of a knowledge economy.

There a variety of terms that have been used to describe some or all of the practices associated with high road management strategies. A recent paper identified the wide range of labels that have been used to identify all or some of the features that, in this chapter, are categorised as features of 'high-commitment workplaces'. This list, which while recent is more than likely to be some way from being exhaustive, includes, 'transformed work organisations', 'flexible/alternative workplace practices', 'functional flexibility systems', 'employee involvement systems', 'flexible production systems', 'progressive human resource management practices', 'high-performance work practices', 'high-involvement management' and the straightforward 'new work practices' (Kalleberg, *et al.*, 2006).

In this chapter, the terms 'high-commitment workplace' and 'high-commitment workplace practices' are used. These labels carry greater descriptive weight than 'transformed work organisation' or 'new work practices'. Other labels that are used to describe all or some of the features contained within 'high-commitment workplace' and 'high-commitment workplace practices' have a similar degree of descriptive content, for example 'high-performance organisation' and 'high-performance workplace practices'. For the purposes of this chapter, commitment is used rather than performance due to the nature of the questions asked and the evidence gathered. Interviews provide insight into attitudinal, affective and normative commitment, but are poor evidence on which to base statements regarding performance. The terms chosen also use 'workplace' where others might use 'management'. This is a consequence of the fact that, in line with small science being described as an adhocracy, the development of the organisation of the Institute was an *ad hoc* process of adaptation and adjustment. It certainly was not the imposition of the latest trends in management theory. To the degree that the Institute is a high commitment workplace, this is a quality of the workplace, rather than a conscious quality of management style.

H2 THE HIGH COMMITMENT WORKPLACE

Watson (2003) discusses the origins of 'high commitment' models of work organisation. He locates the advent of a discourse of high commitment in early 1980s exhortations to copy the Japanese model of industrial organisation. The aim was to emulate, in the industrial sectors of the US and the UK, the comparative success of Japanese industry. The popularisation of the idea that there was a new way of organising work that was being increasingly adopted is often traced to a 1985 article in *Harvard Business Review* (Walton, 1985). This new way of organising work was particularly suited to the rapidly changing high-technology late-20th century workplace. That *Harvard Business Review* is a magazine published for an audience of senior managers should not be ignored. Walton (1985) was not simply reporting, but he was also stimulating the adoption of at least the rhetoric of 'commitment' in management of the workplace.

Godard and Delaney (2000) describe high commitment workplace practices, though they use the term 'high performance', as involving "flexible work assignments, cross-training and team work, sustained by some form of performance-based pay, formal employee participation, and supportive H[uman] R[esource] M[anagement] policies (for example, job security)" (p. 483)[32]. Additional features of high-commitment workplace practices include "minimal status differentials" (Wood and de Menezes, 1998, p. 486).

Watson quotes Storey (2001) as arguing that the 'Human Resources Management phenomenon' is "an amalgam of description, prescription, and logical deduction", built on the assumption that "it is human capability and commitment which in the final analysis distinguishes successful organisations from the rest". From this assumption follows the conclusion that "the human resource ought to be nurtured as a valued asset" (Watson, 2003, p. 109; quoting Storey, 2001). There is nothing

[32] A study into the adoption of high-commitment workplace practices in for-profit, public and non-profit places of employment suggests that while public and non-profit workplaces might employ some practices that can be described as 'high-commitment', it is unsurprisingly rare to find performance-based pay (e.g. profit-sharing or bonuses) in these sectors (Kalleberg *et al.*, 2006). As The Institute is a non-profit scientific establishment, we should not expect to find performance-based pay, regardless of the adoption of other high-commitment workplaces practices.

necessary about the assumption, and so the deduction is contingent upon the degree to which this assumption is true. Furthermore, if we imagine 'human capability' as something broader than professional/technical/creative skills, and imagine an economy in which the market in 'human capability' is not a sellers' market, we should not find it difficult to imagine a situation in which an organisation reliant on 'human capabilities' is able to be successful with a strategy of treating 'the human resource' as a mere commodity. Treating labour as a mere commodity would be a 'low road' strategy.

Watson (2003) writes that in the case of an organisation that requires a highly-skilled workforce, perhaps in situations that involve working with complex technology, the choice might be taken to adopt a high commitment model in order to ensure the retention of these difficult to replace staff. At first glance, this would be an explanation for the choice of the 'flat' hierarchical structure of the Institute. However, the passage from Sulston and Ferry (2003), which we revisit several times in this thesis, demonstrates that in large-scale sequencing a significant number of jobs have low formal entry requirements.

> We would recruit unskilled people... This group would have no need of academic qualifications. We judged them on school achievements, interview and something by which I set great store: the pipetting test.
> (Sulston and Ferry, 2003, p. 75)

At the point of recruitment to the task of sequencing a new employee of the Institute carried with them only the general skills of a school leaver and the requisite level of manual dexterity. Further, in the interviews with the managers and supervisory staff there was no explicit discussion of the adoption of management practices derived from elsewhere, at least not until the appointment of the new director. Rather, at least in the understanding of the research participants, the management structure of the Institute grew 'organically' from the seeds planted at its foundation.

It would appear, then, that rather than drawing inspiration from Japanese corporations or the *Harvard Business Review*, the structure of the Institute, and its recruitment of sentiment and commitment, is a product of its origins. It might be big science, but it emerges from the laboratories of biological small science; workplaces with relatively 'flat' hierarchies, with close, informal, interpersonal contact, with autonomy and self-direction, workplaces where engagement with and commitment to the goals of the work are taken as a given. The laboratories of small science can be seen as the models of the high commitment workplace, just as the university and the campus are proving to be the workplaces that some high-tech and creative firms seek to emulate in order to produce an environment suitable for intellectual labour (Kleinman and Vallas, 2001).

Watson (2003), citing Sisson (1993), describes the aims of a high commitment strategy as being:

- the 'development of a highly committed and adaptable workforce willing and able to learn new skills and take on new tasks';
- the elevation of 'the management of people' to a strategic level of organisational decision-making;
- an emphasis on trust rather than rules and procedures;
- the encouragement of managers to become leaders and facilitators of cultural change by 'harnessing the co-operation and commitment of others';
- the move away from hierarchical organisations with a number of tiers of management, separate functions and tightly defined job descriptions to much 'flatter' and more 'federal' organisations;
- an emphasis on the flexibility of function, task, time and reward and on teamworking and 'single status' terms and conditions of employment."

(Watson, 2003, p. 109)

The standard argument in favour of adopting high commitment work practices is that such practices produce an increase in the amount of discretionary effort on the part of the workers. An Australian study (Weakliem and Frenkel, 2006) found that productivity does rise in a linear fashion, in line with increases in morale. This accords with the beliefs and attitudes of many managers, and with the assumptions of the 'high road' model of contemporary management "whose features include "employment security, teamwork, employee participation in decision making, and a relatively egalitarian status system"" (p. 336). Morale, though, is difficult to operationalise and measure, and so such causal and scientistic claims contain within them scope for dispute.

Manufacturing Advantage (Appelbaum *et al.*, 2000) is one of the definitive empirical studies of the high commitment workplace. Appelbaum *et al.* conducted an exhaustive study of the effects of 'high performance' work practices across three quite different industries. Their conclusion was that high-performance work practices increased productivity. Barton and Delbridge (2004) write; "There is a considerable body of research [...] that has reported a positive association between firm-level measures of HRM and organizational performance" (p. 332). The literature is described as 'optimistic', is so much that it is taken as a fact that progressive Human Resource Management can enhance the performance of organisations and employees. In answering the question of why organisations using progressive Human Resource Management succeed, they write; "[F]irms achieve their objectives by using the innovative abilities of individuals more effectively" (p. 332). These workplaces are 'adaptive'. Considering the subjective experience of work, Appelbaum *et al.* argue that there are also benefits for those working in companies that adopted high performance work practices. These are characterised by an increase in the intrinsic rewards of work; increased job satisfaction coupled with an increase in organisational commitment. As a result, Appelbaum *et al.* describe high performance work practices as 'win-win'.

When addressing the benefits of high commitment workplace practices peculiar to the contemporary economy, the argument runs that high commitment workplaces are "better able to cope with rapid technological change and able to offer high-quality work to a labour force that has grown in educational sophistication" (Kalleberg *et al.*, 2006, p. 272). Appelbaum *et al.* (2000) suggest this is a product of the increasing opportunities that high-commitment work practices present for the 'shop-floor' learning of new skills, techniques, and processes. Considering that the Institute is a relatively high-technology employer, we can see how these organisational advantages might well apply. The technology used to sequence genomes changed immensely over the short organisational history of the Institute. Coupled with, though not determined by, these technological changes, the work of sequencing genomes at the Institute has been reorganised and increasingly divided, coming to resemble a production line. If the argument for the advantages enjoyed by high commitment workplaces in a technologically changeable environment holds, and if the Institute is high commitment workplace, this may account for some of the success of the Institute in sequencing the human, and other, genomes. This would be seen especially in the maintenance of employee satisfaction in the face of work environments that are marked by rapid technological change, such as increasing automation.

Theorists who have argued that the high commitment workplace will be a dominant form of employment in the future have been criticised, as have those who laud the development of these work practices. Ramsey *et al.* (2000) suggested that, rather than an uncritical 'win-win' diagnosis of the effects of high commitment practices, the performance gains attributed to high commitment attitudes of workers might be better described in terms of task intensification. Victor and Stephens (1994) argued that the rise of what is characterised of the 'bureaucratic form' was met with intense moral discussion[33]. By contrast, the discussions of new organisational forms, such as high-commitment workplaces, have been dominated by the testing the economic claims, while little attention was being paid to the moral consequences these new ways of organising work. Victor and Stephens (1994) suggest that we should be engaged in just these kinds of discussions. The deskilling that is characteristic of the 'bureaucratic form' "is replaced with an incessant demand for innovation and adaptation". This demands that workers become "self-motivated, continuous learner[s]", or face "rapid obsolescence" (p. 481). The defined job is replaced by a system of flexibility in which "[t]ime, space, and shifting group membership and becoming the primary definers of responsibility and accountability" (p. 480). Furthermore, "[a]t just the time that organizational commitment to the employee has been thoroughly violated, the employee is expected to exhibit feverishly enhanced commitment to the organization" (p. 481). Relationships with fellow workers also take on a new

[33] The discussions in Chapter C, *The Pathologies of Work in Big Science*, owe much to these moral explorations.

character. “Flat organizations force interpersonal relations in more demanding and intrusive modes than ever before. [...] Teams and networks call for new levels and kinds of cooperation. No one can expect to escape the demand to interact and be interactive” (p. 481). These new relationships with fellow workers can also act as an invidious extension of management control, as Finlay (2001) suggested in his review of Appelbaum et al. (2000), in so much as they transform workers into the supervisors of each other.

Furthermore, as Weakliem and Frenkel (2006) describe, there are those who argue that many jobs contain within them no room for high performance. In these views, the majority of jobs do not contain within them the latitude, for example, in which to apply discretionary effort, rather requiring only a basic minimum of performance. This kind of performance can be obtained by ‘low road’ strategies, through existing systems of reward and punishments. They argue that in low-level jobs, particularly in times of unemployment, the fear of job insecurity can be used as an effective whip. In these circumstances, negative sentiment on the part of the workers has little effect on performance. Unless the knowledge economy is a booming economy, and unless the knowledge economy is dominated, rather than merely characterised by, knowledge workers in jobs that are designed so that contained within them is the room for the application of choice, the ‘low road’ will be a part of the future organisation of work in technologically advanced economies.

Some see the echoes of previous ‘revolutions’ in the organisation of the world of work. Godard and Delaney (2000) identify high-commitment management practices as a renaissance of the post-war ‘human relations’ US tradition of industrial relations. This tradition sought to displace the participation of unions in the organisation of work. Indeed, when research participants were asked whether they were members of a union the response was one of bafflement. Why join a union when you are on such informal terms with your managers? Godard and Delaney argue that, despite their emphasis on communication and participation, the rhetoric of high-commitment management “views management as the primary actor in the employment relationship” (p. 485). Furthermore, there is always the argument that labels such as ‘high-commitment’ and ‘high-performance’ are illusions. Rather than representing the adoption of distinctively new workplace practices, whether top-down or otherwise, these labels are instead the symbols of changes in the language and presentation of management.

It can be argued that ideal type of 'small science' is a paradigmatic, but unacknowledged, example of the high-commitment workplace. The popular image of scientists at work often that of people engaged in autonomous, largely voluntaristic labour (Yearley, 1988). This work is constantly changing as the cutting edge of science changes, and collaborations are formed and broken depending on the task at hand. In other words, high technology, flexible work involving a high degree of discretionary labour. However, as we reflected upon at the end of Chapter G, *The Recruitment of Sentiment*, the orientation of the commitment differs at the Institute from that of small science. In the case of the Institute, the commitment is orientated towards the large, concrete collective of the Institute, in some cases through the symbolic embodiment of the Institute in the person of the founding director. In the case of the ideal type of small science, the commitment is, as a consequence of the system of reward and recognition, towards often changing aggregations of people that are much smaller that the Institute; the individual scientists, the collaboration, or the laboratory. In so much as the commitment is to something larger, it is to an abstract collective; the scientific discipline or to science itself.

While science is work, science is a very special kind of work. Unlike the commercial settings that are the empirical ground in which much work on high commitment practices are planted, science is often insulated from the interference of external direction or economic pressures for product. Collins (2003) argues that small science is under little pressure to succeed quickly. Small science "can work toward a goal of excellence, defined by... [its own practitioners], without interference... the creators of an idea can be left to decide whether it is succeeding or failing" (p. 261). This, as Collins notes and as we reflect upon in Chapter B, *Anatomies of Big Science*, does not hold in the case of big science. Internal pressures to succeed quickly are also found across science, in the cases of races for priority. The story of the Institute involves both the external pressures of capitalised, politicised and public big science and a highly moralised race for priority.

The claim that the Institute resembles a high commitment workplace is not a claim made by the managers at the Institute. The language and rhetoric of 'high commitment' and 'high performance' did not emerge during the interviews conducted with managers at the Institute. Certainly, these interviews do contain descriptions of management and workplace practices that in some ways resemble the 'high commitment' ideal type. However, the attribution of the label 'high commitment' to these practices is that of the researcher, not a product of self-description on the part of the research participants.

The Institute is not a factory, despite the increasingly acute division of labour and the increasing automation of sequencing. It is not a university, despite its central focus on the production of knowledge and the degree of engagement engendered. These are broad, crude categories. The high commitment workplace, grounded in the sociology of work, is a category that might give us more purchase in our attempts to understand work at the Institute. Of course, it will not do to replace one unrepresentative but clean-lined picture of work in the big science of the Institute for another. The story of work at the Institute is not without evidence of dissatisfying, even alienating, rationalisation and bureaucratisation. These are illustrated in Chapter I, *Interlude: Dis-Engagement and Dis-Integration.* Looking at the experience of large-scale genome sequencing through the lens offered by the idea of the high commitment workplace is one that offers transferable value; lending insight to other studies of work, studies of science, and studies of work in science.

H3 High Commitment at the Institute

If small science can be considered a paradigmatic, if unacknowledged, example of the high commitment workplace, then we can hear the echoes of the hinterland of work of the founding director sounding through the organisation of work at the Institute. The structure of work at the Institute as originally envisioned reveals the evolutionary origins of the resemblance of the Institute to a high commitment workplace. This is an example of convergent evolution, not common descent.

Professor Ingham describes his ambivalence towards the centralised, big science of which he was a prime instigator; "in principle I would love to have everything totally dispersed and everybody do their bit." Unfortunately, although "we'd started that way [...] as the pressure grew to get it done in a reasonable time then we found ourselves condensing into fewer larger labs [...] there is an efficiency to be gained by doing some centralisation." To continue the evolutionary metaphor, it is common selection pressures, not common origins, which make the Institute appear akin to other models of organising work in a knowledge economy.

Susan, a member of the management board who has been at the Institute since its inception, recalled the intended organisation for work in the Institute:

> [...] the way the original proposal was set out was that basically we would have seventeen teams of ten people doing the same thing in parallel. And that team of ten was based on the lab structure that [Professor Ingham] had built up at the [small science laboratory] [...] So we were just going to multiply it umpteen times.
> [Susan]

Susan described how the original vision of work at the Institute was of a series of laboratories, each based on the successful model of work that Professor Ingham had enjoyed as a small science university researcher.

> Running in tandem, with each of the components run in each laboratory. So the sub-cloning, [...] picking, sequencing, and then the finishing, all done within that little environment.
> [Susan]

This vision is not one of a factory, as was suggested ought to be the structure of laboratories involved in the Human Genome Project. The Institute was not imagined by those actually creating it as the ideal of the Modern work organisation, involving an increasingly acute division, both in terms of individuals and space, of labour. Rather, it was imagined that the reproduction of an absolutely non-factory model of work organisation would generate the increased productivity required to sequence the human genome. Rather than a new model of work, what was needed was the existing form of scientific work, multiplied.

This could not be maintained as a practical organisational ethos. In part, as described by Professor Ingham, this was the result of an internal reaction against an organisational structure that was too flat. According to Professor Ingham, some members of staff at the Institute demanded the differentiation in status that a steeper hierarchical structure provides. And some is ascribed to the technical features of the science of genome sequencing. Susan was asked how long the originally envisioned structure had lasted.

> It didn't even make it out of the [small science laboratory where it was conceived] actually, because the sub-cloning fell down. And people – sub-cloning is a... molecular biology, and you need, I mean, we call it green fingers to do that. So it was clear from, you know, very, very early on that we needed a specialist group to look after that area. The other, the team of ten beyond that, with half shotgun, half finishing work, that made it through... quite a while actually, way into this building. So this building went up in '96, and we moved in, so through to the, almost to the beginning of the scale-up for the human genome. When we got to that we just needed to change the structure completely.
> [Susan]

Professor Ingham, though, as with Susan, makes it clear that much of the eventual industrial character of the Institute was the result of external pressures to sequence the human genome more and more quickly. For Professor Ingham, this reorganisation of the Institute in the face of external challenges was a source of regret. He describes the mixed success of attempts to organise the Institute along the lines of a small science laboratory, and the development of an 'industrial' division of labour.

> But I think in terms of the sense of teamwork I really wanted to have it as nearly as possible like the [successful small science laboratory]. And of course it wasn't, because, as I say, people were doing different kinds of jobs and the levels of qualification and indeed ability were different. I think the sense of working as an individual and yet having a common purpose, we retained. But certainly, obviously, the individual jobs had to be organised in a factory-like way, so that each person did what they could do best.
> [Professor Ingham]

These interview extracts demonstrate the debt that the work practices of the Institute have to small science, even if, more than a decade later, the origins are hidden by the subsequent adaptations. As was argued in Chapter F, *The Factory and the University*, the resemblances between the Institute and other forms of work are matters of perspective; they depend on the hinterland of work fom which the Institute is approached. The descent from small science identified here should remind us that the homologies that we find between work at the Institute ideas of high commitment work are not the result of the deliberate adoption of *en vogue* management practices.

This section of the chapter discusses these homologues, taking a selection of indicators of high commitment work and illustrating the degree of correspondence found at the Institute using interview evidence. These illustrations help us to explore the value to be found in describing the Institute as a high commitment workplace.

Low Status Differentials

Low status differentials between workers, and between workers and management, are a key feature of many descriptions of what makes a workplace high commitment. Chapter G, *The Recruitment of Sentiment*, discusses the ways in which the hierarchy of the Institute is made to appear as flat as possible. By means of informality between workers and management, both in social and in work settings, the appearance to the research participants is of a workplace where status differentials do not weigh too heavily on the conduct of day-to-day work. Professor Ingham, in his description of the philosophy behind his vision, independently captures the ideas of the high commitment workplace in near text-book terminology.

> For myself, I was keen and I think we were somewhat successful in running as flat a structure as possible. I other words, it wasn't tremendously hierarchical.
> [Professor Ingham]

Low status differentials are often seen as being a feature of an inclusive, egalitarian workplace. Cast in this light, we might expect this to be an unalloyed positive for the ordinary worker, if not for the management. However, Professor Ingham suggested that there was resistance to this feature of the organisation of the early Institute.

> In fact we interestingly, in a way driven by the staff, and with a tiny bit of management training, we were forced to make some structures, more than I would have liked. People like it, otherwise things get unfair. If you have a totally flat structure there are people who feel they aren't being treated right, they're not getting exactly the same treatment as someone else. So that means you have to make some sort of grades, promotion structure, and all the rest of it, and then everybody settles down.
> [Professor Ingham]

This is a view towards which scepticism would be a natural reaction, when voiced by management, especially when, as an apparent aside management training is invoked. However, this view was supported by the concerns of some of the research participants. The research participants who seemed to be the most concerned that the hierarchy formalises and hardens were not the team leaders and other members of management, but the technical workers, the bench workers. Cautious optimism was expressed for the increasing bureaucratisation of the promotion and grading structure of the Institute, a process that makes status differentials between technical workers, and between technical workers and management, more patent and unambiguous.

This can be understood in several ways, but it is not the rejection of an egalitarian workplace. Management, and, at the upper echelons, the scientific leaders of the project, may envisage work at the Institute as being part of their vocation. They have been socialised into the values of hinterland of work in small science. Scientific work brings intellectual rewards, some which carry with them grants of status, others of which are less tangible. This attitude towards work can be seen when Professor Ingham describes how his experiences of work have shaped the sort of ethos he wanted to instil in The Institute.

> I mean I personally have always, I was going to say tried, but that's not quite right, I seem to have fallen into a pattern of living to work, rather than working to live, because I have always done things which I feel are enjoyable and or important, to varying amounts at different times.
> [Professor Ingham]

Expecting everyone else to live by the same ethos, particularly when these other people are doing a very different kind of work, one that is not rewarded with recognition or intellectual satisfaction, and who arrive at the Institute from a very different hinterland of work, seems hopelessly romantic. For the scientist, differential reward can be achieved by 'soft' means; publication, citation, peer esteem, awards and prizes. For the technical workers there are no such sources of differential reward. Differential reward here cannot be left unstated, but must be made cold and patent. The 'hard' differentials of remuneration and rank.

It is also the case that members of the management of the science of large-scale sequencing, even if unable to participate in the traditional reward structures of science as a result of their administrative role, are part of a hierarchy of power. They occupy a supervisory role. While they might be happy for there to be an appearance of flat organisational structure under their supervision, the realities of control remain. For technical workers with no claim on the gross distinctions of rank and reward that come with management and supervision, more subtle differentiation is required. Further, some research participants suggest that in a structure marked by informality as part of an attempt to be 'flat' there is ambiguity in the criteria for promotion and reward, leading to suspicions of favouritism.

However, it is clear from Chapter F, *The Factory and the University*, and Chapter G, *The Recruitment of Sentiment*, that Professor Ingham is more or less correct when he says that the management of the Institute was successful in their attempts to keep status differentials at a minimum. Status within an organisation is not solely a product of rank and payment, certainly not within an organisation within the domain of science. The accounts of a tremendous degree of social 'flatness', to be found in descriptions of the canteen, the celebrations, and also the informality of worker-management relations, attest to the degree to which the Institute was able to adhere to the ethos that Professor Ingham brought with him from his hinterland of work in small science.

Flexibility

'Flexibility' is often seen as a key feature of a high commitment workplace. Not so much because, as is the case with flat organisational structures, flexibility generates high commitment. But because flexibility grants the space in which high commitment can be exploited. A flexible work environment in which people possessed a wide range of skills is a clear feature of accounts of work during the early years of the Institute. People who were employed as finishers performed their own pre-finishing laboratory reactions, and also performed the tasks of production when needed.

The flexibility, expected and allowed, created an environment in which there was the ability to move between different jobs. Joe presents an unusually extreme example of the flexible potential of the work at the Institute. Joe, who has been at the Institute since the mid-1990s, has had a wide range of jobs in his career here. Working in the Institute was his first scientific job of any kind and he arrived at the Institute after being made redundant. He was first employed as a 'prepper', which Joe describes as the 'lowest' role in genome sequencing. He describes his career within the Institute.

> Then I went onto sequencing. Then from that, I mean, you did that for a little while. Then from sequencing I was a finisher's assistant kind of thing. So I was doing all the reactions for the finishers. Then after doing that for a little while I then became a

> finisher. Did that for a little while. Didn't like it as much because I just couldn't – I didn't like the just sitting at a desk and stuff – what I didn't like was I couldn't see anything completed. [...] So I gave that up and then I moved into the QC team. [...] and then we've moved on to doing what we're doing now.
> [Joe]

When Joe was interviewed he was involved in the development of new laboratory robotics. Joe's path through the different work tasks of the Institute is extreme, but illustrative. Many of the research participants have been employed within the Institute in more than one job. The change in job does not always involve promotion to supervisory rank, but can involve a move along the sequencing chain of production. This is normally up the informal hierarchy, though there are movements down the chain when those moves are coupled with promotions to supervisory roles. There are also moves from large-scale genome sequencing to the small science research laboratories, and back again.

These accounts place flexibility as a quality of the workplace, of the Institute. Indeed, taking this benign reading of flexibility at face value is something that Victor and Stephens (1994) warned us against. Flexibility is not just, or even mainly, a quality of the workplace. Flexibility is also demanded as a quality of those conducting the work, transforming high commitment into high performance. The idea of broadly skilled workers, to be found in the early days of the Institute as a consequence of the ethos of Professor Ingham, has largely been replaced by increasingly specialised work roles.

As the Institute, after the accomplishment of the Human Genome Project, reorientates itself towards 'post-genomic' small science research, there is an example of one group of workers for whom 'flexibility' is an employment imperative. The degree of utilisation of members of the Pre-Finishing team has fallen. An effort is being made to broaden the skills of people working in pre-finishing in order for them to be temporarily deployed to other teams and laboratories when necessary. One might read this as an application of a post-industrial version of the Taylorist principles of 'scientific management', rational calculative maximisation of human resources. And, through this, an intensification of work. Colin, a member of the Pre-Finishing team, describes how this is experienced.

> [W]e're being farmed out a little bit now and again when other areas in a sense, and they just say – we've all been learning some new things which is great, so. [...] We're quite flexible, we're going to be really flexible – but you know, we're quite flexible now. We can just go off and help other teams, and everyone does things differently and everyone uses different equipment, so it's quite handy really.
> [Colin]

From outside work at the Institute we might take a cynical view of the attempts to maximise the utilisation of laboratory workers. But this is not the way in which these moves are experienced at the bench. The recruitment of sentiment, the generation of high affective and normative commitment, hides the differences in the interests of employers and employees. Colin does not feel that these changes are attempts to intensify the exploitation of his labour. The pre-finishing team regard these moves with little negativity or cynicism. These developments are seen as an increase in their opportunities, not an increase in exploitation. The expansion of these opportunities not only increases their employment prospects, more than that, these opportunities, through increasing the variety of work, improve the quality of their working lives.

In a different workplace or at a different time, changes such as those being experienced by the Pre-Finishing team would not have been experienced in this way. We could suggest that that the experience of the members of the Pre-Finishing team, who do not have a criticism of their transformation into a continually re-deployable reserve, is a product of the diminution of class politics in a post-industrial economy and culture. However, there is something more specific to the arrangement of work at the Institute occurring. We recall the comments of Victor and Stephens (1994) on the 'dark side' of the new organisational forms of work; the demands that workers become "self-motivated, continuous learner[s]" or face "rapid obsolescence", that "[a]t just the time that organizational commitment to the employee has been thoroughly violated, the employee is expected to exhibit feverishly enhanced commitment to the organization" (p. 481). Those working at the Institute have had their sentiments recruited into the idea of the Institute. Therefore, these were not disaffected workers being asked to, in effect, intensify their labours. These were not, for the most part, workers entangled in coercive-alienative or remunerative-calculative models of compliance (Etzioni, 1961). Rather, the recruitment of their sentiments, orientated towards the collective of the Institute, draws these workers into a normative-moral model of compliance. These workers are high commitment, but this should not be seen as unproblematically 'win-win'.

Supportive Human Resources Policies

Most organisations that arrange their work according to the principles of the high commitment workplace do not have a grand historical project through which to integrate and motivate their workers. In this, the Institute is an unusual example. Most high commitment workplaces cannot draw on the exceptional moral claims of science, that the advance of knowledge is for the good of all humankind, in order to recruit the sentiments of their workforce. Supportive human resources policies, though, are a possibility open to any workplace.

There exists within the Institute the potential to move jobs. In the case of Joe, this is explicitly in the pursuit of increased job satisfaction. Despite climbing the sequencing chain of production, he found the work of Finishing uninteresting and unengaging. He was able to move to a different job in search of engagement. Elizabeth too moved roles in order to seek job satisfaction. She moved from Production, which she likened to a factory, to work in one of the small science laboratories at the Institute. She was surprised to find the work in the small science laboratory that she joined was not much more interesting. Nevertheless, what this story illustrates is not that the work of technicians in small science laboratories is over romanticised, but that the Institute was accommodating to the ambitions of its staff. Elizabeth was able to move to a different job within the Institute, taking a route that was neither a promotion nor part of an expected climb up the sequence chain of production.

Supportive human resources policies are not only, or even mainly, about putting in place structures in order to allow workers to seek to maximise their job satisfaction. In a high technology, rapidly developing workplace such as the Institute, it should be expected that technology overtakes the necessity for some jobs. This was the case for Julia and Maggie. Julia and Maggie began work at the Institute pouring slab gels for the sequencing machines. A key development in the development of automated sequencing technologies was the introduction of capillary sequencing. This made the gel sequencing machines obsolescent, and with it the requirement for a team of gel pourers working shifts around the clock. While the technology that they worked with might have become redundant, Julia and Maggie were not. Through a series of jobs in other teams, they found permanent roles in the Pre-Finishing team. Julia and Maggie suggest that this kind of accommodation was made for all those who wanted to remain part of the Institute.

While there was little overt discussion of work practices that are 'family friendly', several research participants cited flexible working hours as playing an important role in the combination of work at the Institute and childcare. Sophie, a member of a Production team, described this as a key reason for taking a job at the Institute.

> More stabilised hours because I used to do a lot [...] when you've got younger children I supposed that used to fit, it fitted in then. [...] but then as they got older and sort of grew up you wanted something like say seven to three or something like that. Seven to half three, so you can go out at weekends. That is why I changed probably.
> [Sophie]

The Institute is also a place of education; the science of the jobs does not necessarily remain a black box to those working on large-scale genome sequencing. Susan, a member of the board of management, speaks with pride of the way in which the Institute is a place where people learn.

> The other thing we have run are sort of more educational seminars to, and a series of lectures, to try and introduce people who've got, you know, no scientific background to some of the science behind what they do.
> [Susan]

Do people enjoy that, then?
[Researcher]

Yes they do. Yes they do. And it's actually been very rewarding, throughout the whole project, the number of people who have gone back to, sort of night school, or do degrees.
[Susan]

Professor Ingham expands on this theme.

We did gradually put in more effort to [engaging employees with the scientific aspects of their work], and courses were put in to teach people. Because people come and ask me; "How does this work? Why are we doing this?" And I would explain, and we realised people wanted to know. And we found, as a further illustration, what was going on is that we found that we would take people who'd maybe dropped out of university or maybe just had A-Levels and they would become, having not been very enchanted with academic learning, and some of them became very enthused again and started to go on day-release courses to get their degrees and so forth. And so I had the very interesting feeling that we were running, not for all, but for a proportion of the people, a sort of apprenticeship system, where you come in through that way. And the thing is I'm totally sympathetic to that, because I know that I didn't like doing my degree at all, and I think that working with one's hands, actually doing things as you go along, as you learn. It's not the only way to do it, but it's a very satisfying way to do it. So people I think are, on the whole and increasingly so interestingly as the things settles down, have found themselves in a very benign environment I think in that way. They were free to grow, either doing the jobs that they did best, because there is a lot of quite intellectual sort of puzzle-solving work in sequencing and the finishing process. And people get very good at that and really enjoy it. And I do myself actually, it is the sort of thing I rather like doing. It's mindless in the sense that it's repetitive, but as [Nobel Prize Winner] and [Senior Genome Scientist] used to say, "no two pieces of DNA are alike" [laughs].
[Professor Ingham]

The Institute is self-consciously understood by its managers as a workplace that offered intellectual as well as financial opportunities. However, while the lectures remain, some respondents complain that the support from within the Institute for members of staff to take degrees has fallen away. This can be understood in light of the end of the rapid growth of the Institute, the increasingly acute division of labour that accompanies improved laboratory protocols and automation, and the end of the seemingly endless supply of money which was allocated to the defence of the human genome from appropriation by privateers.

The theme of staff development is taken further by Susan.

> [T]here was potential to grow and do everything. So [Professor Ingham's] idea was that people needed no training whatsoever, and, you know, some of the criteria that we used for selecting people were basically that they were bright, that they had manual dexterity, there seemed to be some sort of positive correlation with whether they'd worked in a bar, pretty much [laughs]. But basically they, you know, they needed no scientific background. [Susan]

We can read the positive interpretation of statements such as this, that the Institute is an organisation that develops people into workers well suited to a knowledge economy. However, we can read the flip side of these recruitment principles; that through this the scientific workforce is de-professionalised, even proletarianised. Rather than read this de-professionalisation as the passive result of advances in the automation of the laboratory, it can be seen as a managerial process that walks in lock-step with these technological developments. Automation and de-professionalisation are part of the same process of reducing the demand for skilled labour in big biological science.

The negative interpretation of the recruitment ethos of the Institute might win adherents from those who emerge from a hinterland of work in small science, particularly small science *circa* 1990. However, as we have established, the view from small science is a restricted view from a peculiar and unusual workplace. The leading figures at the Institute are not unaware of this perspective, or the truth that this view might contain. It was when discussing the decreasing proportion of people in the workplace with PhDs as science gets bigger that Professor Ingham found himself stressing his ambition to run the Institute with as flat an organisational structure as possible. It is by moving further down the hierarchy, to examine the Institute through the perspectives of those who emerge from hinterlands of work outside the world of small science, that we can understand the way in which this ethos shaped the experience of work.

Team Work

One of the roles of team work in high commitment workplaces is to generate a sense of collectivity, to intertwine the interests of individual workers with their colleagues and with their employers. While work at the Institute is organised into teams, many accounts present a narrative of 'autonomous', individualised work. Pat, who works as a 'prepper' in a Production team, interrupted the researcher when discussing the necessity of working at part of an interacting team.

> So you could arrive there and leave there without –
> [Researcher]
>
> Without seeing anybody yeah, can do.
> [Pat]

Pat was asked to describe the qualities that a person would require in order to work in Production.

> I suppose you have to be able to work by yourself.
> [Pat]

Self-direction is perhaps even more the case in Finishing, where the work is carried out on computers, which enables the atomisation of the working environment. One of the team leaders suggested to me that there is no reason why the work of Finishing could not be conducted at remote locations. For those working in a laboratory, in, for example, the Pre-Finishing team, at least some kind of co-ordination is required. But this co-ordination takes the form of the arrangement of work practices *around* the other people working in the same lab, not the arrangement of work practices *with* these other people.

As we see in Chapter G, *The Recruitment of Sentiment*, work at the Institute produces a strong idea of being a member of a collective. This collective, though, is the Institute, not the team. On a day-to-day basis, self-organisation is a principle feature of work at the Institute, not co-operative teamwork. This is despite the fact that the work of each of these individuals only makes sense in the context of a vast co-ordinated effort. At the level of actual work practices, rather than in terms of the community within which people work, the predominant experience is that of 'independence' at work. This self-direction is to a degree far greater than one might imagine is experienced by technical workers of similar rank and qualification in a small science laboratory. Despite the image of small science as autonomous, for technical workers such an environment demands a much greater degree of integration into the rhythms and habits of the team. And these would be defined by the idiosyncratic autonomous self-direction of the scientific leaders of these small science laboratories.

Self-direction is not the same as autonomy. For management, the question of commitment is a question of control. Through the recruitment of sentiment, which generates high affective and normative commitment, those working at the Institute can be relied on to supervise themselves. In so much as the role of team work in a high commitment workplace is to build into work demands for interpersonal relations that will hide the nature of the relationship between employer and employee, and to transform workers into the supervisors of each other (Finlay, 2001), the thoroughgoing engagement of those working at the Institute, as described in Chapter G, *The Recruitment of Sentiment*, supervises them from the inside out.

H4 CONVERGENT EVOLUTION, COMMON DESCENT?

Work at the institute is high commitment, yes. That much is evident from Chapter G, *The Recruitment of Sentiment*. But in important ways the Institute differs from the models of the 'high commitment workplace' found in the literature of the sociologies of work and management. There are some similarities, some correspondences between the experience of work at the Institute and the models of high commitment work. These are the product of convergent evolution. Kleinman and Vallas (2001) have shown us that there is a general trend for convergence in the forms of work in the academy and in knowledge-based industry. Faced with similar 'selection' pressures as knowledge-based industries, particularly dealing with new technology, retaining skilled workers, and motivating workers to exert discretionary effort, features of the Institute come to resemble those of the 'high road' models of workplace organisation. These originate not from industry, but are the product of the descent of the Institute from small science, drawing from this hinterland a whole set of ideas about what work should be. In the case of team work, as the Institute has diverged from the model of work in small science, so it has diverged from the model of high commitment work.

The Institute has low differentials in status. This is to be remarked on, especially when we consider the differences in formal accreditation between the most qualified members of the workforce, who possess PhDs, and the least qualified, who have arrived at the Institute with a handful of GCSE-level qualifications. Through this chapter and Chapter G, *The Recruitment of Sentiment*, we see the way that this works to produce workers who are bound into the values and goals of the Institute. The management of the Institute has made deliberate efforts to develop their workers, both in terms of redeployment when the racing of technology reduces the necessity for some jobs, which develops a sense of loyalty, and in a far less obviously instrumental way, through a variety of educational opportunities, which engage the workers and make the Institute something more than a mere place of work.

The absence of substantive teamwork at the Institute appears to be a key divergence from the high commitment models. However, this 'independent', self-direction is possible as a result of high levels of affective and normative commitment; real commitment. The absence of heavy-touch supervision, in line with the relatively flat culture within the Institute, is part of a process of recruiting the sentiments of those working at the Institute that produces workers as the supervisors of themselves. The lack of teamwork prevents the commitment being orientated towards the immediate work group. But this absence leaves those working at the Institute as committed to the goals and values of the larger collective, the Institute.

In many ways, the experience of working at the Institute is exceptional. Taking part in a grand historical project, working in science, taking the noble side in a race for priority, and working in an organisation that is, or has done, all of these things are all aspects of this exceptionalism. These cannot be replicated in other workplaces of technologically advanced economies. But, regardless of the origins of the work practices of the Institute or the particular modes by which sentiment was recruited, the development of a highly committed work force has allowed the Institute to cope with the rapid changes, both in technology and in mission, that are understood as characterising work in post-industrial economies. Commitment was integral to the early success of the Institute. Then, a small science ethos created an environment in which workers developed a wide range of skills and worked long hours to ensure the success of the Institute. Commitment continues to matter as the Institute has grown and changed, to ensure that the Institute is able to meet the challenges that are posed by the scientific landscape after the Human Genome Project. Some of the ways in which the commitment of the workers to the Institute is being threatened by the changes that follow the accomplishment of the sequencing of the human genome are discussed in Chapter I, *Interlude: Dis-Engagement and Dis-Integration.*

For all the engagement, we should remember to bear in mind the warnings of Victor and Stephens (1994) that these changes are not, by some magic, a means by which the different interests of employer and employee are reconciled. As Edwards (1979) reminds us, the notion of loyalty and commitment are not new.

In the case of the entrepreneurial firm, personal loyalties hide the nature of the economic relationships in the workplace. Nelsen (1997) reminds us that, for all the experiments in improving the quality of working life, whether it is by the development of small work groups, through increases in 'autonomy', or by means of participatory modes of management, the fact remains that it is the superiors who ultimately retain the right to introduce, and revoke, new work practices. This should leave us with no illusion that the power and command that is vested in hierarchies at work remain.

[I] Interlude: Dis-engagement and Dis-integration

As an interlude, this chapter is a separate movement, a change in direction. The explorations of the empirical evidence in Chapter F, *The Factory and the University*, Chapter G, *The Recruitment of Sentiment*, and Chapter H, *The High Road to the Human Genome*, describe engagement and commitment, movements away from the image of 'worker-as-machine' of the rationalised factory. In those explorations, we have seen the glimpses of counter-narratives. This chapter joins the currents of these counter-narratives, changing direction while remaining a part of the body of the thesis. From engagement, we move to discussions of disengagement, from integration into the collective of the Institute, this chapter turns our attention to the dis-integrating aspects of the workplace.

I1 INTRODUCTION

Many research participants temper their accounts of engagement, of the recruitment of their sentiments, with a description of the way that things have changed. Throughout Chapter F, *The Factory and the University*, Chapter G, *The Recruitment of Sentiment*, and Chapter H, *The High Road to the Human Genome*, we see how these descriptions of change are woven into the accounts of the research participants with the changes that produce the present workplace serving as a contrast by which to measure the past. These changes have a common theme; 'dis-engagement' and 'dis-integration'.

In section I2, *The Triumph of Accomplishment?*, we examine the disengagement that accompanied the loss of the unifying moral mission to sequence the human genome. For those working on the day-to-day tasks of large-scale genome sequencing, the triumph of accomplishment was momentary, with the effect of accomplishment on their experience of work leaving their sentiments unengaged.

In section I3, *Growing Apart*, we explore the way in which the growth of the Institute has produced its own dis-integrating effects. As the Institute grew, it lost the capacity to recruit sentiments by its similarities with a family. More than that, as the Institute grew, the division of labour not only grew more acute, it was written into the geography of the Institute. People became physically separated. The Institute found new direction after the accomplishment of the Human Genome Project in a diversified faculty of smaller science research. This dis-integrated the Institute, no longer were all those working at the Institute bound together.

In section I4, *Living to Work, or Working to Live?*, we ask what this interlude has demonstrated. Throughout this chapter, we should be aware of the tendency to romanticise the past, to indulge in 'Golden Ageism'. We should be careful to wear lenses that correct for the attribution of such a colour to the past arrangements of work at the Institute as we read the re-presented accounts of the research participants that make up this chapter. Even if we dismiss the accounts of the past as reconstructions rather than accounts of what was, this does not itself diminish the accounts of the present, of work at the Institute now being disengaging and dis-integrating.

Section I5, *Postscript: Resisting Recruitment*, provides us with further contrast, as we examine the account of Abi. Abi resisted the recruitment of her sentiment, resisting taking the goals and interests of the Institute as if they were her own. Her lack of engagement does not mean that she is dissatisfied at work. Mills (1962 [1963]) reminds us that dissatisfaction and alienation are not synonymous, and in Abi's case her evident satisfaction with work at the Institute is of the extrinsic kind rather than the intrinsic satisfactions described throughout Chapter G, *The Recruitment of Sentiment*.

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I2 The Triumph of Accomplishment?

The Institute was the site at which one of the most important pieces of late twentieth century scientific work was conducted. The sequencing of the human genome was not just a triumph over the mysteries and complexities of nature, but a victory over the interests of those who would privatise knowledge. As noted in Chapter G, *The Recruitment of Sentiment*, the exceptionalism of this mission and the moral dimension of the race recruited sentiment into work at the Institute. Victory in the race, and the accomplishment of the sequencing of the human genome left a sense of pride, but diminished the active role played by these exceptional forces in recruiting sentiment.

The research participants locate some of the disengaging and dis-integrating trends of the Institute in the end of the Human Genome Project. As Chapter G, *The Recruitment of Sentiment*, demonstrates, the human genome is special, engaging scientific object. Research participants are far less engaged by the process of sequencing other genomes, such as that of the zebra fish.

Elizabeth, who works in Finishing, describes the merits of the Human Genome Project as compared to the subsequent work performed by those employed in large-scale genome sequencing.

> [I]t's just less inspiring to be honest, dead fish. [Laughing] they don't inspire any kind of pride. I always [say], you remember the Human Genome Project? I did some of that. I don't say I work on zebra fish.
> [Elizabeth]

Neneh, who worked in Finishing during the Human Genome Project suggests that there is some excitement in sequencing the zebra fish, but then admits to herself that it is not as exciting as the 'big one'; the human.

> And again, zebra fish is still quite exciting because it's a modern organism. But then the human genome was the sort of big one

and it was the first one[34] and it was sort of a little bit more exciting I guess than the ones we're doing now.
[Neneh]

However, though Claudia describes the sense of feeling that her work is worthwhile has been slipping away, she suggests that there is nothing inherently dis-engaging in working to sequence the genome of the zebra fish. Claudia, a member of the Pre-Finishing team, suggests that the exceptionalism of the human genome is not due to anthropocentrism, but due to a sense of the sequencing project having scientific worth. In this, Claudia is not representative.

> I was feeling really, you know, like I say, really flat the other day. And the one thing our big boss did actually say was we're the only company in the world sequencing zebra fish. [...] I didn't know that. And he was saying how important it was, and it was like, 'Oh wow!' You know, make- like I say, I reckon that would help, if they could keep us informed of how we benefit people still.
> [Claudia]

Claudia sees that the specialness experienced while working to sequence the human genome is not something that rests entirely, if at all, with the intrinsic properties of the human genetic sequence. Rather, at least some of this sense of performing special work is a result of the way in which work is organised, recognised and rewarded. Scientific work might be worthwhile, but when the work of science is divided and routinised, the sense that the work is worthwhile can only be experienced if the organisation of work communicates this.

Considering the accomplishment of the sequencing of the human genome, Helen, a member of a Finishing team, describes how this moment in the history of the Institute was experienced once the celebrations had ended.

> Yeah, I think people... people weren't into it as much as they had been, and people were first starting to think, well, maybe it's time to move on now the human's finished. Yes. My team started to split up and move in different directions. Yes.
> [Helen]

[34] The human genome was not the first genome sequenced at the Institute. In developing the techniques and technologies that would be required to sequence the human genome, the smaller genome of a model organism was sequenced.

As the team that Helen worked in disintegrated, so those at the Institute experienced dis-integration. With the sequencing of the human genome came the end of one of the features of work at the Institute that had recruited the sentiments of those who worked. No mission was articulated that could take its place as a goal that bound those working there to the Institute, to keep them committed. The end of the Human Genome Project was also the end of the race. Elizabeth commented on the effects of victory in her account when talking about the zebra fish and the mouse sequencing projects.

> I mean it's not that you don't feel proud of your work, but it's not got the same kind of feeling, your camaraderie and everybody, especially when there was the race. Everybody likes a competition [laughing].
> [Elizabeth]

As we argue in Chapter G, *The Recruitment of Sentiment*, the feeling of working on something special when sequencing the human genome cannot be separated from the celebrations of work at the Institute. Or, indeed, from the feeling of being an important part of the Institute, or from the feeling of being part of a community of equals. As this chapter moves beyond the exceptionalism of the human genome and the race, we explore the ways in which research participants identify changes in these features of the Institute to account for their experiences of disengagement and dis-integration.

13 Growing Apart

In the majority of accounts, research participants locate disengagement and dis-integration as a product of the growth of the Institute. Alice's account intertwines a description of a loss of community atmosphere as a product of the growth of the Institute with descriptions of changes in the organisation of the Institute that are seemingly independent of growth.

> I'd say that it was a lot more of a community when we were a lot smaller. I can't remember how many staff there were when I first started, but you pretty much could recognise everyone by sight and you knew most people. And we'd regularly, pretty much celebrate in little milestones, I don't know, a party every one or two months. So there was quite a lot of socialising. Now we don't really have that and as you get bigger yes, you don't. I don't know most of the people here now.
> [Alice]

The end of the social events that bound together the people who worked at the Institute is part of the triumph of accomplishment; completing the Human Genome Project took away a uniting mission that gave the punctuation of work by collective celebrations meaning. The growth in size of the Institute makes these celebrations less practicable and less likely to work to recruit sentiment and instil commitment. Alice says that, contrary to her earlier experiences of the Institute over the past decade, she now does not know most of the people working at the Institute.

There are still some parties, of course. Whether involved in historically important scientific projects or not, most workplaces in the UK have a Christmas party. But Claudia describes how the character of these events is very different to the celebrations that marked the pursuit of the sequence of the human genome.

> When I got here everyone used to go to all the balls and the functions and it used to be really good, and everyone used to really look forward to it. And now it's like, 'Are you going to the Christmas ball?' And it's like, 'Piss off, not going there'. It's like, 'Oh, right. Okay'. 'Why would I want to go there?' But when I first started everyone was really into bonding and

enjoying each other. I mean, you still get the sports and that, but not everyone does that. I don't, yeah. I don't know, it just seems weird. It's, like, gone all flat really. Something that was bubbly when I got here has just gone.
[Claudia]

Alice suggests that once the Human Genome Project was complete there was less of an organisational impetus to develop and maintain a collective ethos. For Alice this is reflected in her description of the feel that large-scale sequencing at the Institute has, since the triumph of accomplishment, developed into a workplace with higher staff turnover.

> Yes, and it's something that [, through organisational change,] they want to encourage. I get the impression that they probably don't want anyone to see people who have been here for eight years. They want to see a much higher throughput of staff really. [...] Yes, I think now they want to really just encourage more graduates and they don't really want to see you staying in the job for a long time. They expect you to move on really and they've done this by [organisational changes that mean] you're not going to be able to achieve a higher pay any more. It used to be quite a wide pay band and now they've really narrowed it so that the only way to earn more money would be to be promoted and to keep moving on.
> [Alice]

Promotion is an altogether more difficult prospect than movement within a pay band. The opportunities for promotion are limited by the available vacancies and restricted by competition. Movement within a pay band relies on the demonstration of competency and experience in the conduct of the job that a worker is already performing. For Alice, as a member of the Finishing team, career advancement within the Institute means taking one of two paths. The first is a move up within the organisational hierarchy of the Finishing team, into management. The second is a move up the intellectual/scientific hierarchy[35] of the chain of sequence production, from Finishing and into Annotation.

> You either take a team leader role and do the management side or my path here would be Annotation I guess, yes. [...] I don't

[35] As we see in Chapter D, *Case*, the sense of the Institute as containing hierarchies which not only differentiate within teams but between teams in the sequencing chain of production is felt by research participants.

know really. I think probably I would like to work in management but there's fewer opportunities. It's probably a case of someone has to leave because there's only so many people with, so many teams, and it takes a team leader to leave. Whereas there are probably vacancies that come up in Annotation really.
[Alice]

Brian, a manager of the Pre-Finishing team, describes the effect of growth on the integration of those working at the Institute. In this case, through the way that everyday social activity is arranged.

I think it was a great thing that we had to do back then was pass everyone. Everyone said "Hi". Everybody went for a coffee together, we used go down the pub for lunch on Friday. Whereas things like that just don't tend to happen any more or if they do they happen on a micro scale.
[Brian]

In Brian's account, when the Institute was smaller it was possible to have a personal, social relationship with almost everybody who worked there. As the Institute grew, it dis-integrated. People can only have those kinds of personalised relationships with a limited number of people. Later in that interview, Brian says that there used to be a degree of physical face-to-face contact between people that has now been lost.

Well, it used to be in the first building that we worked in that you had to walk through the lab to go for a coffee. So everybody knew everybody, no matter what teams we were in or what you were actually doing. Everybody knew everybody and that was a really good atmosphere then. Whereas now you can walk down the corridor and not see anyone. And a lot of people now don't recognise 90 per cent of the people here.
[Brian]

Elizabeth describes the same developments.

So it is quite weird, in a way, having gone from- it was quite a small community that everybody- I mean it was also a complete gossip, you know, a grapevine, because if somebody did something the entire place knew, and they all knew who you were talking about. Which was fun occasionally, unless it was

> you they were talking about [laughing]. But it has changed, because there is just a lot more people doing things that you have no idea about [...] [W]hen it was all Sequencing and Finishing you interacted with somebody in every department very much. Because if you needed something doing, you knew the person to go talk to. Whereas there are whole sections in the building, whole groups of people that I never, ever have to have anything to do with.
> [Elizabeth]

Joe found an interesting analogy to explain what had happened as the Institute grew in size.

> I think it has changed the atmosphere, yeah. It seems less friendly. Less, yeah. Just like any other job, in a way. You could be working for, like, a London bus company, and you wouldn't know half the drivers, you know, at all the depots. You'd only know your depot, and I think that it's becoming like that.
> [Joe]

Rather than being integrated into the Institute as a whole, the social and sentimental connections of people working at the Institute are increasingly limited to their immediate work colleagues.

Brian argues that it is not the growth of the Institute that has led to this dis-integration. He suggests that the cause can be found in the fact that people have been moved around and physically divided, in 'separating teams out' and in 'building design'. Regardless of whether Brian is correct in his suggestion that the integration of the Institute could have been maintained as the Institute expanded, his account of dis-integration is not untypical. The process might not have created atomised individuals, but it did shift the sense of the collective, the 'we', from the Institute to that of immediate work and social groupings. This was supported by Neneh's reply to the question of whether there was a 'community' at the Institute.

> [T]hese days there's probably distinct communities, I would say that.
> [Neneh]

Elizabeth describes how the change in the size of the Institute has changed her experience of work.

> It's changed, the size of it and the fact that because of the size of it it's had to become a lot more like a company and there is a lot more officialism and red tape and you don't know everybody.
> [Elizabeth]

Elizabeth was asked if she could provide examples of these changes. Her response began by discussing the change from a 'laid back' atmosphere to one where there are defined targets. However, the concrete example that Elizabeth provides, with more than a hint of nostalgia, is one of dis-integration.

> I think just the sheer size of it has meant that there is a disconnection between the higher levels. [...] I mean I still say hello to everybody. I have not stopped saying hello to [the head of sequencing] or [the founding director] if I see them but I don't think there is that- you know you don't seem to- its got so big that you don't know people in every department.
> [Elizabeth]

Joe also described the change from the 'laid-back' atmosphere.

> It was a nicer atmosphere, yes. But it was easy-going. I mean, the work got done, but nobody was there, like cracking a whip over you to get it done. Whereas now it's- I wouldn't say it's cracking a whip but there is more pressure to get the work done on time.
> [Joe]

This is made manifest in the increasing visibility of targets.

> It's more target orientated. There are product- you know there's schedules for all different clones and different organisms to be done by and within budgets as well, of course.
> [Joe]

The development of the Institute repeatedly reminds us of the development of the firm, from craft workshop, to entrepreneurial firm, to Fordist production, and latterly, to diversified post-Industrial organisation. Edwards' (1979) account of the expansion of entrepreneurial firms describes the demand that this expansion places on the forms of control. "[T]he expansion of formerly entrepreneurial firms undermines the personal sway that an individual capitalist could hold over his workers. The method of control came into conflict with the requirements of production. Pressure built up for more regularized and structured management practices, for methods that did not depend on the extensive personal intervention of the capitalist" (p. 30). In the analogous development of the Institute, this means pressure, targets and budgets, and decreased reliance on the recruitment of sentiment.

Helen connects the dis-integration of the Institute to the increasingly acute division of labour, as teams become more specialised and contained. But she identifies this division as having effects beyond any increased efficiency in the performance of immediate work tasks.

> So when I started they were just breaking up the team [...] And so they would socialise with them all, even though they were splitting them up when I started. So we knew all the people that were providing us with the work, and so if we had a problem then we just like... you could mention it at tea, you know, went for a break or something.
> [Helen]

That idea that the division of the work of sequencing genomes into increasingly discrete jobs has had an effect on the integration of those working at the Institute is a common theme in the accounts of the research participants. Many of these locate this dis-integration around changes in the everyday, mundane social activities that bind the workplace together, such as those that take place over a cup of coffee or tea, rather than around the exceptional socialising of celebrations.

The collectivity of the Institute has also been disrupted by the developments after the end of the Human Genome Project. The Institute has ceased to be a dedicated sequencing centre and has diversified into 'post-genomic' smaller science research built on the resources and infrastructure that the Human Genome Project put in place. This diversification has transformed those involved in sequencing from being those whose work was the essence of the Institute, to being those whose work is a sideline, a service even, to the main work of the Institute. The transformation of genomic resources, such as the sequence of the human genome, into scientific knowledge, is the new purpose of the Institute. Joe describes the change.

> [T]he major projects aren't there. So it's more research. Which, to bring people in to the research I think they had to make it into an [research] institute to get more of the actual research fellows to come in here.
> [Joe]

The Institute acquiring a new reason for existence has shifted the focus away from genome sequencing. This shifting focus has left those working on the sequencing of genomes feeling side-lined. Helen describes the way the feeling of integration into the Institute has changed.

> So, whereas we were the main thing to begin with, what we were famous for, now it's getting less and less. It's kind of more pushed to the side I think. [...] We don't feel as important to the whole thing any more.
> [Helen]

Neneh articulates similar feelings of marginalisation.

> I mean, we were the core at that point, that was basically what we were here for, to start for, was to sequence the human genome. And then when that was completed and then out of that obviously came the biology, and the focus did change, then we didn't- I was going to say we didn't feel quite as valued, but it was just the way it was. But it was difficult to take. Because you did feel that kind of like you were something special and then you were kind of, sort of, side-lined.
> [Neneh]

This 'side-lining' of the sequencing work of the Institute has, necessarily, gone hand-in-hand with the shifting focus to smaller science research. This has required the recruitment of a 'university'-style faculty. Or, as Julia puts it, an increasing number of 'bigwigs'. Julia and Maggie suggest that there was a much more 'flat' hierarchy of esteem when the Institute was concentrating on sequencing.

> But I mean when we first started it was a lot smaller and it was more- there weren't so many really bigwigs and all that. You know, it was more your basic researchers, a lot of, you know, not scientists and whatever. Or the ratio was a lot lower. Yes.
> [Julia]
>
> A lot of lab assistants, weren't there?
> [Maggie]

With the diversification of the Institute into 'post-genomic' research, those who worked in sequencing feel that they have been side-lined, that they no longer have personal, social connections with many people beyond their own teams, and that the hierarchy of esteem has been made much more steep with the recruitment of a post-genomic research faculty. With these changes, considered in light of the conclusions of Chapter G, *The Recruitment of Sentiment*, it should be no surprise that the experience of work at the Institute is increasingly disengaging and dis-integrating.

I4 Living to Work, or Working to Live?

Does dis-engagement and dis-integration matter? Chapter H, *The High Road to the Human Genome*, suggests that it matters more than merely colouring the subjective experience of work. Work into which sentiments are wholeheartedly given is experienced as engaging rather than alienating. Engagement reaches its apotheosis in the description that Professor Ingham gave of his connection with his work as an academic scientist.

> I seem to have fallen into a pattern of living to work, rather than working to live.
> [Professor Ingham]

Steeped in the culture of his hinterland of work, dominated by the image of the university, Professor Ingham speaks positively about being engaged in work. But the world of small science work is highly peculiar, which necessitates a highly personal connection with the products of work.

As we see in Chapter H, *The High Road to the Human Genome*, the idea of alienation finds its way into contemporary management debates, even if the language of alienation does not. Commitment produces functional effects. In Chapter G, *The Recruitment of Sentiment*, Helen suggests that when those sequencing genomes are alienated from the products of their work they would have less interest in producing quality work. This is supported by the arguments in Chapter C, *Pathologies of Work in Big Science.* If this is the case, then the disengagement and the dis-integration experienced by those working at the Institute might act as accelerants, as reinforcing feedback, to these processes. If workers are not, through the recruitment of sentiment, supervised from the inside out, then the organisational response will be the increasing visibility of targets and other means of overt control.

It is in the extraction of discretionary effort that disengagement and dis-integration find their most important objective effects. As Jill describes, once people would work long hours because they wanted to, because they identified their interests and values with those of the Institute. Work at the Institute is now a 'nine-to-five' job. Brian describes the same change.

> No, there was no expectation of anyone doing long hours or anything. It was just the ethos was to stay and get the job done. That's what everyone was there for. Whereas now it's like, do your hours and go. A lot of the staff seem to be like that anyway. And again now some of the managers are as well.
> [Brian]
>
> Even people who have been here since that sort of time?
> [Researcher]
>
> Yes
> [Brian]
>
> So it's not just the community that's changing because the people have changed?
> [Researcher]
>
> No. It's not, unfortunately.
> [Brian]

As we read, Brian's account of the change in the discretionary efforts of those working at the Institute contains an additional lament. That lament is that changes cannot be located in a changing makeup of the workforce but must be seen as changes in the Institute itself; what it does and how it organises this[36]. It is not just that there are new workers who arrive disengaged and dis-integrated, but that the Institute is now, relatively to the experience of work over the past decade, a disengaging and dis-integrated workplace.

We must bear in mind the warning sounded at the start of this chapter. We should avoid falling into 'Golden Ageism'. Nevertheless, even if we were cynical readers of interview transcripts, and concluded that the engaging, integrated workplace that the participants describe when referring to the past is a romantic

[36] A counter argument is put by Jill; "But it was a different era for all of those of us who had just started. We were all single, we were all just out of, more or less, straight out of university, we socialised a lot together, we went to the pub every weekend, and as lives change and we settle down and we have children, priorities change in terms of what we want to do with our spare time and our work time." Jill suggests that as the Institute has developed, so its workforce has matured and it is this maturation that, relatively speaking, has produced a dis-engaging workplace. From a workplace that itself feels like a family, to a workplace dis-engaged as a result of family?

fiction, this does not change our reading of disengagement and dis-integration in the accounts of the subjective experience of work in the present. Regardless of whether there was a contrasting past, the accounts of the present describe a workplace in which research participants do not feel part of the larger collectivity, in which their sentiments are not recruited.

If we do take the accounts of the past as, more or less, accounts of the subjective experience of work as it was, then we can begin to make claims about the process of change as well as the nature of the present. These accounts describe a change in the experience of work at the Institute, the overall trend of which can be summarised as being from one in which there was a sense of collectivity and into one characterised by a sense of comparative fragmentation. As we see in Chapter G, *The Recruitment of Sentiment*, this collectivity was supported by the personality of the founding director, the special nature of the Human Genome Project, integration through celebrations and the deliberate attempt to maintain a 'flat' organisational structure. The sense that there were few social barriers between the most senior figures in the Institute and the most junior was a sign of this collectivity in action. This aspect of working at the Institute is diminished by challenges to all these supporting factors; there is a new director, the Human Genome Project is complete, the celebrations have run dry and the organisational structure as experienced by the research participants is increasingly hierarchialised and structured.

I5 POSTSCRIPT: RESISTING RECRUITMENT

Not all the research participants found their sentiments recruited in the first place. For Abi, a member of the Production team, the triumph of accomplishment did not produce the nostalgic responses that characterised the majority of the accounts of the research participants. Abi presents a particularly unengaged account of the 'team building' approaches to developing a campus-like community ethos at the Institute.

She begins by describing how the lightly structured working environment was not felt as an equally positive arrangement by all the people working at the Institute.

> You know, you're under no pressure. There's nobody on your back going, 'But where were you? I was looking for you at eleven o'clock, where were you?' But it works to a disadvantage to us, the people that do the normal thing, because it was like they get paid shed loads more money because they've been at university. [...] they sit in font of a computer all day and then they'll have like six or seven coffees. They'll go off, do this and then – ooh! I must do this football for two hours. Which you don't have to payback because that's 'team building'. [...] Or badminton or Frisbee or cricket or you know, things like that.
> [Abi]

As Abi illustrates, and as Nichols and Beynon (1977) pointed out to us in their critique of Blauner's (1964) argument that increasing technology would liberate workers, the laudatory accounts of new working practices often write large numbers of workers out of the picture. In this case, the freedom and autonomy of the more senior members of the Institute is a freedom that must be accommodated by 'the people that do the normal thing'. Abi was asked if she got opportunities to take part in these team building exercises.

> Oh, you get – yeah. Oh god, yeah. You can – you can do what – you know, I mean we've got football teams and everything else like that. But that was always looked on – like our boss, because she was – our boss, she was the head of the sports and social club. So she was either reffing or she was doing this, that and the other. And you'd say, 'Well, hang on'. You know,

> they'd moan about the fag breaks and stuff but they'd go, 'Well, [X has] been on for two hours doing thingy and then he's come back and got his lunch and gone.'
> [Abi]

Abi argues that it appears that some leisure and social activities are placed in the privileged position of 'team building'. Some non-productive activities are virtuous, others are wasteful. And virtue appears, in this account, to correspond with the interests of those who occupy positions of power within the Institute. Abi's dissatisfaction with attempts to create a collectivity with a university-like feel go further. While for some, these team building activities might be perks of working in such a campus-like environment as the Institute, for Abi, these feel like undignified impositions.

> But that's team building. You know, that's for the good of the [Institute]. But because you don't play football or you don't do badminton or you don't do this, that and the other, I mean like the stupid parties they have for – you know, three million mega bases or whatever it is. [...] A huge cake, bouncy castles, free booze. Put a tenner in our pockets[37], you know, don't bother about that because I'm not – I ain't got the time to do that. And it is, but that's the bit that's like a campus. [...] You know, oh – we'll have a party. You know, we can have free booze, we can have a barbecue, we can do all that. And the bosses look down on you more for not going to these things and not being involved in these things than anything else really. And it's like well, I come in to work [early] in the morning. I want to go home [early in the afternoon]. I'm not bothered about going to a party and bouncing the castle; I'm [40] years old, for crying out loud, ha!
> [Abi]

For Abi, these team building activities become duties rather than rewards. Abi imagines her work as an exchange of time for money rather than a participation in a community of scientific work. A 'tenner in her pocket' is a more suitable reward than a free party for the achievement of a project goal.

[37] To illustrate the difference between the engaged and the unengaged, we can refer to the interview with Claudia. Claudia says almost the exact opposite to Abi: 'It's like, I don't want extra wages. Come and pat me on the back and make me feel good about what I do, because I do work hard, and sometimes you just feel, 'Well, why should I if it goes unnoticed?' Why don't I just sit back and think, 'Ah, sod it'.

From Abi's account, it seems that the path to success at the Institute is to play football and to attend parties. Since the completion of the Human Genome Project this culture of reward through social gathering has diminished.

> Yeah. I mean it's not so much now; I mean that was when we were – that was also on human genome and that lot. But yeah. But that was it, you know, oh – having a party. All stop work. But this time – which is time if you come in at eleven o'clock in the morning. It's like hang on a bit, I've got to go. Another two hours, got to go to a party? And – but yeah. That was what I could never – that was more of – I could never get my head round that bit. Yeah. [...] As long as – you know, you have to get involved in the football, you have this, that and the other. And nine times out of ten the – you know, the team leaders are, I mean you know, and they can off to the gym for a couple of hours and come back but you disappear and some – oh, where were you then? Not now it isn't but [...] there you are.
> [Abi]

We find that Abi uses her background, her hinterland of work, as a way of providing some mitigation for what she feels was a too negative tenor to her interview. Her hinterland of work shaped her expectations of the experience of work at the Institute. Work is an exchange of time and effort for money; it is not meant to be a place of parties, sports and autonomy. Abi's account of work does not consider the possibility of intrinsic satisfaction through work. Considered in bare economic terms, from the perspective of the material interests of the worker, the most efficient worker is the one who receives the greatest monetary reward for the least input of time and labour. And at the Institute, for Abi, this is a rewarding calculation.

> Well, it's still the best job I've ever had and I wouldn't want to leave. [...] I want to be here until I retire! But, yes. Definitely. I mean like I said I've done hairdressing, I've worked in a factory, I've done this, that and the other and worked with kids. You know, which you do – I mean you do everything when you've got kids of your own. You've got to do it. But this is definitely, you know, the most money for the least work. [...] And I still find things to moan about!
> [Abi]

Though Abi resists the recruitment of her sentiment, she presents an image of extrinsic satisfaction. She is committed to the Institute; she wants to be there until she retires. But her commitment is not 'real' commitment, of the affective or normative sort, but, comparatively, rational and calculative. Working at the Institute is the best job that she has ever had. It is 'the most money for the least work'. Work at the Institute is a job, and a job is an exchange of time and labour for money. For Abi, the recruitment of sentiment does not hide this relationship. More, efforts to recruit sentiment from the staff, to instil commitment, are regarded with suspicion, particularly as it appears to Abi that those who benefit most from these activities are those already well placed in the hierarchy of the Institute.

Part Four: Context and Conclusion

Through this thesis we are engaged in the pulling apart of threads. In Chapter A, *Primers*, we tease from the literature the important strands of history, pulling away the mythic language and an economic vision that goes no further than the stock market. In Chapter B, *Anatomies of Big Science*, the various aspects in which science could be big are pulled apart. The meanings of alienation are differentiated in Chapter C, *Pathologies of Big Science*. Through Part Two: *Strategy and Method*, in Chapter D, *Case*, and Chapter E, *Conduct*, the mess of real qualitative social research is combed into something neat enough to be described. Part Three: *Illustrating and Accounting*, contains the most complex task, the analysis and re-presentation of the accounts of research participants into forms that provide us with insight into the sociology of work in big science.

Part Four: *Context and Conclusion*, pulls the threads of the thesis together. Through Chapter J, *The Institute and the Knowledge Economy*, the experiences of those working at the Institute are connected to debates regarding the future of work and economies. In drawing together these treads and addressing the future, questions of optimism and pessimism are asked as the Institute is offered as a demonstration.

[J] THE INSTITUTE AND THE KNOWLEDGE ECONOMY

"The knowledge economy conjures a world of smart people, in smart jobs, doing smart things, in smart ways, for smart money, increasingly open to all rather than a few" (Brown and Hesketh, 2004, p.1). The phrase 'knowledge economy' suggests the end of proletariat, those who contribute to the wealth of the economy through the application of their bodies. The economy will be transformed from one that necessitates the manipulation of the material world into one that is dependent of the manipulation of the symbolic. Optimistic visions of the future such as this might command space in political discourse, but views of a more pessimist nature also find expression. This chapter places the study of the Institute in the context of such visions of the future of work.

J1 INTRODUCTION

Biotechnology is seen as one of the key sectors in knowledge economies, particularly that of the UK. The Human Genome Project is understood as producing a key infrastructural resources on which the future successes of biotechnology, indeed, biocapitalism, will be built. The knowledge economy, with its stress on knowledge and ideas over the material, is seen as a manifestation of post-industrialism. This chapter examines the position of the Institute within the knowledge economy, drawing from this the ways in which the Institute can serve as a 'de-monstration'.

Daniel Bell (1974) was one of the first writers, outside of science fiction, to suggest the coming of a post-industrial society. Bell suggested that we would see the rise of new technical elites; professional, scientific and technical labour becomes increasingly important. Watson (2003) argues that the "central weakness in Bell's thesis, however, lies in his assumption that there is anything novel about the centrality of knowledge to economic and working life" (p. 65). Watson points out the very existence of industrial society itself was the result of the application of scientific, technical and rational-calculative modes of thought to economic life. Bell's ideas, argues Watson, can be seen as a late twentieth century continuation of Weber's rationalisation thesis; decisions are increasingly taken by credentialed experts on the basis of technical rationalities.

This sense of continuity and connection, as compared with revolution and discontinuity, is one that runs through this chapter. Despite the role of the Institute in laying the foundations for a successful knowledge economy, in key respects the way in which the work was conducted was resolutely Modern. As a result, this chapter questions the assumption that a knowledge economy will be dominated by new forms of post-industrial knowledge work.

Knowledge economies are often understood by their move from a reliance on the material and the physical, to the creation of wealth by use of the mental and the symbolic. According to Watson (2003), Castells (1996) makes a distinction between an *information society*, which is not new, and an *informational society*, in which the engines of productivity and power are not plant, labour and capital, but "the generating, processing and transmitting of knowledge-based information" (p. 65). Societies of this kind, it is suggested, would involve a change in the nature of class society. The diminishing working class would no longer pose a threat to capitalism, rather, the challenge to capitalism would come from new social movements built around identity, the symbolic, rather than class-based material interests.

If, as Castells (1996) and others argue, there has been a change in the way that society generates wealth, we can also expect changes in the organisation of work. Brown and Hesketh (2004) describe the standard vision of the knowledge economy. "The competitive advantage of leading-edge companies in the knowledge-based economy (KBE) no longer depends on mass production of standardized goods and services that are made, monitored, distributed, and sold by vast armies of blue-collar and white-collar employees, but on technological innovation, applied knowledge and the intellectual capital of a highly skilled workforce" (p. 16). These changes in the organisation of work open up the space in which workers can use initiative, display creativity, and apply contextual knowledge. Hence, commitment is important, as we see in Chapter H, *The High Road to the Human Genome*, and the adoption of high commitment, high performance management strategies can be understood as adaptations to the demands of the knowledge economy.

In section J2, *From Lisbon to Erewhon*, we explore the more radical claims for the knowledge economy; that the advent of knowledge economies is a change of the scale of the Industrial Revolution. This rhetoric does political work. The suggestion that there has been a move from the material to a world of ideas is one that prescribes certain policy decisions. Section J3, *Knowledge Economies and Knowledge Workers*, examines some of the literature that has explored the meaning of the knowledge economy and the term 'knowledge worker'. The question of who is a knowledge worker, and who is not, appears to depend on the degree of optimism in the potential of knowledge economies. This section argues that we see a persistence of old forms of work, and non-knowledge work; that the contemporary economy is marked by continuity with the past, not discontinuity.

Section J4, *The De-Monstration of the Institute*, concludes this thesis. This section examines the position of this study of the Institute in debates around the future of work. The question of optimism and pessimism is raised. Are we to be optimistic with regard to the potential of the knowledge economy, of big, industrialised science, of new forms of work? Or do we see these things as the carriers of problems and pathologies?

J2 From Lisbon to Erewhon

The pursuit of the Knowledge Economy became the policy of the EU at the Lisbon meeting of the European Council in March 2000. The aim was to provide a new impetus to economic growth in the EU, and in the process "making the European Union (EU) the most competitive economy in the world and achieving full employment by 2010" (Europa, 2008). The EU expert group, which included several sociologists of science, wrote that since 2000, the pursuit of the knowledge economy 'has been a continual preoccupation of EC and member-state policy actors" (Felt, *et al.* 2007, p.14).

The EU expert group summarise the political agenda as demanding an increased use of science to drive economic growth.

> The term, "knowledge-based economy" prioritises the instrumental use of scientific knowledge for competitive economic advantage. Science is seen as both the key factor of new production and as traded commodity-product in itself. UK Prime Minister Tony Blair explained in November 2006 that a knowledge economy is "an economy where we do not compete on wages – how can we when China's wage costs are 5 per cent of ours? – but on intelligence, on innovation, on creativity".
> (Felt, *et al.* 2007, p. 14)

The EU expert group suggest that these priorities demand a change in society.

> The shaping of society is further visible in the idea of the knowledge economy as articulated in the Lisbon Agenda, and underpinned by the linear model. This then leads to implicit or explicit assertions that "Science is the solution, society the problem". Society has to become more entrepreneurial, become more accepting of, or even keen on, new technology. The 21st century version of the Chicago World Exhibition: "society has to conform".
> (Felt, *et al.* 2007, p. 22)

The strategy for achieving a knowledge economy, for making society conform, rests on three 'pillars'.

> An economic pillar preparing the ground for the transition to a competitive, dynamic, knowledge-based economy. Emphasis is placed on the need to adapt constantly to changes in the information society and to boost research and development.
>
> A social pillar designed to modernise the European social model by investing in human resources and combating social exclusion. The Member States are expected to invest in education and training, and to conduct an active policy for employment, making it easier to move to a knowledge economy.
>
> An environmental pillar, which was added at the Göteborg European Council meeting in June 2001, draws attention to the fact that economic growth must be decoupled from the use of natural resources.
> (Europa, 2008)

The political pursuit of the knowledge economy does not just demand that governments act through altering the budgets they set for funding councils and higher education. Rather, the transition to the knowledge economy is seen as all encompassing, as revolutionising. For some, the knowledge economy is being heralded as a change as profound as that which accompanied the Industrial Revolution. Consider the speech given by David Potter, the founder of the computing company Psion. He talks of the way in which 'the chip' facilitates software, information, and knowledge, likening it to the invention of the 'Gothenburg' (sic) printing press.

> And software isn't just software - software is the virtual machine. It is weightless, infinitely variable; it can drive automation and robots; it can facilitate remote communications at the other end of the world; and replace mechanical systems; it can investigate and query and deal with the abstract.
> (Potter, 1999 [2003])

This speech is not simply a speculative after dinner speech to a business round table. It is hosted on the website of the Prime Minister. The most important phrases in this extract are 'weightless', 'infinitely variable', and 'replace mechanical systems'. As with the extract from the summary of the Lisbon strategy above, with its talk of decoupling economic growth from the use of natural resources, there appears to be a belief that economies can exist apart from the material. The notion that wealth can be created without the material transformation of the world is, literally, a form of magical thinking. The transformation of, for example, Nike, into a branding corporation rather than a manufacturing corporation does not mean that their products are no longer manufactured. The outsourcing of manufacture does not signal a move away from manufacturing except for the particular companies that do the outsourcing. But in the less restrained imaginations of the knowledge economy, 'wealth creation' can be shorn of its dirt, its labour, its muck and brass.

Potter continues in this theme when setting out his four propositions[38]:

> First, Value and Wealth derive from The Process of Creating, not Making things.
> Second, Information is replacing Capital. Third, the Process of Making Things is increasingly automated.
> Fourth, Digital Communications are transcending distance. Fifth, Today, Bits of information are replacing material Atoms. (Potter, 1999 [2003])

The message is that the material is irrelevant. All we need in order to generate wealth is our imagination and the manipulation of symbols. In a more restrained continuation of this logic Powell and Snellman (2004) use the car, the model object of Fordist production, to illustrate the changes involved in the development of the knowledge economy. "A new car today is less and less the product of metal fabrication and more a smart machine that uses computer technology to integrate safety, emissions, entertainment, and performance" (p. 201). But for all this, it is still a manufactured physical object. People do not drive ideas.

Burton-Jones (1999) describes the development of a knowledge economy as being a "transformation of a world largely dominated by physical resources, to a world dominated by knowledge" and suggests that this "implies a shift in the locus of economic power as profound as that which occurred at the time of the Industrial Revolution" (p. 3). If Burton-Jones is right, then sociologists of the early twenty-first century should find themselves in a situation analogous to that occupied by the founders of sociology. Perhaps we should hope so. We might, then, either be due a new age of sociology, with the transformation of the social

[38] The transcript of the speech at Number10.gov.uk contains the curious capitalisation seen here.

arrangements and institutions of this new knowledge economy the sites of prime sociological interest, or even the birth of a new discipline to cope with the changed world. Indeed, Savage and Burrows (2007) argue that in order to cope with a world of 'knowing capitalism' the empirical techniques of sociology will need to be radically innovated.

Rather than the Industrial Revolution, arriving amid the trappings of Modernity, such as mass employment, urbanisation, a democratisation of politics, it is claimed that "[w]e are in the early stages of a 'Knowledge Revolution'" (Burton-Jones, p. 3). This 'revolution', which reshapes our economic lives, takes place amid what is imagined as the reshaping the world. We have problems combing apart the threads of contemporary social description and explanation, among which we find the labels; globalisation, post-industrialism, post-Modernity, post-Fordism, the network society, and, indeed, the knowledge economy. To understand these threads it helps to arrive at some sense of priority. It does not require one to be a Marxist for one to accept the Marxian axiom that the arrangement of our economic affairs is the pre-requisite for all other forms of organisation. Understanding the most basic form of our economic affairs, the way that we work, is therefore a task suited to a sociology that meets the challenges posed by the much trumpeted, apparently dramatic changes in the world[39].

In a 'weightless' economy, Schumpterian creative destruction is destruction without real consequences. The failures of a weightless knowledge economy will produce no equivalent to the post-industrial towns of northern England, merely abandoned software platforms and redundant domain names. But economies are not weightless, regardless of the fact that a narrow band of privileged workers deal in the symbolic rather than the material. Further, as described in this thesis, the Institute is a mass organisation that bears the marks that characterise

[39] Narratives of dramatic change can themselves change the world, even if they are inaccurate descriptions. There appears to be a keenness to justify government policies, whether on work and welfare or terrorism and security, by suggesting that the 'rules of the game have changed'. That the vast majority of these policies are regressive, and rely on the dragooning of a rhetoric of drastic, discontinuous change from a past historical period to the present in order to defend these policies, ought to tell us to take narratives of such shifts in the arrangement of the world with two fistfuls of salt.

Modernity. A key workplace of contemporary biological science is therefore an expression of Modernity. Yet, it is also imagined as the seed of the knowledge economy, which, its prophets predict, will sweep these forms of social organisation away.

The propaganda for the knowledge economy as a revolution is curious. As a second extract from the Potter speech shows, despite its presentation as an inexorable, irresistible change in our economies, we must act in order for the revolution to be completed.

> The effect of the virtual machine is so profound that it is changing the entire economic structure. It is therefore critical that we do identify it as the revolution it is so that we can facilitate and accept change in our society.
> (Potter, 1999 [2003])

The knowledge economy is both human-made and beyond the control of human beings. The actions of humans are only accepted into the narrative where they are designed to carry the revolution of the knowledge economy forward. Society must be made to conform.

One way in which society will be made to conform to the demands of the knowledge economy is through education. In these visions of a knowledge economy, education becomes economic and social policy (Brown and Hesketh, 2004). Through education, and as a result, the increase in the skills and talents of the population, the trend towards increasing inequality that is seen in post-Industrial technologically advanced economies will be reversed. As with the immaterial economy, social justice is a good that can be produced without reference to structures of society, only to the ideas within people.

The bolder rhetoric of the knowledge economy suggests that we are entering a new, distinct period of history. This thesis suggests that such periodisation of history is a problematic simplification. The example of the Institute demonstrates that a workplace that is held to be one of the foundation stones of our coming knowledge economy incorporates features that are characteristically Modern.

These features remind those both inside and outside the Institute of the forms of Industrial Age economic life. This, it should be noted, is not a case of the work of the Institute, and as a corollary, work in science, simply lagging behind the trend. The kind of work being done in the Institute was not described as being 'industrial' because it had been left behind. Rather, the industrialisation of science seen at the Institute was a development that takes this particular instance of scientific practice away from the traditional form of work in science. This form of work can be seen as the ideal of 'knowledge work'. In other words, the example of the Institute shows that, however smoothly history appears to run 'forwards' when viewed from a suitable distance, when considered at close quarters the path from the Industrial Age to the Knowledge Age is a path with many forks and turnings. This thesis does not suggest there are not changes in the way in which we organise our economic life, but rather than these changes are marked by continuity as much as by discontinuity, and by the reappearance, and reinvention, of forms of economic life that have gone before. Given this, the task of studying the knowledge economy does not require a whole new sociology to be rebuilt from first foundations; the world has not become something so radically different we need abandon our tools and theories in the face of an intractable post-Modern world. In the face of these problems, the question is not whether we are living in Modernity, late-Modernity, post-Modernity, or whether we have perhaps never been Modern (Latour, 1993), but that the forms of life described by these labels are marked by threads of continuity strong and distinct enough to render all these worlds intelligible to existing forms of sociological study.

J3 Knowledge Work and Knowledge Workers

We have heard rhetoric like that of the knowledge economy before. This thesis argues that the knowledge economy is not a transformation on the scale of the Industrial Revolution. Such a revolution reorders the entire world, geographically, economically, socially, and culturally. It overturns the class relationships of existing society. Instead, the development of the knowledge economy it is a change, profound though it is, of a kind with the automation of manufacturing in the mid-twentieth century. Then, as now, we were capable of looking into the near future and seeing a reordered world on the basis of these changes in our connection to the economy.

This was met not only with pessimism, but with political optimism. The enthusiasm of contemporary governments for the development of knowledge economies mirrors that excitement found attitude of the British government towards the 'white heat' of technology in the 1960s. The embrace of high technology, it was imagined, would achieve both economic growth and social progress (Edgerton, 1996). The rationale behind the increasing focus on high technology in the 1960s was built on a belief in a Bush-like (1945 [2007]) linear model of economic growth resulting from research spending. This is strongly tied to notions of the industrialism, not the dream of unshackling economies from their connection with labour and material resources. Nevertheless, a turn to science and technology, with all its promise of Modernity, was seen as a path to prosperity and a better society. In the 1960s, this took the shape of sweeping away of the anti-science Establishment that was holding back the potential of the masses.

And, in the 1960s, as now, there are suggestions that a knowledge economy, like the hi-tech economy expected during the 1960s, would do away with the drudgery of work. Senker (1992) recounts a strain of research in the mid to late 1960s, such as that by Lilley and by Fyrth and Goldsmith, which suggested that, rather than deskilling workplaces, increasing automation created a greater demand for skilled workers. Rather than producing armies of routine and low skilled workers, as might be feared, automation would eliminate the need for this kind of work. In that, these researchers argued that the education system would have to be strengthened and aligned in the pursuit of producing a nation of workers capable of filling these positions. This kind of argument reminds us very much of the propaganda for the knowledge economy; greater amounts education is required to realise economic and social improvements. In the 1960s, the working classes were to become Blauner's (1964) un-alienated process workers, in the twentieth century, when the working classes are admitted to the narrative at all, they are to become knowledge workers, committed, engaged and autonomous.

While we did not see a comprehensive revolution of this sort, a liberation through technology, neither did we see the emergence of the dystopic automated world as described in *Player Piano* (Vonnegut, 1952 [1980]). Unlike during the Industrial Revolution, there was not a turning over of the relationships of class and the developments of new ones. Some descriptions of the knowledge economy suggest that highly-educated knowledge workers will form a new class in a post-industrial economy. But Brown and Hesketh (2004), examining the question of employability, find little evidence to support the claim that, in the supposed knowledge economies of the US and UK, there is a constantly growing demand for graduates as knowledge workers. Indeed, they suggest that, if anything, there is an over-supply of graduates[40]. This thesis argues that it does us no favours to see in the adjustments of contemporary society the kinds of changes we could call a revolution[41].

Who are knowledge workers and what is knowledge work? Are knowledge workers those who, as suggested by Charles Leadbetter (cited in Brown and Hesketh, 2004), those whose work produces nothing that can be weighed? This would accord with the bolder claims for the knowledge economy. But this definition would include a great deal of routine, uncreative and low-status work, and excludes some whose work in transforming the physical world demands the continuous and highly-skilled application of knowledge and judgement. Others

[40] If universities are considered the sites of the production of workers, rather than citizens.
[41] Unless we have a book or a policy to sell.

seek to limit knowledge workers to those who have a high degree of education, use IT and have some degree of autonomy. In definitions of knowledge work that revolve around the use of IT, working with, or even creation of, knowledge by workers who do not use IT is not classed as knowledge work.

Benson and Brown (2007) provide a more useful definition of knowledge work. "Knowledge work is identified by the emphasis on information processing, problem solving and the production of knowledge" (p. 122) "The term knowledge work is often used to characterize the shift from routine operational tasks to more varied and complex work" (p. 124). Does this include or exclude the work of the people who sequenced the human genome? The 'work of the Institute' is not the work of any particular individual or job at the Institute. Collectively, the Institute is engaged in knowledge work. But this does not mean that all, or even any, of the people working there should be described as knowledge workers. Of course, there is the problem that the notion of 'knowledge work' may itself be unhelpful. If we describe an IT technician as being involved in knowledge work, do we also describe a plumber or electrician as also being so? Consistency would suggest that we do, unless we are to rig the categorisation of knowledge work in such a way that it means little more that contemporarily culturally middle-class employment. Benson and Brown quote an unpublished paper by Sewell when addressing this problem. Such an approach, concentrating on occupations rather that the practices of work "'belies a degradation in the nature of some "knowledge work" while simultaneously ignoring the increasing cognitive demands placed on many employees in traditional employment'" (p. 124). If it is the case that, as Benson and Brown write, quoting Horibe (1999), "[i]n short, knowledge workers add value to the enterprise through 'their ideas, their analyses, their judgement, their syntheses, and their design' (p. 125) then what do we have but a relabelling of what we used to describe as the professions into the language of the late twentieth and early twenty-first century. The division between routine and non-routine work does not capture the sense of the knowledge economy, of the sectoral changes in work, rather it reiterates the divisions of manual and mental labour.

Benson and Brown (2007) expand and dissect their definition of a knowledge worker by suggesting that categorisation involves the consideration of three axes; variety in the work, interdependence of the work, and autonomy in the pursuit of the tasks at hand. Benson and Brown recommend a definition such as this as it avoids categorisation on occupational or sectoral grounds. But, in the context of a discussion of a knowledge economy, sectoral classifications are both important and useful. If we have knowledge work being conducted within the economy, in collectives such as the Institute, it is important that the fact that the individual workers employment in 'routine' work does not exclude them from consideration as a part of the knowledge economy.

In actually existing knowledge economies, the proportion of workers classed as knowledge workers varies from analyst to analyst. Hesketh and Brown (2004) cite Reich to describe one of the more restrictive definitions of the knowledge worker; the 'symbolic analyst'. Reich's definition excludes the majority of the workforce in technologically advanced economies. In a knowledge economy, the majority of workers remain 'routine production workers', which includes even low- and middle-managers and routine IT workers, or work in 'in-person services', in which value is extracted by the exploitation of emotional labour. This reads as a pessimistic vision of the knowledge economy, one that promises little capability to reverse trends towards inequality. Instead, it produces a minority of knowledge workers, perhaps twenty percent, who occupy the role of an aristocracy of labour. The majority will be production workers or service workers in a culture that lauds knowledge work and denigrates other forms of necessary labour. Knowledge work is, as Brown and Hesketh (2004) title a section of their book, 'nice work, if you can get it'.

Other analysts are much more optimistic. Brown and Hesketh (2004) suggest that writers such as Drucker (1992) put the proportion of knowledge workers at seventy per cent. This is optimistic, but problematic. Such a large number of people engaged in knowledge work is not a claim that is in accord with what Brown and Hesketh find in their study of the economies of the US and the UK. Many people might be employed in knowledge intensive industries, in the sectors of economic activity that characterise knowledge work. This does not mean that the work that they do is knowledge work, or that the knowledge economy has transformed their experience of work. The proportion of the workforce that an analysts sees as knowledge workers appears to determine, or be determined by, the degree of optimism, that the analyst has for the knowledge economy to benefit society as a whole.

Thompson and Warhurst (1998) have argued that the labelling of an increasingly wide range of jobs as knowledge work presents a misleading picture of society. Such descriptions make it appear that there has been a growth in knowledgeable workers, that the nature of work has changed. However, a great number of jobs in the knowledge intensive sectors of the economy involve the performance of routine tasks. This theme is echoed by Alvesson (2004) who claimed that much work in many knowledge intensive firms is routine, and Fleming *et al.* (2004) who found that by 'going below the surface' the growth in occupations that were deemed to be knowledge work revealed a growth in many of the more routine aspects of work" (p. 124). Even the claim that post-industrialism has involved a discontinuous shift from manufacturing work to service work has been challenged. Callinicos (1989) argues that the UK labour market at the turn of the twentieth century contained a substantial services 'sector', largely in the form of domestic servants[42]. Brown and Hesketh (2004) update the critique offered by Nichols and Beynon (1977) that it would be automation that would liberate us at work by reminding the reader of the millions of cleaners, production workers, service industry workers, packers and so on, who are required even in the technological advanced economies of the US and the UK. As Brown and Hesketh put it, labour is always required to 'activate' the value of ideas.

Continuities connect the knowledge economy to the manufacturing economies of the twentieth century. With regard to the development of the workplaces of the knowledge economy, this can be seen in Kleinman and Vallas (2001). Kleinman and Vallas describe the way in which the forms of work in knowledge intensive industrial sectors and academia are coming to resemble each other. They describe the relationship between the bureaucracy of industrial corporations and the

[42] The return of which are lauded in the Sunday supplements as a sign of prosperity.

collegiality of academia as being in tension. These developments are a "shift *away from* the hierarchical constraints that capitalism has historically imposed, *toward* a newer, more flexible, and egalitarian organizational pattern" (p. 460). Though the forms of work are new, they are built from the model of already existing forms of work. Grint (1998) advises us to think of "the post-industrial future not simply as an inversion of the industrial present but as embodying shadows of the pre-industrial past" (p. 320).

These 'new' forms of work promise autonomy and egalitarianism. Kleinman and Vallas (2001) describe the process that Barley (1996) has called the 'technization' of work; that is, as work becomes more complex, those in work come to use ever more esoteric bodies of knowledge, and work is resistant to the imposition of bureaucratic controls. A knowledge economy therefore, should be characterised by forms of work that, in their essence, are resistant to existing forms of capitalist control. But, as Hesketh and Brown (2004) argue, this 'nice work' is not available to all. The knowledge economy is still characterised by routine work (Thompson and Warhurst, 1998).

Moreover, a counter-trend should be expected. If ideas are the engines of wealth creation, then we can expect to see the codification, routinisation, and even automation of the work processes of knowledge work. Not only to make the process more efficient, and not only to allow the deskilling of workers. In a knowledge economy, where knowledge itself becomes a commodity, it would be unwise to leave this important commodity in the ownership of employees, of labour, rather than under the control of the organisation, of capital. This would mirror the development of CNC machine tools, (Noble, 1984). Codification, routinisation and automation all transfer the knowledge from the minds and bodies of the workers, who can walk out of the factory or laboratory, and into physical objects. These could be as simple as instruction manuals. Through these, ideas and knowledge can be treated as commodities, as objects.

J4 THE DE-MONSTRATION OF THE INSTITUTE

The de-monstration of the Institute forces us to consider our position between several sets of oppositional relationships. Just as people approach the Institute from hinterlands of work dominated by imagery of the factory or the university, now that we have had a chance to reflect on the Institute as a workplace in the context of a technologically advanced economy with a claim to becoming a knowledge economy, how do we see its de-monstration? Are we optimists or pessimists? Are we a version of what Mirowski and Van Horn (2005) have called the Economic Whigs, seeing in these new forms of work in science the room for an unleashing of economic potential? Or are we Mertonian Tories, who see in the rationalisation and industrialisation of science a threat to knowledge production? When science is routinised and automated, as it must be when it is industrialised, are we 'black box optimists' or 'black box pessimists' (McNally, 2008)? Do we see the black boxes as limiting or enabling? Which Weber do we read? The one who sees rationalisation as 'infinitely superior to all traditional forms of social organization" (Mommsen, 1989, p. 111), or the one who sees bureaucratic rule as producing "conditions which will eventually precipitate its failure, or more often its petrification" (p. 116)? And, more widely, do we look forward to a knowledge economy of rewarding creative work, in which 'high road' practices predominate, or do we see a future in which the workforce has been divided into a small, stable core of knowledge workers, with the rest a disposable periphery, fractured and divided in an age of post-Fordism, post-industrialism, and post-Modernism, vulnerable and unable to resist exploitation? Or even, are we 'high road' optimists or pessimists? Do we see in these new organisational forms a 'hyper-Fordism', in which responsibility is shifted from supervisors to workers, in which the internalisation of the values and goals of the organisation leads to self-rationalisation, co-opting workers into increases in the intensity of work (Powell and Snellman, 2004)?

To what extent is the work of the Institute 'knowledge work'? As we argue, it would be counter intuitive to suggest that the Institute itself was not involved in knowledge work. The work of science is surely the archetype, the ideal even, of knowledge work. Though some have argued that the work of the Institute falls outside the definition of science (Hood and Smith, 1987), based on the argument that genome sequencing asks no questions and tests no hypotheses[43], it is tightly bound within the orbit of science, and, patently, is involved the production of knowledge. The work of the Institute, science or not, is sectorally part of the knowledge economy.

A great amount of the work of sequencing genomes at the Institute did not require the recruitment of graduates or already trained workers. As Brown and Hesketh (2004) put it; "Knowledge *worked*, then, does not necessarily require certificated knowledge *workers*" (p. 55). Training was found at work, on-the-job, and did not require formal further or higher education. By conventional definitions, this would place the work at the Institute as outside the category of highly skilled work (Brown and Hesketh, 2004). This questions the assumption that an effective knowledge economy requires the expanding production of workers with formal further or higher education. In so much as the a key sector of the knowledge economy is biotechnology, and given that the sequence of the human genome is a key part of the infrastructure that allows biotechnology to realise its possibilities, we can say that the knowledge economy is built on a foundation of jobs that do not require higher education. Brown and Hesketh's (2004) study of employability would suggest that, in its existing form, knowledge economies do not require great masses of graduates. Brown and Hesketh describe actually existing knowledge economies as 80/20 economies, in which eighty per cent of the workforce is not engaged in knowledge work.

So are we pessimists or optimists with regard to the promise of the knowledge economy? The fact that something as quintessentially 'knowledge economy' as the Human Genome Project was accomplished without the need for the recruitment of a great mass of scientists, of graduates, instead relying on the

[43] This definition would exclude other scientific work from their definition of science. At least large parts of the work of naturalists, paleontologists, and astromonmers could all be described in this way. Of course, Hood and Smith (1987) were not writing as philosophers or sociologists of science. They were acting as politicians, seeking to secure resources for a project to sequence the human genome in the face of criticisms that such a project would be bad *science*. If it is not science at all, such criticisms are neutralised. And if it is not science but akin to construction, there is no uncertainty over success.

recruitment of untrained people leaves us in a state of ambivalence. This recruitment suggests that Brown and Hesketh's (2004) 80/20 economy is the correct description, not the optimistic vision of Drucker (1992). Even in a knowledge economy, even in knowledge intensive firms, a great deal of work will involve routine work. There will be no new mass class of university educated knowledge workers ready to revolutionise society. However, neither will people necessarily be left behind. Some will be, as the vision of full employment which was set out in the Lisbon meeting appears not to have met capitalist reality. But the Institute demonstrates that tremendous accomplishments in high technology, scientifically advanced work can be achieved by people who do not arrive at work equipped with a degree.

What of new organisational forms, of the 'high road'? There is a tension here, which can be read through Chapter G, *The Recruitment of Sentiment*, and Chapter H, *The High Road to the Human Genome.* In order for a worker to become affectively, attitudinally and normatively committed to their work, and to their workplace, their sentiments need to be recruited. They need to be engaged. If workers are to be engaged, the tasks that they do cannot be meaningless and they cannot be alienated from the values of the organisation in which they work. This engagement is expressed as intrinsic satisfaction; the new forms of organising work that seem to accompany the emergence of a knowledge economy produce work that is satisfying and engaging even as it is routine.

As we see in Chapter G, *The Recruitment of Sentiment*, when the Institute was in the process of sequencing the human genome it was led by the contingent charisma of the founding director. Brown and Hesketh (2004) describe the move from bureaucratic management to charismatic leadership as part of the changes that are taken to characterise the emergence of knowledge economies. "The charismatic personality represents changing forms of symbolic control in the knowledge intensive organizations. It celebrates those managers who seek to undermine the structures of routine action and rule-following behaviour" (p. 33). But the charismatic leadership of the Institute was not a product of post-industrialism, but a product of the emergence of the Institute from a form of work that had much in common with pre-Fordist entrepreneurial firms. As much as the knowledge economy changes work, it reincarnates past forms.

Yet, as discussed in Chapter H, *The High Road to the Human Genome*, the kind of loyalty and engagement engendered by new forms of work and charismatic leadership hides the nature of the relationship between employee and employer, demands that the worker becomes their own supervisor, and leaves them without the resources by which to resist the intensification of their labour. Indeed, as engaged workers, this intensification is gladly accepted. The Institute, though, is exceptional. The recruitment of sentiment involved factors that cannot be replicated in 'ordinary' workplaces of a knowledge economy. Without such exceptional factors as the narrative of science as a benefit to all humankind, the special nature of the human genome, and the moral dimension of the race, a more tempered degree of engagement is the product.

What of the perils and pathologies of big, industrialised biological science? Watson (2003) identifies writers such as Blackburn *et al.* (1985) as arguing that ICT would allow the severing of "'the Fordist link between mass-production and economies of scale' by allowing 'the production at lower costs of smaller but still economic batches'" (p. 67). Thus, workers would no longer need to be physically concentrated in large collectives. Yet the Institute was just this kind of concentration.

Nevertheless, this is another way in which the Institute can appear as a decade and a half cartoon recapitulation of the history of industrialisation. We start with the craft workshop, the highly skilled, highly green-fingered work of molecular biologists, at a time when sequencing a gene was worthy of acclaim. This work was made more routine with the birth of the Institute, in which a number of craftspeople were brought together, the work was divided into rough chunks, but the atmosphere was still one of a family firm. This process was then industrialised, a development that was seen as one of the successes of the sequencing centres involved in sequencing the human genome (Hodgson, 2000). Post-Fordist diversification comes in two forms. First, the development of a number of innovative small teams; the post-genomic smaller science faculty.

Second, the development of IT and the process of automation leads to a history of big science that might look something like the vision described by Peter.

> So they're going through that kind of parabolic curve, so at the moment the processes are more manual, so it's upping physical bodies to do the task. But at the same time they're looking at, you know, there must be a way to do this process better than just throwing people at it. So, again, it's driving technologies to be developed saying, we don't want to do that step manually, we've reached the level where it's robust enough that it could be potentially automated, and therefore we need to look at how you're going to automate it.
> [Peter]

So, in the case of big biology, where the scaling up of science does not involve the construction of massive pieces of apparatus, as is the case in big physics, but rather does involve the multiplication of labour-intensive bench-top processes, big science contains the seeds of the logic that lead to its own withering away. At least, in so much as the science is big organisationally. The development of IT and automation allows biology to be a big *informational* science while being conducted in the labs of small(er) teams. Biology, in the case of genomics, has moved from craft work, through Fordist industrialisation, to post-Fordism, complete with, as we see in Chapter I, *Interlude: Dis-Engagement and Dis-Integration*, its own small-scale, localised version of the demoralisation of the working class, in the space of a decade and a half. The perils and pathologies of big biological science are increasingly irrelevant as the industrialised form of biology has produced its own redundancy.

What of the cognitive community of science, of the generation of black boxes? Are we black box pessimists or black box optimists? McNally (2008), in her report from a multidisciplinary workshop on 'the transformation of knowledge production in the biosciences', describes the two positions.

> On the black box 'pessimist' side was concern that black boxes are having a deskilling effect on the threshold for entry to the field, with adverse consequences on the quality of proteomics research. [...] the way forward is for researchers to be equipped with the skills to understand the workings of the various black boxes they deploy; and for black boxes themselves to be less

> black [...] black box 'optimists' argued that instances of bad practice are not an argument against black boxes *per se*; that over time black boxes improve, becoming not only faster, more accurate and more sensitive, but also blacker and less amenable to tinkering, and that this is a sign of progress; and that black boxes are a good thing precisely because they empower individuals to perform tasks which would otherwise be beyond their ken. (McNally, 2008, p. 223-224)

It was the black boxing of knowledge that allowed the human genome to be sequenced. It was black boxing that enabled hundred upon hundreds of uncredentialed workers to take part in the accomplishment of sequencing the human genome. In this, it deskilled the job of genome sequencing, but 'upskilled' those recruited to a knowledge intensive workplace that would otherwise not exist. Indeed, it is black boxes, not a creation of a new mass class of knowledge workers equipped with the higher degrees necessary to understand the work task from first principles, which makes the knowledge economy possible.

This study does not exhaust the kinds of labour essential to the sequencing of the human genome, where are, for example, the cleaners, the caterers, the security guards? Nevertheless, in this study we enter the 'hidden abode' of production, get behind the mythic presentation of the heroic men of science, and explore the experiences of the day-to-day workers of large-scale genome sequencing which are most often absent from view in the ways that we 'consume' the genome. As we are reminded by Brown and Hesketh (2004), just as we were reminded by Nichols and Beynon (1977), the lionisation of the transformation of work ignores the continuities that connect these futures of work to the work of the past.

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