COMPUTERISED RESEARCH TECHNOLOGIES IN PRACTICAL RESEARCH SETTINGS

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ABSTRACT

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This thesis is a video-aided ethnomethodological study of computer-aided research in postgraduate-level scientific projects in two disciplines (astrophysics and electrical engineering), drawing on fields including science and technology studies, the sociology of science education and ethnomethodological studies of work. The aim of this study is to explore how computerised research technologies are developed, modified and worked with in scientific disciplines, and the objective has been to investigate some of the ways in which these technologies can be used to address specific research problems, and the work that goes into successfully doing research with them.

A broad overview of the findings of this work is that for sociological accounts of scientific research and education, failing to understand the scientific content of these activities is the same as misunderstanding the activity entirely. What is found through investigating these settings with this idea in mind is that science cannot be understand as *entirely* cultural and conventional as it tends to be portrayed in sociological accounts. Rather, scientists draw on lots of different resources to do with science, programming and the computational tools that allow them to proceed with their work systematically and positively (i.e. in ways that clearly contribute towards the achieving of pre-defined goals). These resources may well include cultures and conventions, but these are better understood as situated *alongside* an array of other features such as conceptual knowledge of science and mathematics, practical understandings of the settings at hand, and so on. Therefore, this thesis aims to present various features of scientific work exemplifying how these resources are used and how their usage fits into wider project and/or scientific goals and objectives.

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DECLARATION

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CHAPTER ONE: Introduction

The aim of this thesis is to explore how computerised research technologies (CRTs) are developed, modified and worked with in two scientific disciplines – astrophysics and electrical engineering – where postgraduate researchers do this as their daily work. The goal throughout is to examine such usage as it happens (i.e. to observe the day-to-day practices of research involving such technologies) and draw upon these observations to understand the features that make this work a feasible endeavour for those undertaking it. More specifically, this thesis aims to investigate some of the ways in which various CRTs – chiefly programming languages, but also various items of physical laboratory equipment that feature software to some degree – can be used to address specific research problems, and the work that goes into successfully doing research with these technologies. Three empirical studies address a range of key concerns pertinent to these aims and objectives, and in doing so, address three major research questions: What resources do users of CRTs have available to draw on in their work (see chapter four)? How does the work of developing, modifying and using CRTs to generate representations contribute to the achieving of wider project goals, and how does this CRT work tie in to other research concerns (i.e. laboratory work, mathematics, scientific phenomena, etc.) (see chapter five)? How do students and researchers using CRTs learn to do so through their practical working with them (see chapter six)?

This thesis – and the research work undertaken in service of it – is grounded in an analytical tradition known as ethnomethodology, which has informed the work from start to finish. It is therefore important to outline some of the ways in which this study bears the stamp of the tradition, by referring to some of ethnomethodology's key principles as outlined by its founder, Harold Garfinkel. The aim of the ethnomethodological enterprise is, as conceived by Garfinkel, to detect "expectancies that lend commonplace scenes their familiar life-as-usual character, and to relate these to the stable social structures of everyday activities." (1964, 227). What ethnomethodological studies are intended to present then are the features of routine activities that *make* them routine to members involved in them. Consequently, a necessary condition of this is that everyday activities are to be understood as "members' methods for making those same activities visibly-rational-and-reportable-for-all-practical-purposes" (Garfinkel, 1967, vii). This is to say that the organisation of members' actions and interactions is to be found in the actions and interactions themselves, in precisely the same way it is displayed for social scientists as well as members.

To further pick apart what these bizarrely-phrased but seemingly innocuous statements might mean to sociology, I co-opt an explanatory tactic often employed in Garfinkel's foundational studies – displaying these terms in practical use, through examples of how they have to come to bear in the activities of members of various settings. One oft-repeated but particularly useful example is Garfinkel's study of the coding practices by which researchers tried to capture the admission criteria used by the University of California, Los Angeles (UCLA) outpatient clinic to select applicants for further treatment, through a form-filling exercise to evaluate their suitability as patients. Garfinkel noted that in order to code the clinic files which recorded the patient's relationship with the clinic, those doing the coding were "assuming knowledge of the very organized ways of the clinic that their coding procedures were intended to produce descriptions of." (1967, 20). As Garfinkel explains of the coders' work:

> No matter how definitely and elaborately instructions had been written, and despite the fact that strict actuarial coding rules *could* be formulated for every item [...] insofar as the claim had to be advanced that Coding Sheet entries reported real events of the clinic's activities, then in every instance, and for every item, "et cetera," "unless," "let it pass" and "factum valet" accompanied the coder's grasp of the coding instructions as ways of analyzing actual folder contents. (1967, 21).

Hence, coders were not filling in their Coding Sheets according to the 'coding rules' that their design suggested, and coders understandings of Coding Sheets relied partially on information not contained within them. However, Garfinkel's approach to coders' activities had no interest in tallying coders' actual entries against the 'rules' outlined by Coding Sheet instructions, and:

> instead of assuming that coders, proceeding in whatever ways they did, might have been in error, in greater or lesser amount, the assumption was made that *whatever* they did could be counted correct procedure in *some* coding "game." The question was, what were these "games"? (1967, 20).

In approaching coders' activities as 'successful' at producing the Coding Sheets they did, Garfinkel's topic of study was not the Coding Sheets themselves and how closely coders' activities mirror the formal written expectations of the instructions for their use. Rather, Garfinkel's ethnomethodology is oriented to understanding the features by which the work is successfully achieved – in this case, through *ad hoc* features not explicitly written into the instructions, but which were nevertheless essential to coders' understandings of coding entries. As such, Garfinkel stated that for a researcher (such as himself) to "grasp the relevance of the instructions to the particular and actual situation they are intended to analyze" (Garfinkel, 1967, 22), i.e. the activities of coders at the UCLA outpatient clinic, it is essential that they understand precisely these *ad hoc*-ing practices and the various features of them. It is ethnomethodology's aim then to take members' criteria of using and understanding features such as *ad hoc*-ing as the topic-at-hand, rather than generating their own criteria based on a sociological interpretation of what coding entries should look like according to the formal instructions accompanying them. Indeed, Garfinkel claimed that "As long as this programmatic question ["What is *their* game?"] is neglected, it is inevitable that person's usages will fall short [as adequate explanations of activities]. The more will this be so the more are subjects' interests in usages dictated by different practical considerations than those of investigators" (1964, 246).

In the same way, this thesis attempts to address some of the features of the activities of using CRTs as part of scientific education and research projects, and is ultimately able to do so by relying similarly on the understandings of those activities that members share and make available through doing and talking about the activities themselves. It is however true that whilst everyday mundane practices such as standing in a queue, or buying a newspaper, or filling in a form are understandable to ethnomethodologists by virtue of their 'lay' nature[1], understanding the seemingly more esoteric field of scientific research from a member's point of view may at first appear to demand more technical knowledge than a researcher trained in the social sciences can be expected to have. However, insofar as foundational ethnomethodology is rooted in Wittgensteinian ordinary language philosophy[2] it is entirely possible to understand the activities and interactions of members of scientific research communities, for a number of reasons.

Caton (1963) highlights a philosophical distinction between the types of language used in everyday life – ordinary language – and the types of language used in more specialist settings such as occupations, hobbies and special interests – technical language. Whereas ordinary language is the kind of language used when buying a newspaper or filling in a form, technical language is not known as universally, and

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[1] This is not to make light of ethnomethodology's achievements in the understandings of routine and everyday actions. Rather, the intention here is to highlight that when attempting to understand members' activities in everyday commonplace settings, no extra technical knowledge is required of the ethnomethodologist, and this serves to open the setting out to a somewhat more universal extent. Put simply, if the setting is so general as to be understandable to any possible members entering into it (in that it can be taken for granted that the majority of people understand what it is to stand in a queue or buy a newspaper or fill in a form), then an ethnomethodologist is likely already a member themselves, and can understand the scene as such.
[2] This affiliation is addressed in more detail in chapter two.

can only be used "easily or naturally" (Caton, 1963, vi) with fellow colleagues or hobbyists who share knowledge of that more specialist terminology. However, Caton notes that "technical language is always an *adjunct* of ordinary language" (1963, viii), in that the technical content being communicated is, unavoidably, carried and rendered sensible to other members through ordinary language usage:

> physicists and mathematicians, for example – people who necessarily employ large amounts of very technical language – do not find it necessary to devise new kinds of questions in order to cause their colleagues to explain what they are saying: new questions to be sure, but not new *kinds* of questions…'Do you mean *rings* or *commutative* rings?' differs from 'Do you mean *rings* or *engagement* rings?' only in that the things the person may have meant are different. The 'Do you mean…or…?' is not different. (Caton, 1963, ix).

This distinction gives ethnomethodology an in-road to the investigation of settings populated by members sharing a technical language largely unknown to the investigator. It is at least possible to understand the ordinary aspects of the scene if not the technical ones – to understand how people like postgraduate science students talk to each other, if not the finer detail of what they are talking *about*. Perhaps most crucially, it is also possible from this distinction to at least understand which aspects of language and action are ordinary, and which lie in the more unfathomable realm of the technical.

However, it is also possible to go further, and begin to understand more about the technical content of scientific education and research itself, as accountably expressed through the activities and interactions of its members. As Elliot notes, "Science inevitably starts from the experiences of everyday life as the phenomena to be investigated. Where else could it start?" (1974, 23). For the student of science *as well as* any ethnomethodologists lurking in the shadows, the same setting is available for the seeing, albeit from different levels of understanding. Elliot goes on to state that a working scientist:

> emphatically does not see only, e.g., protons and electrons. How could he? And not only does it *not matter* that he does not, he *cannot do so*. And his not doing so is part of his getting on with his work properly. (1974, 24).

As such, not only do students of science see, literally, the same things as anyone who cares to observe them, this orientation to the technical tools of science – the meters that provide readings, the dials that must be set to specific values, the computer programs that process data, the graphs that display results, and so on – is totally essential in getting the scientific work done. Commonsense understanding is, then, an integral feature of scientific work (although it would be short-sighted to suggest that it accounted for the sum total of it), making much of the activities of scientific education and research understandable through reliance on the ordinary features of the work alone. In practical action as well as language, "the 'technical' cannot be divorced from the 'ordinary', and…their relationship invites not criticism, from either side, but empirical investigation." (Turner, 1974, 9).

However, ethnomethodology can go further still and begin to broach even these remaining technical boundaries, through a fundamental ethnomethodological policy established by Garfinkel, named the "unique adequacy requirement of methods" (Garfinkel and Wieder, 1992, 182)[3]. Garfinkel further distinguishes between a weak and strong use of the requirement. As Garfinkel and Wieder note:

> In its weak use the unique adequacy requirement of methods is identical with the requirement that for the analyst to recognize, or identify, or follow the development of, or describe phenomena of order*[4] in local production of coherent detail the analyst must be vulgarly competent in the local phenomenon of order* he is "studying." (Garfinkel and Wieder, 1992, 182).

Put simply, the weak use "requires that analysts be, or become, competent at performing the practices they set out to study." (Lynch, 2006, 510) to the extent that they can begin to recognise some features of the order and organisation of settings in the same way as members themselves do. This requirement, without which ethnomethodological research falls apart, is satisfied almost by default in non-technical settings, where research concerns "manifestly ordinary practices that the researcher can do as a matter of course" (Lynch, 2006, 510), such as performing and recognizing commonplace features of interaction (greetings, requests, membership, and so on). The strong use however requires that the ethnomethodologist provide accounts of settings using only concepts found in the settings themselves, and Lynch highlights some of the difficulties of this practically:

> "observing" computer programmers designing software, or "recognizing" that a question delivers an insult to its interlocutor requires the investigator to be privy to the competent performances being "observed." Practices of "observation" (seeing, recognizing, making intelligible, reacting appropriately) already are on the scene.

[3] Indeed, Lynch dubs the unique adequacy requirement the closest thing to "a single, overarching principle of method" (2006, 510) that ethnomethodological research has, in that any ethnomethodological insights must necessarily rely on accounting for (and thereby being aware of and having understood) various features of the order and organisation of members' activities in a given setting. A version of ethnomethodology that did not rest upon a unique adequacy requirement would have nothing to say.

[4] 'Phenomena of order*' is a term taken to refer to a yet-to-be-fully-specified group of working topics of investigation for ethnomethodology – "order, logic, meaning, reason, and method" (Garfinkel and Wieder, 1992, 180) and so on.

> There is no avoiding them if one aims to render an account of the actions "observed". (Lynch, 2006, 511).

This thesis has aimed to adhere as closely to the strong use of the unique adequacy requirement as possible, through a concerted effort at ethnographic preparation prior to any fieldwork, and through use of video ethnographic methods of data capture[5]. The aim has been to render accounts of the settings of astrophysics and electrical engineering education and research with which members themselves would agree. This has been possible utilising the policies of ethnomethodology outlined above, which arm the ethnomethodologist with a standpoint from which to tackle the highly technical world of postgraduate learning in astrophysics and electrical engineering.

Such a standpoint sees the ethnomethodologist armed with two things. Firstly, the knowledge that scientists do not *literally* talk another language – they simply use a different set of words sometimes – and don't act in ways completely at odds with everyday human conduct. Secondly, the knowledge of any limitations on their own part (i.e. our lack of a technical language with which to speak about the work of science) and thereby the means of addressing that limitation as best they can (i.e. through undertaking genuine learning in the technical fields involved[6], to satisfy the unique adequacy requirement of methods as best they can). It is from this vantage point that this thesis has attempted to investigate the work of scientific research involving CRTs, and has been able to expect to begin to understand the features of it that make the endeavour possible and achievable for members.

As mentioned above, this thesis presents a selection of studies that deal with three aspects of the use of CRTs in early-stage scientific-related research projects: the various resources involved in their use, their fit with wider project concerns such as data collection and results outputs, and how researchers learn to work with them. Although these three strands are diverse and draw on different literary bases, the studies reflect a unifying theme. Each strand is grounded in sociological literature from an array of fields (sociology of science education, science and technology studies, computer-supported cooperative work, workplace studies, etc.) which have typically sought to emphasise the 'cultures' of learning and problem-solving in scientific (and related) fields, which underplays the extent to which early-stage researchers (learn to) do science on their own. The interest of these bodies of

[5] More detail is provided in chapter three.

[6] The word 'genuine' here is used to distinguish between actually acquiring a working knowledge of the technical field in question as opposed to simply 'learning to speak the lingo' in such a way as to *appear* technically fluent without actually understanding.

literature is in the ‘sociologically obvious’ phenomena in and around scientific research – phenomena that have a ‘surface sociality’ such as the conversations and interactions that occur between scientists and their colleagues, students and their supervisors, programmers and their end-users, and so on. This, however, is not the only possible topic of investigation. Indeed, this topic may well be (for members) an ancillary issue at best, peripheral to the ‘real’ work of research. Hence, the unifying theme behind this thesis is an attempt to reincorporate the ‘doing’ into sociological approaches to science education and research, by drawing on cases where researchers’ work is emphatically *not* a product of any collaborative interaction.

Overview of the Thesis

To this end, the thesis proceeds (see chapter two) by critically examining the existing state of literature with regard to the general study of scientific knowledge by sociology (particularly the approaches proposed under the various Sociology of Scientific Knowledge (SSK) programs), and by positing an ethnomethodological solution to the problems that these approaches embody. Chapter three builds on this platform by further justifying the ethnomethodological position in regard to the methodologies adopted by various SSK approaches, and honing in on some of the practical issues pertaining to how non-collaborative activities might be available to social research. The portion of the thesis given over to the findings of the research projects outlined above begins with chapter four, which takes as its subject a researcher’s endeavours in astrophysics programming and pays particular regard to the visual aspects and visualisations used in the performance of that work. Chapter five turns to the work of an early-stage researcher in electrical engineering, focussing on the generation and use of representations – a familiar topic throughout SSK – within the context of the researcher’s project and programming work. Chapter six brings the two sites of research – astrophysics and electrical engineering – together to explore some of the issues pertaining to how it is possible for early-stage researchers to perform practical tasks that work towards successful results whilst being as yet inexperienced ‘in the lab’ (and how previous approaches have misconstrued the specific contexts that are relevant to any sort of understanding of these activities). Chapter seven gathers together and summarises the results from these different elements of the overall argument.

CHAPTER TWO: Analytic Approach

The previous chapter worked to outline the aims of the thesis and depicted, broadly, the working process developed and implemented to achieve them. In this chapter, the work is taken further forward through a detailed explication of the adopted analytical approach, and its relationship with previous works in various related fields. The topic of the argument presented here is the constructionist influence on the wider sociology of scientific knowledge (SSK), but a more illuminating starting point for the chapter is a brief history of these ideas. The intent is to frame later constructionist SSK works as reactions to the early sociological approaches to science and scientific knowledge expounded by Karl Mannheim (1960 [1936]) and Robert K. Merton (1957, 1968, 1973) in order to better understand the motivations behind the various constructionist SSK programmes.

A Short History of the Sociology of Scientific Knowledge

The standard story of the sociology of knowledge (and scientific knowledge as an adjunct of that) begins, for most of its subsequent key practitioners and figures[7], with Mannheim, who was nevertheless loathe to deal with certain aspects of it. To Mannheim, a sociology of knowledge's task was to "analyse without regard or party biases all the factors in the actually existing social situation which may influence thought" (1960 [1936], 69). However, this task came with a caveat effectively excluding mathematical and scientific knowledge from sociology's reach, with the assertion that "The historical and social genesis of an idea would only be irrelevant to its ultimate validity if the temporal and social conditions of its emergence had no effect on its content and form." (Mannheim, 1960 [1936], 243). Hence, to Mannheim, sociology was fundamentally incapable of dealing with the kinds of facts and truths advanced through specific disciplines – such statements as "$2 \times 2 = 4$" (1960 [1936], 263) exist in a self-affirming "sphere of truth" (*ibid*.) that is "completely independent of the knowing subject" (*ibid.*). In this way, sociology could have nothing to say of the fields of mathematics or the natural sciences which were "largely detachable from the historical-social perspective of the investigator" (Mannheim, 1960 [1936], 261), and could only account for other realms of knowledge such as philosophy, politics, and cultural ideas.

[7] Notably, Lynch (1993) holds a different conceptualisation of Mannheim's work and its meaning in the historical situation of SSK than does, say for instance, Bloor (1973). However, the standard story, which can be taken to be the story read by Bloor and SSK in general, is presented here so as to correctly ascertain how the various programmes of SSK understand themselves as fitting in to the debate.

This (non-) approach to scientific knowledge was, however, contested by later sociologists such as Robert Merton, who proposed to open out sociology's scope somewhat further. To Merton, Mannheim's sociology of knowledge had fundamental classificatory problems, which treated anything "from folkloristic maxims to rigorous positive science" (1957, 551) identically. The homogeny with which different types of knowledge were characterised under Mannheim's conception served only to bring to the fore pre-existing assumptions between them, and Merton's goal was therefore to develop (and utilise) a better-defined classification that did not presume the physical sciences to be "wholly immune from extra-theoretical influences" (1957, 552). However, again, this did not extend to "the substantive findings of sciences (hypotheses, uniformities, laws)" (Merton, 1973, 268), which were still excluded from sociological investigation. Merton's accounting for science rather aimed to focus on the social and communal practices of the practitioners of science, and to this aim, Merton famously developed a four-fold characterisation of the norms informing the work of scientists.

Firstly, scientific practice is influenced by *universalism*, in that the credibility of scientific works is evaluated against a pre-established universal criteria, and "The acceptance or rejection of claims entering the lists of science is not to depend on the personal or social attributes of their protagonists" (Merton, 1973, 270). Secondly, a *communism* in the production of scientific research, wherein scientific research is seen as the product of entire scientific communities, ensures that "The scientist's claim to "his" intellectual "property" is limited to that of recognition and esteem" (Merton, 1973, 273). This serves to ensure that scientists are not motivated by anything other than the achievement of sound scientific work (for example, financial reward or other personal gain). Third is the rule of *disinterestedness*, which sees that "the [research] activities of scientists are subject to rigorous policing" (Merton, 1973, 276), and that this policing is performed by fellow scientists who are qualified to contest mistakes and thereby ensure that science is, on the whole, free from error. Fourthly, Merton's characterisation of science is informed by an *organized scepticism*, which involves "The temporary suspension of judgment and the detached scrutiny of beliefs in terms of empirical and logical criteria" (1973, 277), which guarantees the removal of non-objective ideological oppositions to scientific work. Merton puts this approach to use in his study of "The Matthew Effect" (1968), which investigated the inequitable distribution of reward[8] given to Nobel laureates in

[8] 'Reward' being a term which Merton (1968) uses to refer to recognition and esteem, as well as other implicated factors such as greater career opportunities, allocation of research funding, the opportunity to work with other eminent scientists, and so on.

science and the research teams in which their scientific work is grounded. In this study, Merton observes that while it is only the most reputable scientific figures that win the ultimate prize, their colleagues, research students and so on, do not. In this way, scientific work – perhaps spanning years in execution and involving many different researchers at many different stages – might be attributed to just one person, reflecting an unfair distribution of reward.

The Strong Programme of SSK

Merton's approach, as outlined above, is one of the first programmatic attempts to provide a sociological account of scientific knowledge, albeit enabled by the adoption of a tactic that sidesteps the issue of what can be said about a universal objective science. However, this approach and the kinds of empirical investigation that follow from it are taken as an untenable position by advocates of the various programmes (as discussed later in the chapter) under the rubric of the wider SSK, which aimed to further extend sociology's reach into the heart of science itself with the development and adoption of a constructionist approach to the field. Taking the 'Strong Programme' of SSK as an example[9], the development of a new brand of constructionist SSK was, in a significant sense, a reaction to the kinds of approach advocated by Mannheim and Merton. Its very name was pitched as a confrontation against Mannheim and Merton's 'weak' sociologies. Bloor, for instance, outlines a philosophical objection to Mannheim's sociology of knowledge, grounded in Merton's original critique:

> These branches of knowledge are so impersonal and objective that a sociological analysis scarcely seems applicable. Time and again in his *Ideology and Utopia* Karl Mannheim's determined advocacy of the sociology of knowledge stops short at this point. He could not see how to think sociologically about how twice two equals four. (Bloor, 1973, 173).
>
> It is as if Mannheim said to himself, 'When people do what is logical and proceed correctly, nothing more needs to be said'. But to see certain sorts of behaviour as problematic is to see them as natural. In this case what is natural is proceeding correctly, that is, via or towards the truth. (Bloor, 1973, 179).

[9] Though there were other programmes pitched at this time in SSK, the focus here, for the time being at least, is squarely on the strong programme (as developed and expounded by the Edinburgh school of SSK - a research group and approach developed by and involving David Bloor, as well as other SSK luminaries such as Barry Barnes and Donald Mackenzie) and not any other flavour of SSK (such as the Bath school's Empirical Programme of Relativism, led primarily by Harry Collins). The justification is in the interest of exploring the argument in an appropriate depth, rather than presenting a broad historical snapshot of the state of SSK in the 1970s and 1980s, although some of these other flavours will be brought into the argument at a later point (notably, the work of Collins (1985, 1990)).

However, although agreeing with Merton on this issue, Bloor's strong programme held that Merton's proposed solution was similarly inadequate. Bloor and others in the Edinburgh school of SSK (Barnes and Mackenzie particularly) argued for the inclusion of 'social interests' as integral parts of the structure of claims to knowledge, "and that it was these, not the [Merton's] 'norms' which most influenced the construction of scientific knowledge" (Bartley, 1990, 376). As Kaiser notes of Bloor's motivations for the development of the strong programme:

> Merton had criticized Mannheim for excluding scientific knowledge from his sociological investigations; then Merton had proceeded to study science as a self-enclosed social unit. To avoid the problems which this Mertonian approach seemed to engender, Bloor decided to go the other way (Kaiser, 1998, 73).

While Merton conceded that certain elements of science were informed by social norms (i.e. the rewards system, the motivation of scientists, the policing of scientific work, and so on), the content of scientific knowledge, in its ideal form, was free from such influences. Indeed, the very criterion for scientific knowledge to be universal and true for all time was it's *not* having been perverted by such social factors. Hence, the strong programme was an attempt to carry forward Mannheim's work into realms even Mannheim was reluctant to explore, whilst not limiting a sociological approach to scientific knowledge to merely accounting for errors in scientific knowledge.

Bloor's solution to these problems lies in a reading of Wittgenstein's ordinary language philosophy (Wittgenstein, 1974), with particular focus on what Wittgenstein has to say on the issue of how to continue a number sequence. Here, an example is discussed of a person (perhaps a student) attempting to fill in the next few numbers of a number sequence, having been given the first few numbers (perhaps by a teacher) with which to start. For a realist perspective "the correct continuation of the sequence, the true embodiment of the rule [for completing the numerical sequence] and its intended mode of application, exists already" (Bloor, 1973, 181). By contrast, Bloor takes Wittgenstein's focus on the negotiation of the sequence – say for instance, how the student and teacher negotiate an agreement on how to continue the sequence – as an argument that "mathematics can be seen as invention rather than discovery. There is a sense in which mathematics comes into existence when and as it is done" (Bloor, 1973, 188)[10]. Hence, Bloor's interest lay in how scientific knowledge became constructed through social interactions such as the negotiation of agreement on how to continue a number sequence, with each

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[10] This is a crucial point on which the overall argument presented here hinges, and will be returned to in more depth in the discussion that follows.

individual agreement constructing mathematics anew. This focus on 'construction' as the guiding principle of Bloor's approach to knowledge was drawn from a more general programme for the sociological study of knowledge advanced by Berger and Luckmann (1991), which stated that:

> the sociology of knowledge must concern itself with whatever passes for 'knowledge' in a society, regardless of the ultimate validity or invalidity (by whatever criteria) of such 'knowledge'. And in so far as all human 'knowledge' is developed, transmitted and maintained in social situations, the sociology of knowledge must seek to understand the processes by which this is done in such a way that a taken-for-granted 'reality' congeals for the man in the street. In other words, we contend that *the sociology of knowledge is concerned with the analysis of the construction of reality*. (Berger and Luckmann, 1991, 15).

As Turner notes, successors of Mannheim such as Berger, Luckmann and Bloor "popularized new terms of discussion, notably the phrase "the social construction of reality." These usages implicitly challenged the idea that the sociology of knowledge could be only the sociology of ideologies" (1991, 22). In this way, Bloor treats scientific knowledge as having been constructed out of an array of social practices (and other auxiliary features). In making philosophical objections to the realist works of Mannheim and Merton, Bloor shifts the analytic focus squarely on how these activities might work to contribute to the construction of scientific knowledge, drawn from the works of Wittgenstein, which are taken to show "how a behavioural theory can begin to come to terms with those features of logic and mathematics which have always seemed most resistant to anything but a Realist [...] interpretation" (Bloor, 1973, 190).

It is this imperative that heralded a formalised strong programme of SSK (Bloor, 1976) and the advent of constructionism as a driving force with which to tackle this newly-opened black box. Bloor's defining statement of the strong programme (as outlined in Bloor, 1976) organises the programme around four tenets – causality, impartiality, symmetry and reflexivity. Firstly, the tenet of *causality* recommends that a sociology of scientific knowledge should be "concerned with the conditions which bring about states of knowledge" (Bloor, 1976, 5), recognising that these many not all be 'social' causes[11]. Secondly, a sociology of scientific knowledge should be *impartial* "with respect to truth and falsity, rationality or irrationality, success or failure" (Bloor, 1976, 5) in that both sides of these dichotomies require explanation.

[11] It should be noted that this is point of departure from Berger and Luckmann's (1991) social construction of reality project. Whereas Berger and Luckmann aimed explicitly to take only the social features of knowledge construction as their topic of investigation, Bloor (1976) clearly hoped to expand this out somewhat to include other features.

Related to this is the third tenet of *symmetry*, wherein the same types of cause can be utilised to explain "say, true and false beliefs" (Bloor, 1976, 5)[12]. Lastly, Bloor outlines the tenet of *reflexivity*, which states that a sociology of scientific knowledge must provide causes and explanations that would, in principle, apply to sociology itself - "an obvious requirement of principle because otherwise sociology would be a standing refutation of its own theories" (Bloor, 1976, 5), in that sociology could not claim to explain any other form of knowledge if it could not explain itself. From this position, Bloor outlines the mission and purpose of a new strong programme of SSK:

> it must help to show how and why people think as they actually do. It must help show how thoughts are produced and how they achieve, keep and lose the status of knowledge. It must shed light on how men behave, how their minds work and the nature of opinion, belief and judgment. It will do this only if it makes an attempt to show how mathematics [and other types of knowledge] is built up out of naturalistic components: experiences, psychological thought processes, natural propensities, habits, patterns of behaviours and institutions. To do this it is necessary to go beyond a study of the outcome of men's thinking. The task is to go behind the product to the acts of production themselves. (Bloor, 1976, 138).

Bloor goes on to note, "it is men who govern ideas not ideas which control men. The reason for this is simple. Ideas grow by having something actively added to them. They are constructed and manufactured in order that they may be extended" (1976, 139). According to Bloor's reading of Wittgenstein, this must be so because there is nothing inherent in a rule that dictates what application may ultimately be made under the wider activity of 'completing a number sequence'. Hence, the *construction* of scientific ideas, and the many practical activities which such constructing might consist of, becomes the topic of investigation, and it is this emphasis that was taken up most fervently by Bloor's Edinburgh school peers (as well as other SSK researchers), in various conceptual and empirical projects.

Bloor exemplifies just what a sociologist may say about universal truths in his exploration of the conventional aspects of a seemingly innocuous proposition: $2 + 2 = 4$. Although this apparent fact[13] may seem incontrovertible, Bloor argues that it is imbued with conventionality at every turn, where conventions can be taken to

[12] The two tenets of impartiality and symmetry can be taken as an explicit revision of Mannheim and Merton's approach, which Bloor saw as only accounting for instances where scientists had got their science wrong. According to Bloor (1973, 1976), neither rational nor irrational science can be taken to be a natural way of proceeding, and therefore, both have *reasons*, which must be explained by a sociology of scientific knowledge.

[13] Presumably the choice of $2 + 2 = 4$ as the fact to be scrutinised is intended as a throwback to Mannheim's description of the universal truth of $2 \times 2 = 4$, but with the added benefit of having available a formal mathematical proof that might be understood fairly easily by a sociological audience.

mean “shared ways of acting that could in principle be otherwise. They are contingent arrangements, not necessary ones” (1996, 21). Hence, Bloor’s approach, shared by the Edinburgh school and the wider programme of constructionism in SSK, is to make clear the ways in which conventions impact upon apparently unimpeachable, necessary truths – “Demonstrating conventionality therefore involves demonstrating alternative possibilities” (Bloor, 1996, 21). Drawing on his particular reading of Wittgenstein, Bloor discusses a ‘naïve’ empirical proof of the proposition, perhaps the first which we might make about it: a person points to a set of two apples, to which he incorporates a further set of two apples, and then counts the apples as a single group. The two sets of two apples together make four apples. However, Bloor argues that this is an inadequate proof, in that it serves only to “produce a truth about four apples, rather than establishing a timeless necessity about the number 4” (1996, 23). Even a formal mathematical proof (one such example is represented in Bloor (1996) should the reader wish to see what such a thing looks like) of the proposition does not do anything significantly more to ensure the universality of the proposition, in that:

> if the person producing the proof, or the person reading the proof, weren’t already in a position to apply the equation that $2 + 2 = 4$ to the symbols of the proof, then they could neither generate it nor assimilate it. (Bloor, 1996: 28).

Hence, with any sort of ‘proof’ that we may be able to draw, from naïve to formal, there is nothing conclusively decided about whether that proof counts for all time or only for the specific objects (i.e. apples) and concepts (i.e. ‘2’, ‘4’ and ‘addition’) we are trying to prove, at the time of our proving.

Mackenzie, another Edinburgh school luminary, adopts this approach in his study of the historical development of nuclear missile guidance technology, and factors affecting that development. Mackenzie's key question is:

> How deep does the flexibility of the technical [i.e. the construction of technical facts] go? If we dig deep enough, can we not find a solid foundation of technical fact, matters that rationally cannot be disputed? Is there not, ultimately, a sphere of the technical that is genuinely insulate from politics and the clash of organizational interests? (1993, 340).

Here, Mackenzie's aim is to present several stories of how social factors (i.e. political, cultural, institutional, etc.) have affected the development of nuclear missile guidance technology, via discussions of such things as prevailing military strategies in the USA as reactions to apparent and supposed threats, and changing notions of minimum CEP (circular area probable) requirements for missiles (i.e. the accuracy with which a missile could be delivered to a specific target). In presenting these stories, Mackenzie outlines his key finding:

> The single most important lesson of this book is the fallacy of this technological determinism [i.e. that modernization necessarily equals better weaponry]. What we have found is that technological change is social through-and-through. Take away the institutional structures that support technological change of a particular sort, and it ceases to seem "natural" - indeed it ceases altogether. (1993, 384).

Hence, the fundamental findings of the strong programme are re-iterated in Mackenzie's study – that social construction pervades every aspect of scientific and technological development, and that in a significant sense our understanding of what scientific knowledge is (and what it involves) collapses as soon as these social struts are taken away.

Empirical Programme of Relativism

Working from this kind of conception, with the social construction of knowledge as the topic of investigation, various other empirical projects were conducted under groups with a different twist on SSK than the strong programme – for instance, Collins' (1985) work on the building of a transversely excited atmospheric laser (TEA-laser), which was carried out under the banner of Bath University's Empirical Programme of Relativism (EPOR). Here, Collins' interest falls on how the ability to build a laser is transmitted from scientist to scientist. As Collins notes of his observations, "no scientist succeeded in building a laser by using only information found in published or other written sources" (1985, 55). By contrast, working lasers were achieved where laser-building scientists had had personal contact and discussion with experienced laser-builders. Collins therefore claims that the scientific work of building a TEA-laser is an matter of acquiring the relevant 'tacit knowledge' – "our ability to perform skills without being able to articulate how we do them" (Collins, 1985, 56). However, these skills are precisely what scientists seek to omit from the accounts of their laser-building. When a TEA-laser is successfully built and lasing, any of the problems experienced throughout the practical hands-on work of building a laser are retrospectively attributed to errant human influences:

> One moment nature is obscure and recalcitrant, the next moment everything works and nature is once more orderly. The earlier obscurity and recalcitrance, which demanded so much human intervention to regulate, is then displayed as a *defect* in the human contribution. (Collins, 1985, 76).

As such, the human activities (such as trial and error experimentation, or wrangling effective results out of inadequate equipment) that are so fundamental to building a

TEA-laser become non-canonical elements of accounts of the building – humourous anecdotes at best, and omitted entirely at worst. Hence, to understand what exactly scientific work involves, it is necessary to "distance oneself from the standard view of experimentation in science and escape from the railroad of common sense to see the conventional nature of this reconstruction of 'what really went on' in an experiment" (Collins, 1985, 75). In focussing on the personal discussion and communication between scientists as they transmit the ability to build a TEA-laser, Collins aim is to do precisely this, thereby 'demystifying' the behind-the-scenes work of science.

Collins was later to further this empirical theme with a similarly informed study of (amongst other scientific research fields) crystallography, which emanates from the following position:

> Building scientific knowledge is a messy business; it is much more like the creation of artistic or political consensus than we once believed. The making of science is a skilful activity; science is an art, a craft, and above all, a social practice. (Collins, 1990, 3).

This work, as with Collins' earlier TEA-laser study, argues that "For scientists, a few words in a journal article can represent months or years of effort [...] We forget the confusion involved in reversing entropy" (Collins, 1990, 154). This is to reiterate that journal articles are not an accurate representation of the work of science. Indeed, Collins outlines a defining experience in his own participation and understanding of scientific work:

> I learned that when one first melts up an ingot in a furnace it is important to make sure that there are no air gaps in the mixture and that this is done by switching on a mechanical vibrator attached to the frame of the furnace but, more important, by bashing the frame very hard, very loud, and very long with a pair of old pliers that happen to be lying there."This," I remarked to myself, "is real physics." (1990, 177).

The point is made and remade that scientific work is invariably extracted away from scientific knowledge, and Collins' objective is to outline the ways in which this is done, for the purpose of 'demystifying' those elements of scientific knowledge which are presented (to scientists as well as lay people) fully formed and perfectly universal. Despite the distance that the Edinburgh and Bath schools sought to put between themselves, it should be clear from the empirical findings, methodological approaches and conceptual reasoning behind both projects that they are unified by one primary interest. Their singular concern is with the social construction of all things, particularly knowledge, and that investigating and accounting for the social features of how knowledge is generated, utilised and disseminated is the route by which to explain that same knowledge.

Actor Network Theory as Reaction to SSK

The constructionist tendencies of the various SSK programmes did not however satisfy all researchers interested in accounting for scientific knowledge. For instance, Bruno Latour (who had originally made his mark on the SSK with an influential laboratory study co-authored with Steve Woolgar (1979)) developed a new approach – Actor Network Theory (ANT) – very much as a reaction to staunchly constructionist studies such as those presented above. Latour's problem with an extreme constructionist SSK that the constructed nature of knowledge was emphasised above all else, and was taken to have stronger implications than it could philosophically support. As Latour notes:

> the excitation went quickly sour when we realized that for other colleagues in the social as well as natural sciences, the word construction meant something entirely different from what common sense had thought until then. To say that something was 'constructed' in their minds meant that something was not true. They seemed to operate with the strange idea that you had to submit to this rather unlikely choice: *either* something was real and not constructed, *or* it was constructed and artificial, contrived and invented, made up and false. (2005, 90).

By contrast, Latour's ANT holds a more relaxed view of the construction of knowledge, in which "to say something is constructed means that it's not a mystery that has popped out of nowhere, or that it has a more humble but also more visible and more interesting origin." (Latour, 2005, 88). Hence, ANT is pitched as a "sociology of translation" (Latour, 2005, 106) that mediates between and ties together the many different factors involved in the construction of knowledge (some of which may be social, some of which may not). This, to ANT practitioners, stands as an alternative to an SSK that is effectively a sociology of 'transportation', which presents an understanding of science as a purely social activity and then criticises it for being constructed thusly. To ANT however, a single-minded focus on the criticism of the constructed elements of knowledge neglects a far more interesting question – "Is it *well* or *badly* constructed?" (Latour, 2005, 89) – which may only be answerable through leaving these assertions at the door. As such, the single abiding tenet of ANT is "that actors themselves make everything, including their own frames, their own theories, their own contexts, their own metaphysics, even their own ontologies" (Latour, 2005, 147), and the sociologist's task therefore becomes one of reflecting this agency as accurately as possible in their accounts.

Latour (1988) puts this approach to practice in his historical investigation of the field of microbiology, as traced through nineteenth century journal articles. In this study, the ANT approach requires that:

> there should be a complete symmetry between the terms used to describe human and non-human actors. The first choice of term does not matter, but once we have chosen one for human actors, we shall stick to it when we address the nonhuman actors. If we "negotiate" with the microbes, then use the words for the hygienists or the ministry. If we "discover" bacilli, then "discover" the physicians or their colleagues. When this rule of method is applied, we soon realize that the distinction between science and society is an artifact caused by an asymmetrical treatment of human and nonhuman actors. (Latour, 1988, 262).

Hence, to Latour (and other ANT advocates), the value of the approach lies in its equal treatment of actants, whoever or whatever they may be. This view of actants is intended to ensure that no one (typically social) element of construction is revered above all others and, furthermore, that the account as a whole preserves the goings-on under scrutiny as holistically as possible.

In one of the more influential ANT studies of scientific knowledge, Callon (1986) studied how three groups of actants – scientists, fishermen and scallops – worked to negotiate a problem of low scallop yields in a French scallop farm in the 1970s, tracing their interactions through various stages of translation (i.e. in terms of how each group translated their issues to other groups). Callon characterises a group of three researchers as indispensible to the problem-solving activity. Using their knowledge of scallop farming techniques as applied to Japanese scallops, they were able to draw together three distinct groups – scientists (who were interested in understanding more about scallops biologically), French fisherman (who overfished scallops but were aware of the long-term risks of doing so and seeking a more sustainable solution) and French scallops (of whom little was known with regard to their breeding and maturation processes). From here, the three groups could begin to negotiate an 'interessement', whereby the actants were locked together in a possible solution, bringing together the biological knowledge of the scientists, the practical skills of the fisherman, and the willingness of the scallops to accede to the demands of farming. As Callon notes:

> The collectors [a method using net bags in which Japanese scallops were known to grow in captivity but which had not yet been applied to French scallops] would lose all effectiveness if the larvae 'refused' to anchor, to grow, to metamorphose, and to proliferate in (relative) captivity. (Callon, 1986, 209).

The proposed solution (i.e. to farm higher yields of scallops through the use of collectors which provided them with a better breeding environment) was then applied, and evaluated against the criteria of whether the three researchers could mobilize each group to contribute positively towards the resolution of the problem, or they couldn't (say for instance, the scallops dissented and refused to breed and grow in the provided conditions). Callon's interest then lies in this negotiation between groups of actants and how they can be brought together as part of a solution to what might be characterised as a scientific (biological) problem:

> The three researchers talk in the name of the scallops, the fishermen and the scientific community. At the beginning these three universes were separate and had no means of communicating with one another. At the end a discourse of certainty has unified them, or rather, has brought them into a relationship with one another in an intelligible manner. (Callon, 1986, 223).

SSK and ANT as Two Sides of the Same Problematic Coin

So far, this chapter has aimed to outline two of the conceptual driving forces in the contemporary sociological study of science, and present a selection of studies that put these approaches to use. The argument of the chapter from here on is that neither of these approaches – SSK and ANT – have engaged with the content of scientific knowledge, due in no small part to their loyalties to constructionism as the guiding principle (and finding) of their work. Their empirical work in this respect is very much motivated by philosophical concerns, as outlined by Zammito:

> From the outset the Strong Program pursued confrontation with *philosophy* of science even more than with sociology of science. The Strong Program undertook to displace philosophy by sociology: its tenor and its reception cannot otherwise be accounted for. (2004, 137).

Moreover, their efforts in resolving these philosophical issues with empirical investigation present difficulties in terms of how solid a grounding these projects are built upon in the first place:

> Many of the first group of students of laboratories used their observations to make philosophical arguments about the nature of scientific knowledge, but framed their results anthropologically. (Sismondo, 2004, 86)[14].

Hence, in undertaking empirical studies with the construction of knowledge in mind, Bloor (1976, 1996), Collins (1985, 1990), Mackenzie (1993) and other strong

[14] With this in mind, later sections of the chapter will address how the issue may be handled differently; chiefly through discussing how exactly an ethnomethodological position seeks instead to take a Wittgensteinian position of addressing empirical problems with empirical methods.

programme advocates invariably present the single philosophical finding that knowledge is constructed. In their critique of this form of SSK, Callon (1986) and Latour (1988) find, similarly, that knowledge is constructed. The following argument aims to explicate how this finding, reiterated throughout SSK and ANT, is symptomatic of an inability to escape a fixation on the purely social factors of scientific knowledge (i.e. how it is constructed from various social processes *and nothing more*). Although such a fixation is excluded programmatically from both the strong programme (via Bloor's (1976) causality tenet) and Actor Network Theory (via Latour's intent for ANT "to dispute the project of providing a 'social explanation' of some other state of affairs" (2005, 1)), this has, apparently, been difficult to maintain in practice. Such a fixation on explicitly social relationships might seem natural and unproblematic – surely this is exactly what a sociologist is *supposed* to investigate! Yet as an accompaniment to be taken alongside a constructionist approach (as this fixation appears in studies under both banners of SSK and ANT) it engenders a destructive 'ironic' stance towards scientific knowledge that devolves the activities of science into arbitrary social constructions with no necessary link to the scientific knowledge that is purported to be the topic of investigation. From this point, the task of sociology (either SSK or ANT) becomes to tell its audience (which may of course include the scientists it takes as the topic of study) just how wrong scientists have got it when they have attempted to explain their own actions. For instance, Collins outlines the two projects of his presented above as follows:

> This book shows how ships get into bottles and how they get out again. The ships are bits of knowledge and the bottles are truth. Knowledge is like a ship because once it is in the bottle of truth it looks as though it must always have been there and it looks as though it could never get out again. (Collins, 1985, vii).

> Building scientific knowledge is a messy business; it is much more like the creation of artistic of political consensus than we once believed. The making of science is a skilful activity; science is an art, a craft, and above all, a social practice. (Collins, 1990, 3).

What this demonstrates is that Collins' background in the constructionist strong programme is taken to somehow ensure more of a claim as to what can be said about scientific knowledge than scientists themselves hold. Through making a topic of how a ship (i.e. a bit of knowledge) could possibly get to be in a bottle (i.e. truth), Collins' (1985) aim is, squarely, to tell us what scientists either do not know themselves or have actively omitted from their own accounts – the constructed nature of their universal truths. As Turner notes of Collins' work, "This account is a redescription of the activities of scientists, which establishes that their descriptive practices with respect to their own activities are not manifestly better than other descriptions" (1991, 26). Lynch argues further that this irony in fact characterises the constructionist SSK project:

> the most common refrain from "laboratory studies" is that scientists act differently than their reports, biographies, and methodological writings say they do. This is often taken as support for an ironic contrast between an official version of logically defensible and consensually validated science and an actual science that is "in fact" messy and contentious. (1993, 270).

This irony is perhaps best exemplified through Collins' (1985) idea of scientific experimentation as a purely 'tacit knowledge'. Collins finds that whereas 'tacit knowledge' is crucial to the replicating of a TEA-laser, it is precisely this knowledge that scientists work to omit from their laser-building accounts. Collins, and other SSK and ANT practitioners in their respective accounts, deliberately presents his readers with an ambiguity as to whether scientists might be actively *deceiving* other scientists and lay audiences in the hope that such a deception will preserve the idea of a single universal scientific knowledge[15]. Similarly, Collins' (1990) revelation that science is an artful and – above all – social pursuit relies on the assumption that science has previously been (wrongly and deceptively) presented as a linear progression from wrong theories to right ones, dealing with an ever-increasing bank of irrefutable data. This assumption does not hold. If the doing of science is "much more like the creation of artistic or political consensus than we once believed" (Collins, 1990, 3) then it may well be our (Collins', and SSK more widely) once-held beliefs that are at fault rather than the provided (scientific) explanations. In short, the issue at hand is not science's deliberate covering-up of the truth about 'what really goes on', but sociology's failure to accurately establish 'what science really is' in the foundations of its straw man argument.

Despite its positioning as a reaction to an overtly sociological SSK, ANT fares no better in this respect. As Callon and Latour note of the ANT project in relation to their own empirical studies:

> we require the observer to use a single repertoire when they [both society and Nature] are described. The vocabulary chosen for these descriptions and explanations can be left to the discretion of the observer. He can not simply repeat the analysis suggested by the actors he is studying. However, an infinite number of repertoires is possible. It is up to the sociologist to choose the one that seems the best adapted to his task and then to convince his colleagues that he made the right choice. (Callon, 1986, 200).

[15] Certainly this ambiguity has been seductive to multiple studies too numerous to list here, but which are addressed in the literary backgrounds to the results chapters presented later in this thesis – chapters four, five and six.

> In the beginning, I claimed that I could discuss that indisputable science and provide an explanation of bacteriology because I agreed to recognize it for what it is, a nestled series of reversals in the balance of forces, and because I agreed to follow it wherever it led and to whatever groups it constituted, crossing as often as necessary the sacred boundary between "science" and "society." (Latour, 1988, 148).

What these quotations demonstrate is that a sociology of translation still, necessarily, takes control of what can be said about scientific knowledge from scientists. For Callon and Latour, it becomes the sociologist's task to choose a lexicon with which to frame their accounts (Callon, 1986), and to "recognize science for what it is" (Latour, 1988, 148) – the social and other processes at play that resolve what might become accepted as scientific knowledge (in short, anything *but* the content of scientific knowledge itself). Callon (1986) explicitly instructs that the sociological observer *must not* simply repeat the explanations of scientific work provided by scientific actors themselves, and Latour (1988) positions himself as the leading authority on bacteriology, over and above bacteriologists themselves. As Lynch notes:

> Commonly, constructivist [constructionist] theories depict socially organized actions as though they actually or potentially pursued tangible objectives, were based on clear-cut interests, and involved deliberate choices of means to facilitate those interests and objectives. This is suggested when everyday terms like *invention*, *inscription*, *manufacture*, *machination*, *manipulation*, and *intervention* are theoretically preferred over equally familiar idioms like *discovery*, *description*, *observation*, *testing*, *proving*, and the like. (1993, 266).

As such, the choice of lexicon is more than mere translation for ANT – it is equally as 'transportational' as the sociology of transportation it aimed to supersede. As with SSK, this inherent constructionism turns out to be a peculiar defect in the programme that colours the claims that are made about the activities and knowledge under investigation, and it is on these grounds that SSK and ANT can be said to be two sides of the same constructionist coin, despite being superficially opposed to each other. Under this conception, neither SSK nor ANT are able to account for the content of scientific knowledge, because it is explicitly excluded at a programmatic level in favour of novel approaches to the social features of scientific work that emphasise only the socially constructed nature of things that come to be called scientific knowledge. In light of this conception of the deficiencies of constructionism as an approach to scientific knowledge, the remainder of this chapter will aim to rebuild an approach more sensitive to the features of scientific knowledge and activity that lie outside of a purely social sphere, starting again from foundational Wittgenstein (1974)[16].

Re-reading Wittgenstein

To briefly reiterate, Bloor's take on Wittgenstein is such that "In principle, each application of a rule is negotiable, and the negotiation (or lack of it) is intelligible in terms of the dispositions and interests of the rule followers themselves: that is where agency truly resides." (Bloor, 1992, 271). Under this conception, the following of a number sequence is conventional each time through, such that when asked to complete a sequence with a rule, any 'naughty schoolboy' wishing to cause trouble for his teacher by diverting from the teacher's expectations of how the rule is to be continued is effectively creating a new system of mathematics. The acceptance of the naughty schoolboy's claim to a new mathematical truth is then an issue of wider social norms such as the cultural reproduction of Western (or global) numeracy, the acceptance (or otherwise) of authority in an educational institution, and so on. This, to Bloor, is how the features of society come to bear on what we may – wrongly – believe to be a universal truth. As Button and Sharrock summarise, to Bloor and constructionist studies in general "scientists' methods for establishing objective findings actually consist of *the employment of rhetorical techniques for persuading others to agree and rhetorical techniques for displaying consensus*" (1993, 5). Hence, the constructionist focus becomes one of agreement and consensus on the negotiation of a 'fact', and it is SSK (and, I argue, the similarly constructionist ANT's) project to trace which 'facts' become accepted as 'science' and knowledge more generally, or not.

There is, however, much opposition to this staunchly anti-realist perspective, hinging on a critique of this 'anything goes' reading of Wittgenstein. Lynch for instance notes that "For Bloor, Wittgenstein's pivotal move was to reconceptualise the central topics of epistemology as empirical problems for social science research" (1992, 218), thereby attempting to settle philosophical problems with empirical investigation. This, Lynch argues, is an unsatisfactory and impossible enterprise, and not one advocated by Wittgenstein himself:

> By citing intuitive examples from ordinary usage and constructing imaginary "tribes" and language games systematically different from our customary usage, Wittgenstein is able to problematize

[16] Although some are discussed in the present chapter, for a fuller account of some of the sticking points between constructionists and others (chiefly, ethnomethodologists) on the topic of Wittgenstein's philosophy, readers would do well to start with the interchanges between Bloor and Lynch in Pickering's (1992) (ed.) "Science as Practice and Culture".

> epistemology by showing the variations, systematic ambiguities, and yet clear sensibilities in everyday usage. (Lynch, 1992, 256).

The concern here is that Bloor takes Wittgenstein's problematisation of epistemological issues around what understanding various forms of knowledge might require as a *solution* of sorts, which might be applied – unproblematically – to scientific activity. Hence, as Button and Sharrock note, the constructionist argument is forced into an untenable position that suggests "that making something 'real' simply involves agreeing that it is so, and that, accordingly, the very constitution of the object is apparently done in a conversational or discourse interchange." (1993, 6). Wittgenstein's argument is that:

> there is an inclination to say: every action according to the rule is an interpretation. But we ought to restrict the term "interpretation" to the substitution of one expression of the rule for another. (Wittgenstein, 1974, §201)

While it is grammatically clear that 'interpretation' refers in this case to some statement that cannot be evaluated as 'right' or 'wrong', a continuation of a number sequence is *not* an interpretation. Rather, it is an *expression*, which may be evaluated against the criteria of the number sequence itself, in terms of whether the actions of the student continuing the number sequence accorded with what was *meant* by the rule or not. The real question at hand then, for this conceptual case, is not one of whose interpretation is accepted – no such interpretation is ever made! – but how one particular expression of the rule can be 'meant' (and consequently understood) in the interaction:

> We say for instance, to someone who uses a sign unknown to us: "If by '$x!2$' you mean 'x^2', then you get *this* value for y, if you mean '$2x$', *that* one." – Now ask yourself: how does one *mean* the one thing or the other by "$x!2$"?
>
> *That* will be how meaning it can determine the steps in advance. (Wittgenstein, 1974, §190).

As such, the constructionist argument "plainly diverges from the concern that Wittgenstein develops. His argument is directed towards *what it makes sense to say*" (Button and Sharrock, 1993, 12-13). In these terms, a sociological approach might investigate the features of a shared understanding of what might be meant by the given rule for completing the number sequence, rather than pursuing a line of argument whereby anything that could be said out of the limitless array of possible answers is true under some alternative conception of the rule at hand. This crucial element of Wittgenstein's argument is perhaps best expressed thusly, by briefly stepping aside from the issue of number sequence continuation and into ordinary language[17]:

> It is only in normal cases that the use of a word is clearly prescribed; we know, are in no doubt, what to say in this or that case. The more abnormal the case, the more doubtful it becomes what we are to say. And if things were quite different from what they actually are – if there were for instance no characteristic expression of pain, of fear, of joy; if rule became exception and exception rule; or if both became phenomena of roughly equal frequency – this would make our normal language-games lose their point. – The procedure of putting a lump of cheese on a balance and fixing the price by the turn of the scale would lose its point if it frequently happened for such lumps to suddenly grow or shrink for no obvious reason. (Wittgenstein, 1974, §142).

Here, Wittgenstein's point is that our language use is grounded in mutual understandings of what we are talking *about*. To create a new mathematics every time we provide a different continuation of a sequence defeats the point of the wider activity as one in mathematics, which is concerned with the skill a student might have in working with concepts found in only *one* (the 'accepted' *and* the 'correct') mathematical system. Just as it would be futile to continue to buy cheese by weight if cheese itself was not of a fixed mass, it would be futile to teach students how to continue a number sequence if *any* continuation could be justified[18]. Hence, as Lynch notes, "far from making science and mathematics safe for sociology, Wittgenstein made things entirely unsafe for the analytical social sciences" (1993, 183), and therefore this calls for a different kind of sociology other than a constructionist SSK or ANT based purely on agreement and consensus.

Taking Wittgenstein's problematisation of the social sciences as a foundation, we can begin to rebuild an idea of how a social science concerned with (scientific) knowledge might ultimately take shape by exploring a key work of one of Wittgenstein's successors, Peter Winch (1990)[19]. Although the argument precedes the constructionist programmes in SSK and ANT by nearly twenty years, Winch advocates the sceptical treatment of any social science holding an ironic approach to the subjects of its study. This aspect of Winch's argument is expressed through a critique of E. E. Evans-Pritchard's (1976) anthropological study of witchcraft in an

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[17] Ordinary language philosophy is a field in which Wittgenstein was prolific, and is organised around the ways in which people can and do use meaningful language in their everyday interactions with each other.
[18] In this case, perhaps the first mathematics lesson would conclude that 'anything goes', and no more would need to be said.
[19] This work, "The Idea of a Social Science", can be read as an application of Wittgenstein's problematising, more directly centred on the social sciences specifically. Indeed, the title itself casts incredulity on the sociological enterprise – the very idea!

African tribe called the Azande. To summarise briefly, Winch takes issue with Evans-Pritchard's critique of the rationality of the Azande tribe with regards to their witchcraft and oracle-usage (used to forecast the futures of Azande people) on the grounds that Evans-Pritchard does not do enough to understand the rationality that these Azande actions have *as* actions performed in a framework of Azande society and beliefs. Here, Winch argues that in comparing (unfavourably) Azande activities against Western ideas of rationality, Evans-Pritchard has failed to account for those same Azande activities in a way which will increase our understanding of them. As Hutchinson *et al*. note of Winch's position:

> If one is blind to the description of the action as would be understood by the competent actor – what the action is, given the theoretical setting, given the actor's purpose – then one has simply failed to establish what they are doing. (Hutchinson *et al.*, 2008, 96).

Given this position, it is useful to consolidate Winch's perspective as part of the wider field of what a possible sociology of knowledge might look like by returning once more to the unifying issue of continuing a number sequence. Here, Winch poses a particular variation of the 'naughty schoolboy' problem, whereby person *A* writes a sequence '1 3 5 7' on a blackboard. *A* asks *B* to continue the sequence, and *B* obliges, writing '9 11 13 15'. At this point, *A* claims this is a wrong continuation and corrects B by writing '1 3 5 7 1 3 5 7 9 11 13 15 9 11 13 15', and an argument ensues:

> There would undoubtedly come a point at which *B*, with perfect justification, would say that *A* was not really following a *mathematical* rule at all, even though all the continuations he had made to date *could* be brought within the scope of some formula [which would no doubt be more convoluted than the formula $(2n - 1)$ that *B* had supposed *A* must have meant]. Certainly *A* was following a rule; but his rule was: Always to substitute a continuation different from the one suggested by *B* at every stage. And though this is a perfectly good rule of its kind, it does not belong to arithmetic. (Winch, 1990, 30).

Hence, the ability to apply a rule is not signified merely by someone *formulating* a rule – in Winch's conceptual number sequence continuation, every time *B* thinks he has got the right answer, *A* is able to formulate a new rule that is mathematically consistent but that also disagrees with *B*'s provided answer. Rather, it is the idea of "whether it makes sense [for a rule follower] to distinguish between a right and wrong way of doing things in connection with what he does." (Winch, 1990, 58). With particular regard to how this might come to bear on a sociological study of scientific activity, Winch's argument is in essence that:

> to investigate the type of regularity studied in a given kind of enquiry is to examine the nature of the rule[20] according to which judgments of identity are made in that enquiry. Such judgments are intelligible only relatively to a given mode of human behaviour, governed by its own rules. In a physical science the relevant rules are those governing the procedures of investigators in the science in question. For instance, someone with no understanding of the problems and procedures of nuclear physics would gain nothing from being present at an experiment like the Cockcroft-Walton bombardment of lithium by hydrogen; indeed even the description of what he saw in those terms would be unintelligible to him, since the term 'bombardment' does not carry the sense in the context of the nuclear physicists' activities that it carries elsewhere. To understand what was going on in this experiment he would have to learn the nature of what nuclear physicists do; and this would include learning the criteria according to which they make judgments of identity. (Winch, 1990, 83-84).

Hence, a sociological approach to scientific knowledge and activity must be willing and able to take on board the finer detail of the *content* of that knowledge and activity, as opposed to merely investigating the surface sociality and relationships that are most easily accessible to those with a sociological background. However, an approach such as this does not have to dive headlong into a seemingly esoteric scientific world, where participants may seem to rely on a highly complex system of technical language and undertake activities that may seem to frustrate any understandings an 'outsider' (such as a sociologist) might make of them. One possible way to begin to unravel these apparent mysteries whilst avoiding "the antinomies of the realist—constructivist [constructionist] debate" (Lynch, 1996, 319) is through adopting an ethnomethodological perspective, as traced through the re-reading of Wittgenstein presented above and via the further work of Winch. The argument from here on will build on the outlining of ethnomethodology's key principles presented in the first chapter, and aims to show just how with an ethnomethodological perspective it might become a feasible task to understand the work of science and scientific knowledge, from its basis as an ordered and organised field of human activity, grounded in ordinary language and action.

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[20] It should be noted that neither Winch nor Wittgenstein intended to claim that all action was equal to (and nothing more than) 'rule following'. As Hutchinson *et al.* note:

> the word 'rule' is not a theoretical term, it is a perfectly ordinary English word, and Winch uses it as such: there are innumerable activities – such as the spelling of words in English which are obeyed many times on every line of this book – that are extensively or in some aspects rule governed. To state this is not to offer any theory of writing or of English spelling; it is merely to describe, state a truism about writing. (Hutchinson *et al.*, 2008, 44).

Ethnomethodology and Scientific Practice/Knowledge

An ethnomethodology of scientific practice and knowledge (as outlined, briefly, in chapter one) must necessarily begin by investigating scientists' ordinary mundane activities, in that it is through such activities that the less easily accessed (for ethnomethodologists at least) elements of scientific work are performed. To reiterate for the present argument, Caton provides a useful philosophical framework for precisely this relationship between ordinary and technical, beginning with the proposition that "technical language is always an *adjunct* of ordinary language" (1963, viii). How this may be so is that whilst scientists may indeed speak about different things than we (i.e. non-scientists) might be used to in our everyday goings-on, their language is structured and ordered by the same principles that we are *all* aware of:

> In ethnographic studies of science and other specialized practices, "familiar" activities like giving orders, asking questions, and giving instructions provide an initial, although far from sufficient, basis for grasping the intelligibility of technical actions. (Lynch, 1993, 182).

Ethnomethodology is not however limited to the study of language use, and the same analytic focus – how 'the technical' takes place through the application of 'the ordinary' – is applied to the praxiological elements of scientific work:

> Scientists rely upon a 'syntax' of practices and methods which are accredited as 'correct', 'sufficient to the task at hand', 'properly conducted by prevailing standards' in just those ways in which any concerted activities are warranted by a collectivity. Furthermore, the working scientist trades in objects and procedures which are not formulated in the categories of any scientific theory. This is not proposed as a limitation or defect of science – how *else* could it proceed? (Turner, 1974, 9).

Notably though, not every aspect of scientific work can be equated so straightforwardly to abstracted social interactions such as 'giving orders' or 'properly conducting by prevailing standards' and so on. The fact of the ordinariness of the actions under scrutiny provides ethnomethodology with a springboard from which to base more probing enquiries. As Lynch notes:

> Like the sociologists of scientific knowledge, ethnomethodologists try to transform the traditional themes in epistemology into topics for empirical research. But instead of advocating a "sociological turn" in which philosophy's problems are given sociological explanations, ethnomethodologists initiate a "praxiological turn" through which they turn the sociological aim to explain social facts into a situated phenomenon to be described. Sociology's loss becomes society's accomplishment. (Lynch, 1993, 162).

Crucially, to investigate such mundane actions is, emphatically, not to make the same sociological 'cop out' as previous approaches – Mannheim, Merton, SSK and ANT – which actively exclude the content of scientific knowledge from their remit. Rather than taking this base sociality of actions and interactions in scientific settings to be the end result in and of itself, to ethnomethodology, these occurrences are treated as a platform from which to build towards a direct engagement with scientific knowledge as it is dealt with by scientists. Indeed, Hutchinson *et al*. make the claim that to outright reject the possibility of such a direct engagement is to run the risk of misrepresenting the activities under scrutiny as constructionist accounts have invariably done:

> insofar as there is or might be any project of *understanding* human being(s), that is going to have to proceed by cases – considering mindful human beings in action, engaged in specific human practices – and courts failure if it doesn't begin by engaging with the 'order' inherent in/reconstructed by those practices. (Hutchinson *et al*., 2008, 34).

Hence, being acutely aware of (perhaps even sharing) members' own perceptions of how their activities and talk are constituent elements of the work as it occurs gives the ethnomethodologist the tools with which to account for the highly technical nature of the activities and talk, in the same way as members themselves might – would! – account for them. Lynch summarises the aim of ethnomethodological work on scientific practice and knowledge noting that "Ethnomethodology's descriptions of the mundane and situated activities of "observing," "explaining," or "proving" enable a kind of rediscovery and respecification of how these central terms become relevant to particular contextures of activity" (1993, 200). Hence, the empirical question to be addressed is not whether a formulation – perhaps a written 'recipe' for building a TEA-laser (Collins, 1985) – truthfully or falsely represents the activities under scrutiny (i.e. the building of the laser), and what else must be added to a formulation to achieve the desired result (i.e. the social transmission of 'tacit knowledge'). Rather, the activities are investigated in terms of "how they act as pragmatic moves in temporal orders of action" (Lynch, 1993, 190), and the key ethnomethodological concern is, simply:

'why this, now?'

Having shown the failures in previous approaches, and the requirement for a sociology of scientific practice and knowledge to deal with this key question, the remaining chapters presented in this thesis will be organised around this pervading concern[21].

Concluding Remarks

The present chapter has aimed to outline some of the more influential sociological approaches to scientific knowledge, and critically appraise the effectiveness of these programmes and their subsequent empirical studies. The constructionist SSK (of which the strong programme was and still arguably is a leading proponent) seemingly has nothing to offer beyond the claim that scientific knowledge is arbitrarily constructed (and thereby false), and even this statement is highly contestable. As Lynch notes:

> Their [constructionist] studies do not empirically demonstrate that "scientific facts are constructed," since this is assumed from the outset. It would be more accurate to say that they demonstrate that a constructivist [constructionist] vocabulary can be used for writing detailed descriptions of scientific activities. (Lynch, 1993, 102).

However, the actual *value* of writing about scientific activities in this way is uncertain – it seems as if constructionists produce so many of these accounts simply because they can! – and as such, there is no reason as to why a constructionist account of science should be the ultimate goal of a sociology of scientific knowledge. Similarly, although ANT was originally proposed as a means of resolving the problems in the sociologically elitist ironic SSK, it does not come much closer (if it does at all) to a clearer understanding of scientific knowledge. In focussing its investigations on the history of theories in bacteriology, Latour (1988) ignores the *content* of those same theories, and in topicalising the negotiation between various groups of actants involved in a scientific problem, Callon (1986) ignores the *things* that each group (including scientists) find interesting and relevant enough to talk about. Hence, SSK and ANT, make every effort to investigate everything *but* scientific knowledge itself, and just as with Berger and Luckmann (1991), they thereby become sociologies of *what becomes accepted as knowledge*, which is a fundamentally different operation than a straightforwardly sociology of knowledge. In this respect, SSK has a simplistic approach whereby the question is 'what factors make this version of knowledge

[21] In chapter four, the question will come to bear as the issue of why an astrophysicist performs specific coding activities at particular points in his working process. In chapter five it will take shape as an investigation into the purpose and usage of representations to an early-stage researcher in electrical engineering who (amongst other things) makes specific choices with regard to data collection given what is available for the seeing on a computer screen. In chapter six the concern will address why both the astrophysics and electrical engineering research projects under discussion can be (and are) one step in the educational career of future scientific researchers.

accepted?'[22], whereas ANT might be argued to be a little more sophisticated, in that it demonstrates a story of knowledge production and how a particular version of knowledge might be hard to resist given the conditions of its construction. Nevertheless, they both fall short of delivering a sociology of *knowledge*, and we are left still unclear as to what makes the activities in question specifically *scientific* and not from some other sphere of organised action. The question we are left to ask is; if all the people under scrutiny are doing can be boiled down to *nothing more than* basic social relations, then what sets these people apart from, say, economists? Or lay people? Or witch doctors? Clearly there is a need for an approach that can work to characterise such activities as specifically scientific, and it has been the aim of this chapter to show ethnomethodology's suitability in this regard.

Starting again from Wittgensteinian origins (and following those origins through the work of Peter Winch), the ethnomethodological project sees no need to take constructionism as the only possible route to understanding the work of science. Instead, it begins with a focus on the ordinary language and action used in scientific activities and follows this up by seeking to understand concepts drawn from scientific knowledge itself, and incorporate them into its account of the order and organisation of the setting. As Lynch notes, "far from being a chaos from out of which order is constructed, the locally organized and reflexive details of actual conduct in a laboratory are orderly and describably so." (Lynch, 1993, 319), and it is the aim of the following chapters to provide exactly this kind of description.

[22] It is also highly contestable as to whether Bloor (1976, 1996) and other strong programme advocates (e.g. Collins (1985, 1990) and Mackenzie (1993)) routinely break their own causality clause, which states that their sociology should seek more than just social explanations for scientific knowledge. Perhaps they do adhere to it to an extent – Mackenzie (1993) for instance at least broadens his scope to incorporate technological, political and institutional factors into his explanation– but the use of concepts drawn from scientific knowledge itself remains a glaring omission to these studies.

CHAPTER THREE: Methods of Study

This thesis builds on video-aided ethnographic studies of two postgraduate early-stage researchers conducting projects in the fields of astrophysics and electrical engineering respectively, paying particular regard to their use of computerised research tools. The aim here has been to understand some of the features of working with computerised research technologies (such as programming languages) as their use occurs in the day-to-day work of settings combining scientific research and education. Approximately twenty hours of video has been recorded, spanning the course of several days in each of the subject disciplines. These videos encapsulate an array of distinct tasks occurring throughout a 'normal' working day[23], including the use of programming languages, the analysis of visual results, the interplay between just-collected experimental data and data already processed through a program, and so on (see chapters four, five and six for further details). These video recordings are supplemented with ethnographic field notes collected at the time of videoing, gathered through observation and participation in the two students' work on these days, and with a significant degree of preparatory work[24] designed to furnish these video-aided ethnographic materials with the level of scientific competence necessary to understand finer details of the activities at hand.

This report is based on activities captured by the video recordings, and are further 'fleshed out' through the knowledge gained through the ethnographic preparation, as well as through the familiarity and depth-of-knowledge that repeated viewings of the tapes has afforded. The video and ethnographic materials have been subjected to a non-linear ethnomethodological analysis, in what Lindwall terms "iterative and ongoing cycles" (2008, 60), going back and forth between stages of watching and re-watching tapes, transcribing and re-transcribing key episodes, and presenting and

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[23] That is, 'normal' in terms of its consisting of activities already designed to contribute towards the projects aims and objectives as they currently then stood, excepting of course the atypical presence of a video camera and an ethnomethodologist (the effects of which will be addressed later in the chapter).

[24] This work has included: interviewing participants and their peers and supervisors about their project work (and their particular approaches to it) and their role in wider research projects and groups; learning various elements of undergraduate-level textbook science and mathematical techniques; acquiring a rudimentary working knowledge of two of the more ubiquitous programming languages (MATLAB and Python), and; taking a selection of undergraduate lectures across all four years of the University of Manchester's MPhys degree (topics included theoretical physics, mathematical requirements for physicists, and various aspects of astrophysics including stellar evolution, galaxies and early universe cosmology). As such, it is difficult to put a figure to the amount of hours spent doing this preparatory work – indeed, the preparatory work has continued (and still does continue) throughout the analysis and presentation of the research, with each iteration prompting more 'preparation' to understand previously unnoticed features of the video data.

re-presenting findings. This analytic work has resulted in three distinct studies, each under a different theme, each organised around various elements of relevant activities in the settings under investigation. The aim is not to comprehensively account for the research work undertaken on the days captured on video but the characteristic ethnomethodological pursuit of describing and unpacking several features of how that research work is ordered, organised and achievable by members involved, and the provision of conceptual respecifications of relevant phenomena in light of existing sociological work on them. However, one unifying focus is on how the non-collaborative research work under investigation (in both astrophysics and electrical engineering settings) is made possible, despite this supposed 'problem' of non-collaboration, as an attempt to show just what else might be involved in scientific research work besides interaction with research colleagues. As such, this chapter deals with the methodological issues that bear on research involving non-collaborative work and settings, delineating, presenting and justifying a video-aided ethnographic approach to understanding how computerised research technologies fit into wider scientific research settings.

Situating Ethnomethodology Against Previous Methodological Approaches in SSK and ANT

One useful way of addressing and justifying the efficacy of a video-aided ethnographic approach to this particular problem is to outline the methodological foundations of previous approaches – for the argument of the present thesis, this will focus on Bloor's (1976) reflexivity tenet for SSK, which has been widely adopted in empirical work throughout the Sociology of Scientific Knowledge and by related fields including Actor Network Theory – to ascertain what exactly they *do* and *don't do* for the studies resulting from them. To reiterate briefly a point from the previous chapter, this tenet insists that any sociological approach to scientific knowledge must provide causes and explanations of knowledge that could, in principle, apply to sociology itself. This has been a recurring theme in SSK and ANT works in which authors devise creative methods of highlighting aspects of their own knowledge construction, as well as that of the scientists they aim to study[25]. Without wishing to delve too deep into the history of the reflexivity argument[26], for the present chapter

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[25] One particularly striking example of the meta-levels reached in this area is in Pinch and Pinch (1988), where Trevor Pinch can be found arguing with himself on the topic of just how reflexive he must be in his work.

[26] This argument is neatly encapsulated in an unresolved exchange between Collins and Yearley (1992) and Woolgar (1992) and Callon and Latour (1992), in which Collins and Yearley provide a critique of the radical extremes to which SSK and ANT have taken the tenet, and Woolgar and Callon and Latour offer subsequent rebuttals claiming that Collins and Yearley's problem is that they are not radical *enough*.

it suffices to say that SSK has, since its inception, fostered a radical approach to reflexivity in which new literary forms not typically associated with academic research (such as the representation of author's inner methodological musings as a dialogue (Pinch and Pinch 1988), conversational styles of writing accompanied by histories of key events in the formulation of the thoughts being expressed (Woolgar 1992), or anthropomorphising scallops as active participants in solving a scientific problem (Callon 1986)). As Lynch notes of the approaches adopted by studies such as those mentioned above, "Reflexivity, or *being* reflexive, is often claimed as a methodological virtue and source of superior insight, perspicacity or awareness" (2000, 26). Hence, presenting information in a novel way is seen by SSK and ANT to be a means of sidestepping any thorny issues relating to the justification of a sociologist critiquing a discipline they are not professionally competent in. However, Lynch's argues that reflexivity in this literary sense is superfluous since reflexivity is "an unavoidable feature of the way actions (including actions performed, and expressions written, by academic researchers) are performed, made sense of and incorporated into social settings" (2000, 27). Indeed, Lynch finds it impossible to imagine what an *un*reflexive study might consist of, in that it seems irrefutable that academic researchers (as well as everybody else) routinely engage in thinking about and conceptualising their work before, during and after doing it. Hence reflexivity – the active act of analysing your own actions and work – is ubiquitous and unavoidable, and it follows then that "it no longer makes sense to distinguish reflexive from unreflexive language or action" (Lynch, 2000, 42).

To summarise, Lynch (2000) characterises SSK's and ANT's propensity for 'extreme' methodological and presentational tactics as unnecessary – "there is no particular advantage to 'being' reflexive, or 'doing' reflexive analysis, unless something provocative, interesting or revealing comes from it" (Lynch, 2000, 42). This chapter argues that the adoption of these radical forms of reflexivity is used by authors as a way of buying results cheaply. Throughout SSK and ANT, novel literary forms are implemented to absolve authors of the need to understand the contextual detail of the activities under scrutiny – if Callon (1986) is not able to tell us anything specific about the biological understanding of the life-cycle of scallops, he can at least make his work original through a literary technique often used in fiction that ascribes them with human qualities ('willingness', 'refusal' etc.). In a significant sense, radically reflexive accounts in SSK and ANT do not so much say anything about scientific knowledge as they do about sociology; they might be conceptualised as exhibitions in a showcase of the outer limits of sociological reasoning. While radically reflexive accounts in SSK and ANT may provide innovative ways of researching and writing about research, it is not clear that any such thing is needed, especially so when they are adopted at the expense of accounting for features that make the setting under scrutiny specifically one in science. Indeed, in light of the remarks made in the previous chapter, the drive for radically reflexive forms of representation seems only to further distance SSK and ANT from the scientific knowledge it purports to study, excluding a possible in-road to that knowledge methodologically as well as conceptually. Hence, what we find missing from SSK's and ANT's methodological arsenal is the same thing we find conspicuously absent in its conceptual repertoire (see previous chapter) – a tool for accessing scientific understanding without recourse to focussing on the sociological issue of how to present sociological research.

This thesis has taken ethnomethodology as a basis for providing exactly this orientation towards scientific understanding, and it may be useful to outline and justify the impact ethnomethodology and associated approaches have had on the research work undertaken. As Garfinkel notes, the ethnomethodological aim is "to detect some expectancies that lend commonplace scenes their familiar, life-as-usual character and to relate these to the stable structures of everyday life" (1964, 227) – how this kind of aim may come to bear on studies of scientific understanding is dealt with in chapters one and two. For present purposes, a general methodological directive for ethnomethodology is to be found in Garfinkel's claim that "Any setting organizes its activities to make its properties as an organized environment of practical activities detectable, countable, recordable, reportable, tell-a-story-aboutable, analyzable – in short, *accountable*"[27] (1967, 33). Hence, the key methodological goal for this thesis has been, simply, to find out more about what features of order and organization make the understanding of various scientific activities accountable for those directly involved in them. An approach such as this is, naturally, not limited to the study of verbal accounts but has a wider applicability to *whatever* resources members rely on to make sense of their settings and activities – hence, there is nothing in principle to exclude non-collaborative action from investigation under ethnomethodology, in that even in non-collaborative settings, members act accountably. As Schegloff comments in an interview with Čmerjrková and Prevignano of conversation analysis (a discipline very closely affiliated with ethnomethodology):

> The most important consideration, theoretically speaking, is (and ought to be) that whatever seems to animate, to preoccupy, to shape the interaction *for the participants in the interaction* mandates how we do

[27] Indeed, it is these same features of organization that provide for the natural and inescapable reflexivity that Lynch (2000) draws upon in his argument.

> our work, and what work we have to do. (Čmerjrková and Prevignano, 2003, 25).

More specifically oriented to scientific research, Lynch notes that:

> Ethnomethodology's descriptions of the mundane and situated activities of "observing," "explaining," or "proving" enable a kind of rediscovery and respecification of how these central terms become relevant to particular contextures of activity. (Lynch, 1993, 200).

As such, the methodology utilised here is directed towards uncovering (for myself and explicitly *not* members, for whom such things are already uncovered and well known) the reasoning behind members' courses of action when they undertake various non-collaborative activities in scientific research and learning. The 'trade-off' here is that in order to recognise the accountability of members' actions when they are undertaking non-collaborative work, what is required of ethnomethodologists is an understanding of the contextual factors *of* that work (cf. the discussion of the 'unique adequacy requirement of methods' (Garfinkel and Wieder, 1992, 182) in chapter one), which is perhaps not so straightforwardly acquired in settings involving complex technical knowledges and skills. As Hughes and Sharrock note:

> The thesis that the identification of actions must necessarily be in the language of the social actor is seen as having very serious consequences for the status of knowledge about the social. What is being proposed is more than simply urging social researchers to investigate the ideas and beliefs of whom they study. The argument is about the nature of the concepts used by social science to explain its phenomena. (Hughes and Sharrock, 1997, 146).

It may be useful at this point to clarify how these motivations to encapsulate members' own sense of the accountability of their doings are of vital importance to any sociological understanding of those activities and that work. The following chapter in this thesis (chapter four – "Scientific Findings") is concerned with the use of visualisations, visual resources and visual aspects of scientific research involving computerised research tools. Taking this focus as an example, Lynch (2011) has investigated a related topic of scientific understandings of images of nanotechnology stored in online galleries. Here, Lynch points out that while recent images of nanotechnology have little aesthetic value, they express a 'craft'. For instance, images of the nano-guitar, manufactured by Cornell University in 1997 as a showcase of nanofabrication techniques, may appear uninteresting and unrealistic to the casual observer but have a greater significance to a nanotechnologist who is aware of the feats required in creating (and taking a picture of) a tuneable and playable guitar that is no larger than a human blood cell. The lesson to be learned here is that it is impossible to appreciate scientific work fully without understanding the scientific (and not just social) elements of the knowledge and activities that make up the endeavour. Further drawing out the example of the visual components of the scientific work discussed in chapter four, as Coulter and Parsons note, "we must acknowledge that 'seeing' is akin to an *achievement* and is not any sort of activity, process, or 'undertaking'" (1990, 255). Hence, the empirical work represented by this thesis generally (chapter four as well as chapters five and six) displays an interest in scientific knowledge and understanding as reflected in its investigations of the *activities* that contribute to the *achievement* of scientific knowledge and understanding. The efforts expended to do this research are necessarily and inextricably bound up in knowledge and understandings drawn from science itself. Moreover, given that the activities under investigation are overwhelmingly non-collaborative and it is not immediately obvious as to where the accountability of members' actions may lie for themselves, it has been vital to find some way to ascertain and accordingly utilise the resources that members draw on when undertaking non-collaborative work in these settings.

This ethnomethodological (and Winchian – see chapter two) focus on satisfying the 'unique adequacy requirement' is not done simply for posterity or a sense of justice in representing members' own understandings. In drawing on the same sets of resources as members, ethnomethodologists are able to provide an alternative to SSK and ANT approaches to scientific knowledge, refraining from adopting the same concepts as constructionist programmes and bypassing the need to support those concepts with a radically reflexive methodological scaffolding.

Some Practical Issues in Video-Aided Ethnography

Turning now to some of the practical aspects of video-aided ethnography as a method for investigating non-collaborative work in highly technical settings, it is prudent to work through some concerns pertaining to the method and its applicability to the chosen sites of research. Several relevant issues are discussed, including how the work done towards this thesis has dealt with recording and observing action and interaction, how it has analysed that action and interaction, and how it has transcribed and represented those actions and interactions for the purposes of explanation.

It is important to note from the outset that the particular brand of video-aided ethnography adopted by this thesis is in a significant sense reliant on the technologies used to capture the data. As Schegloff notes of the origins of the related field of conversation analysis in an interview with Čmerjrková and Prevignano:

> There's no question that without tape-recording it [conversation analysis as a field] would not have thrived. It's just improbable that it would have thrived as it did, and taken on the character that it has. (2003, 17-18).

Furthermore, Lindwall suggests that it is a mistake to argue "that the same kind of analysis could have been performed without the video, just not that reliably. Considering the amount of detail presented in analyses, and the iterative, non-linear, and continuous nature of the analytic work, this suggestion is highly improbable" (2008, 61). Hence, it is crucial to conceive of the work as being not just enhanced through the addition of video into existing ethnographic techniques, but *technically enabled* through the integration of video data and ethnographic observation in ways sensitive to the settings at hand. In light of this, it will be important to briefly outline the data collection strategies used to generate materials for the studies presented in chapters four, five and six. As mentioned above, an extensive programme of ethnographic preparation was undertaken prior to beginning any fieldwork. Using this preparation, it was possible to identify appropriate and relevant settings for investigation, and further, more specific, ethnographic preparation was undertaken in these settings – practically, this meant spending time with the postgraduate researchers who had agreed to participate in the research, learning about the scientific aspects of their particular assignments and how they were situated within wider research networks and projects. In this respect, I endeavoured to follow the advice of Heath, Hindmarsh and Luff, who note:

> even when video is the principal source of data analysis, fieldwork is invaluable [...] Fieldwork early on in the process can help you develop a familiarity with the/characteristics of the setting that may be critical in deciding when and how to record, where to position equipment and how to deal with problems that might arise in securing a clear visual image and good quality sound. In addition, for studies of highly complex organisational environments, fieldwork can be essential to enable you to become familiar with the basic features of the setting and the activities involved. (2010, 49-50).

As such, having undergone such a process of familiarisation, I was able not only to decide what aspects of the work were most relevant and interesting in terms of my own research, but how to establish a technical data capture routine to acquire useful and usable video data[28]. Based on information acquired from this familiarisation

[28] For instance, one problem that I was able to surmount given the initial exploratory fieldwork in the electrical engineering setting concerned optimum microphone placement – the postgraduate researcher's own research equipment was known to pick up the frequencies sent and received through my wireless microphone, thus distorting his data – knowing this, the postgraduate researcher and I were able to find a suitable technical solution that ensured that the microphone did not interfere

process, a video data capture routine was designed to capture relevant aspects of the ongoing work of both postgraduate researchers, taking into account Heath, Hindmarsh and Luff's (2010) advice on the three related relevant concepts of 'finding the action', 'avoiding the action' and 'framing the action'. Given the topical focus on non-collaboration, cameras were not set up to capture interaction between people – indeed, under Heath, Hindmarsh and Luff's (2010) conception, no action was to be found there since very little interaction between people occurred and hence that is not where the work of postgraduate science research using computerised research technologies gets done. Rather, a single camera was trained on computer screens from behind researchers as they were using their computers, with the intent of capturing on-screen action as well as bodily gestures such as hand movements when using computer keyboards and mice and head movements and body orientations. Throughout the recordings, I was present (although largely not on camera), sitting with researchers as they undertook their day's work. Although this ensured that the camera and myself did not comprehensively 'avoid the action' (Heath, Hindmarsh and Luff, 2010)[29], it did put me in an ideal position for taking ethnographic notes throughout the video data capture process, as well as being able to ask questions of the postgraduate researchers and participate in discussions when they occurred. In terms of 'framing the action' (Heath, Hindmarsh and Luff, 2010) (literally, what I chose to be 'in shot' when designing and implementing the video data capture routine), for the astrophysics video data, the work was situated solely at the postgraduate researcher's desk and consisted solely of computer work (i.e. programming, analysing images on-screen, reviewing and comparing results, etc.). For the electrical engineering video data, a more flexible video data capture routine was required, since the day's work in this case saw the postgraduate research involved in a multitude of dispersed tasks (i.e. constructing objects from which to take data, collecting data from the objects as placed in an anechoic chamber, processing and reviewing data at a computer, showing results to supervisors in the office, and so on). As such, the video data of the electrical engineering setting was captured in a variety of different ways, including training the camera on the computer work as it occurred, as well as on myself and the postgraduate researcher as we discussed the results and their significance to his project, and even on the data collection of objects and bodies in the anechoic chamber (for which I posed as a 'model' carrying various metal objects to be detected by the setup).

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with his data collection process, but that retained enough sensitivity to pick up a reasonable quality of audio for my own purposes.

[29] The justifications for my presence on camera are explored in more detail later on in the chapter.

In both the astrophysics and electrical engineering settings, the camera was connected to a wireless microphone, which allowed for high-quality sound recordings of talk and ambient noise where they occurred. The camera used was capable of high-definition (HD) recording, which allowed recordings to capture an adequate level of detail to see on-screen action and account for it analytically. The importance of HD in this respect cannot be overstated – it provided the capacity for detailed and precise observation of an array of elements of screen-work, including reading lines of code on-screen, reading command logs that tracked various human inputs with peripheral devices, analysing on-screen data and so on. This would have been lost entirely with a standard resolution of video image. Finally, I endeavoured to maintain content logs throughout the video data capture, noting down tape reference numbers and time stamps of entries (which in line with Jordan and Henderson's recommendations consisted of "a heading that gives identifying information, followed by a very rough summary listing of events as they occur on the tape" (1995, 43)) and general ethnographic observations. Where events were happening in such a way as to make ethnographic note-taking impractical (i.e. too fast to capture fully at the time, or when I was posing for data capture in the electrical engineer's anechoic chamber and consequently unable to write), I also added any further relevant ethnographic information to the content logs as soon as possible post-videoing.

What this type of data allows for is access to the "seen-but-unnoticed features" (Garfinkel, 1964, 229) of action and interaction – things which zip by unremarkably for those involved, but which for outsiders (such as an ethnomethodologist) require a little more investigation to understand and situate in the unfolding order and organisation of the setting. As Lindwall notes of his own work, "repeated viewings of video recordings have [...] been necessary for coming to grips with the seen but unnoticed details of the practices investigated" (2008, 61). Goodwin (2011) concurs that video data, to some extent (see below for a critical reflection on this point), preserves some relevant aspects of the ongoing action for the ethnomethodologist to slow down or focus in on relevant aspects of it, or watch it repeatedly – all of these functions are unavailable without the use of a video recording:

> Videotape records are frequently most useful because of the way in which they preserve limited but crucial aspects of the spatial and environmental features of a setting, the temporal unfolding organization of talk, the visible display of participants' bodies and

> changes in relevant phenomena in the setting as relevant courses of action unfold. (Goodwin, 2011, 179).[30]

Bezemer *et al.* (2011) deal with this issue in their work on communication in surgical theatres, noting that:

> Much of what nurses and surgeons do is instantiated in the subtle and fine grained detail of body movements such as the positioning of a retractor, or a shift in gaze from operative field to scrub nurse. Thus video analysis produces a much richer and nuanced account of communication than what can be captured on-the-spot and in field notes by researchers, or what can be recollected and re-articulated in interviews with the participants after the observed event. (Bezemer *et al.*, 2011, 315).

Hence, whilst it is probably unfair to pit ethnographic observation against video recorded data – the choice of method should always be tailored to suit the particular research setting at hand – video work (supplemented by ethnographic notes) is strongly suited to settings in which technical knowledge and complex skills may overwhelm an ethnographer, even one armed with a good deal of preparatory knowledge.

There are, of course, limits to video-aided ethnography that it is has been useful to be aware of. For instance, Jordan and Henderson (1995) highlight the limits of video technology in providing a comprehensive representation of a setting. Firstly, any video data capture technology will inevitably be limited by a maximum capturing screen resolution, and although the setup used here is of HD quality, it still does not compare to the resolving power of the human eye, and this may have accounted for missed details at any point in the video research. It is hard to say if this is the case or not, or how detrimental to the quality and accuracy of any given piece of research this may, due to the nature of the problem. Moreover, the video technology used has no way of encapsulating the array of sensory resources available in a setting other than sight and sound, and as such, other input such as temperature and smell are lost and cannot be accounted for in the research. However, the effects of these conspicuous absences may be negated somewhat by their relevance to the setting at hand. Certainly in my own video research on astrophysics and electrical engineering, the absence of a reproducible sense of heat perception and smell had no discernible effect on the resulting findings, since they were not vastly important to the postgraduate researchers' or my own understanding of the setting in the first place. Jordan and Henderson (1995) also note that the camera will necessarily have an

[30] Although the examples Goodwin (2011) chooses to demonstrate the video research techniques are all drawn from episodes of collaborative interaction and conversation, see above for the argument that this is not a pre-requisite for video research and can just as easily apply to non-collaborative settings.

audio, visual and temporal perspective that is different from participants, and the camera may 'hear' or 'see' things that participants do not (and vice versa) and do not necessarily record events as they unfold (i.e. recording can be paused). Overall, Jordan and Henderson (1995) make a strong case that video research does in fact 'lose' reality in certain aspects, and still reductively transforms lived experiences into data. But they also suggest that "video loses less, and loses less seriously, than other kinds of data collection" (Jordan and Henderson, 1995, 53), especially when rounded out with supplementary field notes and observations which may go some way towards filling in these gaps.

One other common textbook complaint of participant-based social research is the intrusiveness of the observer into a setting and the effects this may have on the action and interaction that occurs there, and this issue therefore requires an individual treatment. Foundationally, and assuming that research projects display cameras overtly to participants so as to be ethically sound, video-aided ethnographic observation will always intrude into a setting, by virtue of the camera having a physical presence even if the operator remains absent. I endeavoured to make the presence of the camera as overt as possible at the outset, setting up whilst researchers were working, and alerting them to what exactly it was that I would be filming (i.e. showing them what the camera was trained on). At times, participants made references to the fact that they were being filmed, and joked with peers and colleagues about events that the camera was known to be capturing. However, beyond these explicit references, there were few discernible (to me at least) effects on participants' actions and interactions relevant to their work as it was undertaken. Related to this concern, Rooksby (2011) makes a strong argument justifying the use of mocked-up settings, arguing that there is no real requirement for ethnomethodology and video work that it reflect 'authentic' work[31]. Rooksby (2011) questions this idea of the 'authenticity' of a setting – what it might mean and what it is supposed to add to social research – through designing a 'mock' setting of research, whereby two software developers are asked to simulate how they might work collaboratively to design, on paper, a particular piece of software when given a

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[31] This bears a similarity to one of the topics on a lecture course in conversation analysis at which I was present in 2009, wherein Wes Sharrock worked through the idea that given audio data of a phone call between two seemingly 'normal' everyday conversers, how could we tell that they were not Russian spies speaking in code (and what does it matter if they are). The results of this hypothetical problem were that if two Russian spies were expert enough at disguising their code as everyday conversation that we (as everyday conversers as well as conversation analysts) could not tell the difference from their code and from an everyday conversation, then their talk can be treated as an exercise in everyday conversation regardless of the meaning it held for those speaking and understanding it.

few brief requirements by Rooksby himself. What Rooksby (2011) notes is that the fact that he does not want this piece of software to be built and that the participants are fully aware that Rooksby is a social researcher interested in how people collaborate on the initial design aspects of software does not render the resulting video data useless. Indeed, it provides opportunities for Rooksby and the software developers to orient explicitly towards the camera and the research project topically. The software developers are mindful of the fact that some of their actions happen off-screen and even ask Rooksby (as cameraman) if he was able to capture certain actions and whether he would like the developers to go through them again for the benefit of the camera. It is crucial to note that this capacity for topicalising the video-research as a methodological priority is *added* to the mock task of designing a piece of software, and does not *replace* any aspects of it – it's 'authenticity' as a task in software design is irrelevant to how the participants go about undertaking it, and moreover, there are grounds for arguing that it's 'inauthenticity' may in fact make the research more video-ethnographer-friendly. As Jordan and Henderson (1995) note, the issue of the effect of a camera and observer is undoubtedly worthy of acknowledgment, although it is certainly nothing to be viewed with suspicion:

> it might be reasonable to say that the kind and amount of camera interference is something researchers should attempt to assess for each particular project. It should neither be ignored nor considered fatal. (56)

Using these ideas, I worked to acknowledge and, at times, explicitly orient to the camera and my own presence as an intrusion into the postgraduate researchers' working days, enabling me to prompt and react to discussions and explanations where needed, and in ways that did not ultimately harm the validity of resulting data. The aim of the research presented here has not been to capture a day's work as it would happen without intrusion from an ethnographer with a camera – rather, it has been to see, hear and record several aspects of the work as it happens, and throughout there has been no reason to assume that my obvious intrusions into this work have redirected the course of the work itself, despite being *recognised* and *oriented to*. This reflects a facet of ethnomethodology identified by Heath, Hindmarsh and Luff (2010) in which any work being undertaken is made, through its very performance, visibly and/or audibly open to be acknowledged by all. Even when the postgraduate researchers are not explaining their work to me, it is there for the seeing[32]. Hence, rather than impinging upon the supposed 'authenticity' of a task

[32] This is to say that anyone, not just an ethnographer, watching the postgraduate researchers would be able to see the same things as I saw, and indeed, perhaps those same things would have been visible *in principle* had *no-one* been there to

unfettered by ethnographers and cameras, I have found that my presence and the presence of the camera, as with Rooksby (2011) has prompted discussion and explanation where none would otherwise be offered, and these discussions and explanations have provided me with extra materials with which to construct solid re-descriptions of the settings at hand.

The Argument Thus Far as Informative of the Empirical Studies

Chapters one, two and three, although each dealing with distinct topics, are to be taken together as informative of the empirical studies that follow. Chapter one outlines the broad aims and questions that the thesis aims to address, and provides a brief introduction to the possibilities ethnomethodology heralds in terms of understanding technical languages and knowledge, such as that found in scientific (and related fields) research. This is largely due to an adherence to a strong unique adequacy requirement, which ethnomethodologists must use to familiarise themselves sufficiently with the concepts and knowledges that inform members' work in their setting. Chapter two situates this ethnomethodological position against more commonplace (constructionist) sociological approaches to scientific knowledge and activity, positing ethnomethodology (with the unique adequacy requirement in mind) as resolving inherent problems with constructionism that come from its routine decontextualising of settings. Chapter three has built upon this justification by pointing towards the practical concerns of undertaking ethnomethodological research, with particular regard to the use of video recording equipment as a research tool. In this way, chapters four, five and six are implementations of the argument thus far, as applied to two different settings (early-stage research in astrophysics and electrical engineering) and three different thematic concerns (crudely, visualisations, representations and education). Hence, the following three chapters can be taken to serve a dual function. Firstly, they provide substantive findings with regard to how early-stage researchers in astrophysics and electrical engineering do their work (and learn how to do so). Secondly, they demonstrate the possibilities heralded by video-aided ethnography and ethnomethodology as a means of accessing those findings, given previous approaches' neglect of non-collaborative activities and contextual features of settings generally.

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observe them (suspending for the moment any philosophical talk of trees falling in unpopulated forests).

CHAPTER FOUR: Scientific Findings

Though computerised visualisation is a relatively recent topic in the broad field of science and technology studies, for some disciplines, the production and use of visualisations has been routine for some time. The vast majority of work on science and programming that falls under the banner of science and technology studies concentrates on the constructing and constraining elements of social relationships and laboratory cultures on the results of scientific work (see chapter two for a selection of empirical studies with these concerns, from various strands within the broader SSK). Most often, in more recent studies, this is reflected as a distinction made between scientists and programmers and other members of computing project teams, who are depicted as having different sorts of knowledge and different skills which must be brought together (through social and cultural interaction) to successfully solve problems. However, the work presented here is formulated into an argument against imposing this distinction between scientists and programmers, on the grounds that to do so has the effect of overlooking entirely how contextual features of the work itself (including such things as the scientific phenomena being dealt with) shape the work as it happens. Moreover, this overlooking serves to misconstrue the activities that form the work of computer-aided science, thereby distorting the fundamental features of the object of investigation. With this framework in mind, the present chapter aims to look at practices surrounding programming work using visualisations, taking a masters-level project in astrophysics as its subject. Any programming work has inherently visual aspects – programmers have always been able to draw on visual elements of their written code to facilitate their work (see for instance Button and Sharrock, 1995). However, programming work involving visualisations gives programmers access to a new set of visual resources which tie various visual properties of images and on-screen displays to specific work tasks. Here, 'looking for' and 'finding' become important enterprises, and this chapter unpacks some of the practices by which a programmer looks for and finds features of his work, the images and the programme through close analysis of the various activities that generate and construe an adequate representation of the topical astrophysical phenomenon.

The continuing spread of computing throughout social life has already had a significant impact on the natural sciences, and this has even initiated the appearance of a new form of scientific research, computational science, in which the use of computers to either simulate phenomena or to automate the gathering and analysis of scientific data will become an alternative to research and experiments (for social studies of computational programming work in both science and other settings see

Button and Sharrock 1994 1995 1996, Knuuttila 2006, Knuuttila *et al*. 2006, Knuuttila and Boon 2009, Martin and Rooksby 2006, Merz 2006 and Rooksby *et al*. 2006). This chapter aims to look at the computer as a tool in astrophysics, based on video-recordings of a student-researcher trying out a programme that he has written to convert electronic input relayed from an orbital telescope into a set of images to be classified. The student-researcher's task then is to develop and improve (through manual input) the programme's ability to identify 'gravitational lenses' by checking the classificatory performance of his programme against his own identification of gravitational lenses from visual images displayed on-screen. The central focus will be on an assortment of problems that the student-researcher meets in trying to organise manual input into a database in relation to a series of galactic images displayed on the screen and in relation to his understandings of how the programme is working. The student-researcher's problems relate to being able to single out instances of an astronomical phenomenon, gravitational lenses, in on-screen images of areas of space, and using his decisions on how to classify specific images to test the dependability of the output from his automated lens-recognition programme.

This research finds itself broadly situated within a body of literature (science and technology studies) largely dominated by approaches that take as their focus the interactions between scientists as they go about their work, noting how these relationships ultimately construct and constrain the work involved in scientific projects (such studies are discussed more pointedly in chapter two, though Collins, 1985 and Callon, 1986 serve as suitable examples). As computer-aided science has become more apparent in routine scientific work, so it has become increasingly pertinent to social studies of science, which seek to incorporate these new scientific techniques into their existing repertoire (Agar, 2006; Bruun and Sierla, 2008; Hine, 2006; Rall, 2006; Voskuhl, 2004). Such studies present the work of computer-aided scientific projects as comprising of distinct expertises including both practical hands-on skills and distributed sets of conceptual and theoretical knowledge, which are combined and consolidated through collaborative efforts to ensure successful outputs. However, in accounting for the work of computer-aided science in such a way, these studies tend to discuss the work of programming as an entirely separate set of activities to the work of science. Agar for instance claims that historically, "one difference that [the introduction of] computers made to science was deepening the division of labour – and expanding one side of the division, professional computing services" (2006, 900). Similarly, Hine argues that:

> This division of labour [between science/knowledge and computing/programming] is conventional in [the] development of information systems. The database developer is responsible for

> identifying 'user requirements', and is expected to get to know users and find out what their needs are. (2006, 281).

On the 'shop floor', scientific projects and the problem-solving work they involve are depicted by studies like these as unfolding in the shape of a cultural challenge of, amongst other such things, finding ways to facilitate project completion through soliciting the aid of different skills and expertises and managing group work according to members' capabilities and abilities. This is exemplified by the following quotations:

> This particular problem had nothing to do with acoustics or digital-signal processing. Rather, it was a problem that required those mystical skills which enable 'computer wizards' to rescue and manipulate their machines from the most hopeless situations [...] My informants would refer to those who were capable of successfully manipulating computers as being 'wizards' who always knew a 'trick', an obscure command, or another solution to a problem. (Voskuhl, 2004, 405).

> Feynman[33] is everywhere in this story [...] Against the odds, as the problems increased in size and complexity, his team continued to improve [in their ability to provide the calculative power necessary for the project]. (Rall, 2006, 955).

What these two accounts (and those of Agar, 2006, Bruun and Sierla, 2008 and Hine, 2006) work to achieve is a sense of scientific knowledge as distinct and separate from (albeit related to) the practical skills that constitute the project work. Hence, the scientific work is achieved through the bringing together of disparate skills and knowledges into a unified, though distributed, solution. However, what Voskuhl (2004) seems to neglect is that the mystical skills of 'computer wizards' are not mere tricks of programming, but are in fact the work of *doing* acoustics and/or digital-signal processing with computers (which was the ultimate purpose of the work Voskuhl purports to have observed so closely). Similarly, if Feynman is everywhere in Rall's (2006) story, it is a version of Feynman as a manager of human computing team that does not in any way refer to our more familiar notion of Feynman as a physicist engaging with scientific knowledge (through computing). Though the distinction is seemingly innocuous, it is nonetheless important. These two Feynmans seem to exist in a quantum state of sorts – they both occupy the same positions in space and time, related, but (as Rall (2006) implicitly implies) *they are not the*

[33] The Feynman under discussion here is noted physicist Richard Feynman, and Rall (2006) investigates his work as the manager of a computing team on the project of building the atomic bomb, which first comprised of a) untrained scientists' wives, then b) computer-trained WACs (Women's Army Corps) and finally c) soldiers with computer training and full knowledge of the project objectives. Here, Rall tracks the make-up of this developing computing team against its effectiveness as a problem-solving unit.

same[34]. Hence, the research presented in this chapter takes the position that accounts such as these cannot be said to be discussing scientific work. In characterising scientific work as a purely social and cultural phenomenon and discussing only those features relating to how different elements of problem-solving teams work (or do not work) together, all sense of the context characterising the project as specifically scientific is lost. In a significant sense, the way that scientific problems and work are accounted for in these studies makes them interchangeable with any other types of problem or work[35].The focus is squarely on how various skills and expertises are brought together, and what those skills or expertises may consist of or look like as they are put to use in problem-solving work is typically (although not always – see the list of references at the top of page 57 for works that demonstrate alternatives to this constructionist style of approach) left untouched. Indeed, this is even recognised by such studies themselves:

> Recordings of real-time actions and interactions of the project members would have contributed to an in-depth understanding of the circumstances through which knowledge networking solutions were produced. This could have been accomplished through video-recording, but many of the interactions, decisions and deliberations in research projects were difficult to capture in real time, even with a video camera, because they were not fixed in time and space…What is more, in software development much of the crucial interaction occurs when engineers browse, study, modify and integrate artefacts that have been developed by colleagues. These activities dominate the experience of most software engineers and constrain many of their decisions, but there is little overt, bodily behaviour to be observed: only mouse and keyboard use. (Bruun and Sierla, 2008, 140).

Here, Bruun and Sierla make two complaints: firstly, that people won't stand still long enough for their interactions to be videoed (this issue is addressed in the previous chapter), and secondly, that what *does* take place in a static setting –

[34] They are not the same in that they do not do the same things, they do not use the same technical languages, they do not talk to the same people, they do not draw on the same fields of knowledge to achieve their work, and so on. Rall's (2006) Feynman is taken to be nothing more than a project manager of sorts, and this idea of Feynman does, in no way, necessarily rely on our more familiar notion of Feynman the brilliant physicist.

[35] In other words, if computer-aided scientific work was merely a product of social relationships and the bringing together of distributed knowledges and skills, then what is to distinguish this type of work from, say, work designed to solicit and process insurance policies and claims for customers, or even the work that goes into completing quests in an MMO (Massively Multiplayer Online) game with friends? The question then is that if the content and context of those problems encountered in each setting is left out at the expense of highlighting only social and cultural factors in the construction of problems-to-be-solved and their solutions, how are we to know exactly *which* setting we are talking about?

mouse and keyboard use – is not of any interest to a social study of science. However, it is precisely this arena of little overt bodily behaviour in which the work of programming-for-a-scientific-project takes place, and to ignore the array of activities that go on in this arena is to ignore the scene as an endeavour in science. Hence, this chapter presents a student-researcher's work on an astrophysics project involving the use of a programming language as a means of re-aligning previously alien ideas of programming and science. The aim here is to re-integrate the activities of programming and science where they are already combined, through discussing various features of that work with particular regard to those features pertaining to visual aspects of the programming. This is achieved by adopting a strategy of *not* resolving only to look at what may well be auxiliary issues to members' endeavours in this regard (i.e. the social and cultural work of science), thereby avoiding abstracting them away from each other prior to investigation.

The attention to the visual features of computational work reflects a growing interest in how visualisations are used and engaged with in such settings (Amann and Knorr Cetina (1990), Burri and Dumit (2008), Carusi *et al*. (2010), Lynch 2011, Mößner 2011, Ribes 2011), and it has been suggested that such visual work might even constitute a new 'black box' for social research that might only be opened with new approaches that deal with visualisation-based science on its own terms (Woolgar, 2011). This chapter attempts to take one such approach, grounded in Coulter and Parsons' claim that "'seeing' is akin to an *achievement* and is not any sort of activity, process, or undertaking" (1990, 255). It is with this in mind that this chapter explores the student-researcher's focus on the visual aspects of his work with regard to the activities of 'looking for' and 'finding' (or 'not finding'), as part of a set of activities that contribute towards a final achieved 'seeing', which occur and re-occur as prominent features throughout.

Background to the Study

A basic account of the method the researcher in question (HR) used to identify gravitational lenses was to find peaks of radiation emission relating to each object in each of the images of his 2148-strong dataset (which consisted of 537 possible lensing events, each of which having 4 images describing a different electromagnetic radiation profile). This information was then used to ascertain if there was a visible (to HR) distortion of the radiation emitted by each of the objects and from that make the decision as to whether the image represented a gravitational lens or not[36]. The

[36] A gravitational lens is a phenomena whereby light and other electromagnetic radiation (ultraviolet rays, radio waves, optical range wavelengths, x-rays, etc.) is

video data under consideration in this chapter captured HR working with and developing a basic programme he had already written to do this task with only 80% accuracy. To improve this accuracy to which the programme could identify lenses and non-lenses, HR worked on manually inputting information about the coordinates of the two radiation peaks on an image so that the computer would know approximately where to look (and crucially, where not to look) to find the peaks. The reasoning for this is that some images may contain anomalies which confuse the computer's ability to make a decision, so if the programme is told which of the two peaks are relevant (and to ignore all others), then it should be able to make more definite decisions about whether images represent lenses or not.

Having chosen how to go about improving his programme, HR wrote a piece of code to allow him to look at each of the 2148 images in turn and record the coordinates of where the peaks are on the image – this is the element of his work captured on video. The process can be boiled down to the following (ideal) steps: he looks at the image to see if the position of the peaks is obvious (as is the case in figure 1, in which there are two clear peaks with a clear lensing interaction between them). For more ambiguous cases, HR can use other images of the same system in other wavelengths to cross-check those against the image being worked on (see figure 2 for various relevant features of HR's on-screen work). Then, having determined where the peaks are 'by eye', HR can record the location of the first peak by clicking on it with the left mouse button, then do the same for the second peak with the [right mouse button], then keystroke n to move on to the next image and repeat the process. Various elements of 'looking for' and 'finding' activities come to bear on HR's work, and it is possible to see these repeated and sustained practices in the video data of the day's work, which serve to highlight some of the ways in which HR's work is characterised by a reliance on visual resources. It is these elements that this chapter hopes to go some way towards unpacking, through an exploration of such features as: making code visual; highlighting for visibility; finding through looking; finding visual utility in images; arranging for comparison, and; visual diagnostics.

'bent' by the gravity of another high-mass object nearer to us in our line of sight. Therefore, a lensing system can be identified by the presence of an interconnected distortion between the radiation that each object emits, and a non-lens can be identified by the absence of this feature.

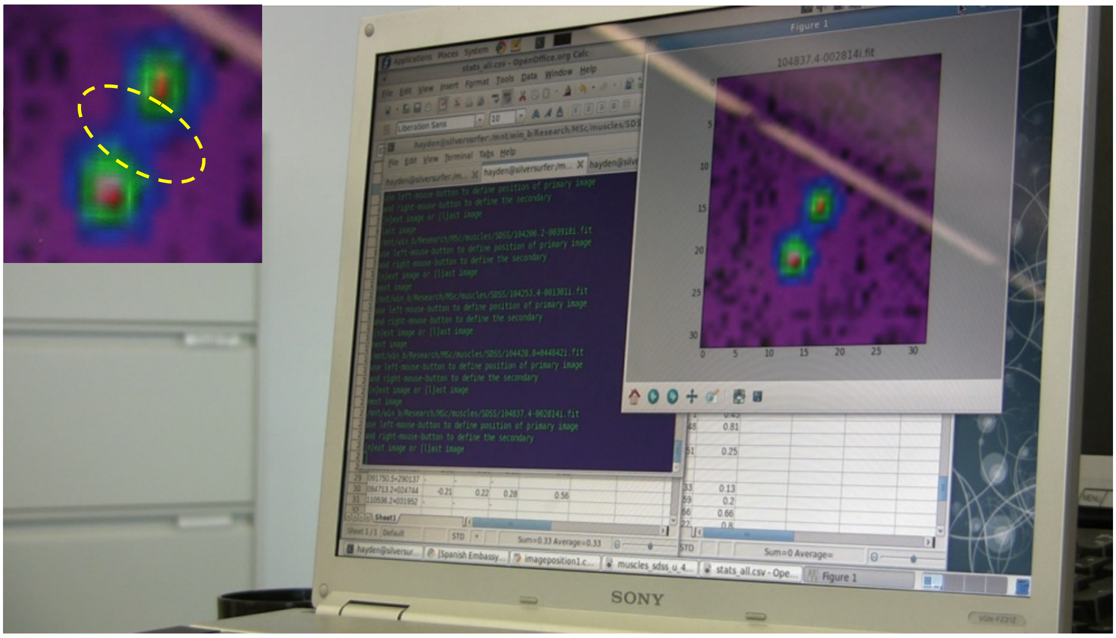


Figure 1 - A 'good' lens with a clear lensing interaction (highlighted)

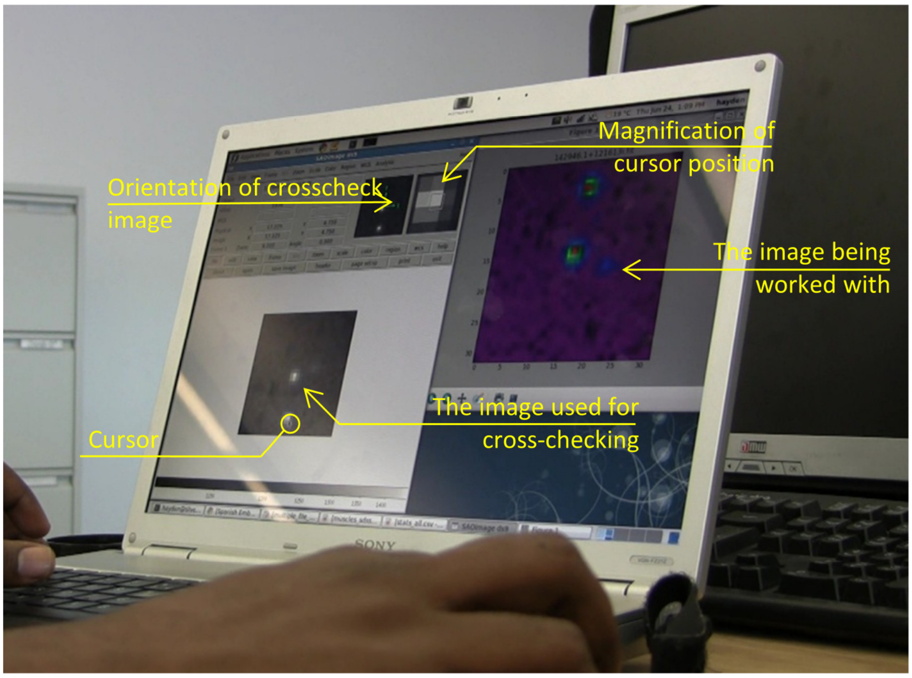


Figure 2 - Cross-checking in another wavelength, with various relevant features of SC's on-screen setup highlighted

Making Code Visual

Code is scripted text which serves as a list of operations (and the instruction to run them) collated under the larger structure of a programme, and is written in a dedicated programming language (i.e. a software package for mathematical and computational processing) which a computer can understand and act out. However, it is also vital that *programmers* can read and understand code if they are to write programmes that do what their work requires, and as Davis and Hersh note of the work of mathematics (which has a direct relationship to the work of programming in a number of ways):

> The layman might get the idea that a skilful mathematician can sight-read a page of mathematics in the way that Liszt sight-read a page of difficult piano music. This is rarely the case. The absorption of a page of mathematics on the part of the professional is often a slow, tedious, and painstaking process. (1981, 281)

Familiarity and skill with a programming language is often essential to being able to absorb and understand the vast amount of code that might make up a programme, but as Button and Sharrock note, "Part of this is to visually organize the code so as to make explicit the way in which the programme will process information and display the reasons for processing information in a particular way" (1995, 234). As such, one method by which programmers can access an understanding of their code is through use of comments. Comments, by their nature, never form a functioning part of the programme. This is to say that they don't *do* anything – their absence or presence has no effect on the programme. However, their use in programming is common, and not only for reasons related to collaborative work, where documentation such as comments act as guides for future users. Button and Sharrock suggest that to some programmers such documentation is the 'dirty work' of programming, and that programmers see documenting for the benefit of others as "something of a clerical task" (1996, 381) that is not usually required in the 'real' task of getting a programme to work. By contrast, HR *uses* comments in a variety of ways to facilitate *his own* navigation through and working with his programme. For instance, HR divides his code into separate sections, delineating at what point one specific coding task becomes another by making a border of blue[37] commented hash marks at the start and end of each section. Practically, this means that HR can easily search for and find specific sections of the programme, relying on visual clues like these which provide the resources to ascertain where specific coding tasks begin and end. HR also uses comments as labels for distinct coding tasks – as visual tags that make the subsequent code more understandable. For instance, HR has a section of code that appears as follows:

[37] Comments in the programming language HR is using – Python – appear in blue, which further visually distinguishes them against other code.

```
BEGINNING OF PARSELTONGUE[38] SCRIPT
```

This comment serves to mark out the following code as something other than other typical Python language – since ParselTongue is different to Python (and HR has only a limited familiarity with either) it is useful for HR to have a reminder to read the following code in ParselTongue and not Python; the comment is a label to aid the understanding of the section of code that follows it. However, comments are not just labels for code, and can also *situate* code as part of a wider process. For instance, HR leaves the following comment:

```
#now mask out a few pixels around this peak position, to detect
the second peak
```

As Button and Sharrock note, the visual organisation of a programme can serve "*as an account of the computational organization of the program*" (1995, 248), and comments such as this help HR to navigate through the master code screen, in that they give some indication as to where in this screen HR must be if *this* is the section of code he's currently looking at. The comment above, by implication, relates to a section of code that *must*[39] *be after* (for instance) the section that deals with finding the first peak on an image. As such, if HR was to find a need to search for the specific code dealing with finding the first peak, the comment is a resource for ascertaining whether HR has to look before or after (and also, *how far* before or after) the section of code currently on screen. In this way, comments are navigational devices; signposts that point programmers in the right direction, helping them to find they're looking for against an otherwise undifferentiated background of barely comprehensible script.

[38] ParselTongue is a scripting interface designed for allowing Python to do complicated data reduction (i.e. turning long strings of numerical information into images) with techniques from another software package (Astronomical Image Processing System, or AIPS) (Kettenis *et al* 2005).

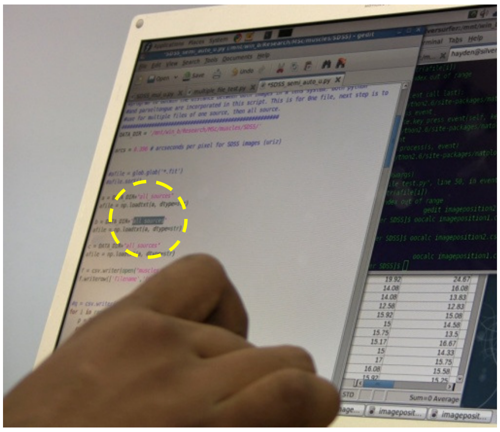
[39] To use the word 'must' is not, of course, to say that HR could not have constructed the code differently (say, arranging the code such that the second peak was detected first and the first peak detected second). Certainly, shifting sections of code and their associated comments around would do nothing to change the ultimate output, so in a sense it would not matter where in the program these sections of code were situated. However, HR ascribes a sequentiality to these sections, and one reason as to why this ascribing is done is so as to create an organisation in the code that HR (and anyone else) can read, understand and even intuit. Hence, the first peak is detected first, and the second peak is detected second.

Highlighting for Visibility

Another feature of HR's work is that it is not contained in just one display on-screen. HR's work relies on the successful integration of information from a variety of sources (i.e. his database of manually inputted peak coordinates, file directories of images, the master code screen, and more). As such, part of HR's work involves adopting various practices to facilitate the transition between screens, including creating a temporary visibility arrangement through highlighting. In one instance, HR is working on editing and adding to a variable in the master code so as to integrate his new database of peak coordinates into it (effectively, he is telling the computer not to look at the raw images, but to use the coordinate information in the database to direct where it focuses with regard to the two peaks). This editing involves making two copies of the variable below:

```
a = DATA_DIR+'all_sources'
afile = np.loadtxt(a, dtype=str)
```

This copying of variables reflects a known feature of programming work – Martin and Rooksby note that there is "a propensity towards re-use and economy in finding solutions rather than working out a solution from scratch" (2006, 8) – and HR goes on to edit the copied versions of this variable so as to change their variable names and associated data (from 'a' to 'b' and 'a' to 'c', from 'afile' to 'bfile' and 'afile' to 'cfile' etc.). But perhaps most crucially, the `'all_sources'` script must be changed to reflect where HR wants these variables to pull his manual input data from. To do this, HR must check the filenames of these databases, which involves navigating away from the master code screen temporarily to the database itself (which features the filename in its title bar). However, prior to moving screens, HR highlights the `'all_sources'` script in the new variable 'b' – whilst holding the left mouse button down, he drags the cursor over this piece of code, with the effect of making it stand out against the background of the other code on-screen. HR then goes to the database to retrieve the filename and upon his return to the master code screen, is able to use the highlight as a guide to reorient himself quickly and easily to the section of code that this filename should replace – the `'all_sources'` script in variable 'b' is changed to `'imageposition1'`, and variable 'c' is changed to `'imageposition2'` accordingly (see figure 3 below for an image of HR doing the highlighting work, and a representation of the section of code after editing). Here, highlighting is used as a quick, easy and temporary marker which "acts as a place holder in the unfolding organization of the code. It can hold the structure in place as it is forming" (Button and Sharrock, 1995, 242). Techniques such as highlighting for visibility are non-intrusive to the development of the programme (in that they do not change the instructions themselves) but can nevertheless provide enough of a visual emphasis on the script-to-be-changed to make it more 'findable' and hence easily editable.



```
a = DATA_DIR+'all_sources'
afile = np.loadtxt(a, dtype=str)

b = DATA_DIR+'imageposition1'
bfile = np.loadtxt(b, dtype=str)

c = DATA_DIR+'imageposition2'
cfile = np.loadtxt(c, dtype=str)
```

Figure 3 - Highlighting 'all sources', plus the finished edit of the section of code under development

How to Find Through Looking

Clearly, recoverability is a key issue for HR – he has to be able to find a number of things including specific images, various databases (and particular information within them), filenames, sections of code, and so on. Often, the location of the thing HR is looking for at any given point is not (and cannot practically be) defined exactly and the best possible direction can only be phrased as 'somewhere within this database' or 'somewhere in this set of images'. Various practices of 'looking for' items such as these can be found in HR's work, and these practices draw on lots of resources available within the structure of the working process both inside and aside from the programme. As Martin and Rooksby note of programmers working with code, "knowledge of the code base is knowledge of your way round it, how things might be connected and what the implications of changing a piece of code may be" (2006, 8), and this applies to HR's visualisation work in a variety of ways. For some sought after items, finding them can be as simple as entering a filename (or the first few letters or numbers of a filename) into a 'find' form – in one instance, HR is looking for a specific image file in his database of peak coordinates, and, having the filename of the sought after image on-screen, he can copy the first few numbers of the filename into the 'find' form, keystroke [Enter], and the computer skips through the database directly to the desired file (see figure 4 below).

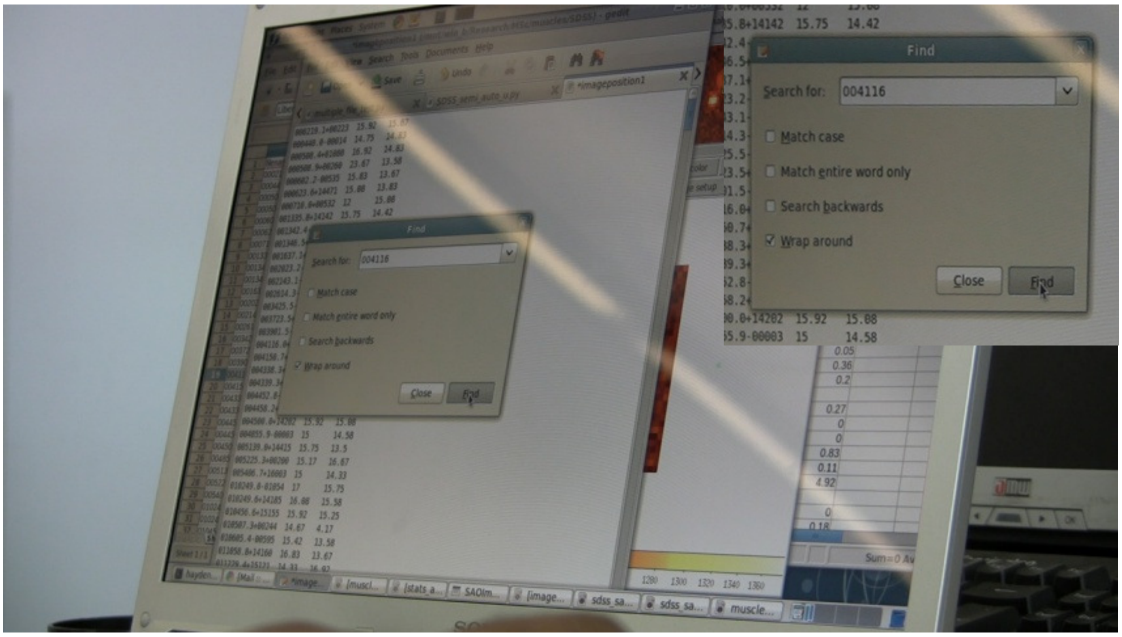


Figure 4 - A 'find' menu

However, there are also cases where a simple solution like this is unavailable and HR cannot rely on a simple ‘call-and-response’ communication with the computer – as Suchman notes, “The problem is not simply that communicative troubles arise that do not arise in human communication, but rather that when the inevitable troubles do arise, there are not the same resources available for their detection and repair” (1994, 185). In these cases, HR has to rely on other (visual) resources to solve problems, and one such case occurs when HR has made a mistake with where he has clicked on an image (image 1) and only realised he had done so after having gone on to the next image (image2) (see the storyboard of this series of events in figure 5 below[40]). At this point, HR has to find some way to go back and re-examine image 1, delete the information pertaining to where he mistakenly clicked, then re-process the image and move on. He does this by temporarily stepping out of the confines of the manual input/image processing work to recall the image. Here, HR is now working outside of the programme and has to call up images using the master code screen. Effectively, he has to start the manual input programme again, but can choose at which point in the sequence of images to do so: if the value of the variable ‘i’ is changed to 309 (as it is in the video), then the programme will call up the three hundred and ninth image in that set. So, HR chooses a value of ‘i’ that he thinks relates to image1 (`i = 309`), only to find that the image that this value of ‘i’ brings up

[40] Readers may find it useful to pay attention to the inset images, which display the particular astronomical objects HR has available on-screen as the events unfold temporally.

is *not* the one he was searching for. Here, HR has to draw on other resources to ascertain the value of 'i' for the specific image he *does* want; chiefly, the fact that he has already seen this newly-recalled unwanted image currently on screen and can use those recognisability of its visual properties to work out its relative position in the sequence. The image on-screen at this point was recognisable as the one *after* the image he needs to redo – he can see image2, but he wants to be able to see image1 – and as such, HR can infer that the value of 'i' he actually requires to continue with his work is *one less* than 309 (so `i = 308`). Here, HR has to draw on visual properties of the images on-screen (i.e. does it look like the one he wants? If not, does it look like one he recognises? If so, can he pinpoint where in the sequence this unwanted image is and thereby infer the relative position of the image he *does* want?) to tie specific images to their specific points in the process. As Goodwin notes, "visual phenomena become meaningful through the way in which they help elaborate, and are elaborated by, a range of other semiotic fields" (2001, 179) such as sequential organisation, and by relying on various findable visual properties of the things he is looking for, HR is able to draw on a set of resources that makes his working with visualisations more manageable and possible.

Figure 5 - Storyboard of events

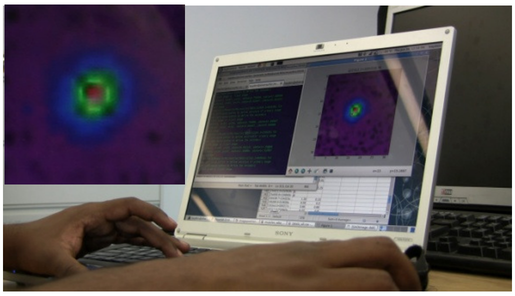


Figure 5a – HR clicks in the wrong place on image 1

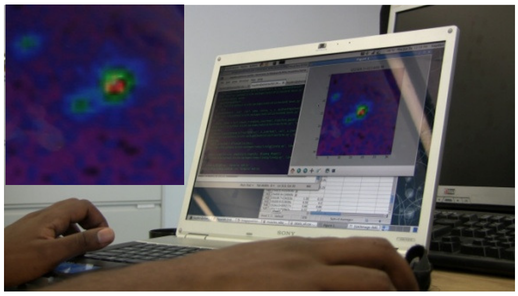


Figure 5b – Before he realises the mistake, he has moved on to image 2

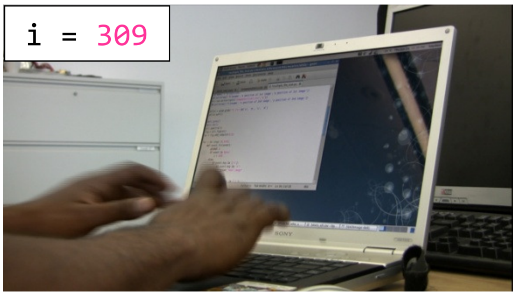


Figure 5c – He changes the value of 'i'

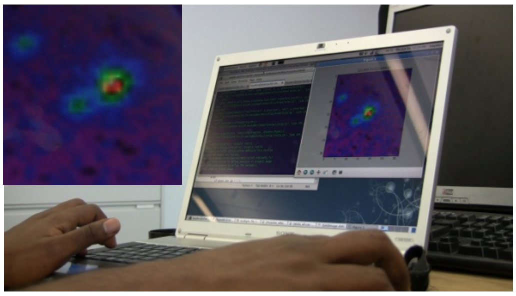


Figure 5d – 'i' = 309 cues up image 2; the image *after* the one he wants

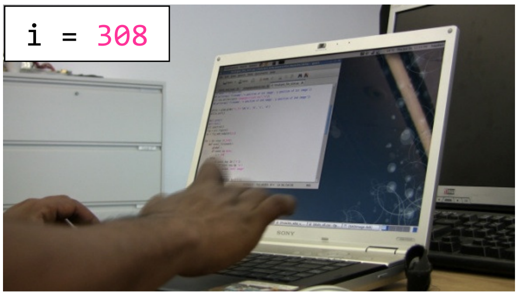


Figure 5e – HR edits 'i' to cue up the image *before* i = 309...

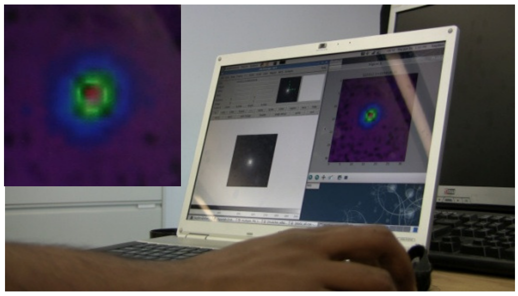


Figure 5f – ...and this brings him back to image 1, which HR can then analyse and process properly

Finding Visual Utility in Images

Ultimately, HR's programme is hoped to have the capability of distinguishing between gravitational lensing systems and other non-lens objects, given an input of images of those objects in one or more wavelengths. As it is, at this point in HR's work the programme is in the process of being developed and its capacity to do this is therefore in question. As Lynch notes of his own work on biology lab science, 'artifacts' – "moments in the work, where the ordinary transitivity of practices was found to be problematic" (1980, 111) – "were not collected and analyzed in lab research, but "fell out" as occasioned troubles in "visibility" or "interpretation"" (1980, 120). However, for HR, the possibility of artifacts is is more expected given the uncertainty around the programme's ability to perform classifications. HR is mindful of such artifacts appearing in his results as questions-that-have-yet-to-be-addressed – are the images the programme identifies as lenses *actually* lenses? Are the other objects it identifies as non-lenses *actually* non-lenses? Are the images for which the computer produces a 'je' error[41] *actually* ambiguous? All of these questions are only answerable upon the production of a set of results, and to answer the question of whether or not the results the programme produces are (likely to be) accurate, HR has to be able to classify the images himself. This allows him to match results to images and make an informed decision about how well the programme is able to perform, which is something the programme cannot yet do itself. In one instance, HR comes across a 'nice' image (see figure 6 below) during his manual input work which he picks out because of an interesting feature that is clearly visible on it – a galactic arm[42]. This feature is interesting to HR for a number of reasons,

[41] A 'je' error in HR's program was an entry in the results that signified that the program was, for whatever reason, unable to make a decision as to whether the image in question is or is not a lens – most likely the program has identified significant evidence for both instances (i.e. the image is a lens, the image is a non-lens) and can't thereby reject either of these outcomes.
[42] The 'objects' in lensing systems are often galaxies. Though there are different types of galaxy, spiral galaxies (such as our own Milky Way) are comprised of a

chief amongst which is that it is rare to see something so well defined on one of these images, which makes it of general interest astronomically. However, the presence of this feature is also of interest to the current programming work, in that it stands as a strong indicator that the image does in fact show a gravitational lens (because at least one of the primary objects is very likely to be a galaxy, which is the case for a good deal of positively identified lensing systems), and would therefore be useful in terms of checking against the result the programme produced for the image. As HR himself explains in this instance:

> "This looks kinda cool, I think this is a gravitational lens and is a-, this one looks very close to the...to the...so you- you tend only to have one bright lens: another one and this [the secondary object] one looks close to the galaxy cos you can see some sort of galactic arm. So, that might be nice to see what's gonna happen."

For HR, images like this, where there are some criteria for judging the imaged object to be a 'strong' or non-lens, are very useful in terms of getting the programme to work. Goodwin notes that it is particularly important to attend to "the contextually based practices of the participants who are assembling and using [...] images to accomplish the work that defines their profession" (2001, 163). With this in mind, it is clear that being able to spot these 'strong' images as they come up becomes a key element of the work of programming for HR, since this allows him to capitalise on his ability to make scientifically-informed visual classifications of single images, which when combined with the programme's capacity to process lots of images in a short space of time (and with quantified statistical information that indicates how accurate it judges its results to be) provide adequate resources for further improving the programme. Here, what HR can spot at a glance is what his programme has (as yet) no ability to 'see', and HR uses this asymmetry in his and the programmes' capabilities as a means of improving his semi-automated technique of classifying lenses and non-lenses.

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central concentrated 'bulge' of stars and a flat rotating disc of stars, dust and gas. This disc features long thin 'arms' of stars, which appear like a spiral due to the property of their rotation, and it is to one such arm HR is referring here.

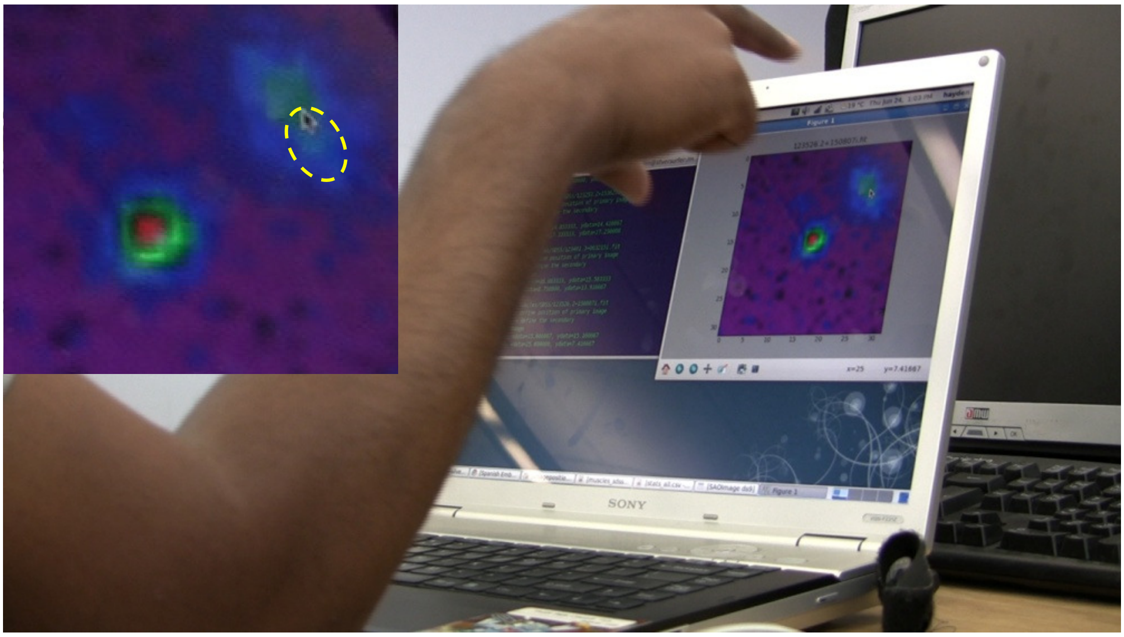

Figure 6 - A 'nice' image featuring a galaxy with visible arm (highlighted)

Arranging for Comparison

For HR, this day's work is an attempt to improve the programme's ability to classify lenses and non-lenses (in other words, to reduce the amount of 'je' errors in the results, which currently occurs for around 20% of cases). Consequently, one question that HR needs to address is if this day's work is contributing to this objective or not, and finding a way of checking this becomes an issue for HR. In one instance, HR compares the results produced by the two different versions of the programme: version 1 (the original programme, which takes basic data from all images) and version 2 (the 'new' programme, which integrates information about the peak coordinates defined by HR through his manual input). This is intended to reveal more about what is happening in the new version of the programme, and HR makes use of the fact that both versions of results have a fundamental comparability – there are entries for each of the same individual images in both versions. Amann and Knorr Cetina note that, "Analyzability is not just imposed upon the visual record by labelling and other techniques. Rather, it is *built into* the record from the beginning through the way the experiment is designed" (1990, 107). In much the same way, HR has designed the day's task such that he can correlate the two results (from version 1 and version 2) for single images and use the difference in results to judge whether the new programme is better, worse or similar in terms of its ability to classify lenses and non-lenses.

However, to amplify this comparability and make it more visually apparent (and therefore more practically achievable), HR arranges the two results screens side-by-side on the computer desktop, such that the results for individual images are broadly on a level plan (see figure 7). With this configuration of the two versions' results on-screen, HR makes an at-a-glance comparison of the first few cases – so far, the results look as expected, in that there appear to be less 'je' errors in version 2 than in version 1. Hence, it appears that the programme can now classify more images than it could before, which was of course the very purpose of the manual input work. However, looking more closely, HR begins to compare individual cases from both versions' results, accenting these cases by clicking on cells within the row (which has the effect of drawing attention to individual lines on each display so as to enable an easy shifting of gaze between them). Here, HR highlights the cells in case three in version 1, then the cells in case three in version 2, and this allows him to see that for this case, version 2 produces a 'je' error whereas version 1 produces a valid result. It is this fact that prompts HR to pick out case three specifically in the first place – he is looking to see and compare what happened in version 1 for cases where the newer version of the programme can now no longer make a classification. This finding has worrying implications, chiefly that the programme's capability to make a decision should have been *improved*, across the board, and the fact that it has worsened in a select few cases is a possible cause for concern. HR goes through some more case-by-case comparisons for cases in version 2 resulting in a 'je' error, and finds that this is not just a one-off anomaly, but occurs in a number of cases. HR eventually attends to case nineteen (see the magnified inset section of figure 7) and explains:

> "[The programme] gives me one [a 'je' error] here- oof! Thissa bad one. This is bad... I'll just have to go through the data to...it seems that it's not as ideal as I thought."

Here, because case nineteen has a particularly strong numerical result in version 1, the presence of a 'je' error in version 2 has a stronger resonance for HR's work, and this instigates a diagnostic approach as to why this problem is occurring (see section on visual diagnostics below). As Lynch notes of his biology lab researchers, when their experiments failed to work, questions remained: ""Did we do it correctly? Is there anything we could have done that would have made it work?" Such questions arise in the absence of a possible authoritative resolution by means of comparisons to a standard" (1980, 160). HR however can make such questions answerable, in that he *does* have a standard (of sorts) to compare his new results against, although perhaps it is not an *authoritative* standard – he is able to use an earlier version of results as a *sub*-standard (the comparative criteria being that the old results should be worse than the new). From looking at how this comparison is made, it is clear that there is a marked difference between what HR can see at first glance (i.e. that version 2 *is* an improvement) and what can be seen on closer inspection (i.e. that that improvement has some concerning caveats which call for further investigation). Through visually arranging the two sets of results for comparison HR allows himself both a broad at-a-glance comparison between a large set of results, and sets the stage for a more detailed a more revealing case-by-case comparison, both of which are required for the positive development of the project[43].

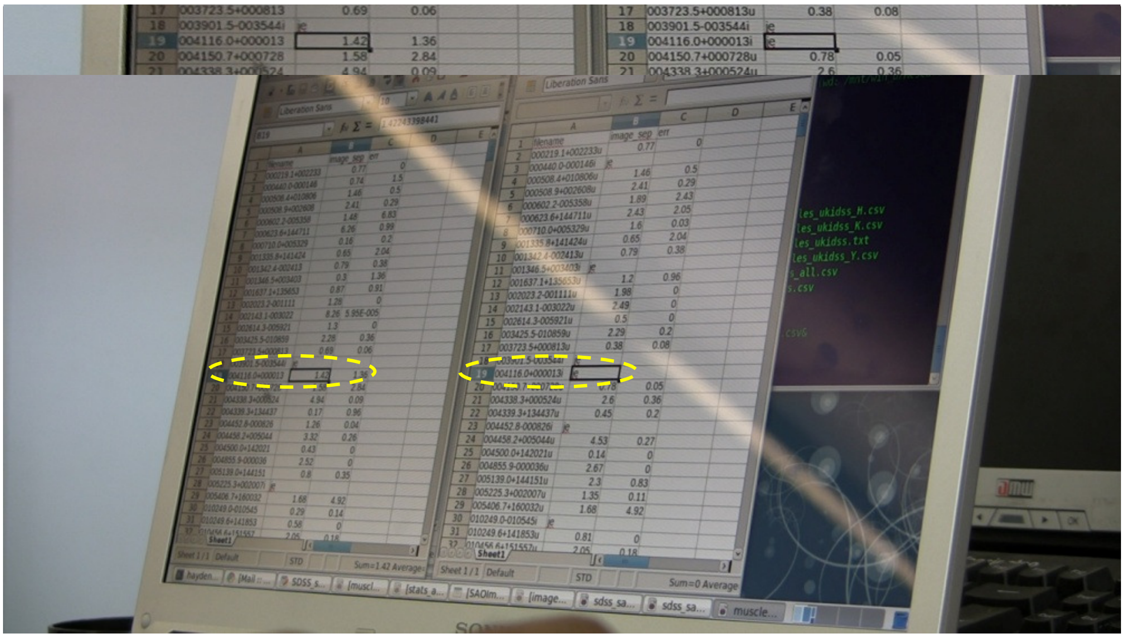

Figure 7 - Comparing results side-by-side, with case nineteen highlighted in each set

Visual Diagnostics

It should be no surprise that, as with any other endeavour, programming work involving visualisations often features problems, and diagnostic work must be performed to look for, locate and solve them. Complex problems might even 'hide' errors from view, and programmers might have to rely on a variety of diagnostic techniques to come to a solution. Working with visualisations, HR is able to use visual resources to diagnose and work on certain problems, and this comes to bear on how HR works to find a reason as to why his newest version of the programme is

[43] Although these results look bad after close comparison, this is not an unrecoverable disaster for HR – it certainly is an upset that means his programmed technique for finding lenses and non-lenses is not working *yet*. However, it also points to a need for further development and improvement, without which the project would be incomplete, in that it could not be said to achieve the set objectives.

producing 'je' errors where there were no errors in the original untreated results. As outlined in the previous section ('*Arranging for Comparison*', page 71), whilst checking results case-by-case, HR notices that case nineteen is problematic in exactly this way, giving a 'je' error in version 2 of the programme, but a valid result in version 1. However, the question remains as to why this should be, and which version of the programme has made the correct call – perhaps programme 2 is *right* to call image nineteen a 'je' error if the object is genuinely ambiguous (i.e. that it is quite simply difficult to tell whether it is or is not gravitational lens)? Or perhaps, as the weight of evidence of unexpected 'je' errors in version 2's results suggests, the programme is somehow not using HR's manual input as he would like it to? The uncertainty necessitates a diagnostic approach to the results and programme, and to resolve this problem HR calls up the original image for case nineteen (see figure 8 below) in an attempt to classify it with his own visual judgment. As Knuuttila notes of particular types of programmes used in syntactic analysis called 'parsers':

> above all, the parser must function well, which means that a parser must be able to carry out some of the tasks (i.e. syntactic analysis) that humans can. To do this, parsers do not necessarily have to be 'psychologically realistic,' and it is highly probably that they will not be so. (2006, 47).

Here, HR is attempting to ensure that his programme functions well by pitting his own abilities against the 'psychologically unrealistic' programme's, and from a quick visual analysis of the image, HR can see that the image for case nineteen looks to be a great example of a gravitational lens. From his new evidence, HR achieves the conclusion that version 2 must be mistaken in its classifying of image nineteen as 'unclassifiable', and therefore it is something in the programme that is at fault (and not the image or the lens itself). As HR notes at this point:

> "This is weird; this is a really good lens! It gave me an error on something that supposed to be, well, perfectly fine. Oh boy. This is not going to be good."

This is a significant problem for HR's project, and HR must work to understand *why* it is not able to classify certain lenses that he can easily classify himself. As Lynch notes of his biology lab researchers, for them, "the most interesting (and problematic) artifacts were not definite "things," but were "possibilities" [...] As possibilities they were not, as yet, specific features of any microscopic scene, but were tied to readings of the scene" (1980, 114). This is exactly how HR uses visual clues to diagnose problems – he infers from various visual properties of what can be seen on-screen the possibilities of *what might be happening*. As it stands, the next obvious possibility as to what might be happening (given that any coding errors could be discounted on the grounds that the programme was patently able to produce a table of results, which suggests that it was integrating the peak database information sufficiently) is that maybe HR's manual input – his clicking on the two peaks in each image – was to blame in some way. HR opens the two databases of his peak coordinates (*x* and *y* coordinates of where he clicked on the primary peak, and *x* and *y* coordinates of where he clicked on the secondary peak) to ascertain exactly where on the image he had clicked previously. This information can then be compared against the image itself, since the particular screen in use features a cursor magnification function that allows HR to more closely inspect the area around the cursor and thus locate the peaks more precisely (see figure 2 above and figure 8 below). Comparing where he previously did click on the image against where he would now having taken more time and attempted the task with more precision in identifying the peaks, HR finds that his original clicking was not accurate enough: the coordinates recorded in the database are quite some distance from the coordinates of the peaks as they appear under the magnified cursor. Therefore, HR draws the conclusion that his original manual input was simply not accurate enough and will need to be re-done if it is to be of any use in terms of improving the programme's capacity to classify lenses and non-lenses. Retrospectively, HR's accuracy inadequacy is comparable with Suchman's concept of a 'garden path result', whereby during the course of his manual input work, HR:

> takes an action that is in some way faulted, which nonetheless satisfies the requirements of the design under a different but compatible interpretation [i.e. that two clicks have been made, regardless of their accuracy]. As a result, the faulty action goes by unnoticed at the point where it occurs. At the point where the trouble is discovered by the user [or programmer], its source is difficult or impossible to reconstruct. (1994, 170).

Here, however, HR *is* ultimately able to diagnose and work towards reconstructing the source of the trouble and finds the problem and its solution through *looking more closely* at that which he (as he understands it now) had rushed through carelessly before. This endeavour finds HR checking if the programme can produce what he himself can identify visually, and finds the issue is one of his own precision placement in a visual field; his accuracy with the manual input, which confuses the programme's inability to decide what is, what isn't and what it hasn't got enough information to call a gravitational lens.

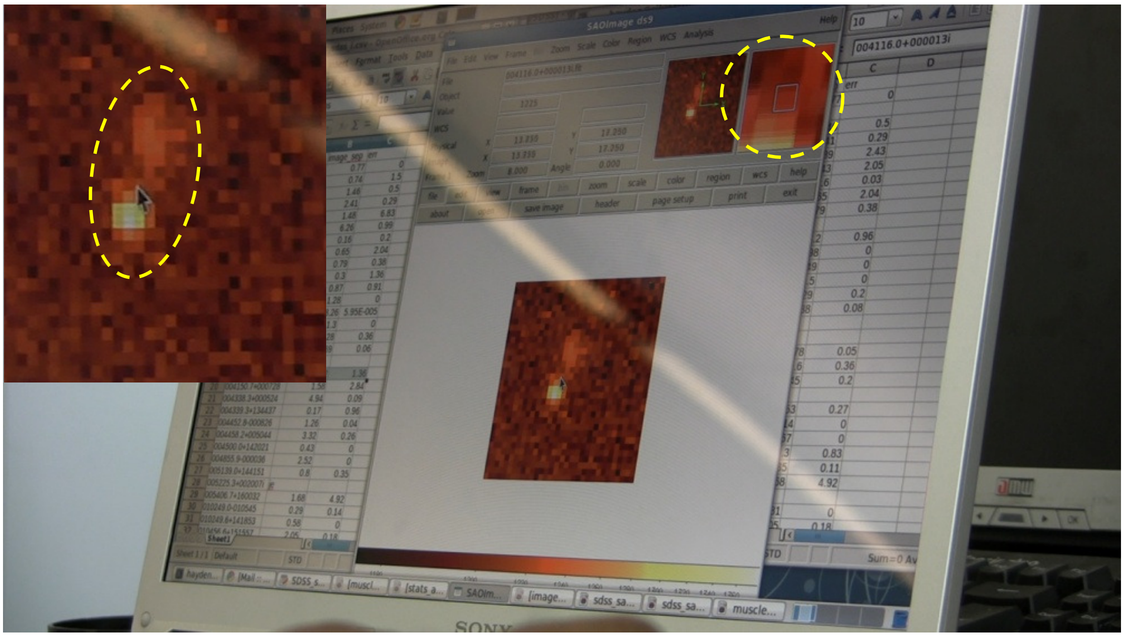

Figure 8 - The image for case nineteen - the clear distortion of the radiation emitted by the two objects indicates a good lens. Also note HR's use of the magnification display to closely analyse this distortion

Concluding Remarks

This chapter has presented some of the practices of 'looking for' and 'finding' that arise as part of routine programming work involving visualisations in astrophysics. As Garfinkel *et al.* note, "Situated inquiries are practical actions and so they must get done as vulgarly competent practices" (1981, 139), and it is precisely these practices that this chapter has aimed to unpack and explore. Invariably, for researchers working with visualisations, these practices are bound up in the various available visual resources that can be utilised[44], not just within code but throughout the visualisations themselves. As Burri and Dumit note, "Visual expertise also creates its own form of literacy and specialization" (2008, 302), and with such literacy comes the necessary skill to use visualisations as resources and as *sources* of resources. Hence, throughout the day's work HR could draw on the clues left as part of comments in his code, temporary visibility arrangements such as those generated by highlighting, the 'sequentiality' of images and visible features of the images themselves, his own ability to distinguish by eye between 'good' lenses and non-lenses, arrangements to facilitate both general (i.e. between tables) and direct (i.e. between individual cases) comparisons of results, and elaborate implications from comparisons of his own classifications and those made by the programme. This

[44] This is, of course, something of a tautology – what is there to be 'found' visually can always be 'looked for', by whatever means.

particular constellation of visual resources is useful to HR because achieving a working programme is the *object* of his work. The project is not designed to look *through* lenses, but *for* them (and to code that ability such that a programme could perform the task), and because of this HR does not have to rely solely on the results produced by the programme to inform his work – the results themselves can be legitimately questioned, and indeed should be. This makes the programme an interplay between the original observed data (the images) and the results, which works as an iterative process that requires a 'building up' of understanding of what effects manual input might have on results and associated diagnostic work about the quality of the manual input. Although this leaves a lot of uncertainty to the day-to-day work of the task – there is no decisive criteria of exactly which iteration might be the last – this nevertheless allows for the development of a programme that given enough time and effort will be able to do the job of discriminating lenses from non-lenses with so few 'je' errors as to make the whole cohort of results useful statistically.

Grounded in the literary background provided in chapter two, through analysing these elements of programming work on an astrophysics project, this chapter has also aimed to explicate some aspects of scientific problem-solving where all sense of the activity would be lost if we look only at the cultural and social elements of it and pay no regard to the context that scientific knowledge (i.e. knowledge pertaining to what a gravitational lens is, what features of lenses HR is looking for in the images, and so on) adds. The instances selected for inclusion in this chapter simultaneously reflect programming activities and scientific activities, inextricably linked – they are presented in this way because this is how they are to be found. An analytic strategy of separating one set of activities from the other[45] runs the risk of misreading and misunderstanding what is going on in a fundamental way. Such a conceptual distinction reflects constructionism's essential antipathy towards realism. This point is not made to involve the research work presented here in any sort of philosophical debate about the correctness or otherwise of realism, and it is not at all supposed that the present chapter could even begin to resolve the matter one way or another. Rather, this antipathy is highlighted because it invariably leads to an overemphasis on the social constructions and constraints of scientific work. This research is not intended as an argument against the existence of such factors and their effects on the work of science – of course, they *do* exist, and they affect scientific work in a

[45] See chapter two for a critical account of approaches that explicitly aim towards this separation, and see also the selection of authors outlined in the opening sections of this thesis for an understanding of how this separating applies specifically to programming and computing work.

multitude of ways – but as simply a display of *other* oft-neglected factors (i.e. the phenomena as it is known to members – in this case, how HR understands the science of gravitational lensing) and an exploration of *their* effects on the work that gets done. The implication of constructionism's antipathy towards realism is that it underplays precisely these elements of the work under scrutiny – the *doing* of software use and development as part of a wider project – as if it were a mere technical nuisance to be taken for granted; the realm of software developers *and not* scientists. Given the particular approach utilised here, HR's work can instead be conceived of as unfolding within a 'twinned'[46] problem-space of phenomena and software, whereby the software constructs and constrains HR's perception of the phenomena – literally, his ability to perceive gravitational lenses – and the phenomena constructs and constrains the use of the software in that his programming work must incorporate an accurate scientific understanding of gravitational lenses in a variety of ways. HR's skill with programming is not developed as a general 'wizardry' to be learned first and then applied to scientific research, as if the former is a necessary evil and the latter the *real* work. Rather, HR's programming is developed, learned and written as code *only as well as it needs to be* for the task at hand: his knowledges of programming and science are integrated, not distributed.

[46] This is, of course, not to limit the problem-space to two factors only. This statement should be considered as part of the argument against limiting sociology's remit to only the interactional features of scientific work.

CHAPTER FIVE: Representations in (the Practice of) Electrical Engineering

This chapter investigates elements of the work of a second-year PhD student in electrical engineering, with particular regard to the generation, usage and interpreting of representations of laboratory experimentation. These findings are based on ethnographic and video data that focuses on a lone researcher working on his project non-collaboratively (although with irregular periods of conversation and interaction between the researcher and the author, in the form of questions, explanations, assistance with practical hands-on tasks, and so on). The researcher's (here on named SC) work captured involves SC's efforts in collecting data from a self-built laboratory setup, which aims to implement a system for using an antennae array to take ultra-wideband electromagnetic readings of various metal objects in an anechoic chamber to determine how effective such a setup might be at detecting metal. From here, SC processes the data into forms that facilitate the making of initial reviews of the data (i.e. graphs), and based on the results of this quick visual analysis, makes a decision as to whether the data is good, bad, worthy of further inspection, revealing of any flaws in the setup itself, and so on. These ad hoc analyses, though formative to a successful piece of PhD research and to a satisfying of the aims and objectives outlined in the project brief, are ultimately designed to be no more than a quick means of transforming the collected data into graphs (and other representational forms such as statistics). This reflects the nature of the work at hand as 'live', in that SC has no intention that the representations he is creating and using for the time being are to be used in publications or in his thesis. They are not 'finished products', and are merely intended to allow SC to gradually develop his understanding and *work further towards* the grand aim of completing a thesis and a piece of valid research[47]. In contrast to 'professional' programming jobs (such as those investigated by Button and Sharrock, 1994, 1995, 1996), the programmes written and used by SC do not have to look neat or even function perfectly – their job is to be quick at interpreting data, and easily-editable so as to be adapted for new data processing tasks as they arise. Furthermore, SC uses these representations as resources for defining (theoretically, mathematically and geometrically) the effects of his setup on the data itself[48], and his resulting interpretations of the setup as it appears in the representations are fed back into his future data collection activities. Given this work, the topic of the present chapter is

[47] The question of how he may do these two things simultaneously whilst still learning how to do research in his field is addressed in the following chapter.
[48] This is to say that SC looks for and recognises various features of the lab setup in the representations.

how SC, as an early-stage researcher, devises and works with representations of various kinds. The aim is to highlight features of SC's work pertaining to how representations are built up, refined, made sense of, reconsidered and related to other experimental activities and theories, as part of the overall project. A selection of these techniques will be analysed here.

Representations have been a long-standing interest in the social study of science (and related endeavours). This extends as far back as the 'classic' laboratory studies and to the various programmes from which they were spawned (see chapter 2 for a discussion of such programmes), and it is useful at this point to briefly outline a selection of such studies and their peculiar orientations to representations. Woolgar (1981) notes that these early studies share a common interest in interests – that their focus is on the negotiation and acceptance of scientists' interests in the (conventional) construction of knowledge:

> The general strategy [of SSK] is to reveal interests as a kind of backcloth of attendant circumstances, and to imply that this revelation throws into better perspective the knowledge claim or event which is at issue. (Woolgar, 1981, 369-370).[49]

With more specific regard to representation, Barnes argues that for sociological purposes, knowledge consists of "accepted belief, and publicly available, shared representation" (1977, 1), and that this knowledge "cannot be understood as more than the product of men operating in terms of an interest in prediction and control shaped and particularised by the specifics of their situation" (1977, 24). Hence, certain laboratory studies of this ilk have taken a primary focus on the representation of various things in research work – examples include the representation of methodological practice in written work (Collins, 1985) and of physical materials in analytic documents (Latour and Woolgar, 1979). Such studies invariably draw on a set of ideas reflected in the presented quotations from Barnes (1977) that hold that knowledge construction is fundamentally a *shared* activity[50], and that the representations in question contribute towards a conventionality of knowledge that is at odds with scientists' own understandings of their work.

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[49] It is worth pointing out that Woolgar (1981), being wary of social interests as a framework for the empirical study of scientific knowledge, elected to turn sociology's reasoning back upon itself, as if to attempt to resolve its own issues before applying them elsewhere. This in itself is a problematic approach, and readers should refer to chapters two and three for a treatment of these ideas.

[50] See chapter 3 for a discussion of the problems with this singular focus on collaboration, and what a social study of non-collaborative work, such as that presented in this chapter, might involve.

To further exemplify how representations are dealt with in these studies, I return more pointedly (than in chapter two at least, which can be considered a more general overview) to Collins' (1985) study of the experimental work involved in replicating a TEA-laser. Collins' focus is in part on the (perhaps imagined) problem of how scientists were able to replicate working TEA-lasers despite having to rely primarily on the incomplete representation of the work of laser-building that was to be found in published papers. Hence, while it was possible to *begin* building a laser from information presented within publications, laser-builders had also to acquire some degree of 'tacit knowledge' ("our ability to perform skills without being able to articulate how we do them" (Collins, 1985, 56)). This was achieved largely through trial-and-error experimentation with components (i.e. mocking up laboratory equipment from available materials such as cut-up polythene bottles), or through social interaction with successful laser-builders (i.e. telephoning them to ask for pointers and solutions). The salient point to Collins was that unless laser-replicators had the necessary social resources to acquire tacit (which Collins implicitly takes to equate to 'unscientific') knowledge of laser-building from other scientists, they could not construct a laser from the representation of laser building contained within publications. As Collins notes of the replicated lasers, "If the device lased then it must have passed through every sorting stage. If it did not lase, then it must certainly have fallen at one or more of the hurdles" (1985, 147), where the sorting is to be found in the tacit requirements of the task. For Collins, publications did not serve as adequate representations of the work of building a laser and, moreover, *misrepresented* the work of laser-building. Hence, new information, acquired through other conventional (and 'less scientific') resources, had to be added to that representation of the methodological features of laser-building in order for lasers to be replicable.

Latour and Woolgar (1979) hold an interest in a different aspect of representation in the construction of knowledge, and focus their attention on 'literary inscription'. Their question to be addressed is: "How is it that the costly apparatus, animals, chemicals and activities of the bench space combine to produce a written document, and why are these documents so highly valued by participants?" (Latour and Woolgar, 1979, 48). Their anthropologically-informed approach to the 'tribe' of endocrinologists noted that at the bench, people (scientists) took materials (such as rats brains) and performed various tasks with them to subject them to a "radical transformation" (Latour and Woolgar, 1979, 49), which turned their focus from the material sample itself to a newly-produced sheet of figures, numbers and images. At this point, Latour and Woolgar note, "the same tubes which had been carefully handled for a week, which had cost time and effort to the tune of several hundred dollars, were now regarded as worthless" (1979, 50). Hence, the:

> final diagram or curve thus provides the focus of discussion about properties of the substance [...] The process of writing articles about the substance thus takes the end diagram as a starting point (1979, 51).

From these observations[51], Latour and Woolgar conclude that the work of research and knowledge construction is to be viewed as an effort of inscription and representation, which is achieved through the use of the laboratory as 'inscription device' for producing highly-prized and valuable pieces of paper.

What Collins (1985) and Latour and Woolgar (1979) present are accounts of research work in various scientific and technical fields that find new ways to talk about the construction of representations from every angle *but* what their content might mean to those creating and using them. As Lynch notes[52], these accounts serve only to "demonstrate that a constructivist [constructionist] vocabulary can be used for writing detailed descriptions of scientific activities" (1993, 102) – what exactly the value of doing so might be is ambiguous. When Collins claims that published work on TEA-lasers does not provide laser-replicators with enough information to build one themselves, this seems undeniable. Yet Collins does not seem to recognise that publications *are not intended as* thoroughly detailed recipes for laser-building. Similarly, Latour and Woolgar characterise their endocrinologists as paper-fetishists, but fail to describe in any meaningful way what import those *particular* papers may hold in scientific terms[53]. Through decontextualizing their understandings of the

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[51] Notably, these observations were 'conducted' by a mock observer, who took his/her 'anthropologically strange' (i.e. supposedly free from assumptions) perspective to this particular 'strange tribe' of scientists. Hence, Latour and Woolgar (1979) were able to present a novel, if overly ironically self-aware, account of the work of endocrinology. This is notable in that it colours the scene with an unfamiliarity which, although Latour and Woolgar claim to work towards puncturing through employing this device, is difficult to take very seriously.

[52] Lynch's argument is about Latour and Woolgar's (1979) study specifically, although I extend it also to Collins (1985).

[53] The point to be made here is that while scientists might well be interested in the inscriptions/representations they work to make (and accordingly in their methods of inscribing/representing), there is more to the technical job of research than this. Lynch notes that "It is as though laboratory work were primarily directed to fashioning and refashioning "statements" and that what any statement is doing were a secondary product of direct operations on the statement's form" (1993, 99). Imagine, for example, the reaction of scientist presented with a bag filled to the brim with till receipts: inscriptions of transactions that have taken place at a supermarket. It is unlikely that the scientist would be filled with glee at the prospect of having acquired a huge amount of inscriptions unexpectedly. Put simply, scientists are not interested in just *any* inscriptions, which leaves a further question of why these particular scientists are interested in some inscriptions and not others.

endeavours under investigation, both Collins and Latour and Woolgar miss out on key factors that characterise the activity and its purpose. Collins does not try to understand the intended role of publications in laser-building (and accordingly, the role that 'tacit knowledge' must assume to resolve laser-building problems). Latour and Woolgar do not acknowledge that scientists are not interested in inscriptions as such, but rather the things afforded to scientific research by having done the necessary work to be in a position to inscribe and use it. Lynch argues that this decontextualizing work "implies a radical separation between the form of a statement and its practical use" (1993, 99), and in this sense, both Collins' and Latour and Woolgar's failures are resolvable through a more context-sensitive approach to the work and products of knowledge-generating research. This is precisely the approach that the present chapter aims to take, elaborating on what this might mean by turning next to a selection of five contextually-sensitive 'stories' of events occurring as part of the work observed.

These stories deal, simultaneously, with three key issues: firstly, what SC's work represents and what purposes those representations might serve; secondly, how the laboratory and setup are available features of representations as mediated through theoretical and mathematical concepts, and; thirdly, issues pertaining to the (re)presentation of data in graphical form. These three issues are difficult to isolate in SC's work, which moves around within them in often complex ways. Hence, the stories presented here highlight various activities that have a bearing on multiple issues, which are brought together as a discussion in the latter stages of the chapter.

Giving the Gun 'a Joust'

The first story concerns an activity taking place at the outset of the data collection activity, wherein SC's aim is to take 'simple' data that should have a strong and clear return that SC can easily positively identify when plotting it on a graph. To do this, SC has suspended a de-activated starter pistol from a length of washing line hung inside his anechoic chamber, which, crucially, is self-built from wooden panels and signal-absorbing foam pieces and hence is not perfectly anechoic. The data is gathered by a vector network analysis (VNA) machine (see figure 9 below), which takes a reading from the transmitters and receivers that are connected to and which are pointing into the central area of the chamber. However, SC's self-built chamber is not fully insulated against ambient background 'noise' coming from metal objects in and around the chamber (i.e. the nails used to join the wooden panels together and the signal-carrying wires and receiving equipment itself), which gets picked up by the receivers along with the metal components of the gun. SC can see from the VNA's screen, which displays ‘live’ readings of what the receivers are receiving, that there are already some significant returns (i.e. spikes on the graph indicating a clear presence of metal objects). However, it is not clear to SC as to which spikes represent the signal returned by the gun, and which spikes are ambient products attributable to the chamber and setup itself.

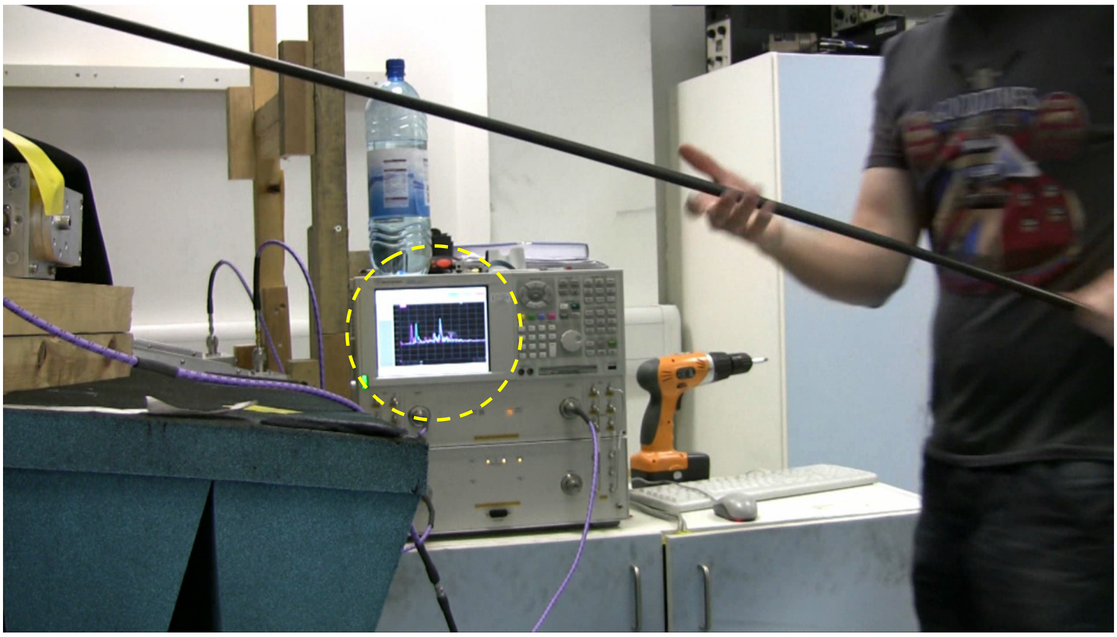

Figure 9 - Giving the gun a 'joust' to see how this affects the VNA display (highlighted)

SC’s solution is to take a long piece of thin plastic pipe and, from his position near the VNA, give the gun what he calls ‘a joust’ – a prod – such that it swings from its fixture on the washing line. This has the effect of making some of the spikes visible on the VNA’s graphical display waver up and down – as the gun changes its orientation to the emitter/receiver setup, it affects how the signal is reflected off it. There are, however, two areas that *do not* waver as the gun is moving (one for each receiver), and these are near the origin of the graph (i.e. the first thing the signal perceives in time). Given their proximity to the origin (i.e. the point on the graph where $x = 0$) and the fact that they occur uniformly for both receivers, and given that these elements of the received signal are clearly not related to the gun itself, SC concludes that they are most likely to be a product of the metal wires that connect the VNA to the receiver. These un-moving spikes are the ‘cross-talk’ that occurs when each receiver picks up the metal components of the other receiver *before* they pick up any relevant signal from the objects in the chamber. Hence, these elements can be discounted – through literally eliminating them from appearing on the VNA in further experiments[54] – as irrelevant to the data collection activity.

Here, SC is working towards representing the gun as an isolated object, despite the issues arising from the setup he is using (chiefly, that it is not fully anechoic), and therefore has to remove any ambient/background/environmental signal from the data before building a representation from it. Once this is done, SC is clear to think of his data (and any representations resulting from it) as more closely approximating theoretical ideals and as not being so strongly affected by the circumstances of its collection.

Resonant Frequencies as Representing the Sizes of Nails

In this instance, SC is deciding which objects to use as part of an imminent round of data collection. He is choosing between two different nails that were found lying around the laboratory – both are simple objects which are useful in terms of providing easily-understood representations of the success to which the setup can detect metal (in that its abilities are more easily quantified when the objects themselves are more straightforwardly detected). But, SC works towards a decision as to which will show up more clearly on the VNA. The choice is not arbitrary – SC knows that the machine is set up to take data within a specific frequency range (his project being on ultra-low frequencies, and as such, he is uninterested in anything above that threshold), and this has a bearing on which nail is selected. SC measures one nail to be 10.5cm, and doing a quick mental calculation, SC estimates that this will produce a resonant frequency at around 1.5GHz. What this means is that in the resulting representation of the data, whilst the nail will be 'visible' to some extent across a range of frequencies (in that there will be spikes in the graph at particular points), the strongest spike will be at 1.5GHz for this nail. However, the other nail is 7.4cm long, and this gives the object a naturally higher resonant frequency – visually, it will appear on the VNA display further away from $x = 0$. There is a consideration that for the longer nail, its relative lower frequency will mean that it does not appear very clearly within the central area of the range that the VNA is set up to display. Hence, the signal produced by this nail may appear too close to the

[54] Practically, this was achieved through a process call 'background subtraction', where SC takes a reading of the chamber with no metal objects, then resets the VNA to a base value – this is similar in concept to how you might recalibrate a set of weighing scales with a bowl on them, so as to be able to weigh accurately the amount of flour you wish to use *without* accounting for the weight of the bowl itself. SC had not yet done this background subtracting before placing the gun in the chamber, hence the un-moving spikes appearing on the VNA which appear as a problem to be addressed.

origin to be clearly discernible, whereas SC argues that the shorter nail would appear more comfortably within this central range on-screen. So, the decision is made – the 7.4cm nail is the better choice, because the laboratory setup will be better able to represent it on the VNA display and in the subsequent graphical output as the data is processed through SC's programmes.

Here, SC has made a choice about which object to use for his data collection activities on the grounds that it will make for a better representation. For SC, his representations are built up from decisions such that things that happen at the data capture stage and are worked through with later programming (see later sections in this chapter for more elaboration on these later stages). Furthermore, SC is able to work out where this particular nail should be visible on the VNA display, and has background subtracted away any irrelevant data that might confuse his visual analysis of the information – in this way, SC can be sure that what is being represented is *nothing more* than what he places in the chamber[55], in this case a bottle of water with a 7.4cm nail taped to the back of it. This 'cleans up' all representations that are to be built out of this data, and goes some way towards ensuring that SC's graphs and analyses are uncluttered by complicating factors and clearly represent the experiment and phenomena free from other interferences.

What Antennae Can (Be Said to) See Through Time

One issue that became pertinent in terms of interpreting the data was the relationship of the data on-screen to the temporal order of physical measurable properties of the setup itself (i.e. the distance from the transmitter to the object, the time it takes for a signal to pass between the two, and so on). In one instance, SC shows the author a graph of time and amplitude of signal (such that we can see the changes in amplitude as time passes, on a nanosecond scale) (see figure 10 below).

[55] Or rather, what is being represented is *sufficiently* (if not entirely) free from external signal-producing factors to be able to analyse and talk about any resulting representation as if it was a direct representation of the object and phenomena.

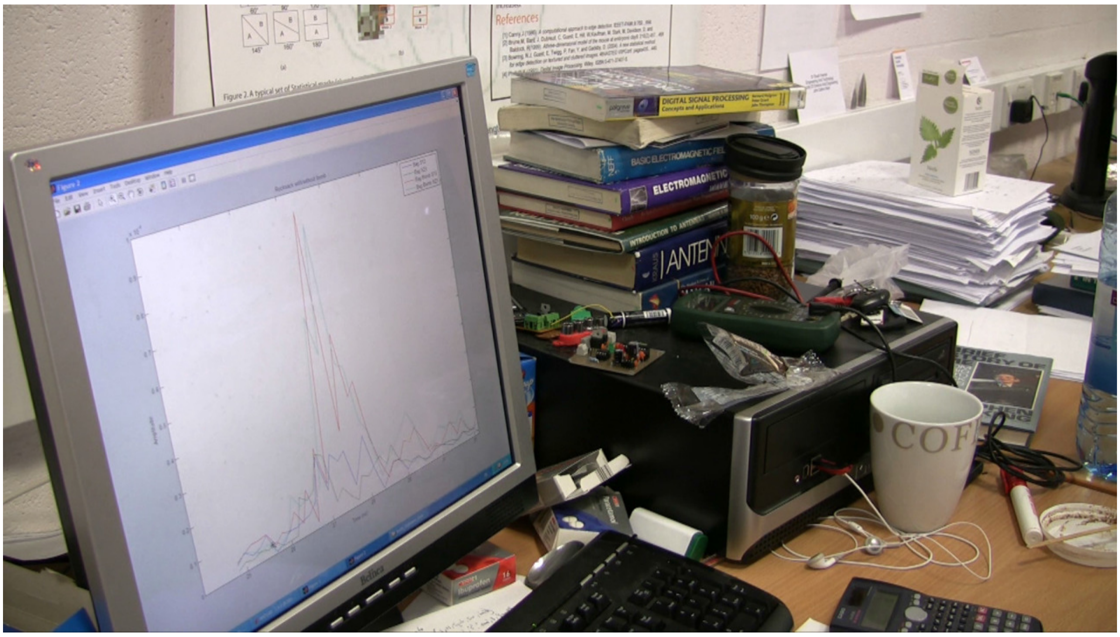

Figure 10 - A 'timeline' graph detailing at which point in time the signal has hit various objects in the data collection chamber

SC explains that as the graph starts from point 0 in time (i.e. when the signal is instructed to send) and increases, it is possible to consider the graphs as representing what the signal 'hits' as it propagates through the chamber and object and back towards the receivers. In this sense, when considering data taken of the author with his back to the emitter/receivers wearing a rucksack containing a metal object, the first small spikes are likely to represent the face of the bag (which the signal hits before anything else) and the metal zip on it. The following 'envelopes' of spikes and activity on the graph represent the signal as it travels through the bag and hits the metal contents, then as it hits any metal objects on the author himself (such as a watch or a zip), then as it bounces off the back wall of the chamber and back towards the receivers. In this sense, the envelopes of activity visible on the graph represent, roughly, different objects (i.e. rucksacks, human bodies, etc.) in the chamber. Knowing roughly where these objects are, what metal components each object features, and the order in which a signal will propagate through them, SC is able to piece together a 'timeline' of what the graph should be showing and what the signal should be detecting. Hence, it is possible to trace the line of the graph from $x = 0$ and identify (approximately), a series of events, i.e. the signal hitting the metal zip on the rucksack at ten nanoseconds, the signal propagating through the author's body at twelve nanoseconds, and so on. This relates the physical objects that make up the data under analysis to the distinct events of the graph such as spikes in amplitude and the temporality represented in the x axis.

Without this relating work, the on-screen display itself is devoid of context and meaningless. It is only a *graph* (and not simply a picture of multiple coloured lines) when it represents some activity or event or phenomena or combination of the three. Hence, the work done at the stage of data-capture and experimental setup is an inextricable part of the resulting representation, and lends a lasting relevance to that representation for those analysing it (cf. Latour and Woolgar (1979), who imply that scientists are interested predominantly only in the generation of inscriptions – representations – and not so much in their actual content). In this sense, it is absolutely vital that SC can move back and forward from the setup to the representation in order to check each against the other and draw meaningful, relevant, useful and usable conclusions from the work performed in service of generating a graph.

Stacking Data vs. Plotting Data

SC also has to be constantly aware of the effects of presentational decisions on his representations and the implications these might have in terms of affecting their explanatory power. In one instance, SC is dealing with a particular set of data that is presented as two lines, under which there is a solid block of colour (see figure 11 below).

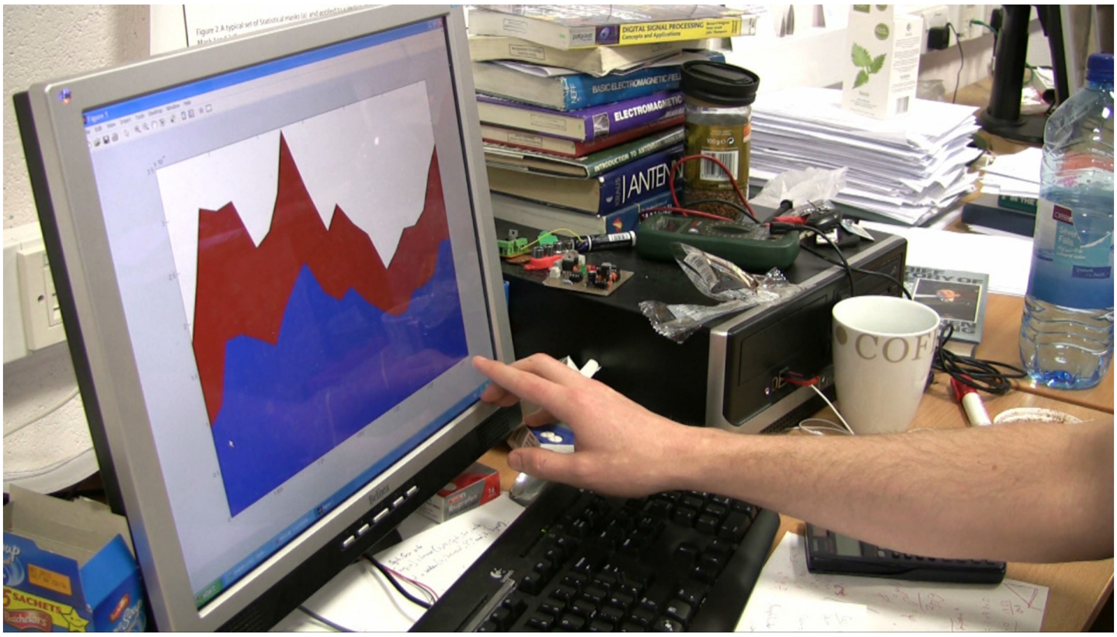

Figure 11 – A ‘stacked’ graph, comparing data taken of a non-metal object (blue) with the same non-metal object plus an added metal component (red)

This way of presenting data is, as SC understands it at the time, a more appealing form of presentation (in that it is clear to see the difference between the two different lines on the graph, such that you can easily spot just how visible a metal object is when compared against a non-metal one). However, having plotted the data, SC checks the values of the first points of data to ascertain whether the representation has been created from the original data without issue. Checking those values against corresponding ones in the original dataset, SC recognises that the graphic representation of the metal object does not match with its original value, and SC spends time working out a reason as to why this might be so. Eventually, SC concludes that the form of presentation is 'stacking' one set of data on top of another, and this has implications on what may be said when comparing the two sets. If both sets of data start from a zero value, then they are directly comparable (and it is possible to say things like "the metal object has clear spikes at specific frequencies, whereas the non-metal data does not"), but if one is stacked on top of the other these kinds of comparisons are not available. SC checks this issue by re-processing the data into a more familiar line (plot) graph (see figure 12 below), and compares the values of one point of data in each – this validates his reasoning, in that it reveals that the plot graph represents SC's data in the desired way, and the stacked graph differs markedly from this. Hence, by noting the actual value contained in the dataset and checking these against the two different available representations, SC is able to decide which of the two representations is most relevant for the project and the claims he is working to make. Whilst the stacked graph appears to be more aesthetically pleasing and clearer to understand[56], it in fact distorts the data in such a way as to render any explanations of the kind SC has in mind that are based on it incorrect. By contrast, the less visually appealing (less 'publishable') plot graph is the more analytically relevant one, and in order to preserve the validity of any future representations, SC resolves to use the plot graph from here on.

[56] A consideration which SC makes based on the 'publishability' of certain graphs more than anything.

Figure 12 - Comparing a stacked and plot graph of the same data

Deciding at Which Point to 'Chop' a Graph

In one instance, SC opens up a new set of data and states an intention to find a point at which to chop the graph off at the beginning. Given that the antennae pick up signals from each other and from the wires that connect them to the VNA and that even with background subtraction there is typically lots of extraneous 'noise' at this point, these elements of the signal (which appear temporally before, and hence left of, the first initial reflection from the object in the chamber) can be removed – 'chopped off' – from the representation. This, in effect, discards them as artifacts of the setup that are irrelevant to the phenomena under investigation. The decision of at what point to chop the data off is linked closely to the circumstances in which the data was collected, in that after analysing the early part of the signal SC is able to identify, approximately, the time at which the signal *should have* reached the metal object, look to see if that has produced anything like a specular reflection in the representation, and on that basis chop off all data that occurs before that initial specular reflection. The choice of where to chop the graph is different for each dataset, but it is not arbitrary. Rather, it relates to the physical features of the data, in that there is an obvious conceptual and empirical difference between data taken from the author's body carrying a starter pistol in a rucksack on his back and data taken of the author carrying that same gun in his jacket pocket on the front. The VNA perceives this difference in its efforts to locate the metal, and therefore SC has to factor this in to his ultimate choice about which elements of a signal are artifacts and which represent the data closely. Further weight is added to these choices when considering that the values available in the ultimate representation are used to inform statistical operations that SC uses to indicate such things as the average values of amplitude across the range of data or summation values that help determine a threshold above which the presence of a metal object is likely. This statistical work, which is done *on* the representations rather than as a formative part of them, makes it doubly important that SC is able to think clearly about what it is his setup consists of (i.e. artifacts that occur up to 160 nanoseconds for instance, then 'real' data of a human body carrying a start pistol in their front jacket pocket from thereon), in that without doing so, no meaningful information can be derived from the representation at all.

The point to be made here is that presentational elements of producing representations carry import as to the explanatory possibilities revealed by those representations. Analytic work does not only inform findings, but *how those findings are best represented*, and in this sense, the separation of the form of representations from their content and context renders them meaningless, both to science and to ethnomethodology. Artifacts are written out of representation not simply for clarity or aesthetic concerns, but so as to ensure those representations are accurate and directly related to the phenomena and objects under scrutiny. In this way, SC must be at all times aware of the circumstances of his data collection when processing data into representations and continuing to work on them, because these activities are as much reliant on the conceptual and theoretical context of the wider project as any of SC's other efforts in the lab and on-screen.

Concluding Remarks

This sections aims to highlight ways in which the selection of 'stories' presented address the three interrelated issues outlined above. To briefly reiterate, the issues are: what exactly SC's work represents (and how SC can know he is representing those things accurately) and what those representations are for; how the laboratory and data setup are available to SC as features of representations which have conceptual (theoretical, mathematical, geometric, etc.) attachments, and; the effect of various (re)presentational forms on the ultimate conclusions SC is able to draw from his work.

It is clear from the stories presented above that SC's work is only possible through his having a keen sense of how his experimental work 'fits into' the wider project brief in terms of its conceptual implications, and which bits may be discarded as irrelevant and meaningless in terms of the project as a piece of valid research. Cartwright highlights the relationship of theoretical and empirical knowledge in physics, noting that "To get from a detailed factual knowledge of a situation to an equation, we must prepare the description of the situation to meet the mathematical needs of the theory. Generally the result will no longer be a true description" (1983, 15). Quite unproblematically, SC is able to discount things such as the cross-talk between antennae as artifacts of *his* setup – this is not to say that SC assumes that other setups would not have to deal with cross-talk or that he does not recognise it as having had an effect on the conclusions he has been able to draw. Rather, SC understands these factors as irrelevancies that have no bearing on the phenomena *as it would play out under ideal conditions*, and which therefore have no place in a simplest-possible account of what had taken place in the laboratory. Hence, it is difficult to take seriously the importance that Collins' (1985) imbues upon his claim that the written reports of scientific (and, by extension, other similar) research are not accurate representations of the practical 'tacit' work done in the lab. To be sure, they *don't* represent this work, and they fail to do so without apology from practitioners. Collins (1985) is preoccupied with the question of whether it is possible to replicate a scientific operation solely on the basis of reading about it, and Collins thereby discounts practitioners' understandings of what their documentation and reportage achieves. It is precisely the practitioners' disregard for accounting for every single practical move made in the experiment that makes their research better accounts of what their work has achieved, rather than misrepresentations of the moment-by-moment course of their constitutive activities. As Lynch notes:

> To point to differences between "methods" accounts and the technical details of the actual performance is not to fault the methods accounts for their "insufficient detail," but is to take notice of unformulated features of practical action that are relied upon in the methods account (1980, 93).

Hence, written accounts of research work serve a different purpose entirely, and one which is not to be evaluated on how pedantically various research activities in scientific and related fields may be described[57]. Cartwright further elaborates, taking physics as her example:

> There is no difficulty in writing down laws which we suppose to be true: '*If* there are no charges, no nuclear forces … *then* the force between two masses of size *m* and *m'* separated by a distance of *r* is Gmm'/r^2.' We count this law true – what it says will happen will happen, does happen – or at least happens to within a good approximation. But this law does not explain much. It is irrelevant to cases where there are electric or nuclear forces at work. The laws of physics, I concluded, to the extent that they are true, do not explain much. (Cartwright, 1983, 72-73).

[57] Perhaps this task (and associated criteria for measuring success) is sociology's own.

This is to say that there are no fundamental problems with boiling down physical processes enacted in real laboratories, with all their environmental variables and ambient noise, to their relevant constituent values, and then taking these representations of that physical real work as concepts, values and variables to be taken up by such concepts as mathematics (i.e. to be treated as variables in algebraic formulae, put into relation with each other, used to reveal new concepts, and so on). However, Cartwright (1983) asserts that these laws – representations of physical processes – hold unproblematically only as far as they are not treated as representing *the full story* of the work that has gone into producing them[58]. What the stories presented above suggest is that SC does not take his representations to be any such thing – where Latour and Woolgar (1979) claim that researchers prize their representations as an end-result above all else, SC is seen to be constantly moving back and forth between understandings of the phenomena-in-abstract and the data-in-situ in order to make meaningful sense of the representations he generates. In their examination of the relationship between models and representations (as they are known to sociologists), Knuttila and Boon note:

> we have aimed to show that the Carnot-model of the ideal-heat engine is not constructed as a *representation* of actual heat-engines by some kind of obvious resemblance or similarity with it, for instance, as about its mechanical working, or as about the observable and measurable properties of real heat engines. This is contrary to what some versions of the pragmatic and semantic views of models would suggest. Rather, the Carnot-model is constructed as an hypothetical engine affording reasoning in view of a certain purpose (2009, 15).

Hence, while SC's aim to is represent the goings-on of his work with the principles of UWB metal detection, his representations do not have to display every element involved in their generation, and indeed *should not do so* (i.e. they will be *distorted* by any data that represents aspects of his experiments that are irrelevant to the working of the principle itself). Hence, SC is able to understand the limitations of his representations as explanations of what has happened in his anechoic chamber, and the limitations of his data as tools for building up representations of a phenomena happening in an ideal circumstance (i.e. unaffected by the many natural distortions that SC's lab – any lab! – is subject to). Indeed, understanding these limitations

[58] This is to say that the author does not wholly agree with Cartwright in her claim that the laws of physics do not explain much. The author's response to Cartwright on this issue would be to argue that their utility as explanations depends on what you take them as explanations *of*. Certainly, the laws of physics do not explain how to perform successful experiments, but there are other mysteries which they may be called upon to clarify (such as how to approximate the resonant frequency response of a given length of metal).

allows SC to more clearly work towards resolving them, which ultimately is the purpose of his project. As Kuhn notes of 'normal science'[59]:

> even in those areas where application is possible, it often demands theoretical and instrumental approximations that severely limit the agreement to be expected. Improving that agreement or finding new areas in which agreement can be demonstrated at all presents a constant challenge to the skill and imagination of the experimentalist and observer. (1971, 26).

If this type of work seems reductive, in that SC's efforts are, inarguably, to further elaborate on already-established principles, then Kuhn also notes that:

> The man who builds an instrument to determine optical wave lengths must not be satisfied with a piece of equipment that merely attributes particular numbers to particular spectral lines. He is not just an explorer or measurer. On the contrary, he must show, by analyzing his apparatus in terms of the established body of optical theory, that the numbers his instrument produces are the ones that enter theory as wave lengths. (1971, 39).

This is to say that SC's equipment is, in a sense, no good if it *just* detects and identifies metal accurately on complex bodies. Rather, SC also has to justify and prove that detection capability through situating his experimental work comfortably within conceptual arenas such as theory, mathematics, and so on. Lynch notes of his study of the work of neurobiologists:

> For lab members, *making a phenomena happen* in lab work was more of an active seeking for the thing or result [...] it is not enough to avoid mistakes, since success requires a management of circumstances so as to *bring out* an intended result (1980, 167).

SC's work bears similarities to Lynch's (1980) characterisation, in that his lab work revolved entirely around the issue of whether he could coerce UWB signals to detect metal under certain obstructive conditions (and this was to be achieved not through affecting the signals themselves in any way, but through re-arranging the setup to be used to do the detecting). This is facilitated through SC's utilisation of 'metadata'. Lin, Poschen and Procter describe the ontologies that their study focuses on as "a means to represent formally, and in a machine-readable format, metadata ('data about data')" (2008, 1). SC incorporates this kind of metadata by writing his

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[59] Kuhn takes 'normal science' to refer to research which is "firmly based upon one or more past scientific achievements, achievements that some particular scientific community acknowledge for a time as supplying the foundation for its further practice" (1971, 10). I characterise SC's work as 'normal electrical engineering', in that the theoretical principles he is dealing with are well-established, but require experimentation in order to reveal their applicability and feasibility as solutions in 'the real world' outside of the laboratory.

representation-producing programmes such that the resulting representations, when accompanied by a contextual understanding of the work done, can be used to ascertain certain facts about how accurately his representations are representing his data. These understandings can be then fed back into – swallowed up by – future data collection activities and inform the progression of the project by capitalising on successes (i.e. taking more data using a setup that functions well) and working to address failures (i.e. pointing out areas for improvement and possible ways to do that improving for setups that have provided poor data). Hence, although SC's work is not intended to question the phenomena (and its workings) itself, and would be unlikely to be able to do so given the circumstances of its conducting, it *is* able to take small steps towards an application of the principle under investigation as a general tool for detecting metal with UWB signals whose use might be realised in a wide array of situations (i.e. at football matches, at airports, and so on) outside of the lab.

In summary, there is a particular quote of Lynch's which demonstrates the overall aim of this chapter in light of the critique of constructionist studies (such as Collins, 1985 and Latour and Woolgar, 1979) that Lynch and the author share. Of these studies:

> It is not as though these [constructionist] accounts *deny* that science is accomplished in laboratory settings, but that they do not provide detailed access to the practical achievement of day-to-day inquiries. They instead give decontextualized versions of methodic production and logical reasoning [...] and abstracted "social aspects" of science's institutional and administrative organization (Lynch, 1980, 5).

It has been the aim of the present chapter to help 're-contextualise' sociology's approach to empirical work of this kind, through the displaying of features of the practical achievement of day-to-day enquiries that occur in research work in fields in and around science, and to show the possibilities and value of doing so.

CHAPTER SIX: The 'Space Between': Moving from Dependant Learning to Independent Doing in Electrical Engineering and Astrophysics

This chapter explores how early-stage researchers in natural science and engineering move from learning about their disciplines to position themselves as practitioners of it. It draws on observations, made over the course of several months, of the work of one PhD-level postgraduate in electrical engineering and one masters-level postgraduate in astrophysics, as well as video recordings of both of these at work. It aims to show how early-stage research is done by these researchers (i.e., how they can produce 'professional' quality research despite still learning what it takes to do so).

There already exists a broad array of literature covering science education at this early-stage research level, where students come from a 'studying' background and move towards 'doing' science for themselves (see list at the bottom of page 99). However, this thesis finds that the treatment this peculiar 'space between' has had from sociology is over-reliant on concepts such as 'enculturation', 'dissonance' and 'reality-shock'. Concepts such as these account only for the cognitive and cultural problems that early-stage researchers face, entirely neglecting elements of the work that characterises the endeavour as specifically one in early-stage scientific education and research, and not some other thing. Hence, this chapter aims to reassess the applicability of such concepts by presenting a selection of examples of how these students deal with various aspects of early-stage research.

Socialisation – 'enculturation' – is a process of preparation for autonomous practice, and the studies in this thesis focus on researchers who are learning and teaching themselves how to make independent use of the science and related conceptual knowledge they have been taught. This chapter initially illustrates this theme with two case studies of early-stage researchers working out how to use and elaborate on their existing understanding of their disciplines to organise activities that will stand as positive contributions to their allotted projects. The transition from undergraduate to 'professional' researcher is often treated as abrupt and discontinuous, whereas the studies reported in the present chapter seem to suggest that this account does not adequately encapsulate the work going on in these settings. This theme is developed in relation to the way in which the transition from learning to practice is often assumed to be marked by a "reality shock" (Delamont and Atkinson, 2001, 88) – an unexpected revelation that much of what is learned in education cannot be immediately and simply applied in professional practice. Applied to students of science and related research endeavours (such as electrical engineering), this presupposes a conception of natural science students as naive about the difference between being presented with science that is shaped to facilitate the teaching and learning of it (often using problems with known solutions) and confronting a problem which has no currently known solution. The early-stage researchers described in this and other chapters treat it as a matter of course that their projects involve them in themselves figuring out how to solve the problem they have been assigned, using the conceptual backgrounds they have already learned to piece together a practical understanding of how their experiment ought to work, whether they need to learn more about their respective backgrounds to better address the problems at hand, whether their design for the experiment should work, and, if it does not, what further resources the science can supply to understand why it does not.

The chief finding is that researchers' introductions to these surroundings are not experienced as a reality-shock, where all vestiges of instructional learning are suddenly removed, leaving students to either successfully adapt to the unfamiliar cultural norms of independent (although supervised) research or fail. Indeed, the application of concepts such as 'reality-shock' neglects the settings to which they are being applied, in that it does not explore what students may or may not know (and how they must therefore react) as they encounter early-stage research for the first time. Rather, the findings presented in this chapter suggest that the transition, if it is even a 'transition' at all and not a 'continuation', happens over a much more drawn-out period (with no definitive start- or end-point), ensuring that researchers don't experience this as any sort of perturbation at all. This is partially because being an undergraduate learner does not necessarily preclude accurately imagining and understanding the work of scientific research, and partially because the process of doing the work itself guides researchers' activities. As such, this stands as a very different account of this stage of scientific education, which does not have to rely on sharp conceptual distinctions between a 'learning stage' and a 'research stage' only traversable through cultural adaptation.

The aim of this chapter is to investigate a specific stage in scientific education and research, where education starts to become research-based and students make their first forays into working on research projects of their own. Although there already exists a selection of studies that have looked at various settings in postgraduate research-based education (e.g. Campbell, 2003; Delamont and Atkinson, 2001; Delamont, Atkinson and Parry, 1997, 2000; Parry, Atkinson and Delamont, 1997; Roth and Bowen, 2001; Roth *et al*., 1997; Wisker *et al*., 2003), these typically advocate the use of related concepts such as "enculturation" (Delamont, Atkinson and Parry 1997, 325), "dissonance" (Wisker *et al*., 2003, 93) and "reality-shock"[60] (Delamont and Atkinson, 2001, 88) to explain the work of student researchers in these settings. Here, enculturation is referred to as a process of socialisation into a scientific research setting, reflecting an "intergenerational transmission of research problems, bench skills, techniques, equipment and other resources" (Delamont, Atkinson and Parry, 1997, 325), handed down from supervisors to novitiates. This process however is characterised as being far from straightforward, and students coming from a background of taught lessons and stage-managed lab demonstrations may struggle to adapt to the not-so-clearly-defined role of early-stage researcher, experiencing a dissonant "clash or gap between [their own] perceptions and approaches and possibly also outcomes" (Wisker *et al*. 2003, 93) and those of their supervisors. As such, the fact that students are not yet enculturated into their research settings may result in a reality-shock for them, whereby students discover that:

> experiments and other forms of inquiry do not always – or even frequently – 'work': that is, they do not habitually produce useful, or even usable results. This comes as a shock, because undergraduate laboratory experiments have been chosen and stage-managed by lecturers so that they do, routinely, produce 'correct' results.' (Delamont and Atkinson, 2001, 87).

Such concepts frame postgraduate success as hinging only on how postgraduates adapt to whatever cultural norms and values inform professional work in that field, with the implication that the only work being done in such environments is cultural and social. The present argument suggests that the use of such concepts neglects almost entirely the elements and features of the work that mark it out as specifically *scientific*, and what these enculturationist accounts are therefore explaining is, at best, one small aspect of what it is to work on a project in an early-stage education and research setting. Delamont and Atkinson argue that:

> this transition [from undergraduate to postgraduate] is accompanied by a sense of reality-shock. Initial encounters with the difficulties and uncertainties and 'real' research readily lead to expressions of frustration and disenchantment. The new research student discovers that the experiments conducted as an undergraduate were stage-managed mock ups: the one-off student practical contrasts with the repetitious, time-consuming and often inconclusive work of research. (2001, 104).

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[60] The term 'reality-shock' has been appropriated by the aforementioned studies from its origins as a study of the hazards newly-trained primary school teachers could expect to face (Wagenschein, 1950), which itself was taken up as a concept in nursing to describe the struggles that recently qualified nurses have upon encountering the job *as* a job for the first time (Kramer, 1974).

Therefore, the only learning that needs to happen for an early-stage researcher to succeed is learning how to overcome the 'shock' of having their textbook knowledge challenged on a daily basis. Traweek provides further clarification on the enculturationist perspective by looking at the use of textbooks in undergraduate education and why these textbooks seem to become obsolete and are superseded by the advice and information acquired in the laboratory through social relationships. She notes that pre-PhD students:

> learn from textbooks whose interpretation of physics is not to be challenged; in fact, it is not to be seen as interpretation…they also learn, from stories in their textbooks, that there is a great gap between the heroes of science and their own limited capabilities. (Traweek, 1988, 75).

It is interesting to compare such steadfastly sociological perspectives against a quote from the introductory chapter of one of the more well-known comprehensive undergraduate physics textbooks[61], outlining the essence of a scientific approach to the physical world:

> To make an idealized model, we have to overlook quite a few minor effects to concentrate on the most important features of the system. Of course, we have to be careful not to neglect too much…We need to use some judgment and creativity to construct a model that simplifies a problem enough to make it manageable, yet keeps its essential features. (Young, Freedman and Ford, 2008, 3-4).

One question might then be; where is the reality-shock? If the creativity and judgment involved in scientific work is acknowledged in the textbooks that undergraduates are said to rely on, such as Young, Freedman and Ford's where *could* it even originate? Therefore, this chapter explores how enculturation, dissonance and reality-shock do or do not play out in the stage of scientific education where students are tentatively bridging a tentative gap between learning by relying on lectures and lab demonstrations and self-reliantly embarking on an individualised research project that is their own responsibility.

The present chapter examines the work of two postgraduate-level research projects in natural science and engineering, presenting a few of the features arising out of these projects as a critique of the sorts of studies outlines above. Both projects – one in the field of electronic engineering, one in astrophysics – were approached ethnographically and against a background of significant ethnographic preparation[62].

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[61] At the University of Manchester at least, where it is recommended to every first year undergraduate as a key reference covering almost every topic they can expect to encounter throughout their degree.

[62] Refer to footnote 24 in chapter three (page 42) for further details on what this preparatory work has entailed.

Additional contextual information that was more specific to each project was picked up throughout the fieldwork period in both cases, and the observational and video elements themselves were relatively short (i.e. a few weeks) - this was similar to Hughes' conception of a 'quick and dirty' ethnography (see Rouncefield *et al*. (1994) for an example). For the electronic engineering fieldwork, the work captured includes several days of an iterative activity of data collection in the laboratory, using a programming language to present and interpret data, then re-designing key details of the lab setup for future data collection activities. The second project was a piece of programming work in astrophysics, and this project was approached primarily using video to capture, in detail, a day's work. The astrophysics work captured on camera consists of one full day's work on the researcher's project, focussing attention on the work being done on-screen with the programming language as well as capturing the talk between himself and the author about various features of his work.

Electrical Engineering

Turning first to the project in electrical engineering, this piece of work was undertaken by a student/researcher called SC and was titled 'Non-Intrusive Detection of Concealed Weapons by Transient Electromagnetic Response Analysis'[63]. SC's project was to investigate the possibilities of using ultra-wideband (UWB) signals to detect and possibly also identify (through characteristic properties of the detected signals, which might be useful in distinguishing between specific weapons such as knives and guns and benign metal objects such as zips and belt buckles) metal components in objects concealed on human bodies[64]. To this end, SC was given a project brief outlining some of the scientific requirements of the project, as well as areas that were as yet unaccounted for. Chiefly, whilst the principle of using UWB signals for metal detection was workable under controlled lab conditions, it had not yet been successful in terms of detecting simple metal objects (such as a thin piece of wire) in simulations of 'real' conditions pertinent to the security applications the project was designed to inform (i.e. complex metal shapes concealed on human bodies in rucksacks or pockets and so on). Given this brief, SC was expected to design, build and utilise a laboratory setup (and accompanying data analysis

[63] One intended application of this work is as a security device and SC often framed his practical efforts against the 'real world' example of an airport security scanner, in terms of whether his methods and setup could be effectively used in this setting.
[64] This use of human bodies in the data collection process was, in fact, enabled by my presence as an ethnographic observer – having a 'spare' body in the lab gave SC the opportunity to develop new ideas for data collection which he had previously not been able to test, i.e., metal weapons (or at least convincing but disarmed substitutes) in various locations (i.e. pockets, rucksacks, in outstretched hands, and so on) on a real human body.

programmes and routines) entirely on his own. Although a supervisor was available for help should any be required, the primary supervisory contact did not believe the project would be ultimately successful – to SC's supervisor, it was emphatically *not* SC's abilities that were in question here, but the feasibility of the principle outside of controlled laboratory conditions[65]. As such, SC's work was done with little day-to-day input from his supervisor beyond this initial brief and formal and informal discussions instigated by SC (for instance, when he came across a particularly thorny problem, or was unsure of his interpretation of a dataset, and so on). For all intents and purposes, SC's practical efforts in data collection were designed and conducted by SC himself. SC was allotted a space in a shared laboratory in which to build and construct his setup (the component parts for which were funded through SC's funding body and purchased through the department of electrical engineering's acquisitions system), and could use the lab or office computers to process any collected data and work towards interpreting it accordingly.

Of the project itself, the distinguishing characteristic of UWB signals when compared with existing metal detection systems is that the wide frequency range accessible when dealing with UWB signals has the potential to allow for characterising specific objects as weapons or as benign and non-threatening (as opposed to merely detecting an amount of metal on a body). This is due to the UWB signal's particular array of reflections. As the signal is emitted by an electromagnetic emitter/receiver working at ultra-low frequencies, it hits a body (and any metal object on or near it) and bounces back towards the receiver, carrying information about the various specular reflections of any object it passes through. A metal object acts as a 'mirror' to any emitted signal and will consequently have a strong specular reflection corresponding to its particular size and shape (i.e. it will 'resonate' at a particular frequency that can be detected and recorded) and also have a 'tail-off' of late-time response (i.e. how the resonating object 'rings out' after the signal has hit it). The extra information provided by the late-time response element of the signal can be used to further distinguish between various objects – each object (a gun, a knife, a belt buckle, etc.) can be expected to have a unique (or at least identifiable) late-time response in addition to its strong specular reflection, with spikes at various different frequencies. In this way, the specular reflection and late-time response can be used to identify specific objects, and it is SC's hope that graphs plotted from data of this kind could be used to identify the size of various metal objects such that it might be

[65] Notably, this did not rule out any opportunity to produce a successful PhD. Indeed, in one sense, *proving* that the principle of UWB metal detection could not hold in practical usage (i.e. that complex metal shapes cannot feasibly be detected on complex non-metal bodies) would be enough of a result on which to base a successful PhD thesis.

possible to say that the signal shows, for example, a 5cm metal tube that may be the barrel of a particular type of gun, or a cluster of short lengths of metal between 2cm and 5cm which might indicate an explosive device filled with shrapnel.

However, various environmental variables affect the clarity with which the specular reflection and late-time response of an object may be detected. For instance, while it is relatively straightforward to 'see' a simple metal object like a short piece of wire in the chamber, the strength of the signal is decreased (i.e. the detector's ability to 'see' is obscured) by, chiefly, the complex shapes of metal that SC is attempting to detect (i.e. a deactivated starter's pistol and a mock 'bomb' made from a lunchbox filled with a variety of nails and screws and candlewax), the presence of large non-metal objects (such as a human body or a rucksack to contain the metal objects) which SC attempts to detect the metal against, and the orientation of metal and non-metal objects towards the receiving equipment (i.e. if they're facing the machinery directly, or side-on, and so on). Additionally, SC's lab setup – an open-cube 'anechoic' chamber constructed by SC himself out of plywood and sound-proof foam – is not entirely anechoic and picks up an array of ambient (background) interference from such things as the nails used to hold the chamber together and even the detecting machinery and cables which themselves are largely made from metal. Hence, the technical achievement SC is working towards is a system whereby a strong specular reflection and late-time response may be picked out from various complex data sources taken from 'realistic'[66] simulations containing both metal and non-metal objects (i.e. a human carrying the starter's pistol in their pocket, or carrying the mock 'bomb' in a rucksack on their back).

a. <u>The 'Real' Lab and the Instructional Lab</u>

One key point of contention that comes to bear from Delamont and Atkinson's (2001) account of postgraduate research is the idea that new research students only discover the difference between 'real' research work and instructional laboratory demonstrations as they begin to undertake research activities for themselves. By contrast, SC's work is built upon an *a priori* understanding of the differences between the two laboratories in a way that ensures his approach to research work is entirely shock-free. Although SC had, as any former undergraduate in science and engineering, spent time learning his discipline in instructional labs, his PhD project brief made explicit references to the various untested factors his work should aim to address. From the outset, SC was acutely aware of the areas in which there was no

[66] In terms of the kinds of thing an airport scanner, for instance, might have to deal with.

possibility of a demonstration from his supervisor or – there were certain elements of his research work that he would be the very first to attempt. Indeed, a sizeable portion of SC's work took the form of finding ways he himself could demonstrate the feasibility of the practical application of the principle *to* his supervisor. Lynch and Macbeth note of classroom science demonstrations that they are "a hilarious version of physics" (1998, 289), in terms of their ability to represent the real work of discovering research. It would be difficult to argue that anyone involved, teacher or student, could seriously believe that the demonstration is in fact an original scientific discovery (and indeed, in Lynch and Macbeth's (1998) studies, neither party treats the activity in this way)[67]. However, to criticise classroom science demonstrations for their inability to reveal anything about the 'real work' of scientific discovery is unfair – this stage-managing of results and the replication of existing scientific knowledge is precisely what makes them fit for purpose as demonstrations for students[68].

Lynch and Macbeth (1998) show that it is a mistake to equate demonstrative science with discovering science, and in this way, SC's activities seem a little less hilarious (although perhaps not entirely devoid of mirth). Whereas the project at hand is clearly designed to result in SC's learning-to-research (in that it was developed as a project suitable for a PhD student to undertake), it is nonetheless a piece of work where a successful outcome involves the resolving of certain issues that have not yet been investigated. The hoped-for and expected value of SC's project work lay in its being a decisive step from the sanitised 'lab conditions' to the as yet unrealisable 'real-work security application'. As such, the concern here was to introduce, in a small way, simulations of the kinds of real-world concerns that test the limits of the theoretical principle of UWB metal detection against a selection of environmental factors known to have a distorting affect on UWB signal detection. In this way, SC could engage with known theoretical principles that are proven to work under controlled conditions, and test their limits against 'real-world' simulations of his own devising, where SC could work to understand the impact and implications of his setup on the results achieved. It is the acknowledgement of the purpose of the project as fundamentally different from the classroom demonstrations of yore that enables SC

[67] Certainly, the students for whom the demonstration is intended to elicit an understanding have not yet seen the phenomena even if their teacher is well aware of what to expect, but this is, in part, precisely what characterises the setting as one of instruction rather than discovery. To confuse one for the other is to misunderstand the purpose of the setting on a fundamental level.
[68] One relevant question might be how could students of science at this stage in their education (i.e. where demonstrations are likely to take place) be expected to interpret original results from pioneering research work, without having 'learned their trade' beforehand.

to frame his activities in a way that specifically addresses the aims and objectives set out prior to undertaking them. As such, SC's research work forms a bridging middle ground between theory and application, allowing him to engage with both whilst learning how to do so.

b. From Bench to Desk to Books (and Back Again)

SC's research work was split between three main sites of activity: in the lab for data collection using the anechoic chamber and detector setup, at computers for processing and interpreting data, and in books to relate practical activities to existing theoretical principles and programming techniques. These activities were tied together by, and built up through, iteration. A round of data collection would provide material for interpretation at the computer, which may require further situating through consulting literature (i.e. to understand the reasons behind the results a particular graph has suggested, or to figure out how to write code that will address a pertinent problem). From here, the results of these efforts were fed back into the next round of data collection, for example, inspiring SC to change the orientation of the emitters and receivers in ways that might improve the setup's ability to detect metal on complex non-metal bodies, or to create new simulations and models to place inside the chamber and attempt to detect (i.e. if it was impossible to see a 7.4cm nail strapped behind a one gallon bottle of water, would it be possible to see a bigger object such as the starter's pistol?).

In light of the iterative nature of SC's research work, it is clear that a large part of SC's work was directed towards relating physical events (such as the author standing on the chamber holding a rucksack containing a bottle of water and a deactivated starter's pistol) to geometric or mathematical (and more) interpretations of the events that the data is made up from. For instance, given that relative position and angle of the signal emitters and receivers (and the angle and position of the 'mirroring' metal object being detected) has a direct affect on the strength of the return signal, SC is able to equate various features of the setup to their salient geometric properties.

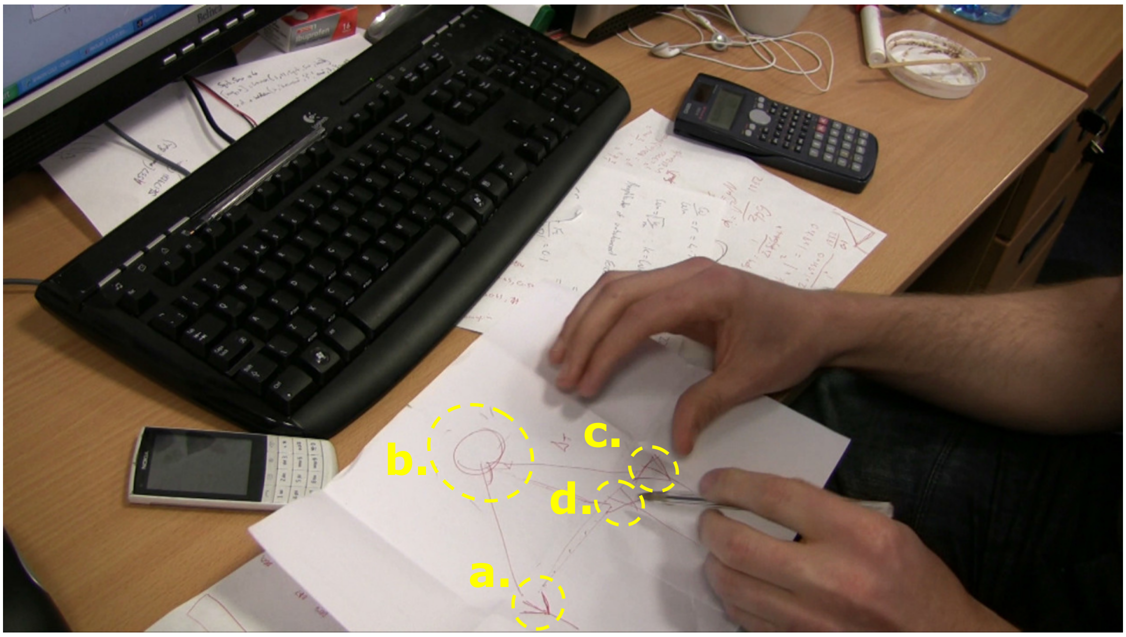

Figure 13 – Representing the anechoic chamber and detector setup geometrically and mathematically

In the diagram shown in figure 13 above, the emitter (labelled a) sends a UWB signal in the direction of the object, which is reflected off the object in the chamber (labelled b) – here, the object is treated as a one dimensional line, for ease of understanding[69]. The signal is reflected back omni-directionally (i.e. in all directions), but it is strongest at the reflected angle of incidence (i.e. the angle at which the signal hits the object originally). Of the two detecting antennae, one of these (labelled c) is positioned so as to receive the strongest signal as it is reflected back along the reflected angle of incidence[70], and it is this receiver that accounts for the bulk of the specular reflection (i.e. the strongest frequency of resonance that the signal produces from the metal object). In the physical laboratory setup, the other antenna (labelled d) does still pick up a portion of the reflected signal because of the

[69] SC recognises that the orientation of the metal object in other dimensions does affect how strong the signal can be received back from it, but for the purposes of thinking about the basic properties of the setup, this would be an unnecessary level of pedantry (especially so considering the difficulties SC himself has in measuring the effects of this empirically, it being highly contingent on a whole host of environmental variables).
[70] The term 'angle of incidence' refers to the angle at which the signal hits the object, and accordingly, the angle at which it is reflected back off it. Although SC recognises that the signals he sends (and their reflections) are omni-directional, this is used as a simplified model of a system which provides SC with the mathematical tools with which to begin interpreting his physical laboratory setup, and from there work towards furnishing that simplified model with increasingly complex explanations of relevant 'interfering' phenomena.

omni-directionality of the reflection, and it is this antennae that helps amplify the late time response of other resonating frequencies carried in the signal (through adding together the frequency response of both receiving antennae). SC's diagram is intended to display the basic geometry of the setup, which can be expressed mathematically as a number of triangles, a fact which SC uses to process the data in various ways. For instance, SC had had to incorporate the difference in length the signal travels to each receiver (i.e. the differing lengths of the sides of each triangle formed by the emitter, object and one of the two receivers), convert this into a time dimension (such that SC could ascertain the difference in time that each receiver received their elements of the signal, which is a function of the different lengths they have to travel) and converge those values through incorporating a time-shift into the equations used to process the data. Effectively, SC is using geometry to ensure that the signal can be understood as having been received by each emitter simultaneously, even though this did not physically happen. In this way, the geometrical properties of the setup feature as (simplified) explanations that inform the iterative process of research work as outlined above.

Another example of this tying together of different elements of research work can be seen when SC is interpreting a graphical output that compares a metal nail taped to the back of a water bottle[71] against 'background' data taken of the bottle of water without the nail (see figure 14 below):

[71] This is intended to approximate the same kind of distortions and obscuring that a human body would do to a metal object. In metal detection terms, human beings are not much more than oddly shaped bodies of water and can be treated as such. Hence, if a small metal object can be detected against a bottle of water, the same technique could be promising in terms of extrapolating its use out to more complex shapes and bodies that distort and obscure signals in similar ways.

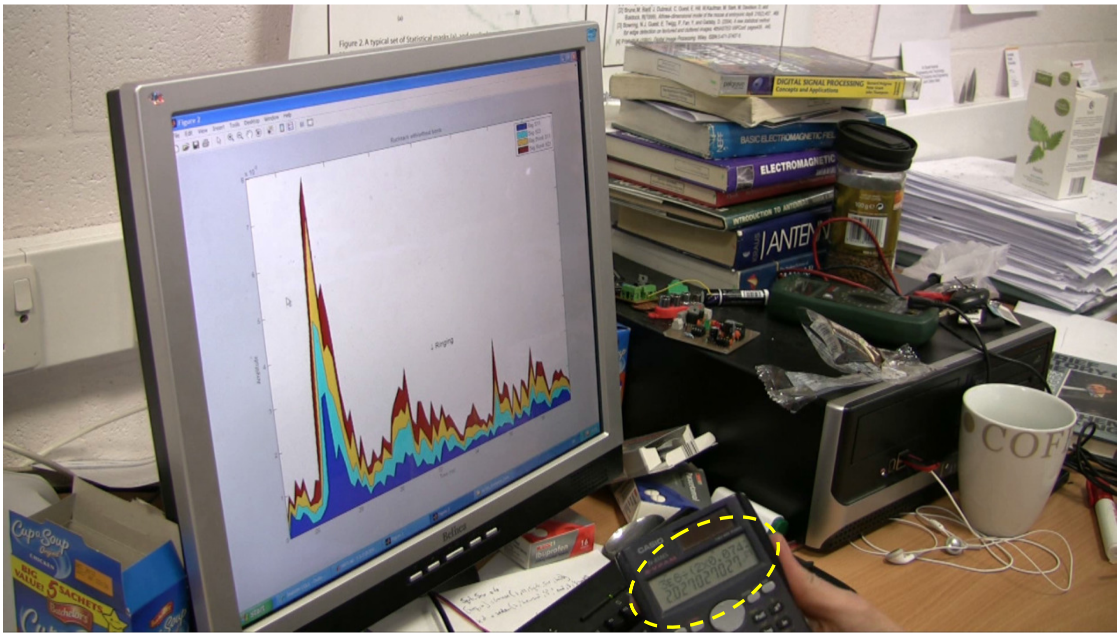

Figure 14 – Working out the expected frequency of the specular reflection of a nail of given length

Here, the graph displays two bluish areas (each of which represent a 'picture' of the bottle of water as taken from the perspective of one antenna), and red and yellow areas (which represent a 'picture' of the bottle of water with nail, again from two different antennae). The *x* is the time in nanoseconds, and the *y* axis is the amplitude of the return, such that this graph shows how the strength of the return signal as it travels (in time, as well as in distance in metres, which is an easy calculation to infer from the given data) to and from the emitter and receivers[72]. The graphical output programme SC has written also produces a graph not visible in figure 14, where the *x* axis is frequency and the *y* axis is amplitude, such that the graph shows the spread of the overall frequency return for the entire time the receivers were collecting data of the given object(s). In the graph visible in figure 14, there is a clearly visible prominent spike – the low-frequency specular reflection as it first hits the object (i.e.

[72] It may be difficult to see with any clarity on the image provided (which has been made smaller so as to fit into the text appropriately), but the labels SC leaves in the graph do not in this case relate to the actual data being worked with. Although SC is in fact comparing data of a water bottle against a water bottle with a nail taped to it, the legend on the top right of the graph has labels that relate to a previous processing of data of a rucksack and the rucksack with the mock bomb in it. Similarly, the text towards the middle of the screen – "↓Ringing" – has no relevance to the current interpreting and analysing activity and is a vestigial coding artefact from a previous run-through. These remain in the current graph only because they're unimportant to the actual analytic work, and would only hold significance if SC were to show these to anyone else such as a supervisor or as part of a publication (at which point they would have to be altered or explained away).

the first thing in time that the signal hits) – and some less-easily-picked-out spikes as the signal continues to 'ring out' (i.e. resonate) at higher frequencies. After having identified these particular relevant features of the graph, SC is keen to work out where the frequency of the specular reflection of the nail *should* lie as according to theoretical principles, in order to see exactly to what extent the obscuring water bottle has affected the frequency return of the signal. To do this, SC takes the salient feature of the nail (its length, which had been previously measured to be 7.4cm), and uses this value perform a calculation that transforms this value into an expected ideal frequency return in gigahertz. This calculator computes a value – just over 2GHz[73] – that can be compared against where the prominent spike on the frequency-amplitude graph (the second graph, not shown in figure 14) lies, and in doing this SC notes that there *is* a prominent spike at approximately 2GHz on the graph. Hence, it is possible to conclude from this data that the bottle of water does not overly obscure the detection of a relatively small metal object, and this has implications on his next round of data collection, for which SC decides to further test the limits of UWB metal detection against increasingly complex bodies.

These two examples show the routine back-and-forth between practical data collection, the analytic work of interpreting graphs, and the theoretical work of using geometry and mathematics to evaluate the work in terms of its fit with known, ideal and expected values. What can be drawn from these examples is the idea that literature and theory are *complementary* to research work, rather than *supplementary* as they are often portrayed in enculturationist accounts. Hence, at this stage of postgraduate education, textbook knowledge isn't *replaced* with practical skills or tacit understanding, they cyclically inform one another. Indeed, the very value of SC's particular project is the combining of the two – to test the pure theory realised in controlled lab settings against messier and more environmentally realistic conditions, which hasn't yet been achieved. Inextricably linked, the moving back and forth between these three elements of research activity constitutes the work at hand – they are not simply frustrating and irrelevant asides that have to be dealt with, they *are* the work of researching at this level. Moreover, conducting research in this way enables SC to simultaneously learn more about how to do so.

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[73] Given the limits on the size of the image as it is represented here, the calculation (highlighted in figure 14) is:

$$3 \times 10^8 \div (2 \times 0.074) = 2027027027$$

The 0.074 value equates to the size of the nail in metres, and the answer is in units of Hertz (Hz). Hence, converting to Gigahertz (GHz), the answer is to be understood as 2.027027027, even though SC does not perform this conversion using the calculator.

For instance, SC's interpretations of his data and how well they fit with ideal theory provide cues as to what SC should do next when collecting more data – he can identify, in the graphs, problems relating to the setup and its ability to 'see' metal on various objects, and conceptualise ways to work around these for the next round of data collection. This facilitates his work as an endeavour in learning, where he can draw inferences and conclusions from his data that relate not only to the principles at hand, but to *how he is to address them to achieve success*. Hence, working through these ideas, SC is, unproblematically, able to take small steps towards becoming adept enough at the research skills required for successful electrical engineering research, as well as simultaneously conducting this kind of research.

c. <u>Working With and Writing About 'Real' Research</u>

One common theme in enculturationist accounts is postgraduates' usage of 'creative solutions' to write out failures from their final reports, deceiving whoever reads those reports into thinking that the research process has been free from error[74]. However, there are some features of SC's work that do not support such a claim in any way, and throughout his project, SC utilised failed results as an impetus for improving his practical hands-on laboratory experimentation. One instance saw SC analysing a recently produced graph of a nail suspended from a washing line hung in the anechoic chamber[75], and postulating reasons as to why the results did not correlate with theoretical predictions:

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[74] Interestingly, these creative solutions are also posited as part of cyclical process that *generates* 'reality shock', in that new researchers in the field read such reports and therefore expect their own work to proceed in ways similar to other researchers' work as outlined in papers. As Delamont and Atkinson note of successful doctoral students:

> As professional scientists, they learn to write public accounts of their investigations which omit the uncertainties, contingencies and personal craft skills. This leaves the next generation of doctoral students potentially facing the same reality-shock when their own research endeavours do not succeed. (2001, 88).

[75] This particular arrangement for taking data was used as the most basic test of the setup, and when experimental tweaks were made to the detecting equipment (i.e. re-positioning the antennae), SC found it useful to begin collecting data with this benchmark event and from these results, work towards further improving the current arrangement's detection capability.

Transcript 1 – Reasons as to why data of a nail might not be 'spiking' at the expected frequencies

SC: What [frequency] was we looking for? [Picks up and uses calculator] Three over...two times....nought. Point. Seven. Four... Just over two gig...

SC: [Looking at the graph and zooming in on the area around two GHz] Hm... s'not quite there...

[Long pause]

SC: Hm... There's a bit of a shift down in frequency because of the thickness of the... nail.

PB: Yeah.

SC: If it was a very thin piece of wire it'd be almost... on the money, you know?... That might be the cause of the little shift down. The broader *that* [the spread of the spikes of frequency around two GHz] is, the longer the lifetime of it.

Here, SC can clearly see that the number on his calculator diverges from the graphical data on-screen, and is forced (so as to advance the project) to come up with a reason as to why this might be. The existence of an error of some kind is clear, and SC posits one possible reason as to why – the theoretical calculation treats the nail as a one dimensional length whereas in reality it has three dimensions, and this has a known effect on how the signal propagates through it. Hence, the two unmatchable results (i.e. calculator and graph) are given a reason that unproblematises and justifies SC's lab work with real components (as opposed to abstract theoretical point-objects and vectors). Another example can be seen in SC's use of failed results as a guideline for future work:

Transcript 2 – Failures feeding into future activities

SC: [Runs a programme to produce a graph. Pointing to the legend of graph] So them two are *nail*... And that one's water bottle.

[Long pause while looking at graph]

SC: There's no way of *seeing* it.

[Long pause]

SC: We'll have't try it again with something bigger... Strap the *gun* to the *bottle*.

In this instance, the results are unrecoverable – there is no way of *seeing* the nail against the water bottle under the current setup, in that there is not enough of relative difference of amplitude (i.e. strength of signal, or prominence of a 'spike' at certain frequencies) in data of a one gallon water bottle as compared with data of a nail taped to the back of the bottle. Whereas the water bottle and nail arrangement might be expected to produce a noticeably stronger signal return, it does not, and based on this graph, SC concludes that the water bottle obscures the nail almost entirely (or at least enough so that the presence of a metal object could not be definitively claimed). However, this failure does not inspire outright dejection in SC – the problem is not so much with the principle itself, but with the type of data SC has chosen to take in this case. Hence, the principle of UWB metal detection is still workable (or at the very least, not disproven) and better results might be realised by experimenting with the detection of larger metal objects on the water bottle, and SC resolves to go back to the laboratory and test out these ideas, continuing the iterative research work.

What these examples show is that failures and problems are *not* covered up by SC – indeed there is *no need* to attempt to do anything like this, because failures and problems are a vital part of getting the project to work. Unarguably, problems do occur, but SC treats these as opportunities for dealing with the 'messy' trial-and-error work of experimentation, taking these occurrences to be positive issues that are necessary and inevitable elements of a successful research outcome. Moreover, this orientation to project work is entirely acceptable for the dual purpose(s) at hand – producing a PhD thesis and producing valid, publication-worthy research. Neither of these endeavours aims to pedantically chronicle each movement in and outside of the laboratory, and hence, any critique of postgraduate research on these grounds displays a fundamental misunderstanding of the surrounding context of the events under investigation, and it is difficult to take any misunderstanding as sizeable as this very seriously at all. As Greiffenhagen and Sharrock note:

> the temptation is to think that a mathematical paper is a *misrepresentation* of what happens in the 'back' [i.e. the work done to produce the findings of the paper] is based on a view that treats the paper as a description (in the form of sociological or historical report) of the discovery process, rather than as a presentation of the relevantly interrelated mathematical matters. (2011, 858).

In this sense, SC's supposed covering up of his failures and problems does nothing to the accuracy or inaccuracy of his accounts of them, because no such account is ever attempted![76] By contrast, working through problems and failures to make a principle work better is a necessary (and positive) element of experimental work. It is,

[76] Indeed, Greiffenhagen and Sharrock characterise accounts that think otherwise as only "as wise as thinking that Hollywood blockbusters ought to be viewed as documentaries about their own production" (2011, 858).

however, *not* part of a theoretical account of that same work, and it is not a 'creative solution' for SC to exclude failed aspects of the experimental work from any final written account of the principle or the project designed to work towards fully realising the principle. Under no conception are SC's failures part of the phenomena or principles he is dealing with – the fact that they are known as 'problems' or 'failures' (in that they do not match with SC's expectations or theoretical predictions) suggests that they are far removed from it, they are *not the same*. Rather, these aspects of the work are the necessary, inevitable, sometimes tedious, always contributory to, features of project work combining experimental data collection (and all the environmental variables that affect it) with theoretical accounts of the principles under investigation.

Astrophysics

The argument now turns to the second project under discussion, which was a piece of masters-level research in astrophysics undertaken by an early-stage researcher called HR. To reiterate for present purposes, HR's work was titled 'Gravitational Lens Discovery and Investigation Using Optical and Infra-Red Surveys', and was situated in a computer lab populated by a dozen or so other early-stage researchers (both pre-doctoral postgraduates and PhD candidates), each working on their own individual projects. HR worked under a similar supervisory model to SC, and so his work was handled in a very similar way – most of HR's problems were to be tackled alone, although help was available via (infrequent) email exchanges for occasions when HR had need to query whether or not his efforts were in line with the goals and objectives outlined in his project brief. Although HR worked in a room with peers involved in similar astrophysics projects, there was remarkably little interaction between them in the course of their astrophysics work. The conversation and interaction that did occur between HR and other early-stage researchers was limited to talk unrelated to astrophysics – for instance, short conversations concerning world cup matches and the difficulties in arranging travel to a conference. As such, HR completed the substantive (scientific) work on his own.

The premise of HR's project was to develop a programmed (and automated) technique for detecting gravitational lenses[77] using numerical data collected by a terrestrial telescope in New Mexico, USA (as part of the Sloan Digital Sky Survey, which makes its data publicly available online). A basic programme to represent (and

[77] Gravitational lenses are a phenomena in astrophysics whereby optical light and other electromagnetic radiation from a distant galaxy (or other 'bright' object, such as a quasar) is 'bent' around a high-mass (i.e. gravitationally strong) object nearer to us in our line of sight.

process) the numerical data as images had already been prepared prior to my videoing, and one such image can be seen in figure 15 below:

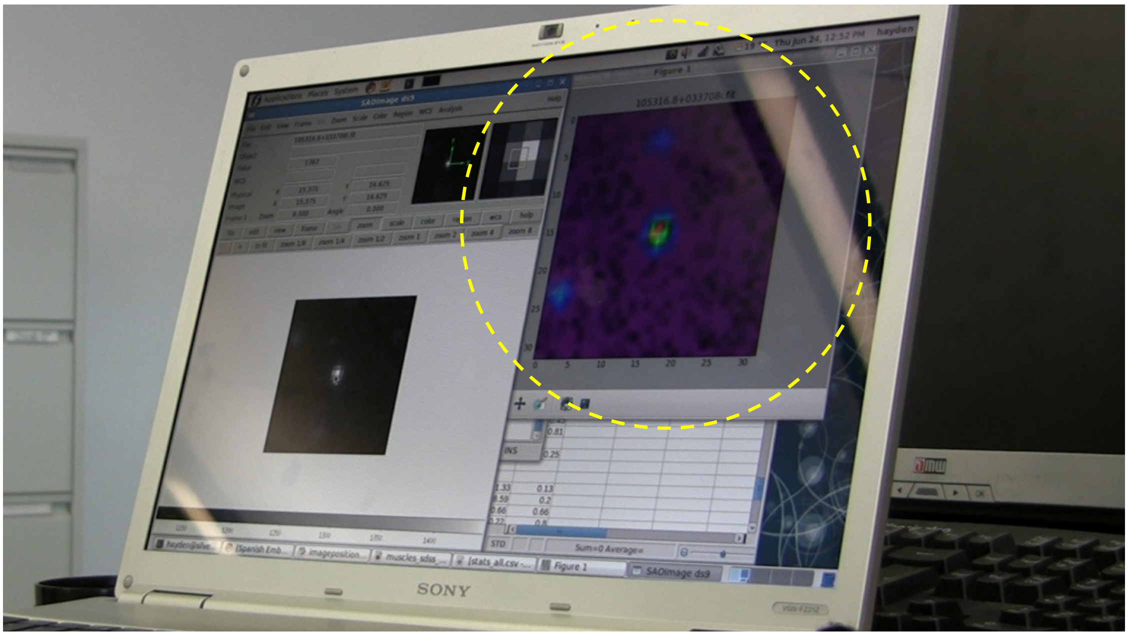

Figure 15 – An example of SC's on-screen resources

As can be seen in the highlighted area of figure 15[78], the images being processed have one or more clear peaks of radiation density – 'brightness'[79] – marking out various cosmological objects (the brightest points of which appear in red), against a dark (purple to black) background of empty space. The interest in these images, for HR, is if there is any identifiable 'interaction' between two radiating objects, which would suggest that the gravity of one or both of the objects is 'bending' the radiation produced by the other, which indicates that the image is of a lensing system. HR's wider project is to investigate the possibilities of combining multi-wavelength optical and infra-red surveys to better detect gravitational lenses, using this dataset to test both his programme's capability and find new lenses as a taxonomical exercise. As it stands on the video data captured, HR has already made a first attempt at this programmed technique, and is able to detect lenses to only 80% accuracy[80]. HR had

[78] Readers may also find it useful to refer back to figure 2 in chapter four, to review the on-screen features that are of particular interest to HR.

[79] Although of course only the visible light produced by objects would make them bright to our eyes – the image processing program written by HR here *makes* various types of radiation density artificially bright against an artificial dark background, for the purposes of making them visually available for work.

[80] This is to say that the computer was unable (given the available information) to make a decision as to whether an image represented either a lens or a non-lens in 20% of cases, on the grounds that neither possibility could be rejected.

however also written a further programme[81] to allow him to provide the images with some referential manual input to work the technique up towards being semi-automated, such that the programme would be able to automatically process large data sets with a minimal amount of manual human input. This instigated an effort towards the semi-automation of the technique, whereby HR could provide the computer with better information as to where the peaks on each image could be found. Practically, this involved developing a programme that allowed HR to record the coordinates of the two peaks most likely to be elements in a lensing system[82] by first clicking the left mouse button on the primary peak, second clicking the right mouse button on the secondary peak, then third using a command (keystroke [n]) to move on to the next image. For visually ambiguous cases, a 'clarifying image' could be called upon to help HR identify what he was seeing on-screen (see image above) – often, seeing the same image taken in another wavelength could clarify where exactly the peaks on each object lay (for instance, optical light can be obscured by dust and debris between the telescope and object, but infra-red can penetrate through giving a clearer picture). The day's work captured was HR providing this manual input to a series of images, checking to see how the results looked afterwards and if they suggested any further work needs to be done.

a. Project Design and Learning Assurance

As with SC's work in electrical engineering, there are several features in HR's astrophysics programming work that stand in contrast with ideas of enculturation and reality-shock. For instance, the very design of HR's project comes to bear, in that it is created such that it can be managed by HR as both a piece of learning and piece of research. Lynch and Macbeth talk of classroom science demonstrations as being activities in their own right, with science as 'thematic to the assembly' (1998, 289). This applies to HR's programming work to an extent, in that programming problems are not necessarily scientific ones, and are treated accordingly. HR's programming problems can be treated as *technical* issues, which will either work or not work but in visible and diagnosable ways – if the programme has an instruction it can't handle, it

[81] Both programs were written in a programming language called Python, although both incorporated script from various other software packages, most notably AIPS (Astronomical Image Processing System).

[82] It is worthy of note that HR was able to and did visually identify the majority of images in terms of whether he himself could see a lensing system or not. Crucially though, he endeavoured to process all images as if they *might* be of a lensing system, since the project brief was essentially to see if a *computer* could tell the difference. Indeed, these visual identifications came to bear later on, when known (visually obvious) lenses and non-lenses were compared case-by-case against the results produced by the computer so as to understand how the computer dealt with some obvious (and some ambiguous) cases.

will say so one way or another. For HR, programming errors are more 'findable' and 'handleable' than abstract scientific ones, in that they are contained within a programming environment with a programming language to structure them – this can be seen in the elements of HR's work related in the *Visual Diagnostics* section of chapter four (page 73). Of course, the 'science' of the problems features as an inextricable element of the work, though it is filtered through the programming language into something that is more easily worked with. Moreover, this is not just a feature of *early*-stage research done as a means of educating future professionals in the field, since programming and computing issues are increasingly paramount in science across all stages (Knuuttila, Merz and Mattila, 2006). Thus, if HR can treat his problems as programming problems, the issue becomes one of whether he can learn to use a programming language to code and act out his ideas or not. And having 'mathematised' the problem in such a way, HR can then relate any programming problems arising throughout the course of the work back to the 'real-but-conceptual' field of scientific phenomena and principles the project aims to deal with.

Given this assurance of HR's learning through how he has designed his own work activities, it is unclear as to how his efforts are in any significant way related to enculturation, dissonance or reality-shock. As with SC's lab work, HR's tasks and problems are to be taken on individually, and the decisions he makes concerning how to proceed rely on a resources available within the work itself (i.e. properties of the visual images on-screen, the fact that the task is contained within a strict self-written program that dictates which mouse-click or keystroke to do next, etc.). This fact seems to limit the effectiveness to which the concepts of enculturation, dissonance and reality-shock might be applied here, in that HR does not rely on cultural integration and communication to provide solutions to his scientific troubles, but draws on features contained within the work itself to guide his further efforts. Rather than explaining HR's work by reference to the presence or absence of the kinds of cultural factors posited by enculturationists, what we find is that the 'science' of the task becomes a resulting achievement built out of the more practical and hands-on work of programming and working with programmes. The point is that in undertaking the work in such a way, HR is *not* doing mere technical drudge-work, and neither is he indulging in 'creative solutions' to work around having to produce genuine scientific research. What he is doing is working through a small but manageable programming task designed specifically to be a masters-level piece of education and research. And of course, the fact that HR's objectives are to be achieved through programming does not mean that he is not engaging with the task scientifically – the programming and the science are one and the same. Rather, HR is starting small at doing real valid scientific work, in a setting where projects require no more than precisely that.

b. Managing Learning Through Preparation

Given then that HR's task is one of programming, one next question might focus on how that programming itself is an achievable task. It is clear from HR's work that there is a significant amount of effort put into *making* it a feasible endeavour, in such a way as to allow HR to rely on the features in his work to guide his further progress through it, rather than solicit the aid of others. One such way in which HR makes his programming work achievable is through incorporating navigational devices into its structure. HR's master programme contains a vast amount of lines of code performing lots of different kinds of function, some of which even refer to documents outside of the programme itself (i.e. databases of 'raw' numerical information from the SDSS, databases of peak coordinates as established by manual input, and so on). The sheer amount of script in the programme necessitates some strategy for navigating through and around it, so as to facilitate its development and any troubleshooting exercises arising through the course of that development. The use of comments – pieces of typed information positioned within a programme but that do not form any functional part of that programme – becomes a crucial activity here, and many previous studies have noted some of the ways in which comments are used to structure programmes (e.g. Button and Sharrock, 1995; Martin and Rooksby, 2008), chiefly how they are used to label specific sections of code. However, for HR, comments also have a cartographic role of situating code within the programme, which is exemplified by one of HR's (many) comments:

```
#now mask out a few pixels around this peak position, to detect the second peak
```

At this point it is valuable to unpack a few of the things that being able to see this comment at a specific point in the code does for HR. This comment, most obviously, labels the section of code following it by its function in the overall scheme of the programme. However, this comment also serves a purpose in that it relates to some piece of code that must be *after* the section that deals with finding the *first* peak on an image, and depending on what coding task he might be looking for, the comment also indicates approximately how far before or after he will have to look from here[83].

[83] This is, of course, approximate, in that the units of 'how far before/after' are given only in distinct coding tasks, which are of course of varying sizes (i.e. the number of lines on a page that that task requires in code). Nevertheless, knowledge of the program and the programming language can give better definition in terms of

These navigational comments, as well as other techniques, find use as prospective diagnostic aids – HR knows his programme is unlikely to work as planned on its first run-through, and therefore invests effort in making it easier to locate and work with problems when they do arise. As such, any techniques that help HR to understand how to work with his programme, such as these, contribute towards the feasibility of the project both as a piece of research and as a piece of work completed for educational purposes.

c. Problem-Solving With 'Common' Resources

In addition to preparing the work so as to make it possible, HR is also able to draw on a variety of 'common' or 'common-sense' resources that complement a burgeoning scientific 'know-how' during the course of that work. One of the aspects of HR's work captured on video concerns a mistake he makes with processing an image – essentially, he clicks in the wrong place – and only registers his having made the mistake after having cued up the next image (see the discussion surrounding figure 5 in chapter four for further details). To reiterate this story for present purposes, in this instance, HR has mistakenly processed an image, clicking on something that isn't a peak, which will affect the results that the computer will ultimately be able to draw from that image[84]. He moves on to the next image and only then realises that a mistake has been made and that he will have to return to the image within the programme to sort it out. This, however, is not so easily done – to return to this image, HR has to go outside of the image processing/manual input programme and into the master code screen to edit the master programme to start the image processing again from the image he wants to re-do. Practically, this is done by changing the value of 'i' – shorthand for image – to a value that matches the position of the image he wants to look for in the sequence of the whole cohort of images, and HR changes the value of 'i' to 309. However, this value of 'i' recalls the image *after* the one HR wants – he has got the value of 'i' wrong. However, using this information about what image `i = 309` brings up, HR is able to work out that

knowing how many tasks lie between the current position in the program and the desired position, and whether those tasks are relatively complex (i.e. built from many lines of code) or simple.

[84] Such a mistake may cause the computer to classify an image wrongly (i.e. as a lens when it is not a lens; as a non-lens when it is a lens; as unclassifiable when it should be classifiable, or; as classifiable when it is not classifiable). However, the mistake may also affect the statistical certainty to which an image can be classified, such that even if the classification matches HR's visual judgment on whether the image is of a lens or a non-lens or is ambiguous, it may be presented in the results as a (statistically) stronger or weaker lens than it is, and this is not necessarily something HR has the capacity to judge visually. Hence, it is important to attend to any such mistakes.

value of ‘i’ he *did* want must have been *one before* 309, and HR runs the programme again with this edit (`i = 308`), bringing him back to the image he wants to re-process.

Here, HR uses an understanding of how images have specific positions in a sequence, and he uses visual properties of those images to work out where in the sequence he needs to get to (relative to a current position). The line of questioning develops according to the resources available on screen and how each question might be answered – first, does the recalled image (`i = 309`) look like the one he wants? It does not, but does it at least look like one he recognises? It does, so can he pinpoint where in the sequence this unwanted image is and thereby work out the relative position of the desired image? In this instance, HR again makes use of various commonly available resources as part of the task-at-hand. These resources, however, are not ‘tricks’ or ‘creative solutions’ with which to simplify and get around the problems of scientific research. Such resources *are* the activities of scientific research, and can be found in any and all scientific work[85]. Commonly available resources such as these make scientific work more achievable as a practical task, but they are also activities that can be easily managed by students in that they can draw on concepts that feature in areas not limited to the technical remit of science (concepts such as counting, sequentiality, and so on). For HR, these concepts are used to make the work achievable – these are the ways in which HR becomes progressively further ‘enculturated’ into scientific practice. Embarking upon such activities allows HR the opportunity to acquire practice with using common resources to achieve scientific goals himself, as professional scientists do, and to experience first-hand a little more of the sorts of work that make up the broader enterprise of scientific research, which is, perhaps, the very point of early-stage research projects in science education.

Concluding Remarks

Undergraduate students might conceivably learn dependently in such a way as to avoid encountering the practical concerns of ‘real’ laboratory work, and professional scientists might conceivably no longer depend on learning in that way for their

[85] This much, the argument presented here agrees with the enculturationists on. The difference seems to be that while the approach adopted here is happy to accept these activities as scientific work, other approaches have implied a critique of such activities for being too ‘informal’ and too ‘messy’ (i.e. Bloor, 1996; Collins, 1985; Mackenzie, 1993; etc.) to be capable of producing scientific knowledge, such that the only explanation for the formalised fundamentals in scientific knowledge must be as a misrepresentation or even a deliberate deception that excises the practical work performed to delete it from the historical record.

research. However, if sociology is to provide an account of those early-stage researchers that occupy the middle ground between these two groups, it is crucial for sociologists to remember what these people appear to know already – that science education is a distinct activity aside from scientific research. The goals of these activities are not the same, and students, scientists, and those in-between know this very well[86]. If the classroom demonstrations that Lynch and Macbeth observe display only a "a hilarious version of physics" (1998, 289), this is no grounds for decrying the whole endeavour of classroom demonstration as ineffectual – their point is to take demonstrations as activities in their own right – not as experiments for research, but as demonstrations for classrooms. Hence, it is patently not sensible to evaluate activities that are not professional exploratory scientific research against different success criteria to the purpose they were intended to fulfil (in the case of the present chapter, early-stage research projects designed as postgraduate qualifications). This bears a relation to Anderson's (1979) comments on how to interpret classroom 'agenda' (i.e. lesson plans written by teaching management staff) when working to understand educational settings. Anderson notes that while it is tempting to conceive of agenda as *descriptions* of ensuing lessons, this is not necessarily helpful to any sensible evaluation of those lessons. Anderson's goal is to:

> show that even a cursory and gross inspection reveals a web of features which could well destroy, divert, delay or dirty any agenda, with the obvious implication that a curriculum innovator who is interested in the fate of his product [an agenda] at the hands of his teacher customers should do some research at the classroom counter. (1979, 46).

As such, when the worth and effectiveness of lessons is stacked against the written agenda, it typically falls short, but this simply shows that our evaluation criteria is at fault (and that, as Anderson (1979) suggests, we might do better with a focus on the practical activities of the classroom).

Arguably, it is confusion over this point that accounts for a lot of the problems this chapter finds with enculturationist studies, in that the very need to utilise such concepts as reality-shock and dissonance as the sole explanation for success and failure at this level of education demonstrates a presupposition that the problem with undergraduates moving to postgraduate study is that they believe that textbook

[86]This is to say that undergraduates do not expect that their laboratory demonstrations could possibly result in new scientific findings, and professional scientists no longer work towards attaining a qualification on a taught course and the forms of learning that such an endeavour requires. Put simply, students do not worry they are not discovering new phenomena, and professional scientists do not worry about cramming for exams.

learning and independent research are one and the same thing. If this is not the case – and the examples presented throughout this chapter suggest that it isn't – then it is difficult to find an area to which the concepts of enculturation, dissonance and reality-shock might in fact apply.

Undoubtedly, early-stage researchers do involve themselves in social and cultural relationships – this is undeniable. What this argument takes issue with is the idea that this is *all* early-stage researchers do. These activities only become recognisable as science education and research when they are brought back together with the technical context surrounding them. As Turner notes, "The point to be made is that the 'technical' cannot be divorced from the 'ordinary', and that their relationship invites not criticism, from either side, but empirical investigation" (1974, 9). Perhaps a more rounded setting can become apparent when considering these two activities under the single umbrella of 'postgraduate education/research' (or, as it has been referred to throughout, early-stage research) and what this chapter has aimed to demonstrate is that whilst one approach might be to explore the differences between the cultures of undergraduate and postgraduate/professional science, this may lead to misunderstandings as to how postgraduates come to deal with their research projects successfully. A better idea of how learning can proceed in tandem with research (see Roschelle, 2008) can be realised through examining not only social and cultural relationships, but also the resources available and used within the work itself throughout the activities that make up the enterprise. Doing this, HR's and SC's activities are akin to those of Kuhn's (1971) 'normal' scientists – they are not doing context-free explorations; rather, they are working within set parameters, boundaries which are further tightened through supervisory guidance and the setting of definite project goals and techniques by which to achieve them. HR and SC are both given a set of conceptual conditions – their theory and mathematics – that for their purposes can be taken as granted, and this enables them to embark upon productive, yet conceptually fairly unrevealing, laboratory tasks.

This understanding of HR's and SC's activities puts forward the case that reality-shock and enculturation have little to do with the success of a postgraduate student, or at least that these are not their only concerns and that a better cultural awareness is not the only answer to their problems. To reiterate a quote from Garfinkel *et al.* that is particularly salient here, "Situated inquiries are practical actions and so they must get done as vulgarly competent practices" (1981, 139). In this sense, HR's and SC's work takes shape as the further addition of more and more technical (conceptual and practical) knowledge and skill to their already existing bodies of 'ordinary' knowledge – of programming, of conducting experiments, of working through mathematical problems, and so on, all of which are ordinary activities to science at least, and are well-known (if not well-practiced) to students and professional scientists alike. The activities that make up the work of early-career research can draw on resources that *everybody* has access to and knowledge of (i.e. comparing, interpreting, isolating variables, etc.). Furthermore, these resources inform how the learning and the research takes shape as one cohesive endeavour, giving students the know-how necessary to understand when it might be time to depend on a textbook or how to independently design their own sub-tasks and so on. These activities are *designed* (by both supervisors and students themselves) to be feasible, in that their goals can be accomplished with resources freely available to students, and perhaps this is the very idea of this level of training – to give students a few more opportunities to deal with these resources and this work, in settings and on projects where the possibility of doing so is assured in advance.

With this in mind, the argument can now return to the enculturationist studies mentioned previously as the accepted approach in the sociology of science education. Given the plethora of resources available within their work itself, it should be no surprise that early-stage science researchers are perfectly capable of working on their own, without having to spend much (if any) time and effort on enculturation. What this chapter hopes to have achieved is to present a few of the practices of students undertaking these types of research projects with the aim of questioning where exactly the dissonance or reality-shock is in them (and accordingly, what role enculturation might play in them). Throughout the time spent examining the work of these students, there has been no evidence of any possible in-road for dissonance or reality-shock in the way they approach their work – indeed, students can rely on features both within the work itself as well as ordinary 'common-sense' features non-specific to the technical field of scientific research to guide their work. The question then is, what *use* are these enculturationist concepts in describing scientific work? Essentially, the issues raised throughout boil down to a fundamental question for studies of postgraduate science education: what is the nature of the thing we are talking about? Many have characterised these settings as particular cultures with their own mores and norms, and this approach has been hugely instrumental in the development of a coherent programme of study around the field of science education. However, it has been the modest goal of this chapter to demonstrate that there are clear and significant benefits to be seen through engaging with scientific knowledge on its own terms, so as to understand the reasons why such cultures might arise around thematic work concerns in the first place. A still smaller goal has also been to simply acknowledge the 'learning-to-research' stage in education as distinct and interesting step for students but one for which sociology hasn't yet given the right kind of treatment, and to take the first small steps towards giving it something more like the attention it warrants.

CHAPTER SEVEN: Conclusions

In light of the aims and objectives defined at the outset, this thesis has worked towards a re-specification of three central themes in the social study of science – the impact (or otherwise) of cultural interaction on scientific research; the role of representation in the generation of knowledge, and; how education prepares students in scientific (or related) fields for the day-to-day life of research work. This is the result of an argument that is worked up over various stages, beginning with an introduction to the research discipline of ethnomethodology and how those principles might be applied to the study of the work of early-stage researchers and their usage of CRTs such as programming languages and machines with software attachments (chapter one). This endeavour is achieved to its fullest through the adoption of a strong unique adequacy requirement, which orients ethnomethodologists to the same conceptual descriptions as members of settings themselves – for present purposes, the unique adequacy requirement allows ethnomethodology to 'speak the same languages'[87] (ordinary and technical) as astrophysicists and electrical engineers. All of this provides ethnomethodology with a unique orientation to the empirical study of the order and organisation of scientific work, and one which retains the contextual backgrounds of that work whilst focussing keenly on the activities at hand.

The argument then proceeds, in chapter two, to situate this ethnomethodological approach to scientific work as part of a wider canon of social investigation which is predominantly informed by the various 'flavours' of works in the Sociology of Scientific Knowledge (SSK) – namely, the Strong Programme, the Empirical Programme of Relativism (EPOR), and Actor Network Theory (ANT). This begins by charting the origins of these ideas in (and as reactions to) the work of Mannheim and Merton. Where Mannheim and Merton were seemingly reluctant to tackle scientific knowledge itself (choosing instead to focus on either 'non-universal' types of knowledge (Mannheim, 1960 [1936]) or failed attempts to create 'universal' scientific knowledge (Merton, 1957, 1968, 1973)), the key proponents of David Bloor's Edinburgh-school Strong Programme (and their Bath-school 'rivals', the EPOR, spearheaded by Harry Collins) made it their explicit aim to find a way for sociology to deal with just that. This was achieved through the proposal and uptake of constructionism as a unifying theme throughout the sociological study of knowledge, which held that all knowledge consists, possibly entirely, of various social and cultural conventions and relationships. Hence, to the Edinburgh and Bath schools, the

[87] Although of course, the aim is not merely to adopt a faux-familiarity with the sorts of technical terms used in scientific (and related) research. Rather, the ethnomethodologist's goal is to acquire a *genuine understanding* of them.

analysis of these social and cultural conventions and relationships was a means to understanding the content of scientific knowledge, and this idea informed several key early laboratory studies (a good example of which would be Collins' (1985) study of how scientists learn to replicate TEA-laser setups). However, this singular focus on the supposed constructed nature of knowledge was taken by Bruno Latour (and ANT compatriots such as Michel Callon) as a failure to ask more interesting and relevant questions along the lines of "Is it [a given piece of knowledge] *well* or *badly* constructed?" (Latour, 2005, 89), and consequently, ANT inspired its own corpus of empirical studies (e.g. Callon, 1986) that aimed to address this (slightly) new question. However, chapter two proposes that the Strong Programme, EPOR and ANT share an identifying fundamental characteristic: an essential and destructive 'irony' that seeks to wrest explanations and descriptions of scientific activities away from science altogether.

Chapter two proceeds by positing a different (ethnomethodological) understanding of the philosophical ideas underpinning Bloor's constructionism, which are found in the work of Ludwig Wittgenstein. Taken alongside the work of one of Wittgenstein's successors, Peter Winch, it becomes clear that context-sensitivity is crucial to any sensible accounting-for of any members' activities, and attention is paid to how ethnomethodology broaches this through a praxiological approach which aims towards describing features of the orderly conduct of activities. Hence, members' – in this case, early-stage researchers in science and related fields – activities are understandable as the 'ordinary actions' of science[88]. This brings the discussion to centre on the methodological concerns pertaining to how it might be possible to observe and understand such 'ordinary actions', and chapter three outlines some key issues relevant to the doing of an ethnomethodology of scientific research activities. Further differences between constructionism and ethnomethodology are dealt with, chiefly around the issue of reflexivity, which is ultimately dismissed in favour of a closer consideration of the accountable aspects of members' activities as they take place in their associated settings. From here, the argument turns to some practical issues related to undertaking video-ethnographic research. Perhaps of most importance, an argument is presented to justify a possible focus on activities that are essentially non-collaborative (such as those of early-stage researchers in astrophysics and electrical engineering), and how exactly a video-ethnographic method might be used to uncover (for ethnomethodology) aspects of work that are not made available through conversation or interaction.

[88] Which are ordinary for practitioners of science at least.

While other types of sociological account (i.e. SSK) would have, and have had, little to say on non-collaborative work, it is found that there is nothing within ethnomethodology that programmatically excludes such a focus, and this aspect particularly is taken forward into the three empirical studies. This is achieved by way of showing that relying only on the investigation of actions displaying a ‘surface sociality’ (i.e. some obvious interaction going on between multiple people) is not necessarily the only thing sociology is capable of. Furthermore, such a focus downplays the extent to which contextual (for early-stage researchers working with CRTs, this might be conceptual, mathematical, theoretical, visual, etc.) understandings of the setting might also serve to order and organise the activities taking place. The three empirical studies are presented with precisely this methodological awareness in mind, and this awareness is made more acutely visible through the choice of early-stage researchers as practitioners of scientific research. For the early-stage researchers observed (HR and SC), their orientation to research activities is more available ethnomethodologically than perhaps it would be for the same activities as they might be conducted by professional researchers. This has been a happy benefit for the ethnomethodological study of these early-stage researchers’ work with CRTs, and has contributed towards the accountability of the order and organisation of their work. To elaborate on the significance of what this fact of HR’s and SC’s work has added to this thesis, it is perhaps useful to compare it to how Harvey Sacks – the key figure in conversation analysis, which is a research discipline that is closely affiliated with ethnomethodology and has grown alongside it – came to settle on a definition of ‘possessables’ (i.e. something you can have) and ‘possessitives’ (i.e. something you can see already belongs to someone else). Garfinkel relates a conversation he held with Sacks, wherein Sacks exclaims:

> Now, Harold, what do I mean by that [‘possessable’/’possessitive’] distinction? That is what I want to *find out*. I don’t want you to tell me…I could find discussions that would bear on what I might as well mean, but that’s not the way I want to learn what I mean…I don’t want to write definitions; and I don’t want to consult authorities. Instead, I want to *find* a work group somewhere…who, *as their day’s work*, and because they know it as their day’s work, will be able to teach me what *I* could be talking about (Garfinkel and Wieder, 1992, 185).

Sacks *did* find such a group who, as their day’s work, oriented to ‘possessables’ and ‘possessitives’ in this way – he found these activities being conducted by Los Angeles police officers, part of whose work was to evaluate whether parked cars had been abandoned or were in the current ownership of some member of the public. Similarly, the choice of early-stage researchers who are still learning their trade (as opposed to professional researchers who are well-versed in it) has been revealing in terms of their activities having been conducted with a constant visible and accountable concern that they might not be doing their work correctly (and how they might be able to tell if this was the case). For HR and SC, part of their work, as an endeavour in research but also education, was to topicalise, problematise and 'figure out' what might count as adequate research activities. Hence, HR's and SC's 'figuring out' work has been particularly revealing from an ethnomethodological perspective, and the findings of the studies presented in chapters four, five and six reflect this.

Chapter four works through various visual resources HR draws on in his astrophysics programming project, seeking to understand these through paying close attention to the conceptual and theoretical aspects of HR's work. This understanding of how HR gets his work done is juxtaposed against accounts suggesting that social and cultural interaction is a necessary pre-requisite to successful computing and programming work (see the constructionist studies outlined in chapter two, as well as Agar, 2006; Bruun and Sierla, 2008; Hine, 2006; Rall, 2006, and; Voskuhl, 2004). This attempt to enrich sociology's explanatory palette with regard to the understanding of non-collaborative research activity involving CRTs is further built upon in chapter five, which deals with CRT usage as it occurs in conjunction with other more 'hands-on' laboratory activities. Focussing on the research work of an early-stage researcher in electrical engineering, this study takes representations as a theme, incorporating how representations are generated and subsequently used (i.e. as findings in themselves, as well as indicators of how to alter future data collection activities for better results) and how they feature as an element in a project involving other (conceptual, theoretical, mathematical, practical) attachments. Chapter six brings the two fields – astrophysics and electrical engineering – together in an examination of how learning is a feasible endeavour for HR and SC as early-stage researchers, even though their learning takes place through the doing of valid research projects which are conducted entirely non-collaboratively. The findings drawn from this educational framing of the research undertaken demonstrate that the resources available to HR and SC – which, importantly, are in no way overtly social and cultural – are pulled in from a variety of arenas in and around their research projects, and are managed in such a way as to make learning an achievable outcome. Specifically, HR's and SC's activities are taken as a critique of common constructionist concepts in the study of science (and related fields) education: namely, 'enculturation', 'dissonance' and 'reality-shock'.

Though the three studies presented as chapters four, five and six deal with distinct themes, they are unified by an overarching question that is worked up throughout each chapter: what is it that makes non-collaborative research with CRTs an achievable task for early-stage researchers? Adopting ethnomethodology as an approach has allowed for a keener focus on the contextual elements of HR's and SC's research work, and it is this kind of approach that the thesis overall has aimed to demonstrate and justify against other alternatives. Ethnomethodology has provided a platform from which the author has been able to question what it is that makes HR's and SC's work orderly and feasible for them – what do they know that *I*, for instance, do not? This question has informed the entire project, from deciding how to fill in the gaps in my own understanding of the relevant contexts and backgrounds to HR's and SC's work (and where precisely those gaps might lie), to choosing specific work sites to video, to selecting excerpts for analysis, to undertaking that analysis, and to fitting all of the above in with (and against) existing sociological literature. The results of this ethnomethodological project are a selection of unpacked activities around the usage of CRTs within scientific (and related) research, which aim to elaborate on the order and organisation of those activities in terms of their fit with the project goals they are conducted in the name of. The hope of all of this has been to show, given the glaring omissions within existing literature – i.e. on the topics of non-collaborative work and the supposedly singular importance of social and cultural factors as full explanations of scientific activities – just exactly what these kinds of unpacked activities might offer to a sociological understanding of scientific (and related) knowledge. With this in mind, if the present thesis has at least made some small movement towards filling in the aforementioned gaps in sociology's understanding, through the undertaking of a selection of empirical studies around the topics of CRT usage in early-stage research, then it has achieved its goal.

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