Objectivity, Reasoning and Interdisciplinarity: Making the Links

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ABSTRACT

Both the production of knowledge and the product, knowledge itself, are social phenomena. This generally accepted fact is generally thought to require relativism, scepticism, and Kuhnian incommensurability, as well as casting serious doubt on the potential of our cognitive traditions to provide us with objective knowledge about an objective world. This thesis exposes and critiques the presuppositions about the nature of reasoning and objectivity which underlie these fears. Combining a Nietzschean, perspectivist account of objectivity with a conception of reasoning drawn from Lockean epistemology and pedagogy, I build a new account of cognitive optimality, dubbed 'Linkmaking'. The phrase deliberately encompasses several meanings. We 'make links' by noticing connections between objects in the world, by linking ideas together to form a theory or a curriculum; by forming social connections, and by developing interdisciplinary practices. I defend the view that we cannot fully address any of these kinds of Link without reference to all of the others. I further show that out best means to critically assess our cognitive groups is to evaluate the extent to which those groups encourage Linkmaking practices.

The major potential challenge to Linkmaking is Kuhnian incommensurability. Having demonstrated the flaws inherent in Kuhn's account, this thesis defends the weaker, Doppeltian form of incommensurability, which grants us insight into the genuine problems which can occur in interdisciplinary research. We then see that the Strong Programme in the sociology of knowledge, inspired by the strong, relativistic version of the Kuhnian incommensurability thesis, has held sway among sociologists because they do not generally study interdisciplinary practices, which highlight scientists' (perspectivist) objectivity. Furthermore, social scientists who accept Kuhnian constructivism doubt their own potential for objectivity, presuming the presence of strong incommensurability where there is none. Undertaking Linkmaking practices both cures this illusion, and improves the cognitive optimality of the group.

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Thesis Introduction

It is now commonly accepted that knowledge is a social phenomenon; but it is less commonly welcomed. The acceptance is tinged with fear that, once we recognize the irreducibly social nature of our knowledge, we must relinquish the belief that we are capable of objectivity, or of reasoned critical assessment of our cognitive traditions. This thesis addresses that fear by exposing the presuppositions on which it rests; that the Cartesian conception of reason is the true one; that objectivity should be conceived as aspiring to a God's eye view of the world; and that a social account of knowledge entails relativism, Kuhnian incommensurability, and skepticism. I shall argue that we need to revise our current conception of objectivity and reasoning. The new account I offer shall have at its core a Lockean epistemology, according to which interdisciplinary practices (broadly conceived) are the key to maintaining objectivity.

We shall begin in Chapter One by showing that the commonly accepted Cartesian dichotomy of reason and tradition is indefensible; and furthermore, that is not necessary in order to maintain realism. It is possible, and desirable, to develop a social account of reasoning and objectivity which can support realism. To this end, the chapter shall delineate and defend Nietzsche's perspectivist conception of objectivity. According to Nietzsche, we cannot achieve and ought not to aspire to 'disinterested contemplation' of the world (Nietzsche 1998: 85). All of our insights are necessarily from a particular perspective. Therefore, our means of avoiding cognitive bias is not best understood in terms of attempting to achieve a single, pure, 'God's-eye-view' of the object under investigation. Instead we achieve objectivity by increasing the number of perspectives that we have on that object. We shall argue that this perspectivism does not entail scepticism. We shall then place Nietzsche's perspectivism in a surprising and yet fruitful relationship with Lockean educational theory. As we shall see, Locke, in *Some Thoughts Concerning Education*, designs a child's curriculum with the explicit intention of exposing as many relationships between different subject areas as possible. This of course has the effect of increasing the number of perspectives that the student has on any given object of inquiry. He then offers similar guidance for adults in *The Conduct of the Understanding*. This is a natural extension of his epistemological claim that knowledge consists in the

connection between ideas. Exposing such relationships improves the learner's reasoning capacity, by enabling them to see 'the light, which the remote and scattered parts of truth will give to one another' (*Conduct*, § III). As we shall see, this Lockean conception of reasoning and the Nietzschean perspectivist conception of objectivity are mutually supporting.

Chapter One shall also answer a predictable objection. Locke has a reputation for scepticism about the possibility of testimonial knowledge, and as such seems an unlikely candidate for the title of social epistemologist; and unlikely material for my project. Yet Locke's reputation in this regard is, as I shall argue, undeserved. Locke's clear distinction between 'testimonial knowledge' and 'received opinion' suggests that his 'scepticism' is better understood as the claim that testimony cannot lead to knowledge unless it has been critically assessed. As we shall see, the critical assessment which Locke recommends would require us to access alternative testimonies. Since ability to do this is dependent upon the arrangement of our social structures, Locke is offering the beginnings of a social epistemology. From these beginnings, I shall develop my own conception of cognitive optimality.

This conception, which I have named 'Linkmaking', is fully introduced to us in Chapter Two. 'Linkmaking' is a deliberately ambiguous term, which can be used to mean four apparently separate processes. Firstly, we 'make links' by noticing connections between objects in the physical world. Secondly, we 'make links' between ideas, as where we design a curriculum, develop a theory, or engage in independent study. Thirdly, we 'make links' socially, by meeting individuals and encountering groups, and by contributing to and drawing information from our cognitive communities. Fourthly, we can make interdisciplinary links between different subjects and areas of enquiry. It would normally be considered a philosophical vice for a phrase to carry such ambiguity, but I regard it in this instance as a virtue. I shall argue that we cannot fully address any one of the above kinds of Link without also addressing all of the others; and that Linkmaking is therefore best regarded as one, unified project. Since optimal Linkmaking is dependent upon the structure and behaviour of our cognitive groups, it makes sense to evaluate such groups with reference to how well they encourage and enable us as Linkmakers. I shall develop and defend the concept of Linkmaking by placing it within the context

of current social epistemology. Firstly we shall see that Linkmaking is incompatible both with foundationalism and with coherentism, instead requiring Haack's intermediate position of founderentism. We shall then relate Linkmaking to Helen Longino's standards of a cognitively optimal community, arguing firstly, that members of Longino's group would be ideally placed to meet each of Locke's requirements for the assessment of testimonial sources, and secondly, that Longino's ideal cognitive group would consist of Linkmakers – not least because their focus on interdisciplinary practices would help to form and maintain the public forums she requires.

It is then necessary to see how Linkmakers could respond to common sceptical concerns associated with a social account of knowledge. We begin this task by examining Frederick Will's account, in *Pragmatism and Realism*, which uncovers three issues of importance to Linkmaking. Firstly, he shows how an individualist conception of rationality has led to scepticism about the capacity of our cognitive traditions to produce knowledge, and provides an alternative, social, account of reasoning. Secondly, he addresses the Relativist Illusion; the concern that our cognitive traditions are incapable of radical self-criticism, and the Coherence Illusion; the concern that cognitive systems 'interpose themselves between objects and aspiring knowers' (Will 1997: 94), so that the enquirers can only access the 'artefacts of practice' rather than the objects that they seek to understand.

We shall then relate Will's account to Thomas Green's *Voices, the Educational Formation of Conscience*. Green enables us to see how the sceptical concerns addressed by Will have affected pedagogy; as educators have assumed that they have a choice between either inculcating their students into the norms and practices of their cognitive traditions, or respecting their autonomy. Seeking to show that this is a false dichotomy, Green offers us an account of norm-acquisition (or 'normation') as the development of conscience, revealing the importance of group membership for self-assessment and for the assessment of the group. Green's account helps the Linkmaker to develop a clearer understanding of how people must be educated if they are to become members of the optimal cognitive communities described by Longino.

Having established the concept of Linkmaking and explored it in relation to contemporary social epistemology, I shall address the greatest potential challenge to the Linkmaking project: namely, Kuhnian incommensurability. Chapter Three begins by presenting the traditional, 'strong' form of Kuhnian incommensurability. It then addresses each of the Linkmaker's philosophical commitments in turn. We then see the extent to which Kuhnian incommensurability contradicts, or supports, each of the Linkmaker's commitments. Whilst incommensurability is incompatible with some of the Linkmaker's commitments, it actually provides considerable support to others. Therefore, we shall not attempt to reject incommensurability entirely. Instead, we shall reveal a fallacy within Kuhn's original 'strong' version of incommensurability, which occurs due to Kuhn's unconscious acceptance of a descriptions theory of reference. After the 'strong' version has been dethroned, the chapter goes on to address the alternative interpretation of incommensurability provided by Gerald Doppelt. We shall find that Doppelt escapes the criticisms that we have levelled at the strong version of Kuhnian incommensurability, but also that Doppeltian incommensurability is compatible with Linkmaking. Indeed Doppelt can offer Linkmaking considerable support, and provide us with useful insights into the genuine cognitive problems that can arise in interdisciplinary research.

Unfortunately, it is the strong version of Kuhnian incommensurability which has most affected fields outside the philosophy of science. For example, it is used to defend constructivism, which has been influential amongst sociologists of science and the social sciences generally. The Linkmaker finds this influence problematic for two reasons. Firstly the sociologist of science who accepts constructivism (also known in this context as the Strong Programme in the sociology of science) is liable to underestimate the potential of scientists to produce objective information about the world, and therefore likely to hold and promote an unnecessarily cynical view of science; possibly to the point of rejecting realism. Secondly and more insidiously, social scientists who accept the constructivist view are liable to underestimate *their own* potential for objectivity, which can only stunt their intellectual progress. For this problem, we attempt a Linkmaking solution. The first step is to defend Haack's alternative to the Strong Programme, which she names the Sensible Programme; according to which the role of sociology of science is to critically evaluate the cognitive behaviours of scientific research groups. Sociologists should therefore be

neither deferential nor cynical towards the scientists they study. The second step is to show, with reference to Robert Parhe, that those sociologists of science who study interdisciplinary behaviours are more likely to adopt the Sensible Programme. In order to defend this claim, we turn to a case study drawn from Peter Galison's work *Image and Logic*. Galison offers a historical account of two conflicting epistemic traditions within microphysics. We can see that, indeed, were sociologists to examine either tradition in isolation from the other they would be likely to reach strong constructivist conclusions. Examining the processes of crossing and re-crossing between these two traditions gives the sociologist a very different picture of events, one which would lead them to endorse the Sensible Programme.

Having established how sociologists of science have been impacted by their implicit acceptance of the strong form of Kuhnian incommensurability, we then address the effect on social scientists. Archaeology is particularly vulnerable to strong constructivist views, not least because of a long standing cognitive split between processual and post-processual approach to archaeology. Therefore we shall examine developments in archaeological theory over the past century; focusing on the debate between processualists and post-processualists. Drawing on Robert W. Preucel's account in *Processional and post-processional archaeologies: multiple ways of knowing the past*, we shall see that each tradition demands different cognitive requirements for archaeological investigation. (They are, in this limited and Doppeltian sense, 'incommensurable' with each other, as the Image and Logic groups are.) This debate has yet to reach a resolution; and indeed, Preucel maintains that 'resolution of competing interests is… the wrong goal' (Preucel 1991: 27). We shall build on Preucel's argument here, by offering a Linkmaker's account of how archaeological theorists can best achieve objectivity. We shall see that it is fully possible for each of the archaeological traditions to exist simultaneously. They are not only compatible but mutually dependent. The Linkmaker's advice, therefore, is that the divisions of archaeology publicly articulate what their cognitive requirements are and why they hold those requirements, developing a debate such as that which we observe in Galison's Image and Logic groups. This is not to say that we should expect a unifying consensus to emerge. Rather, the debate is in and of itself to be recommended, simply because it enables them to make better Links: between pieces

of material culture, between archaeological ideas, between research groups, and between sub-disciplines.

Chapter One: Reasoning and Objectivity

§1: Introduction

The past few decades have seen an increased interest in, and awareness of, the social aspects of knowledge production. However, many epistemologists (consciously or unconsciously) nonetheless retain the Cartesian ideal of reason. According to this ideal, the purpose of reason is to enable critical assessment of our cognitive traditions, and an objective evaluation of the norms and practices arising from them. In order to achieve these purposes, it is often presumed that reason must necessarily be independent of the traditions it is used to assess. The Cartesian ideal of reason is therefore an endowment of human individuals, not a result of education or of social influence of any kind.

As our understanding of sociology and pedagogy has increased, this individualist conception of reason has lost plausibility. Instead, we have come to see that our reasoning processes develop as part of our education in various cognitive traditions. Yet the Cartesian ideal of individualistic reasoning retains much of its influence. One reason for this is that abandoning the ideal seems to raise significant epistemological concerns. Suppose that we acknowledge the following claims:

- We lack any individualistic 'reason' which functions independently of our cognitive traditions.
- Instead, our reasoning capacities arise from our education and training in our cognitive traditions.

Our concern is this. Reason should enable us to examine, challenge, and develop the norms and practices of our traditions; yet it is becoming increasingly difficult to deny that our reasoning skills in fact arise from those traditions. How, then, can those reasoning skills grant us the necessary objectivity to evaluate them? The fear raised here is that what appears to be objective evaluation may in fact be mere bickering within predetermined boundaries, about how well the tradition is living up to its own cognitive rules. What objectivity can such a truncated version of 'reason' grant us?

This leads us to see the greater apparent problem. We use our cognitive norms and practices in order to come to conclusions about the world; conclusions which amount to a large proportion of our propositional knowledge. If we have no objective means of evaluating our norms and practices; then how can we claim to *know* the conclusions to be true? Without individualistic reason, how can our traditions be a source of objective knowledge?

This chapter seeks to show that the Cartesian dichotomy of reason and tradition is mistaken, and that it is unnecessary to retain an individualistic conception of reasoning in order to preserve realism (§2). To defend the latter, the chapter delineates Nietzsche's perspectivist conception of objectivity (§3). Nietzsche understands objectivity not as the rejection of one's personal 'biased' perspective on an issue in order to obtain an ideal 'God's eye-view', but as the development of multiple perspectives on that issue and of the ability to shift between those perspectives. In §3.2, we shall see that this perspectivism is fully compatible with realism. Nietzsche's perspectivism has profound implications for social epistemology and educational theory; implications which (strangely perhaps) are best explored with reference to Lockean educational theory.

Locke's account of reasoning is introduced in §3.3. In *Some Thoughts Concerning Education*, Locke designs a child's curriculum with the intention of providing as many illuminating connections between the different subject areas as possible, as a practical application of his epistemological claim that knowledge consists in the connection between ideas (§3.4). In *The Conduct of the Understanding*, Locke provides similar guidance for the adult (§3.5), recommending a broadening of reading material and of social sphere. I shall link a Lockean conception of the development of reasoning back to a Nietzschean, perspectivist conception of objectivity; claiming that reasoning and (perspectivist) objectivity are obtained as two parts of the same educational process.

Having revealed the riches Locke brings to social epistemology, it is necessary for me to answer a predictable objection: Locke's infamous scepticism regarding the possibility of obtaining knowledge via testimony. I argue (§4) that this should be understood as firstly, the unobjectionable claim that testimonial knowledge does not

amount to intuitive or demonstrative knowledge, and secondly, the claim that in order to attain knowledge via testimony, the testimonial source must be critically assessed. Part of that critical assessment involves comparing the testimonial account to alternative testimonies. The individual enquirer has greater, or lesser, access to such alternative testimonies depending upon the structure of their social groups. In the *Conduct of the Understanding*, Locke advises the reader to seek out the company of those with whom they disagree, in order that they might access such alternative testimonies. On this basis, I shall defend Locke's status as a social epistemologist.

In conclusion (§5), I shall claim that Locke's account of the development of reasoning, combined with a Nietzschean account of objectivity, offers an excellent basic framework with which to assess the cognitive optimality of groups. I shall acknowledge that Locke does not go so far as to delineate any detailed set of epistemic standards for cognitive groups, but argue that he leaves us but a few steps away from achieving this. These steps shall be completed in Chapter Three, *Defining Cognitive Optimality*.

§2.1: Enlightenment - Reasoning and Traditions

My first concern in this chapter shall be to address the Cartesian fear associated with 'epistemic traditions'. As a result of our Enlightenment inheritance, many of us still *contrast* tradition with reason. We assume that to reason is to think independently, to challenge the assumptions of our traditions. Therefore, if we are 'merely' following tradition, then we are not reasoning independently.

This contrast between reason and tradition is unjustified. Furthermore, accepting this false contrast has led to distrust of the influence of teachers upon students, and the influence of research traditions upon researchers. This distrust can ultimately encourage an acceptance of Kuhnian-style relativism. Only once we understand that reasoning and traditions are interdependent, and how they interact, can we transform that discomforting distrust into a meaningful set of epistemic requirements which we can then demand of any cognitive group, before we are prepared to refer to a consensus within that group as 'knowledge'.

This section will delineate the Enlightenment conception of the relationship between reason and tradition, and will then show fundamental flaws in this conception. It shall show that social epistemologists are motivated to retain the Cartesian conception in order to support realism, and then suggest why realism does not need such a defence (a theme which shall be continued in §4). Finally the section shall raise (and then answer) a possible objection from the die-hard individualist; namely, that whilst the production of knowledge is undeniably social, the product, knowledge, lacks a social dimension. We shall show that this objection is unsustainable.

§2.2: The False Contrast

The Enlightenment tradition, which is still highly influential, conceives of reason as an individual's ability to critically assess, and where necessary alter their inherited norms, institutions, and traditions. According to this conception, the use of individual reasoning is the only means to justify one's beliefs, and therefore the only means to attain knowledge. Reason is therefore considered a natural endowment of individual human beings, and not a socially acquired skill. The thought is that if the methods we use to reason about the merits or otherwise of our traditions were themselves formulated by our traditions, we would be stuck in an intellectual stalemate, unable to evaluate our traditions at all.

By implication, those of us optimistic enough to believe that we can and have made use of reason by challenging our traditions (thereby improving the capacity of our institutions to acquire and disseminate knowledge), must conceive of reason as something intrinsic to the individual human mind, independent of arbitrary cultural or intellectual traditions. We have great respect for individuals like Galileo or Darwin, who have instigated revolutions in their chosen fields, taking a rational stand against a cognitive tradition which would otherwise impede its own progress towards comprehension of the physical world. The Enlightenment promotes reason as a means to escape dogmatism and avoid clinging loyally to ideas past their plausibility.

Following this Enlightenment conception of reason, there has been much debate regarding whether it is in fact possible to have tradition-independent reasoning skills. It is notable, though, that the debate has always been about whether such a thing is possible; it has simply been presumed to be desirable. There are epistemologists who

recognize the inescapable role of group influence on knowledge creation, and regret the fact. Part of the reason for this is the apparent tension between a social view of knowledge and a realist epistemology. Richard Rorty's rejection of foundationalism in *Philosophy and the Mirror of Nature* was also his rejection of realism. Additionally, Simon Blackburn (1993) and John Haldane (1993) both believe that foundationalism, and therefore an individualist conception of knowledge, is required in order to support realism (Will 1997: xxxi).

According to the individualist account, knowledge is not inherently social and the individual can in principle discover and know things about the world independently. As Westphal (2002) argues, following Will, this account is still more readily associated with realism than any picture of human knowledge according to which we are essentially dependent upon social cognitive interactions. The implication is that if knowledge were intrinsically social we would be guided by social conventions rather than by worldly facts. However, there is no reason why realism about the objects of human knowledge cannot be compatible with a well-constructed social account of human knowledge. The fear that a social epistemology is incompatible with realism is drawn from the Enlightenment conception of 'an exclusive, even oppositional dichotomy between reason and tradition' (Westphal 2002: 2). This dichotomy is faulty. Whilst individuals make contributions to their social institutions, in doing so they needs must draw upon their training within those institutions. Whilst we are right to recognise and admire individual innovators, we should also recognise that such innovation relies upon 'unappreciated possibilities of modification latent within established, 'traditional' practices' (Westphal 2002: 2). To think of reason and tradition as opposed to one another is to miss the fact that innovation depends upon both. Unfortunately, social accounts of knowledge all too often still consider social influences an unavoidable evil, a risk which we have little choice but to take. It is certainly true that unquestioningly accepting the norms of a tradition and applying them indiscriminately will result in uncritical individuals and stagnant research groups. This point is discussed in connection to Locke's educational theory in (§3.4). However, meaningful questioning and apt discrimination is only possible as a result of training in and familiarity with the norms of the tradition in question. The dependence of reasoning upon tradition is balanced by a corresponding dependence of tradition upon reasoning, in order for the tradition to develop. The 'risk' then is not

social influence per se, but any influence which discourages critical assessment of the tradition taught. Such influence would damage both the cognitive capacity of the individual and the development of the tradition. Therefore, in order to maximize the reasoning capacity of individuals, it is vital to inculcate them into the norms and practices of the traditions of which they are to become a part; whilst making them conscious of the fact that this inculcation gives them the means to question, adapt and improve those traditions.

§2.3: The Die-hard Individualist's Objection

There is an objection, which could be made by a die-hard individualist, which is worth addressing at this point. The individualist might concede that the *production* of knowledge depends upon social factors, whilst maintaining that the *product*, knowledge, belongs to individual enquirers and lacks a social dimension. It is important therefore to demonstrate that this separation between knowledge production and the knowledge product is artificial. There are three aspects to my reply, each a stronger claim than the last, and each of which I will demonstrate in turn. I argue, firstly, that knowledge gained by testimony remains justified by the means of knowledge production, and thereby retains a social dimension. Secondly, knowledge which is gained by individuals as a result of socially inculcated norms and practices is justified by the cognitive reliability of those norms and practices, and therefore retains a social dimension. Finally and most controversially, I shall argue that propositional knowledge itself has a social dimension.

§2.3.1: Knowledge gained by Testimony

In justifying a belief which has a social source, we must refer to the method of knowledge *production*. For example, take my belief, as a layperson, that the reported data in a particular scientific article is accurate. If I am asked to justify this belief, I could reasonably give the following answer: the article was peer-reviewed, the experiments were performed under carefully controlled conditions, and the scientist was trained at a respected university. In order for my belief to be justified, I am therefore dependent on the following:

- Whether the peer review system was optimally used in this instance,

- Whether the conditions of the experiment were controlled using methods *which have been agreed to be sufficient* for experimental accuracy,
- Whether the institution of the university where the scientist was trained merits a good reputation for education and research.

Each of these justifications is social in nature. Were the social situation different, I could not claim to be justified in believing the conclusions of the article. This can be clarified simply by considering the following scenarios:

- I discover that the sole reviewer of the article was the scientist's Ph.D. supervisor.
- I discover that there is growing controversy within the scientific community as to whether the methods used for this experiment are sufficient for experimental accuracy.
- I discover that it is possible to obtain a Ph.D. from the scientist's university by mail order.

It is clear that in each of these scenarios, the justification of my belief has taken a serious blow. Of course, my belief may still be *true*: the conclusions of the article may turn out to be accurate despite the lack of proper peer review, or the controversy over the accuracy of the method, or the university's lack of academic respectability. But I am no longer justified in believing it to be true, since my original justifications have been undermined.

It is important to note that in none of these scenarios have I discovered anything new about the experiments and results described in the article. They remain the same. Rather, the scenarios describe flaws in the social processes contributing to knowledge production. At the same moment that I discover that the *production* of my knowledge was flawed, I also discover that I no longer have the *product*, knowledge. The two discoveries are the same. It is therefore artificial in the extreme to maintain that knowledge *production* is social whilst the *product*, knowledge, is not.

§2.3.2: Knowledge gained as a result of socially inculcated norms and practices

The above case is straightforward. I am not directly involved in the knowledge production, and am therefore dependent upon the accuracy of the testimony I am given. If I later discover that there is reason to doubt the veracity of that testimony, I am no longer justified in my belief. However, the die-hard individualist could acknowledge this, whilst maintaining that the 'problem' is peculiar to knowledge gained via testimony. Were I the scientist performing the experiments myself, they might argue, I would not have any such dependency and therefore my knowledge would not have a social dimension. To address this issue, let us consider an alternative case. Suppose an individual knows, on the basis of nautical star charts, when to expect a certain eclipse of an outer planet. Is this a strictly individualist piece of knowledge?

This certainly isn't a case of knowledge by testimony. I need not in this instance call on another individual person to verify my belief. Yet I do call upon the reliability of norms, practices, and techniques which have been developed by cognitive traditions, traditions to which I now belong. I can justify my belief that an eclipse of an outer planet will occur at a certain time with reference to a star chart; a chart which I can use independently of anyone else. But when asked to justify my belief that the star charts are reliable, then my answer can only be social. I cannot offer a full and accurate justification without mentioning that the star charts have been gradually developed by many other individuals who made them progressively more accurate and effective. Merely in using the star charts, I am applying norms and practices which I have learnt from a longstanding cognitive tradition. Therefore it is inaccurate to claim that my knowledge of when to expect the eclipse is an individualist piece of knowledge.

§2.3.3: Propositional knowledge

Suppose, then, that our individualist were to grant that where I am using norms and practices inherited from cognitive traditions in order to gain knowledge, my knowledge is social. They could nonetheless deny that every instance of knowledge is social. What about my knowledge that I am, at this moment, holding a glass of blackcurrant cordial? Surely such propositional knowledge is, or can be, purely individualistic?

In claiming that I have a glass of blackcurrant cordial, I am making a far richer and more complex claim than initially seems to be the case. I am claiming that the object I perceive is, indeed, made of glass. If asked what 'glass' was, I could give a vague description of its being made from molten sand. But since I have never witnessed sand being turned into glass, my knowledge of what 'glass' is, is social knowledge – as, therefore, is my knowledge that this particular object is made of glass. Even if I had witnessed sand being turned into glass, or even into this particular glass, there are still many things implied by the claim 'this object is made of glass' which I do not myself understand. There are rich implications to the proposition, which I cannot verify myself. I bought the blackcurrant cordial. Yet, my knowledge that the cordial's ingredients include fruit, rather than artificial flavouring indistinguishable by my taste buds from fruit, is based upon the labelling on the bottle and my awareness that it is against various regulations to lie on such labelling, and that the company would likely have been forced to change its labelling by now had they done so. Furthermore, the fact that it is made of blackcurrants is full of rich implications which I cannot myself understand or verify: blackcurrants are fruits of a particular genus, a genetic makeup, a nutritional value, and so on. The apparently simple proposition that I have a glass of fruit cordial is sufficiently rich that it would not be possible for me as an individual to verify it. Even this, therefore, cannot be considered a case of individualist knowledge.

§2.4: Conclusion

Enlightenment ideals promote reason as a means to escape dogmatism; and so it is. Our reasoning skills enable us to critically examine our traditions, and to suggest innovations and improvements which increase their potential to produce and disseminate knowledge. However, it need not and does not follow that we must conceive of reason as something fundamentally divorced from tradition. Reason and tradition are interdependent. It is possible to draw upon the norms of one's own tradition in order to critically examine that tradition. Indeed, it is usually essential to do so. (In Chapter Two, I will use Frederick Will's work *Pragmatism and Realism* to demonstrate that this dependency is not the paradox that it may initially seem to be.) In addition, reasoning individuals may draw upon the norms and practices of other institutions in criticising their own. Awareness of the approaches, standards, and methodologies used by other cognitive groups is an effective means of seeing possible

improvements to one's own institution. Rather than reaching after the false ideals of individualist knowledge, we ought to develop new and more robust ideals. To this purpose, the next section will show that the development of reasoning, and the development of objectivity, requires that individuals draw upon as many perspectives on a given question as possible. As Nietzsche shows us, this is the only proper way to conceive of objectivity. As Locke's account shows us, this is the most effective way to educate our children and ourselves.

§3.1: Objectivity, Reasoning and Multiple Perspectives: the strange fellowship of Locke and Nietzsche

In the previous section, I argued that the Cartesian conception of reason, which regards reason as independent from tradition, is indefensible. I maintained, however, that a social account of reasoning is compatible with realism. The best way to explain this is by re-assessing what we mean by objectivity, and clarifying the relationship between objectivity and reasoning. This section will begin (§3.2) by examining Nietzsche's perspectivist conception of objectivity as put forward in *On The Genealogy of Morality.* We shall show that Nietzsche's perspectivism does not commit him to relativism, and that Nietzsche is in fact a cognitivist. Objectivity is possible, and with it a realist conception of the world; but objectivity is best conceived not as a 'God's eye-view', but as the possession of multiple perspectives upon an issue and the ability to shift between those perspectives at will. This conception of objectivity, as we shall see, has profound implications for social epistemology and for educational theory. We shall find that, surprisingly perhaps, those implications are best explored with reference to Lockean educational theory.

Therefore we shall then look to a general introduction to Locke's account of reasoning in *Some Thoughts Concerning Education* and in the *Conduct of the Understanding* (§3.3). *Some Thoughts Concerning Education* is explored in greater detail in (§3.4), which lays out the child's curriculum in detail, revealing the epistemic commitments that formed it. The curriculum is designed to maximise the amount of appropriate connections between ideas that the child is exposed to. Equally, Locke's recommendations to an adult learner in the *Conduct of the Understanding*, to broaden

one's social sphere and reading habits, are explicitly intended to improve the number of appropriate connections between ideas and the enquirer's 'large, sound, round-about sense' (*Conduct*, § III). I connect both texts back to the Nietzschean conception of objectivity, arguing that a Lockean curriculum both provides a child with multiple perspectives on the objects of enquiry, and provides the means to assess those perspectives.

§3.2: Nietzsche's Objectivity

In *On the Genealogy of Morality,* Nietzsche offers an interpretation of objectivity which, initially at least, seems to involve relativism. He shows that 'objectivity' should not be regarded as 'disinterested contemplation', since it is not possible to avoid viewing an object or an issue from a particular perspective. Rather, objectivity consists in:

> the capacity to have one's pro and contra *in one's power*, and to shift them in and out: so that one knows how to make precisely the difference in perspectives and affective interpretations useful for knowledge. For let us guard ourselves better from now on, gentlemen philosophers, against the dangerous old conceptual fabrication that posited a 'pure, will-less, painless, timeless subject of knowledge'; let us guard ourselves against the tentacles of such contradictory concepts as 'pure reason', 'absolute spirituality', 'knowledge in itself': here it is always demanded that we think an eye that cannot possibly be thought, an eye that must not have any direction, in which the active and interpretive forces through which seeing first becomes seeing-something are to be shut off, are to be absent; thus, what is demanded here is always an absurdity and non-concept of an eye. There is only a perspectival seeing, only a perspectival 'knowing'; and the more affects we allow to speak about a matter, the more eyes, different eyes, we know how to bring to bear on one and the same matter, that much more complete will our 'concept' of this matter, our 'objectivity' be. (Nietzsche 1998: 85)

Nietzsche's 'perspectivism' dismisses the traditional conception of objectivity as the ability to move beyond one's personal perspective (or bias, as a traditionalist might be more inclined to call it) to achieve a pure insight into the matter itself. The metaphor of the eye serves two purposes. Firstly, it is meant to reveal that the ideal of cognitive

'objectivity' is unattainable because it is incoherent: to remove the personal perspective is to remove the active, interpreting force required for 'sight' (or, rather, for understanding). One's perspective, or preferably perspectives, *is* ones insight into the matter. Secondly, the metaphor of the eye serves to provide an alternative account of what it means to be objective. Objectivity does not consist in attaining the 'God's eye view' of the event or the object of enquiry. Rather than attempting to see through the 'eye that must not have any direction', we should multiply our 'eyes', our perspectives, in order to gain a more objective view of the matter we are examining.

It would be easy to regard Nietzsche's perspectivism as a form of relativism. In fact a close examination of Nietzsche's work reveals that he is very much a cognitivist, firmly espousing the view that there are knowable truths about the world (Westphal 1984: 71). Furthermore his cognitivism is fully compatible with his perspectivism:

> In GM III 12... Nietzsche indicates that perspective interpretations are constituted by three main factors: a subject matter attended to by a subject, a direction or point of view from which the subject attends to the subject matter, and 'interpretive forces' activating the perception and interpretation of that subject matter. In those subjects motivated by an interest in truth, that motivation would predominate over other, less perspicuous interests... All of this is completely cognitivist... (Westphal 1984: 81)

Cognitivism does not require a complete knowledge of the object of enquiry; incomplete knowledge suffices for there to be knowable truths. Nietzsche's perspectivism is therefore compatible with his cognitivism. However the combination requires that disparate cognitive perspectives on a given subject matter 'are sensitive to or have access to different aspects of that subject matter' (Westphal 1984: 82). It also requires acknowledging that there is no 'single, completely correct, "beatific" interpretation of anything' (Westphal 1984: 81). Nietszche's naturalism leads to his insistence that the way we see the world, and what we can know of the world, depends upon our position, motivation, sensitivities and insensitivities, competencies and incompetencies. Therefore Nietzsche argues that there is only a perspective knowing, and every cognitive perspective is, by definition, incomplete. Objectivity, then, is not an absolute: we cannot acheive the 'God's-eye-view' which is the

traditional ideal; nor should we aim for it. Rather the best way to move towards greater objectivity is to obtain and use as many relevant perspectives as possible.

Nietzsche's account of objectivity as the use of multiple perspectives has deep significance for social epistemology and for research and educational practices. In order to examine this significance, we must turn to what will seem a very odd choice of bedfellow for Nietzsche, namely the educational works of John Locke. The following section will show that Locke's account substantiates Nietzsche's claim that human objectivity, such as it is, can be achieved by increasing the number of relevant perspectives available to an individual thinker. Further, Locke's account will show that the only means to achieve this are social means. Whilst many epistemologists have feared that the extent of social influence on knowledge creation is incompatible with realism, we find that in fact objectivity depends upon social factors. Furthermore, in the process of demonstrating the necessarily social aspects of objectivity, Locke will provide us with a theoretical anchor with which to differentiate between appropriate and inappropriate interdisciplinary practices.

§3.3: Locke on Reasoning

In *Of the Conduct of the Understanding* (henceforth referred to as '*Conduct*'), Locke argues that for an individual to reason effectively, it is essential that he has a 'full view of all that relates to the question and may be of moment to decide it' (*Conduct*, § III). When our view does not extend to 'all that has a connection' with the object of our investigation (which is the usual situation), we reason imperfectly. Whilst part of Locke's response is to compare human capacities and reasoning unfavourably to that of angels, who can 'in the twinkling of an eye, collect together all their scattered and almost boundless relations' (*Conduct*, § III) he also provides better and worse scenarios for the improvement of human reasoning.

In *Some Thoughts Concerning Education* (henceforth referred to as *'Thoughts'*), Locke maximizes a child's 'view of all that relates to [a] question' by carefully formulating a curriculum designed to reveal all relationships between the information provided, not merely relationships between topics in a given discipline. The curriculum therefore crosses and re-crosses disciplinary boundaries, in meticulous ways. This is effectively a practical application of Locke's theory of ideas as

developed in *An Essay Concerning Human Understanding*, henceforth referred to as the *'Essay'*, an application which has profound implications for social epistemology. Locke makes it clear that maximizing the appropriate connections between ideas necessarily involves crossing subject boundaries. His aim is to instil the habit of connecting every new piece of information with all relevant information which is already available; with the hope that this habit will be carried over into adult life.

In *Conduct*, Locke addresses his points to adults who may as yet lack this habit. Therefore he encourages them to develop it. Firstly, Locke argues that since an individual's view on a particular object or subject is naturally limited, it is necessary to consult with other people to obtain their views. This helps to increase the individual's objectivity. Secondly, it is particularly important to avoid limiting one's studies to 'one sort of men [or] one sort of books' (*Conduct*, § III). As I will show with Locke's example of the 'country gentleman', by 'one sort' of men or of books Locke means both 'one set of opinions' and 'one area of expertise'. Limiting one's studies to one set of opinions on a matter (for example, a political or moral issue) will necessarily limit one's ability to be objective. Such an approach to study reaffirms one's prejudices, by giving the impression that 'everyone' agrees with one's opinion even where this is far from being the case. Furthermore, this can end up limiting the empirical information one has access to, since unfavourable evidence can be excluded either deliberately through bias or due to the author herself having limited her reading to those whose opinions she shares. Limiting one's studies to one area of expertise, Locke argues, will lead to an individual who is unable to be objective because they view all areas of study in terms of their own specialism, however inappropriate it may be to do so. Additionally, they will also be less able to comprehend their own specialism, because they will be unable to import useful concepts and methods from elsewhere. Therefore they miss genuine connections between subject areas, or worse, presume to see connections which aren't there.

§3.4 provides a brief account of the Lockean curriculum and its implications for social epistemology. §3.5 assesses the recommendations from *Conduct* for maximising objectivity. §3.6 concludes that Locke's and Nietzsche's accounts are both sound and mutually reinforcing, and that they serve to highlight the cognitive virtues of interdisciplinarity.

§3.4: Training in Objectivity: The Child's Curriculum

In a Lockean curriculum, subjects are taught in an order which carefully links each subject to the ones taught before, after, and alongside it. For example, geography is taught before chronology, which precedes history. Locke explicitly relates this back to his epistemology:

> Give them first one simple Idea, and see that they take it right, and perfectly comprehend it before you go any further, and then add some other simple Idea which lies next in your way to what you aim at, and so proceeding by gentle and insensible steps, Children without Confusion and Amazement will have their Understandings opened, and their Thoughts extended farther, than could have been expected. (*Thoughts*, § 180)

In addition, the chronology of the Lockean curriculum is designed so as to 'zigzag' between subjects which are based upon empirical investigation and those that are based on abstract reasoning, a practice which Locke considers to be beneficial to understanding both. Therefore, after basic literacy and numeracy is achieved, the child's early education arranged in the following order:

Geography is the recommended starting point. It is studied in the form of a globe, rather than maps. As such it is a direct physical representation of the Earth. The study of the globe is an empirical examination of three-dimensional particular physical object. Whilst it could be said that the globes themselves are an abstract representation of another object, they have a physical resemblance to the planet they represent which means that the child is informed about the planet directly by their senses: it is spherical; it turns on an axis, and so on. This is the sense that I consider relevant to the relationship that Locke is trying to establish.

Arithmetic follows geography. All mathematics more complex than counting is taught after geography has been introduced. This develops the child's abstract reasoning.

Arithmetic is then applied to studies of the globe. This includes learning longitude and latitude, and then map-reading. Here abstract reasoning influences the perception of the physical particular.

Maps of the star systems are then introduced. Knowledge gained from geography is applied to astronomy, which also draws upon and develops mathematical skill. The planets are also studied in the form of globes.

Geometry is only studied once the child has the familiarity with the globes described above. Geometry is then taught in relation both to mathematics and to the physical objects of the globes. This gives the child experience in moving from particular physical objects to abstract ideas.[1]

Locke justifies the above arrangement of the curriculum in explicitly epistemological terms, claiming that the arrangement maximizes the child's potential understanding of the topics. The purpose of the curriculum is not merely to familiarise the child with certain ideas, but more importantly to reveal connections between the phenomena studied. The interdisciplinarity of this approach engrains the habit of connecting every new piece of information with information which is already known. Getting into this habit trains the learner to differentiate between relevant and irrelevant information.

Locke does not directly offer strategies for distinguishing useful from spurious connections. Conceivably this is because Locke considers the ability to recognise relevant connections as similar to the ability to abstract: the connection is that which the subjects have in common, much as 'redness' is that which the ball and the kite have in common. The role of a parent or tutor is to introduce the names of objects and properties in such a way as to highlight similarities and thus aid the child's learning. Offering a definition of abstraction or a detailed explanation of how it is done might be intellectually interesting to the adult, but does not aid the child who is learning how to abstract. Similarly, the role of a tutor is to provide illustrative examples of relevant connections between subjects, showing in each case how the links aid the child's understanding of both subjects. This will encourage the child to seek out more

[1] See Thoughts §178 - §181 for details of this curriculum.

connections when their learning becomes more independent. Possibly, the absence of direct advice on this matter is quite deliberate; it is easier by far, in this case, to learn by doing.

By increasing both the number of appropriate connections between ideas and the likelihood of the child seeking out connections on his own later, the curriculum improves what Locke calls their 'large, sound, round-about sense' (*Conduct*, § III):

> Besides the want of determined ideas, and of sagacity and exercise in finding out and laying in order intermediate ideas, there are three miscarriages that men are guilty of in reference to their reason... The third sort is of those who readily and sincerely follow reason, but, for want of having that which one may call large, sound, round-about sense, *have not a full view of all that relates to the question and may be of moment to decide it.* We are all short sighted, and very often see but one side of a matter; *our views are not extended to all that has a connection with it*. From this defect I think no man is free. We see but in part, and we know but in part, and therefore it is no wonder we conclude not right from our *partial views*. (*Conduct*, § III, my italics)

This passage reveals how we might link Locke's understanding of reasoning with Nietzsche's conception of objectivity. Nietzsche denies that objectivity consists in disinterested contemplation. Rather, objectivity requires the possession of multiple perspectives and the ability to use those perspectives:

> The more eyes, different eyes, we know how to bring to bear on one and the same matter, that much more complete will our 'concept' of this matter, our 'objectivity' be. (Nietzsche 1998: 85)

Nietzsche recognises that objectivity can only approach, not arrive at, completion. Like Locke, he knows that 'our views are not extended to all that has a connection' with our objects of investigation. Whilst Locke does criticise those who lack 'large, sound, round-about sense', and sets out the child's curriculum to improve it, he does not claim that it is possible to perfect it. 'Short-sightedness' is an intellectual flaw belonging to all human beings, not merely to those who have in some way neglected

their education. A good education (or adult programme of study, as recommended in the *Conduct*) can improve our objectivity, not by extending our views to 'all that has a connection' with a matter, but by providing clear examples of such connections and by encouraging the enquirer to seek out more themselves. Locke's account, then, acknowledges and develops the idea that human objectivity is maximised by increasing the number of perspectives available to individual thinkers. Lockean educational theory aims to provide students with as many 'eyes, different eyes' on a given matter as is possible. He does this in *Thoughts* by illustrating links between different subject areas within the curriculum, and in the Conduct by the recommendation that readers do not limit their studies to only one area of study or only one set of opinions. Locke's intention is to improve the reasoning capacity of his students. In doing so he also vastly improves their (Nietzschean) objectivity.

In establishing connections between subject areas, Locke is also providing students with *two* perspectives. All interdisciplinary links provide two such perspectives, and provide the opportunity to link those perspectives in order to gain a greater understanding of the object of enquiry. One example of this, which shall be examined in Chapter Two, is the case of Lise Meitner and the discovery of nuclear fission. This discovery only occurred because the chemists, Otto Hahn and Fritz Strassmann, had sufficient understanding of physics to understand the implications of their results, *and* because Lise Meitner had a sufficient understanding of chemistry to engage with their research. Consider also the example, from Locke's curriculum, of the connection made between geography and mathematics. One can view geography from a mathematical point of view; and develop an understanding of cartography. One can also view mathematics with geography in mind: and this will aid an understanding of geometry. It is clear from this example that the connections not only encourage sound interdisciplinary practices but also improve one's understanding of one's own area of expertise.

Locke defines human reason in relation to the superior capacities of the angels, 'who, in their several degrees of elevation above us, may... have perfect and exact views of all finite beings' (*Conduct*, § III). In addition, of course, Locke would have the omniscience of God to consider; this would amount to the only true objectivity. Therefore Locke retains the ideal of what Nietzsche would call the 'eye without any

direction'; but he doesn't expect human beings to attain it. Rather, he would agree with Nietzsche that *our* objectivity is achieved by gaining as many informed perspectives as we can, both through our education and through our interactions with other people. We have explored this primarily with reference to Lockean pedagogy; for a fuller understanding of Locke's educational goals and how they relate to Nietschean perspectivist objectivity, it is necessary to examine his advice to the adult, as presented in the Conduct of the Understanding.

§3.5: Objectivity in *The Conduct of the Understanding*

In the *Conduct*, Locke addresses an adult rather than the educator of a child, and makes recommendations for self-improvement of one's reasoning capacity and objectivity.[2] This time it is not achieved by undertaking a formal course of study, but rather by committing oneself to broad general reading and to widening one's sphere of social interaction, both of which can broaden a narrow perspective. Locke argues that the natural limitation of an individual's view or perspective ought to:

> instruct the proudest esteemer of his own parts, how useful it is to talk and consult with others, even such as come short of him in capacity, quickness and penetration: for since no one sees all, and we generally have different prospects of the same thing, according to our different, as I may say, positions to it. (*Conduct*, § III)

The parallel between Locke and Nietzsche is never clearer than it is in the above paragraph. The 'position' of other people in relation to the object of investigation is different to our own. Therefore consultation with others is a means of increasing our objectivity, by the inclusion of views or perspectives which we cannot obtain alone. Notice that Locke's claim is not simply that testimony and cognitive interactions are invaluable for the development of an individual's reason. Rather, social interaction must be deliberately arranged in order to maximise cognitive advances:

> In this way we may see the reason why some men of study and thought, that reason right and are lovers of truth, do make no great advances in their discoveries of it…they are very often mistaken in their judgments: the reason

[2] For example, see *Conduct* § III on Reasoning, § XIX on Universality, and § XLIV on Bottoming.

> whereof is, they converse but with one sort of men, they read but one sort of books, they will not come into the hearing of but one sort of notions. (*Conduct*, § III)

In order to maximize our reasoning capacity, it is not only necessary that we talk to other people in general, but also and in particular that we talk to others who hold different 'sorts of notions' to ourselves. This moves beyond the mere necessity of testimony, to the necessity of developing social structures which maximise cognitive advances. The right social structures aid the development of reasoning by providing plenty of 'exercise'. Locke defines 'exercise' as interaction with other individuals who are knowledgeable (*Conduct*, § III). This in mind, he sketches a brief hierarchy of human reasoning. A 'day labourer in a country village' is at the bottom, since he has little knowledge and no interaction with individuals who have more knowledge. He is surpassed by 'porters and cobblers' in cities. This is because their lives in the city provide them with access to more information through testimony; and also because they are more likely to come across various different opinions and different perspectives on a given matter, thus giving their reasoning skills more exercise. With this contrast in mind, Locke criticizes:

> A country gentleman who, leaving Latin and Learning in the university, removes thence to his mansion house, and associates with neighbours of the same strain, who relish nothing but hunting and a bottle; with these alone he converses, and can away with no company whose discourse goes beyond what claret and dissoluteness inspire. Such a patriot, formed in this happy way of improvement, cannot fail, as we see, to give notable decisions upon the bench at quarter sessions, and eminent proofs of his skill in politics, when the strength of his purse and party have advanced him to a more conspicuous situation. To such a one truly an ordinary coffee-house gleaner of the city is an errant statesman, and as much superior to, as a man conversant about Whitehall and the court is to an ordinary shopkeeper. (*Conduct*, § III)

Locke's critique regarding the reasoning capacity of the 'country gentleman' is twofold. Firstly, whilst the gentleman was somewhat informed of 'Latin and Learning', he is geographically cut off from the cities where information flows freely (just as the country labourer is) and deliberately keeps the company least likely to

challenge his ideas or provide him with new information. Their views, their perspectives, are insufficiently different from his view, both in terms of opinion and in terms of information, to provide any assistance in his reasoning processes. Therefore, when the 'country gentleman' is called upon to make political decisions, he is ill-equipped to do so, and inevitably makes them badly. Whilst Locke focuses here (for amusement) on the weaker case of a gentlemen who is not particularly interested in developing his reasoning capacities, his analysis would also apply in the case of an individual who genuinely *was* interested in doing so. The central point is that the cognitive isolation of the country gentlemen diminishes his capacity to reason well and to make appropriate decisions. It is clear, then, that limiting oneself to 'one sort of notions' carries two meanings. The first kind of restriction is limiting the information to which you have access. This can mean reading only the 'one sort of books' which address your favourite subjects, and keeping company only with people who share those interests. These behaviours reduce the amount of information one has *and* reduces the number of potential useful connections one can make between different areas of knowledge. This, in turn, will limit one's capacity to reason well.

The second meaning of limiting oneself to one 'sort of notions' is reading only those authors who share one's opinions and prejudices, in order to avoid hearing a critical voice. This limits one's objectivity, by limiting the number of perspectives we can 'bring to bear on one and the same matter' (Nietzsche 1998: 83). Reading well-informed authors with whom you disagree is useful for sharpening one's reasoning skills; whether it is a disagreement about empirically discoverable facts, or about moral or political values. In both cases, it is necessary to read one's opponents in order to understand the strengths as well as the weaknesses of their position, and of one's own. Lack of prejudice, then, is essential for the production of good judgment, and lack of prejudice is best achieved by as great a familiarity as possible with the viewpoints, or perspectives, of others, even and especially others with whom one disagrees. Optimal reasoning, as Locke earlier argued, relies on having a view upon 'all that has a connection' with a matter: or, in the language of the *Essay*, maximizing the number of relevant *connections between ideas* which involve the matter at hand. Refusing to engage critically with the viewpoints of others will limit reasoning capacity, objectivity and good judgement; so, too, will limiting your studies only to subjects of immediate interest.

§4.1: Locke on Testimony and Received Opinion

The previous section has shown that Locke, like Nietzsche, acknowledges that the development of reasoning and objectivity requires the acquisition of multiple relevant perspectives upon objects of enquiry. Furthermore, Locke realises that such development is achieved primarily (if not solely) by social means. It is in this sense that I refer to Locke as a social epistemologist. However, this title may sit ill at ease with Locke's frequently expressed suspicion of testimony as a knowledge source. Locke has gained a reputation for being passionately opposed to the possibility of gaining knowledge via testimony. Perhaps the clearest example of this is in Michael Welbourne's *The Community of Knowledge* (1981). Welbourne regards Locke's epistemology as fundamentally opposed to a social conception of knowledge. He refers to Locke's famous statement that:

> The floating of other men's opinions in our brains, makes us not one jot the more knowing, though they happen to be true. What in them was science, is in us but opiniatrety; whilst we give up our assent only to reverend names, and do not, as they did, employ our own reason to understand those truths which gave them reputation. ... In the sciences, every one has so much as he really knows and comprehends. What he believes only, and takes upon trust, are but shreds; which, however well in the whole piece, make no considerable addition to his stock who gathers them. Such borrowed wealth, like fairy money, though it were gold in the hand from which he received it, will be but leaves and dust when it comes to use. (*Essay* I, III §24)

Surely, Welbourne argues, this would mean that it is impossible to know that the Persians were defeated at Marathon, since we would merely be assenting to 'the reverend name of Herodotus' (Welbourne 1981: 302). Welbourne regards Locke's theory as amounting to the claim that 'in the process of transmission through testimony knowledge is inevitably denatured'; he contrasts this to his own theory that 'knowledge is essentially commonable... that it is capable of being transmitted from one person to another via speech or writing' (Welbourne 1981: 303). In the course of his article, Welbourne argues that if Locke is correct in his (apparent) belief that knowledge is denatured via testimony, we are left without any conception of a

community of knowledge. He claims that Locke 'has no understanding' of 'the dynamics of a community of knowledge' and indeed that 'he could not even conceive of the possibility of a community of knowledge' (Welbourne 1981: 303). I hope, during the course of this section, to demonstrate just how uncharitable an interpretation of Locke this is.

Firstly, I will acknowledge in (§4.2) that according to Locke, testimonial knowledge (like perceptual knowledge) always falls short of certainty. It amounts to neither intuitive nor demonstrable knowledge. Instead, in receiving testimony, we assent to or doubt propositions on the basis of the expertise and trustworthiness of the speaker, whilst understanding that the conclusions we come to thereby are only *probably* true.

(§4.3) addresses 'received opinion', where a belief is held by an individual simply because it is held by the majority of that individual's social group. Received opinion cannot amount to knowledge, because even if the beliefs in question are true, the individual has insufficient justification for the beliefs. I will then show a clear distinction, present in Locke's work, between 'testimony' and 'received opinion'. (§4.4) compares 'received opinion' to 'testimony'. Testimony can lead to knowledge, provided that the individual has critically assessed the testimonial source. In clarifying such a distinction, Locke provides us with a very basic framework for assessing the cognitive optimality of groups. Taking the distinction between testimonial belief and received opinion as our starting point, we can begin to delineate the differences between a cognitive environment which can provide the former, and one which is only capable of forming the latter.

As we shall see, part of that critical assessment involves seeking out alternative sources of testimony on the subject in question. For a social epistemic problem, namely our tendency to accept common received opinions without question, Locke offers a social epistemic solution. The solution is partly the responsibility of the individual, as they must seek the alternative sources out. But it is largely the responsibility of a social group as a whole, firstly to provide the alternative sources, and secondly to render them accessible.

§4.2: Certain Knowledge and Judgement

According to Locke's account, there are two kinds of *certain* knowledge. There is *intuitive* knowledge, which is the perception of the immediate agreement or disagreement of ideas; for example, that black is not white. This is the most certain and clearest form of knowledge. There is also *demonstrative* knowledge, which is the perception of the agreement or disagreement of ideas, but via one or more intervening proofs (or one proof with several premises). Demonstrative knowledge is just as certain as intuitive knowledge; but it involves propositions which it is possible for a thinker to doubt, before that doubt is removed by an understanding of the intervening proofs. For this reason, Locke regards demonstrative knowledge as less clear, 'like a Face reflected by several Mirrors one to another … with a lessening of that perfect Clearness and Distinctness' (*Essay*, IV, II, §6). The distinction between intuitive and demonstrative forms of knowledge is therefore one of degree of complexity. Take the following two mathematical problems:

(a) $1 + 1 = 2$

(b) If $y= x^4+5x^3-4x^2+7x-2$ then $dy/dx = 4x^3+15x^2-8x+7$

Whilst the solution to (b) is just as certain as the solution to (a), only (a) is intuitive knowledge. Problem (b) requires the thinker to hold several steps of reasoning in her head at once, whilst problem (a) does not. The point is not merely that mistakes are more likely to creep in when solving the latter mathematical problem than the former (although this is important). Rather the key difference is merely the necessity of several steps of reasoning, prior to which the answer was unknown (*Essay* IV, II, §5).

Locke acknowledges that intuitive and demonstrative forms of knowledge are exceedingly rare; that we cannot and should not depend solely upon them in our cognitive endeavours:

> Man would be at a great loss, if he had nothing to direct him, but what has the Certainty of true Knowledge … He that will not eat, til he has Demonstration that it will nourish him; he that will not stir, till he infallibly knows that the

> Business he goes about will succeed, will have little else to do, but sit still and perish. (*Essay* IV, XIV, §1)

Given the limitations of our certain knowledge, we are largely dependent on our judgment. This is where 'the Mind takes … any Proposition to be true or false, without perceiving a demonstrative Evidence in the Proofs' (*Essay* IV, XIV, §3). We are persuaded, but not completely certain. Locke notes that sometimes we rely on mere judgement when demonstrative proof is available. For example, if I do not understand the mathematical proof which shows that the angles of a triangle add up to 180 degrees, then I could merely assess the likelihood of the proposition being true, based upon the testimony of my teachers. Locke criticises this habit, blaming 'Laziness, Unskillfulness, or Haste'[3] (*Essay*, IV, XIV, §3). Generally, however, certain knowledge is not to be had, and therefore the use of judgement is necessary. Therefore it is important to improve our judgement as best we can. This includes developing the capacity to judge testimony well, as the rest of this section will show.

Locke could be said to be sceptical about the capacity of testimony to produce knowledge. However this 'scepticism' is merely an acknowledgement that an assent to testimony is not equivalent to intuitive or demonstrative knowledge. This claim is neither extreme nor contentious. Furthermore, Locke's awareness of the limitations of testimony should be weighed against the fact that he grants the assessment of testimonial sources a central role in the development of good judgement.

§4.3: The Opinion of the Majority

Locke consistently encourages his readers to seek out testimony from people of differing opinions and differing expertise, 'even such as come short ... [of oneself] in capacity, quickness, and penetration' (*Conduct*, §III). Yet he also makes a constant appeal to his readers to ensure that they reason out arguments out for themselves rather than accepting the opinions of others without question. In particular, he warns against accepting an opinion as true simply because it is held by the majority of one's social group:

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[3] However, depending on the complexity of the intervening proofs and the relative expertise of the person giving the testimony to the person receiving it, it is often to our advantage to choose a reliable testimonial source over investigating the proofs ourselves. In cases such as these, we are of course responsible for assessing the credibility of the testimonial source (see §4.4).

'... [one example of] *wrong Measure of Probability* I shall take notice of, and which keeps in Ignorance, or Errour, more People than all the other together, is ... *the giving up our Assent to the common received Opinions,* either of our Friends, or Party; Neighbourhood, or Country. How many Men have no other ground for their Tenets, than the supposed Honesty, or Learning, or Number of those of the same Profession? As if honest, or bookish Men could not err, or Truth were to be established by the Vote of the Multitude: yet this with most Men serves the Turn...

... All Men are liable to Errour, and most Men are in many Points, by Passion or Interest, under Temptation to it...

There is not an Opinion so absurd, which a Man may not receive upon this ground. There is no Errour to be named, which has not had its Professors...

(*Essay*, IV, XX, §17)

Locke, then, *both* acknowledges the centrality of the social to knowledge acquisition, encouraging cognitive social interaction as an essential tool for the development of reasoning and objectivity, *and* encourages his readers to act with extreme caution when receiving information from others; in particular, to avoid accepting an opinion as true simply because it is the opinion of the majority of some group of which we happen to be a member.

Locke's two recommendations are by no means contradictory, but the former seems to amount to an acceptance of a social view of knowledge development, whilst the latter seems to amount to a deep regret that it is the case, that we must rely on our flawed social sources rather than possessing the 'comprehensive faculties... perfect and exact views of all finite beings' that the angels possess (*Conduct*, §III). However, as will become clear, the capacity to avoid blindly accepting majority opinion is a social epistemic problem with a social solution. In order to solve it, we do not merely need to sharpen up our individual reasoning capacity, but to alter our social structures in order to maximise our potential for gaining knowledge by testimony. In order to understand how Locke's two recommendations relate to one another, we must first unpack his distinction between 'testimonial knowledge' on the one hand and

'received opinion' on the other. In the process we shall find that there is no need for Locke's regret.

§4.4: 'Testimonial Knowledge' versus 'Received Opinion'

In the *Conduct*, in the section entitled 'Reasoning', Locke identifies several flaws which prevent individuals from reasoning well. Among these is the problem of accepting received opinions without critically examining them. Locke criticizes those who:

> seldom reason at all, but do and think according to the example of others... for the saving of themselves the pain and trouble of thinking and examining for themselves. (*Conduct*, § III)

This flaw arises from a dependency on the opinions of others to the exclusion of one's own reasoning capacities. In response to this, Locke recommends caution in accepting the opinions of others. However, another flaw, named immediately after, is that we 'have not a full view of all that relates to the question' (*Conduct*, § III). As we have already discussed, the solution to this (insofar as there is one) is social in nature; it depends upon cognitive interactions with others. In quick succession, then, Locke offers two recommendations. The first is not to depend upon others to reason for you. The second, which we have already examined in some depth, is to ensure that you have as many relevant perspectives on the issue as possible. The latter depends upon interactions with others.

Clearly therefore, Locke is making a distinction between 'thinking according to the example of others' and seeking out the testimony of others. The distinction rests on whether an individual has *critically assessed* the testimony of others, i.e. assessed the likelihood of their testimony being accurate, rather than merely accepted it. We can see, then, that the two flaws in reasoning Locke described above are connected to each other. Those who save themselves 'the pain and trouble of thinking and examining for themselves' are also unlikely to critically assess the testimony of others. Therefore, they will merely possess 'received opinions', not testimonial knowledge, even where the potential for such knowledge exists. Locke even goes so

far as to tell us what constitutes 'critical assessment' of testimonial sources. He argues that we ought to evaluate the following:

> ... In the Testimony of others, is to be considered,
> 1. *The Number.*
> 2. The Integrity.
> 3. The Skill of the Witnesses.
> 4. The Design of the Author, where it is a Testimony out of a Book cited.
> 5. The Consistency of the Parts...
> 6. *Contrary Testimonies.*
>
> (*Essay*, XV, §4, italics mine)

In deciding whether to accept an individual's testimony, we have reason to assess their integrity. Are they known to be dishonest? This will make us more cautious about believing their testimony. We must also assess their skill as a witness. By 'skill' in this context, we might reasonably infer 'the ability to interpret the evidence accurately'. This would require a certain amount of knowledge relevant to the context. For example, suppose I arrive at the train station on my way home to discover an unusually large crowd of people filling the streets, turning right out of the station. Being familiar with my home town, I have the necessary skill to conclude 'Ah! There is a rugby match this afternoon'. A less skillful witness would have come to a false conclusion (perhaps the music festival is today), or else no conclusion at all.

In the case of testimony from a book, we must consider the 'design' of the author; the purpose of writing the book. If that purpose is to pursuade the reader of a particular opinion on a matter, they might be guilty of presenting the reader with a unrepresentative account of the evidence, which the reader must balance out by seeking alternative testimonies elsewhere.

Finally we must consider how consistent their story is. Do the various aspects of their account mutually support each other? There is good reason to believe that Locke meant something more by 'consistent' than merely the absence of internal contradictions in the testimony. Whilst analytic philosophers of the 20th century have taken 'consistency' to mean *logical* consistency, i.e. the absence of contradiction, the

coherentist theories of the 19th and early 20th centuries used 'consistency' in a more demanding sense. According to the coherentist sense of 'consistency', a testimonial account, or a piece of justificatory reasoning, is consistent if its various aspects provide mutual support for each other. (As we shall see, this is much as the various entries in Haack's crossword puzzle mutually support each other.) Given Locke's focus elsewhere on 'the light, which the remote and scattered parts of truth will give to one another' (*Conduct*, § III), it is reasonable to conclude that he meant 'consistency' in this sense.

Requirements 2, 3, and 4, directly concern the cognitive virtues (or otherwise) of the person giving the testimony, whilst 5 concerns the internal consistency of the testimony. Thus far, then, our evaluation of the testimony has focused firstly, on the individual providing us with the testimony, and secondly, on the consistency of the testimony itself. However, Locke's further requirements (1 and 6) pertain to the existence of *alternative* testimonies. We may have confirmed the various cognitive virtues of our witness, but we have not completed our assessment until we have also ascertained whether there are contradictory testimonies and how many (we might better say, what proportion) of the testimonies available agree with our witness. Therefore, when receiving testimony, our cognitive responsibilities include assessing not only our witness, *but also alternative witnesses*, where they exist.

It follows from this that any cognitive group which limits, deliberately or otherwise, my access to such relevant alternative accounts would also limit my capacity to complete the critical assessment which is a requirement of my gaining testimonial knowledge. Therefore, membership of such a group (or cognitive dependence upon one) would increase the likelihood of my having to make do with 'received opinion'. Such a cognitive group would also of course limit the 'multiple perspectives' that I have access to, thus also limiting my objectivity and my reasoning capacities.

With Locke's evaluative approach to testimony in mind, let us turn back briefly to Michael Welbourne's account. Citing the common recommendation that 'if you want to know the time, ask a policeman' (Welbourne 1981: 304), he claims that whilst his own theory endorses this advice, Locke's theory rejects it. Welbourne intends this to be a demonstration of the absurdity of Locke's approach to testimony. Yet we can see,

by reading through Locke's list for evaluating testimony, that he would regard it as perfectly possible to gain knowledge of the time from the testimony of a policeman. Three of Locke's evaluations are relevant here. Firstly, if one asks a member of the police force for the time, one has already tested the integrity of the person giving the testimony, by deliberately choosing an individual who has been trained, tested and granted that position of authority. This is of course not a foolproof evaluation, but it is a useful evaluation nonetheless. As regards the skill of the witness, in this case the ability to tell the time, well, surely Welbourne would not claim that the inquirer could know the time as the result of the testimony of a police officer who had no watch? Furthermore, if it was especially important that we knew the exact time, then it would not seem strange to ask several people, in order to reveal any watch which was noticeably fast or slow; in short, we would seek out alternative testimonies. Admittedly, many people would not take this final step; but they would be happy to admit that as a result, their justification in believing the time is not as strong as that of a person who had done so.

Locke has, in embryo, the means to distinguish between a cognitively healthy group and a cognitively suboptimal one. He does not, however, pursue this line of thought so far as to provide us with any set of general standards which all such groups should attain, as for example Helen Longino does. (We shall examine Longino's standards in Chapter Two). It is feasible to speculate that Locke didn't consider it possible to formulate or insist upon such standards; perhaps, that Locke may have retained some of the Enlightenment attitude that reason and group traditions are fundamentally opposed to one another. Certainly Locke claims that:

> all the world are born to orthodoxy: they imbibe at first the allowed opinions of their country and party, and so, never questioning the truth, not one in a hundred ever examines. He that considers is a foe to orthodoxy, because possibly he may deviate from some of the recieved doctrines there. (*Conduct*, § 34)

As we have seen, Locke acknowledges that social interactions are central to knowledge acquisition and the development of reasoning. He combines encouraging social interaction with encouraging caution regarding testimony. He urges especial caution when accepting the opinion of the majority, to take care that we are gaining

testimonial knowledge and not merely received opinion. Locke's pessimism is due to the fact that it is easier for people to accept orthodox received opinion than it is for them to achieve testimonial knowledge. Therefore, he fears that few will make the necessary efforts. I have said earlier that Locke seems to retain some regrets that we must, as human beings, rely on social sources of knowledge rather than possessing the 'comprehensive faculties...perfect and exact views of all finite beings' of the angels (*Conduct*, §3). Of course Locke is right in maintaining that this is beyond us. However, we can be better equipped than Locke imagines in assessing the social sources of our knowledge.

'He that considers', Locke argues, 'is a foe to orthodoxy'. However, to be a foe to orthodoxy is not to be a foe to the tradition which holds that orthodoxy. This shall be demonstrated in Chapter Two, especially with reference to Frederick Will and the case study of Lise Meitner's work. Chapter Five with explore the issue in depth with case studies from physics and archaeology. We shall see that those cognitive traditions which encourage their members to examine and question orthodox norms and practices are more likely to advance towards greater understanding of the physical world. In so doing, we shall reveal how our reasoning skills arise from our cognitive traditions. In fact, meaningful challenges towards cognitive traditions are only possible after individuals have been inculcated with the norms and practices of the tradition in question. In this sense, reason and tradition are thoroughly interdependent.

§5: Chapter Conclusion

This chapter has examined the Cartesian account of reason and tradition, and has demonstrated that the dichotomy does not hold true. We have seen that epistemologists who retain the individualist conception of reason often do so because they consider it necessary in order to preserve realism. However, they are mistaken in this. We can understand objectivity, as Nietzsche does, not as a 'God's eye view' but as the possession of multiple perspectives on a given object of investigation, and the ability to use several perspectives on a matter in the course of an investigation. As we have seen, this perspectivism does not imply a commitment to relativism; it is fully compatible with realism. Furthermore, as we learn from Locke, these perspectives are acquired primarily, if not solely, via social means.

It is true that Locke, recognising the fallibility of our conclusions gained via testimony, refused to grant testimonial conclusions the honorific 'knowledge' within the *Essay*. (He would not grant conclusions reached via sensation this honorific either). For this he has been understandably criticised and considered an opponent of testimonial knowledge. However I consider that this issue of terminology is less important than it is commonly perceived to be. Locke does recognise the possibility of forming justified true beliefs from testimony. The strongest evidence for this lies in Locke's perceptive and clear distinction between 'testimony' and 'received opinion'. In the case of received opinion, an individual accepts the belief of the majority (of some social group) without critical assessment. Whilst this might lead to true belief, it cannot lead to knowledge, since the third element of *justification* is missing. Testimony, on the other hand, involves assessing one or more individual in terms of their integrity, skill, and lack of bias. The testimony itself must be examined for internal consistency (in the more demanding coherentist sense of the term). Finally, the witness' account must be compared to any other existing accounts. Therefore, in order to justify my testimonial belief as far as it is possible to do so, I must be able to access those alternative accounts.

There are two points to note from this. The first is that the difference between received opinion and testimony is that the latter involved *justifying* one's beliefs. This would suggest that Locke recognises a difference between a mere true belief gained from other people, and a 'justified true belief' gained from other people. Within Locke's own writings, the issue is complicated by his refusal to grant the honorific 'knowledge' to anything less than complete certainty. However, those of us, now, who are pragmatists, who accept that knowledge itself can be fallible, have much to gain in adopting and improving Locke's conception of social justification. The most powerful implication of Locke's account is that any cognitive group which limits access to alternative testimonies would also limit the potential of its members (and others who depend upon it) to achieve testimonial knowledge, rather than mere received opinion. This implication, coupled with the detailed recommendations of the Thoughts and the Conduct regarding the cognitive effects of social interactions, lead me to regard Locke as a social epistemologist. Furthermore, I believe that Locke's conception of the development of reasoning, coupled with a Nietzschean perspectivist conception of objectivity; can provide a strong foundation for a structured assessment

of cognitive groups. It remains for us to build that structured assessment; which is the task of the following chapter.

Chapter Two: Cognitive Optimality

§1: Introduction

In the previous chapter, I show that the Lockean conception of reasoning, combined with the Nietzschean conception of objectivity, provides us with a strong basis for the assessment of cognitive groups. In fact combining the two creates a new concept which requires a new name; I shall refer to it as 'Linkmaking'.

To 'make links' is a creative act; and a social act; but it also suggests an engagement with the physical world. Thus the phrase 'Linkmaking' seems appropriate for the realist social epistemology I am putting forward. The phrase raises two questions: what kinds of links does it refer to, and in what sense are they 'made'? Potentially, it could mean:

1. Links between objects in the physical world (such as causal links, similarities)
2. Lockean 'links between ideas'
3. Social links between individuals and between groups,
4. Links between disciplines or areas of investigation.

The first kind of link exists whether it is noticed or not, but it is 'made' (in an everyday English sense) when it is noticed. The second sort of links are made by educators and individuals in their studies and investigations, as we see in *Thoughts* and *Conduct*; in order to maximise our relevant connections between ideas and the number of relevant perspectives we have on any given object of investigation. The third kind is made in the sense that we form our cognitive groups and arrange them in such a way as to maximise our potential to make the first two kinds of links. The fourth encompasses the first and the third, since areas of investigation study physical objects that have natural links, but consist of the work and interactions of cognitive social groups. I shall use 'Linkmaking' to encompass *all four* of these.

This may seem to 'fudge together' four quite separate concepts, but the melding is necessary. The project of improving our Linkmaking is holistic; we cannot *just* improve the number of relevant connections between our ideas (Link type 2), because doing that requires empirical investigation to see whether the connections really are relevant (Link type 1), or else seeking out advice on whether they are relevant (Link type 3), which may in turn rely on whether the advisor is aware of relevant interdisciplinary links (Link type 4). In short, Linkmaking requires not only making links, but making *links between* different kinds of links.

The first part of the chapter is concerned with placing the concept of Linkmaking within modern social epistemology. I shall begin this chapter by establishing the relationship between Linkmaking and Susan Haack's founderentism (§2). We shall see that neither of the two rival theories of foundationalism or coherentism can be combined with the Linkmaking project, and that in fact Linkmaking requires founderentism. Indeed I will argue that Haack's conception of justification can be connected to Locke's. I shall go on to delineate Helen Longino's high standards for an effective cognitive community: the existence of public standards for the criticism of evidence, an uptake of criticism, public standards for the evaluation of theories, and equality of intellectual authority (§3). I shall argue that these standards are necessary to, and benefit from, the Linkmaking project.

The second part of the chapter is concerned with addressing some of the sceptical questions which surround a social conception of knowledge in general, and Linkmaking in particular. We shall learn from Frederick Will (§4) how the Cartesian conception of reasoning has led to scepticism about the possibility of governing our cognitive traditions; and the likelihood of our practices acting as access to, rather than a barrier keeping us from, objects of enquiry. Will's proposed solution is that we become aware of and overcome the Coherence and Relativist Illusions. We shall see that overcoming these Illusions is part and parcel of Linkmaking.

Thomas Green (§5) shall then provide us with a powerful illustration of how the Cartesian conception of reasoning has damaged pedagogy; and show us that the acquisition of norms (or 'normation') is best understood as the development of *conscience*, including an obligation to seek out improved norms and methods.

Conscience is both self-criticism and a disinterested, authoritative voice; something Green explains in terms of our status as members of normative communities: conscience is not the judgement of an individual or a group, but the judgement of a group *member*. This is an important concept to grasp in establishing how group practices are governed, when they function well. Green's account depicts an education which has much in common with the Lockean, but goes beyond the Lockean. Locke concerns himself with social epistemology insofar as certain social arrangements are more likely to lead to knowledge for any given individual. Green, in speaking of education as the development of *conscience*, is also concerned with developing the individuals' sense of cognitive responsibility; including her responsibility for other people's knowledge. This is *proactive* Linkmaking; an education to produce educators, researchers, and effective members of cognitive communities; and, significantly, an education to produce people who are capable of educating themselves.

In subsequent chapters in this thesis I shall defend Linkmaking against possible attack. The major threat to Linkmaking, which I will address in Chapters Three and Four, is the strong form of Kuhnian incommensurability. If incommensurability were real, it would place serious limitations on all four types of Linkmaking; and even though (as I shall argue) there is no such thing as incommensurability in the strong sense, the belief that there is such a thing has been sufficient to prevent optimal Linkmaking in certain areas of enquiry, notably sociology of science.

§2.1: Haack's Founderentism

One effective way of placing 'Linkmaking' within the field of social epistemology is to articulate its relation to Susan Haack's 'founderentism'. What follows is an explication of founderentism and the epistemic problems it answers. The section begins by presenting the two rival theories of foundationalism (§2.2) and coherentism (§2.3). We shall then examine arguments in favour of foundationalism (§2.4) and coherentism (§2.5) respectively. In the process we shall see how Haack reveals the problems inherent in the two rival theories, and how those problems suggest the possibility (and necessity) of an intermediate theory, founderentism; which we shall

explore in (§2.6). Finally we shall relate founderentism to the Linkmaking project, and in particular note how Haack's conception of justification can be connected to Locke's (§2.7). We shall conclude that founderentism, unlike its rivals, provides us with an account of justification compatible with the Linkmaking project (§2.8).

§2.2: Foundationalism

Foundationalism is the epistemological position which states that there is a distinction between 'basic' and 'derived' beliefs. 'Derived' beliefs are justified by the 'basic' beliefs which support them. Basic beliefs are justified, but not by any other belief. Instead, depending on the form of foundationalism espoused, they are justified by experience (which may or may not be empirical); or by there being some causal chain between the belief and the state of affairs that makes it true (Haack 1993: 15); or else they are 'self-justifying'. (The final of these three cannot, of course, apply to empirical knowledge). Some, but not all, foundationalists regard 'basic' beliefs as infallible. The term 'foundationalism' reflects the commitment to 'basic beliefs' as being the epistemic foundation upon which more complex 'derived' beliefs can rest. This support is, in Haack's terms, one-directional; meaning that whilst 'derived' beliefs are supported by 'basic' beliefs, no 'basic' belief can gain any support from a derived belief.

§2.3: Coherentism

Coherentism states that a belief is justified if, and only if, it belongs to a coherent set of beliefs. Some coherentists may add the requirement that the set be 'consistent', and that it be 'comprehensive'. Coherentists do not regard any beliefs as 'basic'. Strict coherentism regards all beliefs within the same set as justified to the same extent; whilst weaker forms of coherentism will allow that certain beliefs within a set can be more justified than others. Coherentists may frame this in terms of how 'deeply embedded' the particular belief is within the set. Alternatively, some claim that certain beliefs have 'initially distinguished status' (Haack 1993: 18), independently of their interconnections with other beliefs (although foundationalists wonder where this distinguished status is supposed to come from). However, all coherentists agree that the justification for our beliefs can only be derived from the mutual support that our beliefs give each other.

§2.4: The Arguments for Foundationalism/against Coherentism

Foundationalists offer the following argument in their favour. Consider a 'chain of beliefs', as follows. Person A believes that p, on the grounds of his belief that q. He believes q on the grounds of his belief that r. He believes r on the grounds of his belief that s. Therefore, he is justified in believing r, and therefore justified in believing q, and therefore justified in believing p, if and only if he is justified in believing s. This is known as the 'infinite regress' argument. Foundationalists claim that unless certain beliefs, 'basic' beliefs, can be justified without reference to other beliefs, then this chain leads to an infinite regress; in which there are 'turtles all the way down'. As Haack points out, there are in fact other alternatives: the chain could go round in a circle, or the chain could end with an unjustified belief. However the foundationalist would be as dissatisfied with these alternatives as with the infinite regress, since neither of them would leave person A with the justified belief that p.

Furthermore, foundationalists take issue with the coherentists' demand for consistency. Firstly, it is arguable that consistency is too strict a requirement for knowledge. Coherentism seems to entail that if an individual has an inconsistent set of beliefs, then none of the beliefs in that set are justified. Surely this is too much to ask. No one's beliefs are entirely consistent, but my knowledge that grass is green is surely not affected by any inconsistencies in my mental chronology of the Second World War. Coherentists might respond that the 'consistency' requirement pertains not to the entire set of the individual's beliefs, but to some smaller sub-set relating more specifically to his belief that p. However there is an alternative way of attacking the consistency requirement, which is to claim, instead, that it is too weak. It is, the foundationalist argues, perfectly possible to have an entirely consistent set of beliefs which are mutually supporting, and yet are not true. Haack calls this the 'consistent fairy story objection' (Haack 1993: 26). Nor can the coherentist respond to this objection simply by adding the requirement of 'comprehensiveness'; as it is possible to create an extremely elaborate fictional universe (as many science fiction writers have). The objection here is simple and powerful; coherentists do not allow non-belief input to justify beliefs; yet without such input, the justification of an empirical belief would seem to be no indication of whether the belief is true.

§2.5: The Arguments against Foundationalism/for Coherentism

Meanwhile, foundationalism faces its own problems. Extrinsic foundationalism, which states that basic beliefs are justified by there being some causal chain between the belief and the state of affairs that makes it true (Haack 1993: 15); is vulnerable to what Haack calls the evidentialist objection. This is the objection that the subject may not be aware of the causal chain in question; and could therefore be justified in believing something without knowing any evidence for the belief. This is highly counterintuitive (Haack 1993: 28).

Another objection made by coherentists, and which applies to experiential as well as extrinsic foundationalism, is the accusation that foundationalists conflate causal relations with logical relations. For example, A's seeing a tree may cause A to believe that there is a tree in front of her, but it does not logically entail that there is in fact a tree in front of her. Justification, the coherentists claim, is a logical matter, and can therefore only occur in terms of relations between beliefs. However, as Haack points out, this argument begs a huge question in assuming that justification is, in fact, purely a logical matter.

A more powerful argument lies in articulating the tension inherent in the demands foundationalists make of basic beliefs. They require that basic beliefs are justified purely by experience, and not in relation to any other beliefs. This requirement is best met by beliefs which are very simple in content. However, foundationalists also require that basic beliefs be sufficiently rich in content to 'be capable of supporting a substantial body of other beliefs' (Haack 1993: 30). These two requirements are in tension with each other. This argument is particularly potent against the forms of foundationalism which claim that basic beliefs are infallible.

§2.6: Why Founderentism?

Haack's position is that the objections against both foundationalism and coherentism are best addressed by the development of an intermediate theory. Whilst she acknowledges that a weak foundationalist position escapes the arguments presented by the coherentists, she emphasises that there remains one insoluble problem with the

weak foundationalist position. This is the insistence that basic beliefs get their justification solely from something other than beliefs. Haack uses a simple example to demonstrate the implausibility of this claim:

> Suppose A believes that there is a dog present, and that he believes this because of his current sensory experience (his seeing what appears to be a dog); then A is justified prima facia…Would not A be more justified in believing that there is a dog before him if he also justifiedly believed that his eyes are working normally, that he is not under the influence of post-hypnotic suggestion, that there are no very lifelike toy dogs around, etc etc.? Surely, he would. But the weak foundationalist cannot allow this. (Haack 1993: 31)

In short, Haack argues that even the most plausible of the existing alternatives within the foundationalist/coherentist debate falls short at this critical point: it is intuitively plausible to regard basic beliefs as supportable, at least in part, by complex beliefs. Foundationalism cannot allow this, because they regard basic beliefs as justified wholly without reference to other beliefs; coherentism cannot allow it for the simple reason that they do not accept the existence of privileged 'basic' beliefs at all. So it becomes necessary to establish a new epistemic theory which can allow for this familiar phenomenon.

Founderentism aims to retain the most plausible aspects of both foundationalism and coherentism whilst avoiding the major problems of each. With regard to foundationalism, the aim is to retain an awareness of the relevance of experience to empirical justification (and indeed to do so in a less 'forced and unnatural' way than the foundationalists do), whilst avoiding the one-directionalism of the foundationalist account, allowing 'derived' beliefs to provide support to 'basic' beliefs. With regard to coherentism, foundationalism retains the view that beliefs can offer mutual justificatory support to each other; but it also allows for non-belief input contributing towards justification. Furthermore, it avoids both of the coherentist's problems regarding consistency: neither being committed to regarding all beliefs within an inconsistent set as unjustified, nor being vulnerable to the 'consistent fairy story' objection.

The concept of justification offered by founderentism is best understood in reference to the model of the crossword puzzle.

> 'How reasonable one's confidence that a certain entry in a crossword puzzle is correct depends on: how much support is given to this entry by the clue and any intersecting entries that have already been filled in; how reasonable, independently of the entry in question, one's confidence is that those other already-filled in entries are correct, and how many of the intersecting entries have been filled in. Analogously, how good A's ... evidence with respect to p is would depend on:
>
> 1) How favourable A's direct ... evidence with respect to p is,
> 2) how secure A's direct ... reasons with respect to p are, independently of the ... belief that p,
> 3) how comprehensive A's ... evidence which respect to p is.
>
> (Haack 1993: 82)

The evidence provided by the clue is analogous to the 'basic' beliefs of the foundationalists. Furthermore, were the crossword puzzle to have no clues at all, it would not be possible to fill in any of the entries. This part of the analogy illustrates the relevance, indeed the centrality, of experience to empirical justification. However, the 'one-directionality' of foundationalism, according to which basic beliefs are not justified with reference to any other belief, is avoided on the founderentist account. Instead, the plausibility of a particular entry being the correct one depends not just on how good a response it is to the clue, but also on whether it fits with other, intersecting entries which have already been filled in. Therefore, I might be justified in believing that I have the correct answer to a clue; but I am *more* justified if the intersecting entries would fit with the answer I am considering. If intersecting, already filled in entries contradict my new answer (if I have inconsistent evidence with respect to p), then my overall evidence is *indifferent* towards p at time t, and I must seek out more evidence before I can have any justified belief regarding p.

When we do seek out more evidence, there are two lines of investigation. The first is to re-evaluate the latest solution's relation to its clue: have we misinterpreted the clue;

missed part of the clue, is there another possible answer? The second is to re-evaluate one or more of the intersecting entries. What if we misinterpreted *the other* clue(s)? If we only have one or two intersecting entries, we are likely to pursue the second line of investigation with as much attention as the first; especially if we are not altogether confident in our answers. However, if there are several intersecting entries already filled in, and especially if each of those entries *itself has intersecting entries* which have already been filled in, then we are much more likely to focus on the first line of questioning (doubting our interpretation of this new clue) than we are to seriously investigate any of the others. Such a wholesale investigation would occur only after we are satisfied that there is no other possible interpretation of the latest clue. This is what is meant by 'mutually supporting beliefs'. The existing entries form a framework. If a new possible entry doesn't fit that framework; well, it is still possible that the new entry is correct, but that would require that there be pervasive mistakes running through the existing framework which have been missed over and over again. It is far more likely that the new possible entry is wrong. However, we can only be confident that there *are not* pervasive mistakes running through the framework, because each new entry has to be justified *in relation to its clue* as well as in relation to the other entries around it.

To apply the crossword metaphor, our justification for the belief that p lies both in the empirical (or analytical) evidence that p and in any beliefs I have which intersect with my belief that p. If (independently well-justified) beliefs intersect with p and are incompatible with p, or in tension with p, then I am less justified in believing p. Say, for example, I have glandular fever, I take homeopathic pills, and then I feel better. On the basis of the empirical evidence available to me, I would be justified in saying that the pills improved my health. However, I have intersecting beliefs: beliefs about what dilution is, and what the properties of water are; which contradict the belief that homeopathic pills can improve my health. (We are supposing here that I am aware of the principles of homeopathy). These intersecting beliefs make me unjustified in believing that the homeopathic pills made me better.

§2.7: How does Founderentism relate to Linkmaking?

I have said that 'Linkmaking' encompasses four kinds of links:

1. Links between objects in the physical world (such as causal links, similarities)
2. Lockean 'links between ideas'
3. Social links between individuals and between groups,
4. Links between disciplines or areas of investigation.

Foundationalism focuses on links of type 1. Making links of type 1 would be regarded as forming the 'basic ideas' which are the foundation of all complex ideas. However, the 'one-directionalism' of foundationalism places it at odds with a holistic Linkmaking project; according to which we cannot *just* improve our capacity to make links of type 1 without simultaneously cultivating links of type 2, 3, and 4. Coherentism, by contrast, focuses on links of type 2, and can easily be read to incorporate links of type 3 and 4. However, Linkmaking defines the optimality of links of types 3 and 4 in terms of their success in enabling us to make links of type 1, as well as type 2. Therefore, a coherentist view is incompatible with the Linkmaking project. Founderentism retains the empirical element of foundationalism whilst avoiding the one-directionalism. It also retains the coherentist virtue of mutual support among beliefs whilst avoiding the potential 'consistent fairy story' objection. This renders founderentism, unique among these alternatives, compatible with a holistic Linkmaking project.

Furthermore, the Linkmaking project can plug an important gap in the founderentist model of justification. A shortcoming of the metaphor of the crossword puzzle is that, when one is filling out a crossword puzzle, it is clear when the entries intersect and when they do not. However, it is not always so clear when our *beliefs* intersect; when the truth of one belief would be at odds with the truth of another belief. The purpose of the Linkmaking project is to bring those intersections, those connections, to light. This requires:

1. Having access to as much relevant information as possible
2. Possessing the ability to recognise *how relevant* information is

In requirement (1), the ambiguity of the phrase 'having access to information' is deliberate. 'Possessing relevant information' would only include memorised

information; however, 'having access to relevant information' would include having appropriate written records, the ability to speak to witnesses, and so on. If we are assessing cognitive optimality, this 'access' is every bit as relevant as 'possession'.

The crossword puzzle metaphor naturally incorporates requirement (1), but lacks sufficient emphasis on the requirement (2). This is, possibly, the result of a tactical move by Haack, who needs to avoid the objection levelled at coherentists that their demand for consistency is too strong. She therefore acknowledges that it is possible to have intersecting, conflicting beliefs and not be aware of the fact; yet denies that this need necessarily result in the beliefs being unjustified:

> 'Inconsistency in one's ... evidence with respect to some belief has the consequence that one is not justified in that belief. To avoid this a subject whose beliefs are inconsistent will have to keep the incompatible parts of his belief-set apart from each other; and this can be achieved only at the price of sometimes failing to take relevant evidence into account – which itself lowers the degree of justification of the beliefs it affects.'(Haack 1993: 87)

This is, prima facie, a plausible response; avoiding the conclusion that any inconsistency whatsoever in a belief set results in all the beliefs in that set being unjustified. Yet the approach has some odd consequences. It implies, for example, that if the incompatible parts of my belief-set regarding homeopathic pills are kept apart from each other (if I don't connect in my mind, say, diluting blackcurrant cordial and making it weaker, with diluting belladonna) then, it seems, my belief *stays justified*; although to a lesser degree than if I connected my beliefs and they *were* mutually supporting. On the other hand, if I succeed in doing the cognitively responsible thing and connecting the relevant ideas together, then my belief in homeopathy becomes unjustified. (Of course, one would hope that I would change my belief in these circumstances, but this isn't necessarily so). I do not deny that it would be more unreasonable of me to insist on the truth of my belief once all the evidence I have in my mind is arranged clearly before me, but surely it is misleading to say that before it was arranged clearly, I was justified? Did I not merely *believe myself* to be justified? The latter strikes me as more plausible. Furthermore, the example illustrates a difference between cognitive choices that result in possessing the maximum

possible number of justified beliefs, and cognitive choices which are *responsible*. It illustrates that the crossword puzzle is not, as it stands, a normative evaluation of what we, as cognitive agents, ought to do to ensure that our beliefs are justified.

In order for us to turn the crossword puzzle metaphor of justification into a normative model of what we ought to do in order to be cognitively responsible, it is necessary for us to consider the following question: how best can we assess whether or not certain of our beliefs intersect? Or, more generally, how can we train ourselves in order to recognise such intersections? Of course, that is what the project of Linkmaking seeks to achieve.

In closing, it is interesting to bear Haack's founderentism in mind whilst reading the following extract from Locke:

> 'He that will enquire out the best books in every science, and inform himself of the most material authors of the several sects of philosophy and religion, will not find it an infinite work to acquaint himself with the sentiments of mankind, concerning the most weighty and comprehensive subjects. Let him exercise the freedom of his reason and understanding in such a latitude as this, and his mind will be strengthened, his capacity enlarged, his faculties improved, and the light, which the remote and scattered parts of truth will give one another, will so assist his judgement, that he will seldom be widely out, or miss giving proof of a clear head and a comprehensive knowledge.' (*Conduct*, §3)

We have already noted in the 'Reasoning and Objectivity' chapter that this extract suggests a form of coherentism, in which the 'remote and scattered parts of truth' can support one another, 'assisting [our] judgement'. Furthermore, his educational writings (not just the *Thoughts* but also the *Conduct*) are designed to link those scattered parts of truth together as often and as firmly as possible, making use both of curriculum design and of social interactions. A Lockean education is in this sense a coherentist education. Yet Locke is, of course, an empiricist, perhaps the archetypal empiricist, and one who took great pains to demonstrate in the *Essay* that all our knowledge is traceable to sensory experiences; a position which foreshadows

foundationalism. Locke never saw a contradiction; Haack, having seen contradictions, has addressed them.

§2.8: Conclusion

We have seen that the problems inherent in foundationalism and coherentism point to the benefits of an intermediate theory; namely, Susan Haack's founderentism. We have also seen that this intermediate theory, unlike its rivals, provides us with an account of justification compatible with the Linkmaking project. We understand that in order for an individual's beliefs to be justified, they must *both* be internally coherent *and* consistent with empirical evidence. The cognitively optimal individual is the one who is aware of these cognitive responsibilities and is able to meet them. However, in order to meet them, they must be members of cognitively optimal *groups*; that is to say, groups in which a consensus amounts to strong evidence in favour of that consensus. We therefore need to establish a solid set of necessary conditions for institutions, the satisfaction of which would lead to a consensus in that group deserving the title 'knowledge'. For that task, we must turn to Helen Longino (1994).

§3.1: Longino on Necessary Conditions for Cognitive Optimality

In this section, we shall explore and defend Longino's conditions for a consensus within a cognitive group to count as knowledge. We shall begin by addressing the sociologist's skeptical claim that it is impossible to isolate cognitive from social considerations in the sciences (§4.2). We shall then delineate Longino's response to the sociologist-sceptic, detailing the requirements for a group's cognitive optimality. It will then prove necessary to resolve the apparent paradox inherent in demanding both diversity and consensus in a cognitive group (§4.4). We shall establish that interactions between individuals, and between cognitive groups, are essential for the development of reasoning and objectivity within such groups. Furthermore, we shall see that a social account of knowledge which focuses upon and encourages such interactions is fully compatible with realism about scientific knowledge. Finally (§4.5) we shall see the specifics of how Longino's conditions relate to Lockean testimonial knowledge, and to the Linkmaking project.

§3.2: Normativity and a Social Account of Knowledge

Longino aims to provide an account of scientific knowledge which both acknowledges the full extent of the social dimensions of knowledge and maintains the normative concerns of epistemologists. She acknowledges the compelling empirical studies done by Karin Knorr-Celina, Bruno Latour, and others, which have shown that scientific procedures are social 'in the sense of involving social interactions' (Longino 1994: 136). Such sociologists have shown that the methods used to certify scientific results involve "a hodgepodge of 'internal' and 'external' considerations" in which, they argue, it is impossible to distinguish between purely cognitive and merely social concerns. The sociologist's conclusion is that

> The normative concerns of philosophers are … idle: the wheels of prescription may spin in philosophy, but they fail to catch the gears in actual epistemic communities (Longino 1994: 136-7)

Having rightly recognized the social locatedness of knowledge-producing individuals, the sociologists offer us a flat choice between relativism on the one hand, and 'demonstrating the epistemic superiority of one among the various social locations' (Longino 1994, p. 139) on the other, an option which comes laden with potential accusations of cultural bias. Longino denies the legitimacy of this choice, claiming that it is a false dilemma 'arising from a continued commitment to individualism in epistemology' (Longino, 1994: 139). In response she seeks to provide a recognizable set of requirements for knowledge which are firmly based in a social account of knowledge development. We shall see that this set of requirements provides further evidence of the centrality of interdisciplinary approaches for maintaining a group's cognitive optimality; including reasoning capacity and objectivity.

§3.3: Reasoning and Objectivity

As Longino says, scientific reasoning is not reducible to calculation or pure logic, but rather consists in 'bringing the appropriate considerations to bear on judgments' (Longino 1994: 141). Furthermore, justificatory reasoning (which is essential in putting forward new theories, or adaptations to existing theories, and indeed essential to justifying a particular interpretation of data) requires that challenges 'both of truth and relevance' (Longino 1994: 141) are constantly made to claims, and that responses are constantly made to such challenges:

> Reasoning, thus, gets its point in a social context – a context of interaction among individuals, rather than of interaction between an individual and the object of her or his cognitions (Longino 1994: 141).

Critical debate between groups and individuals is necessary for knowledge development. In this fundamental sense, reasoning itself is social. Knowledge 'is constructed not by individuals, but by an interactive dialogic community' (Longino 1994: 143). So, knowledge arises from dialogue; but not just any form of dialogue will do. The interactions between members of a cognitive group must amount to 'genuine and mutual checks' if the group's enquiries are to be objective. Longino specifies certain features of a community which are necessary to enable such checks, and therefore to 'enable a consensus to qualify as knowledge' (Longino 1994: 144). Longino identifies four of these conditions:

1. There must be publicly recognized forums for the criticism of evidence, of methods, of assumptions and reasoning.
2. There must be an uptake of criticism. The community must not merely tolerate dissent; its beliefs and theories must change over time in response to the critical discourse taking place within it.
3. There must be publicly recognized standards by reference to which theories, hypotheses, and observational practices are evaluated and by appeal to which criticism is made relevant to the goals of the inquiring community…
4. Finally, communities must be characterized by equality of intellectual authority. What consensus exists must be the result not of exercise of political or economic power, or of the exclusion of dissenting perspectives, but as a result of critical dialogue in which all relevant perspectives are represented. This criterion is meant to impose duties of inclusion: it does not require that each individual, no matter what her or his past record or state of training, should be granted equal authority on every matter (Longino 1994: 145).

These are the requirements of an effective cognitive community. Requirement (1) asks that the evidence, methods, assumptions, and lines of reasoning used must be put

up for public scrutiny. 'Public' might in some cases mean the newspaper-reading public (for example in the reasoning used to justify political decisions in a democracy), but for more specialist areas of knowledge it would involve, for example, conferences and peer-reviewed journals, or less formally, the scrutiny of one's colleagues.

Requirement (2) asks that where there is criticism, it must be responded to. A cognitively healthy group will change in response 'to the critical discourse taking place within it'; meaning that well-placed criticisms should result in a change of belief or practice. Of course, not all criticisms will be well-placed. In the case of ill-informed criticisms the response can be brief, but there must nonetheless be a response justifying one's position, as this avoids complacency. The skill of distinguishing a good criticism from an ill-informed one is best regarded as part of our education in the field in question: it is Green's 'conscience of craft'. But even if we lack expertise in the field in question, we can still assume that a group which *never* adapts as a result of criticism is probably ignoring some well-placed criticisms.

Requirement (3) refines requirement (1) by asking that the group has a set of standards 'by reference to which theories, hypotheses, and observational practices are evaluated'. By adhering to such standards, individuals and groups show themselves to be open to criticism. They offer a clear message: 'Here are our standards. When we fail to reach these standards, which we have agreed to strive for, then our work is inadequate'. This is, I would argue, rather complicated by the fact that cognitive groups do not necessarily agree as to what standards they should be working to; as the controversy in archaeological theory over the past few decades shows[4]. In such a case, the standards themselves are questioned and criticised. However I don't think this affects Longino's key point, which is that a cognitive community which has *no publicly accessible set of epistemic standards at all* fails to be objective.

[4] It is by no means straightforward whether the archaeology case is an example of one discipline or of interactions between two disciplines. However, since processual and post-processual archaeologists share a common disciplinary history as well as a common object of enquiry, they are unambiguously members of the same *cognitive community*.

Requirement (4), the requirement for equality of intellectual authority, needs careful explanation. Longino is *not* claiming anything so crude as 'all opinions are equal', and she is not altogether denying that being an established, experienced old hand in a field is a legitimate source of cognitive authority. But, firstly, consensus within a cognitive group should not be as a result of political factors, and secondly, it must not be the result of the exclusion of dissenting perspectives. It is worth noting that it doesn't take a deliberate attempt at misinformation for political factors or the exclusion of new dissenting voices to occur. The effect is likely to be subtle: for example, relatively weak evidence for a politically popular conclusion may be given more weight than it deserves; and young and inexperienced researchers especially may be reluctant to properly defend any but the most compelling results, if they are contrary to the current consensus. Therefore requirement (4) is a positive requirement to encourage dissenting voices to be raised. Also, whilst experienced researchers with good reputations hopefully deserve them, their reputation should not in itself lend credence to their results. The best tool the academic world has to ensure this is, of course, the double-blind peer review.

Longino has produced an account which provides suitably modest aims for social epistemology as a whole. Whilst we are free to articulate context-independent *conditions* for enquiry, as Longino has; we cannot meaningfully produce context-independent *rules* for enquiry (Longino 1994: 156). Rules for enquiry are necessarily context-dependent, and part of the cognitive development of research communities is the process of producing such rules. Rules of enquiry must be produced with an awareness of the subject matter (including relevant techniques and procedures) and what expectations are appropriate, and what the cognitive requirements are for that particular enquiry. Acknowledgement of this kind of context-dependency need not lead us to relativism, because we can articulate context independent conditions for effective cognitive enquiry. We cannot determine, independently of context, what the cognitive standards ought to be; but we can ask that the standards are public; that criticisms are acknowledged and responded to, and that consensus occurs as the result of critical discourse rather than political influences or the exclusion of dissenting voices from the debate. Whilst scientific enquiry is fundamentally social, it is a mistake to conclude that 'genuine' scientific enquiry is inevitably displaced by non-cognitive social elements. Such displacement would only occur 'in contexts of flawed

discursive interaction' (Longino 1994: 156), where the conditions Longino specifies are not met.

§3.4: Longino, Locke and Linkmaking

How, then, does Longino's account relate to Locke, and to Linkmaking? The 'Linkmaking' approach to objectivity is a holistic project which requires that we uncover links between objects in the world, such as causal links and similarities (Link type 1) and make appropriate Lockean 'links between ideas' (Link type 2) with the assistance of our social links with other individuals and groups (Link type 3) and links with other disciplines or areas of enquiry (Link type 4).

We have said that the third kind of link is made by forming our cognitive groups and arranging them in such a way as to maximise our potential to make the other types of links. It therefore requires that we reconcile realism with a social account of knowledge, something that Longino's account moves towards. (Frederick Will's account, examined later in this chapter, goes into this issue in further depth). Linkmaking also requires that we have clearly defined standards for the cognitive optimality of social groups; standards which maximise our potential to make appropriate links of types 1 and 2.

We saw in the previous chapter that Locke provides us, in embryo, with a means of evaluating the cognitive optimality of groups. He does this by distinguishing between testimonial knowledge and mere received opinion. Only if the individual has critically assessed the testimonial source can they be said to gain testimonial knowledge, rather than received opinion (see Chapter One, §4.4). They must therefore consider, in the testimony of others:

1. *The Number* [of witnesses].
2. The Integrity.
3. The Skill of the Witnesses.
4. The Design of the Author, where it is a Testimony out of a Book cited.
5. The Consistency of the Parts…
6. *Contrary Testimonies.*

(*Essay*, XV, §4, italics mine)

Requirements 2, 3, and 4 concern the cognitive virtues of the individual providing the testimony, but 1 and 6 refer to the existence of contrary testimonies. This provides us with a means of assessing the cognitive optimality of groups. Firstly, the individual seeking testimonial knowledge must have access to these alternative testimonies; but we can also consider how the qualities of the cognitive group will impact on the cognitive virtues of any individual within it. I shall run through each of Locke's demands in turn, and demonstrate that Longino's cognitive group is ideally placed to meet them.

First let us consider: how are we to know the *number* of relevant testimonies? Perhaps in some very limited cases, it would be possible to seek out such testimonies purely within our immediate acquaintance. For example, I would only need to consult immediate members of my family if I wanted to check when my cousin's birthday is, and whether or not she already owned the novel I plan to buy her as a gift. But in most cases of enquiry, it would not be possible for me to discover through my immediate acquaintance all the relevant testimonies. Instead, we come to know the number of testimonies through membership of, or association with, a group of experts on the relevant topic, who gather such testimonies together (for example in journals or at conferences).

How are we to know the *integrity* of the witnesses? Well, we can read 'integrity' in two ways. Firstly we can consider the deliberate attempt to deceive. In that case, in order to assess someone's integrity it is necessary that we have the means to uncover signs of dishonesty. My membership of cognitively sound epistemic groups would be extremely helpful in uncovering such signs: simply because they may have deceived other people before. But, further than this. we can also take 'integrity' to mean something else. We can say that it does not merely consist in 'not being deceitful', but further, that persons with integrity strive to meet a high standard of cognitive enquiry; that their methods and conclusions are sound. But this second sense of 'integrity' requires the independent existence of high standards of cognitive enquiry. It requires, therefore, Longino's 'publicly recognised forums for the criticism of evidence, of methods, of assumptions and reasoning' as well as the 'publicly recognised standards by reference to which theories, hypotheses and observational practices are evaluated'

(Longino 1994:145). Without the existence of such independent sets of standards, it would be extremely difficult if not impossible to evaluate the practices of any one individual. Of course, merely striving to meet the standards is insufficient. The witness must also have sufficient *skills* (in Locke's terms) to meet the standards. Those who lack these skills will find their conclusions rejected by Longino's cognitive group.

Locke is also concerned that in assessing testimonial sources we must be aware of the 'design of the author', where the testimony is from a book cited. Longino's cognitive group is able to address this issue for two reasons. Firstly, it demands that there must be an uptake of criticism; the community must alter its beliefs and theories in response to critical discourse. Secondly, it is 'characterized by equality of intellectual authority', such that dissenting perspectives are actively encouraged. Together, these two demands limit stagnancy of opinion. It would by no means be impossible for any one individual to display a strong bias towards a particular view, but if they failed to respond to criticisms made by others, their work would not be taken seriously (even if they were an established researcher and their critics less so). This of course also relates to Locke's concern that we must be aware of the existence of contrary testimonies in order to gain testimonial knowledge. Longino's 'equality of intellectual authority' is necessary if such contrary testimonies are to be easily available.

§3.5: Conclusion

Whilst Locke reaches towards what we would recognise as a social account of knowledge, in distinguishing between 'received opinion' and 'testimonial knowledge'; Longino provides a far more developed account of how cognitive groups can produce individuals capable of gaining, and contributing towards, testimonial knowledge. Members of Longino's group are Linkmakers. As realists, they are concerned with uncovering links between physical objects in the world (link type 1). The public forums, public standards, and equality of intellectual authority maximise the potential of their social connections (link type 3) to enable them to form accurate links of type 1 and useful links of type 2. Also, the focus on publicly accessible standards maximises the potential for related disciplines and areas of enquiry to take part in the same debates, thus maximizing interdisciplinary links (link type 4).

Furthermore, Longino's account of cognitive optimality addresses the sceptical concern that a social account of knowledge cannot be reconciled with realism. The accusation from the sociologist-sceptic that she addresses is that a social account of knowledge results either in relativism or in cultural bias. She correctly asserts that this is a false dilemma, arising from a (sometimes unacknowledged) commitment to individualism in epistemology. Her response is to create a meaningful set of requirements for knowledge which have firm social roots. However, there are further questions to address about governance within cognitive groups; two radical forms of scepticism to which Longino may still be vulnerable. The first is the accusation that systems of practices, even the public standards espoused by her ideal cognitive group, are necessarily incapable of radical self-criticism. The other is the concern that systems of practice are barriers between inquirers and the objects of enquiry. In order to address these sceptical concerns, we turn now to Frederick Will's account.

§4.1: Will on Cognitive Optimality and Interdisciplinarity

Frederick L. Will, in his work *Pragmatism and Realism* (1997), shows how an account of knowledge which fully recognises the role of social practices in cognition can also provide an adequate defence of realism. His account, rich in common sense, demonstrates how 'governance of practices [can] be performed in thorough dependence upon those practices' (Will 1997: 90). In this section, I use Will's account in order to demonstrate that realism is compatible with a social account of knowledge. This is essential to the Linkmaking project, which demands connections between physical links in the world (link type 1) and social links (link type 3).

Will argues that the confusion about rational governance arises from a flawed and limited conception of reason and rationality. He distinguishes between two kinds of reasoning. Reasoning in the narrow sense is purely calculative: it includes deduction and induction; that is all. Reasoning in the broad sense is not calculative. It includes, for example, observation, data collection, and the analysis of testimony. Will's objection is that these two forms of reasoning have been conflated, suggesting that there is a single 'rational touchstone' applicable to all fields of enquiry (§4.2). This is not the case. Instead, each field of enquiry develops its own forms of *rational practices*. Will argues that practices are best understood as habits rather than as rules for behaviour; and insists that we can speak of 'intelligent habits': ones which are

rationally defensible as appropriate, and which we alter if and when it becomes clear that they are not beneficial (§4.3). Understanding this is the key to seeing how the governance in practice and governance of practices relate to each other.

§4.4 and §4.5 will address the two main sceptical challenges facing this reconciliation of social knowledge and realism, which Will terms the Relativist Illusion and the Coherence Illusion respectively. The Relativist Illusion stems from the belief that systems of practices are necessarily incapable of radical self-criticism; and therefore, incapable of self-governance. Will's solution to this problem is to point out that each individual enquirer is acquainted with a plurality of practices. This prevents any one system having a monopoly over our reasoning processes. Notably for Linkmaking, Will's account emphasises the importance of interdisciplinary approaches for avoiding the 'monopoly' so feared by sceptics. The Coherence Illusion is the fear that systems of practices interpose themselves between the enquirer and the world; that enquirers can only access the 'artefacts of practice' rather than the independent objects they wish to understand. Under this view, a social conception of knowledge is necessarily anti-realist. Therefore, Will's solution to this sceptical challenge is to explain the extent to which systems of practices *arise from* the independent objects of investigation. We shall conclude (§4.6) that Will's account gives us the means to reconcile a social account of knowledge with the claim that we can govern our traditions; and that he gives us valuable arguments against Kuhnian-style relativism, which is the major threat to Linkmaking.

§4. 2: Reasoning and the Social

During the past few decades, we have become increasingly aware of the social aspects of knowledge production. It has become impossible to ignore the extent to which our cognitive practices are social, even those practices which were previously considered to be 'paradigmatically rational' (Will 1997: 63). This has raised important questions about the nature of rational governance:

> To the extent that the processes of observation, reasoning, criticism, judgement, evaluation, and so on, traditionally associated with rational governance, are recognised to be thoroughly determined by, thoroughly rooted in, social practice, it has seemed to many philosophically sensitive writers increasingly difficult to

> continue to maintain for them their claims to rationality. (Will 1997: 63)

The philosopher's instinct to divide (social) practices from (individual) reasoning runs deep. In order to overcome it, we must first arm ourselves with a better understanding of what is meant here by 'reasoning'. Will is quick to distinguish between two forms. 'Reasoning' or 'rationality' can be used in a very narrow sense, to mean formal logic, calculation, inference, deduction, and so on. The second sense is broader: it includes 'observation, the collecting and processing of data, and attending to and assessing testimony, as in a court of law' (Will 1997: 64). Will argues that modern rationalist philosophers tend to conflate the two senses, producing a misleadingly narrow conception of reason, and therefore interpret the question of rational governance as a question of competence in 'reasoning' in the narrow sense of the term. When reasoning is regarded as purely calculative, rational results are solely those

> which can be shown to be derivable by a process that begins with grounds having a certain form, namely, that of symbolic formulae, and proceeds by performing similarly symbolic transformations upon these grounds, transformations … sometimes discriminated into two subspecies, deduction and induction. (Will 1997: 93)

This conception of reasoning has the effect of drastically limiting what counts as rational development; as any *revisions* in the deductive or inductive processes would, by definition, be non-rational. (This would of course include any revisions arising from social interactions.) Therefore, calculative reasoning would, of necessity, be a process undertaken by individuals rather than by groups. In short, social practices would be deemed 'irrational' or at least 'a-rational' even where they actively and successfully assist in, say, the processing of data or the assessment of testimony. This is because such assistance can never amount to an algorithm, or decision procedure, capable of delineating which beliefs are justified. Whilst reasoning in the narrow sense, for example formal logic, can perhaps be seen as algorithmic, reasoning in the broad sense, which incorporates observation, the collection of data, the development of new procedures, the assessment of testimony, and so forth, cannot be algorithmic. Such reasoning does not consist in a pre-ordained decision procedure; and it would be of precious little use to us in investigating new areas of knowledge if it did.

For our purposes it is worth noting that what Will calls reason (in the broad sense) bears a strong resemblance to Locke's 'sound, round-about sense'. It is this which Locke sought to develop using his interdisciplinary curriculum, and via social means in the *Conduct*. Reasoning in the narrow sense is what Locke calls intuitive and demonstrative knowledge (see Chapter One §5). As I have taken pains to establish, Locke is very careful to distinguish between these two types of reasoning, without underestimating the importance and potential of reasoning in the broad sense. Locke also acknowledges that reasoning in the broad sense can only be achieved by social means. Will's account expands upon and defends the point that reasoning is social, and, importantly, that this does not prevent us from using reason to govern our cognitive traditions.

§4.3: Governance of Practices

The tendency among epistemologists to conflate the narrow and broad senses of 'reasoning' can be traced to the pervasive influence of Descartes *Discourse on Method, Rules for the Direction of the Mind*, and *Meditations on First Philosophy*, which have fuelled the assumption that since reason has a role in opposing and revolutionizing practices, reason must be conceived as radically independent of practice. Otherwise, it is thought, we would have no hope of governing our traditions.

However, in order to reason well in the broader sense defined above, including observation, collecting data, and assessing testimony, the mind must be trained, educated, *socialised.* However, as we shall see, there is nothing about socialisation which intrinsically prevents individuals from governing their traditions, or their own behaviour. Socialisation is not, or at any rate need not be, indoctrination. This fear is born of an inadequate and oversimplified notion of socialisation, one which fails to recognise the true relationship between governance *in* practices, and governance *of* practices. It is born, also, of a misunderstanding of the nature of practices themselves; a view which regards practices as standing between the investigator and that which she investigates. (Notably, this fear that socialisation prevents self-governance implies an atomistic, individualist view of education; the antithesis of the educational writings of Locke and, as we shall shortly see, of Green.)

The conclusion that rational governance is a 'myth' only arises if we conceive of social practices as fundamentally independent from an objective world in which those practices developed. But social practices have no such independence. This is best understood by looking at how we learn practices:

> A primary feature of human habits, and likewise practices, is their flexibility, their capacity, as dispositions to act, to adapt to and be modified by a great variety of conditions in the contexts in which they are translated from dispositions into actual performances ... When habits are learned, when practices are mastered, more is acquired by the individuals engaged in the process than a simple connection in action between some specific occasion and some equally specific response. The driver follows a practice when, upon seeing the green arrow on the traffic signal, he moves the car in the direction of the arrow; or when, hearing a rapidly approaching siren, he moves the car out of the main traffic lanes. But this description by no means includes all that he has learned to do, in these and similar circumstances ... One who learns a practice as though he were a beginning soldier being imprinted with elements of close-order drill learns to behave in a way so odd and eccentric that Bergson could make it an important element in his theory of laughter. (Will 1997: 68-69)

Will's point is that to learn a practice is to acquire a disposition to act, rather than a fixed decision procedure. In learning a practice, as opposed to a 'close-order drill', one also gains a 'feel' for the circumstances in which the usual actions would be inappropriate. One learns the 'spirit' of the practice. We cannot, in short, conceive of practices as 'atomistic bits of behaviour' (Will 1997: 69) which appear as entirely predictable response to given situations. Rather, we adjust our practices to our circumstances, and to each other (Will 1997: 70).

Note that Will asks us to consider practices as fundamentally like habits, rather than as systems of rules for behaviour. 'A primary feature of human habits, and likewise practices, is their flexibility' (Will 1997: 68-69). Unfortunately, making this connection may not initially help his argument that governance arises from practices, since 'habits' have the connotation of being unreflective at best. Indeed, to the

Cartesian conception of reasoning, a 'habit of thought' would appear to be oxymoronic, since to have a habit is to do something without making a full conscious decision to do so. Habits can even involve doing something at least partially against one's will: for example, having a habit of biting one's nails. If 'practices' should be considered in the same light, then, opponents may say, so much the worse for Will's line of argument; because surely, even virtuous habits are the opposite of the independent reasoning necessary to promote governance.

In order to answer this objection, we should assess what is meant by 'habits'. Habits are patterns of behaviour which, once acquired, become automatic; though the agent will be aware that they have the habit, and can usually (with effort) alter the habit if necessary. It is important to note two things here. Firstly, that *the possession of a habit* is morally neutral. Acting out of habit is neither intrinsically worse nor intrinsically better than acting in a manner which goes against one's habits: neither implies that the agent has taken, or failed to take, appropriate responsibility for their actions. We can only judge a habit by the results of the behaviours it inspires. Secondly, habits can be, and frequently are, altered when it becomes clear that those results are not beneficial. Locke was aware of this moral neutrality, which is why he both urges parents to teach children by the formation of habits rather than the enforcement of rules (*Thoughts*, § 66), and writes *Conduct* as a guide for adults hoping to cast off old cognitive habits, and gain new and better habits of reasoning and study.

We can see, then, that an 'intelligent habit' is neither oxymoronic nor rare. 'Intelligent habits' are chosen by our educators and by ourselves for their capacity to improve our knowledge or understanding (or to benefit us in some other way; for example the habit of brushing one's teeth). When learning tennis, trombone or touch-typing, we deliberately choose and acquire new habits: holding the racquet in a certain way; how much air pressure to apply; the position of the fingers over the keyboard. Were it otherwise, we would be dependent solely upon conscious memory, which is not the best use of our cognitive resources. Indeed, in the examples given, we might be inclined to say that the student *doesn't really know* how to play the trombone until the actions 'come naturally'; that is to say, until there is no longer a conscious consideration of how much air pressure to apply. However, we would also say that

they don't really know how to play until they gain a feel for when it is appropriate to change the air pressure to suit a particular note. Equally, in learning to perform a practice effectively, we must learn a 'flexibility of response', the

> kind of flexibility of response that the once celebrated boy on the burning deck in a signal degree did not display when he stood fixed amid the rolling flames and booming shots simply because there was no one to tell him that his duty to remain had under these circumstances been overridden. It is this, in a very fine and complex way, that the skilled surgeon is expected to learn, partly through instruction and partly through experience about what is possible, desirable, optional, and necessary, in his craft.
>
> (Will 1997: 70)

In more everyday circumstances, it is this that we learn when 'we learn to cook, do carpentry, make cabinets' (Will 1997: 70) and any of hundreds of other skills that require developing a 'feel' for the material that you are working with; a special kind of 'common sense'. When we cook, we respond both to the social input, such as a recipe book, or the example of our parents, and to the physical processes going on in the pan. Sometimes it is necessary to follow the recipe even when the physical circumstances would suggest otherwise (don't worry, it's meant to look like that), and sometimes it will be necessary to respond to the physical circumstances in a way strictly speaking opposed to the recipe (taking off the onions ten minutes early if they are clearly about to turn black). In this 'we do honour the custom more in the breach than in the observance' (Will 1997: 83). We cannot strictly delineate in advance which case will be which; so learning to cook involves acquiring a 'feel' for ingredients and the physical processes of cooking. But we acquire that 'feel' from the entirety of what Will calls the 'existential situation' (94), not only the physical processes. The existential situation also includes the social landscape; such as techniques, available ingredients, cookery books, instructions from family and friends, opinions on the resulting meals and so on. Having acquired a 'feel' for cookery, we can then challenge the socially received wisdom and decide that scrambled eggs are better without adding milk. But until we have learned about food, we can't do that. Learning the 'feel' for cookery is what Will means by 'governance *in* practice' (Will 1997: 71). Learning when to challenge the cookery conventions is 'governance *of*

practice'.

We can see, then, that governance in practice and governance of practice are closely related, and both are acquired in the process of learning practices. Learning practices, involves socialization and habituation, but as Will argues, these should not be regarded as passive experiences. Rather, in learning practices our enquirer becomes an active member of the cognitive community which developed those practices, and becomes able to offer informed criticisms of those practices where appropriate. We have a small-scale, mundane example of this in the making of scrambled eggs mentioned earlier. But in order to achieve our goal of reconciling a social view of knowledge with realism, we must show that the alteration of practices on a large scale can be rational, and that the changes can emerge from the traditions themselves. We must therefore address and overcome two common mistakes about the nature of governance which Will terms the Relativist Illusion and the Coherence Illusion. In the following two sections, I discuss each of these in turn.

§4.4: The Relativist Illusion

As Will argues, any philosophy of science which is committed to the existence of a knowable objective world must provide an account of how facts about that world can be obtained (Will 1997: 88). What the realist needs in order to achieve this is an account which recognises the social roots of our knowledge, and which shows we can legitimately learn to govern our practices in the act of learning our practices. This account would involve a shift in the traditional philosophical questions about criticism and appraisal. The account would acknowledge that criticism and appraisal are informed by social practices. Will frames the key question thus:

> How can one view the criticism, appraisal, and so on, of practices as being always performed in thorough and essential dependence upon social practices themselves, without entrapping oneself in some sociocentric and in the end fatally sceptical predicament? How, if one views in this way the processes of criticism, appraisal, and so on, including fundamental philosophical forms of these functions, can the results attained be viewed as legitimate or valid ones? ... can the governance of practices be performed in thorough dependence upon

practices?

(Will 1997: 89)

Will refers to this 'fatally sceptical predicament' as 'the Relativist Illusion'. Someone labouring under this illusion believes that systems of practice are necessarily incapable of radical self-criticism, and are therefore incapable of self-governance. Such a person regards any appearance of governance within a system of practices as illusory; believing that systems of practices 'determine the main features of a participant's thought and action so completely' (Will 1997: 94) that the participant cannot summon up the necessary distance to offer any fundamental criticisms.

It is, indeed, characteristic of practices to 'cohere with other related practices in systems'. However, not only do these systems 'vary widely both in magnitude and in the tightness with which the components are bound together' (Will 1997: 91), but they overlap significantly in terms of their membership. As Will argues, the Relativist Illusion arises from a failure to recognise the plurality of types of practices engaged in by any individual, and how this plurality aids in effective governance. Furthermore, this plurality lends itself to the Linkmaking project, by promoting the development of links of type 3 and 4.

To clarify the role of plurality in governance, and for the Linkmaking project, let us take the following example from archaeology. This case is taken from the paper 'Pompeii Purpurissum Pigment Problems'(Clarke et al, 2005). A 2000 year old pigment sample is investigated to clarify its composition. It is pink-purple in colour, and was excavated at Pompeii. Pliny (a contemporary writer) described a purple pigment, 'purpurissum', which was made by dyeing a colourless earth substrate with shellfish purple. The aim of the investigation was to discover whether the pigment sample was an example of purpurissum. A multi-technique analysis was carried out, including the use of mass spectrometry and X-ray scattering techniques. The different techniques gave apparently contradictory results. The conclusion – that the samples were probably a mixture of purpurissum and other, cheaper dyes – was only possible due to a combination of the various scientific techniques *and* the historical source material.

This case makes clear that an archaeologist who uncovers a new artefact might well have access to evidence regarding the origin of that artefact *both* from written historical sources *and* from scientific modes of investigation. Where there is any conflict, it is the responsibility of the archaeologist to reconcile the evidence gained from each of the various sets pertinent to the enquiry. In this way, the plurality of practices benefits the enquiry by acting as a system of double-checks. Will's own example of this is the discovery of nuclear fission:

> When Hahn and Strassmann in Berlin during the fall of 1938 had bombarded uranium salts with neutrons, producing nuclear disintegration and its products, they were informed of this by one set of firmly entrenched practices that told them that they had derived barium in this process, while another set led them as strongly to suppose that such a result, by the means they had employed, was not possible ... Strassmann ... was led to say 'as chemists we are bound to affirm that the new bodies are barium ... [but] as nuclear physicists we cannot decide to take this step in contradiction to all previous experience in nuclear physics' ... Lise Meitner ... [saw] first the way to solve this apparent dilemma ... she was able to suggest a way in which ... the results ... could be satisfactorily and very excitingly explained. (Will 1997: 102)

Here, the plurality of practices with which Hahn and Strassmann were familiar enabled them to see what they might otherwise easily have missed: that whilst their training in the practices of chemistry led clearly to conclusion that they had produced barium, their training in the practices of nuclear physics revealed this result to be too extraordinary to accept without offering a physics-based account of what had occurred, which they then proceeded to do. Hahn, Strassmann, and Meitner were not merely members of one cognitive group but of several of various scopes: including the cognitive group of nuclear physicists, that of chemists, and the wider group of the scientific community as a whole. Each cognitive group possesses connections to many others through the expertise of its various members. Each individual within a cognitive group possesses subtly different expertise. This means both that they have different insights into the physical objects of investigation themselves (in Linkmaking terms, they have Links of type 1), and that they have different social links (Link type

3) with members of other fields, which can give rise to interdisciplinary links or links between different areas of investigation (Link type 4). Radical challenges and radical changes can be made through the influence of such links between cognitive systems. Interdisciplinarity is, therefore, key to avoiding the Relativist Illusion; just as it is key to Linkmaking.

§4.5: The Coherence Illusion

Whilst those thinkers who accept the Relativist Illusion regard effective governance of practice as a myth, those entrenched in the Coherence Illusion are committed to an even deeper scepticism. They are convinced that cognitive systems 'interpose themselves between objects and aspiring knowers' (Will 1997: 94), so that enquirers can only access the 'artefacts of practice' rather than the independent objects they aspire to understand.

Again, this illusion is symptomatic of the philosophical instinct to divide (individualistic) reason from (social) practices. In this case, the anti-realist implication is clear. If we do not have direct access to the objects of our investigation, using only our individual reasoning skills, but are instead dependent upon some socially developed system of practices (such as an accepted experimental process), then our conclusions cannot be justified. In short, someone labouring under this illusion combines a foundationalist's epistemic ideals with a coherentist's conception of the epistemic reality, and is therefore an unhappy creature indeed.

To clarify this, let as take the example of Hann and Strassmann's experiments with uranium salts. Hann and Strassmann were aware that, since their results were consistent with the norms of chemistry but not those of physics, the results were inconclusive. Their subsequent investigation aimed at producing a conclusion which was consistent with both the norms of chemistry and the norms of nuclear physics. This provided us with a response to the Relativist Illusion: the plurality of practices enabled the scientists to recognise an inconsistency and act accordingly. However, those who subscribe to the Coherence Illusion would say that their subsequent investigation could *only* succeed in remedying an inconsistency in their overall belief set. Both sets of practices, they would insist, still stand between the aspiring knowers and the independent objects they are investigating. In short they would argue that

Hann and Strassmann merely achieved coherence, not objectivity.

Will argues that to succumb to this illusion is to misunderstand the nature of practices. Practices are not simply dictated by the groups or individuals concerned, but are formed by the existential situation as a whole. By 'existential situation', Will means both social constructions and the physical world. We do well to remember that practices have firm roots in the independent objects which they were developed to investigate, as well as in social constructions.

To draw on a mundane example, our scrambled-egg chef's actions in whisking the eggs is formed not only by the culinary traditions she is familiar with but also by the fact that eggs are a sort of thing that can be whisked (unlike, say, steak). To take a rather more academic example from Chapter Four of this thesis, there were only so many practices which could assist in the investigation of high energy physics depicted in Peter Galison's *Image and Logic, a Material Culture of Microphysics*.

An apparent difficulty with this approach should be addressed here. In the previous section, we saw the importance of an interdisciplinary approach for providing the multiplicity of practices necessary for overcoming the Relativist Illusion. Yet in overcoming the Coherence Illusion, we note that practices are determined to a great extent by the existential situation concerned. It might initially seem that if practices necessarily develop in relation to the objects under investigation, then the number of relevant interdisciplinary links would be few. Indeed, we have already seen in Locke the danger of forcing inappropriate connections between subjects. However, there are several responses we can make to this.

Firstly, whilst practices develop in relation to the object of investigation, drastically different practices could develop in relation to that object, simply because the practices relate to different properties of that object. For example, wood is both a useful source of fuel and a building material. However, a person building with wood ought to bear in mind that it is also a useful source of fuel, because it will render them appropriately cautious about possible fire risks.

Or, the practices could relate to the same property but use it for a different purpose. For example, persons familiar with the fact that silver vessels keep water fresh during

long sea journeys are well placed to recognise that silver has antibacterial qualities which can be used elsewhere; for example, in bandages for burns and other wounds (Percival et al., 2005:1).

We should also note that a single line of enquiry, regarding a single object of investigation, may require understanding of more than one discipline, as was the case with the Pompeii pigment. Certainly, the description in Pliny's texts is accurate because Pliny had the opportunity to examine the dye and the processes of making it. His account can, in this way, be traced back to the physical object being examined, just as the mass spectrometry and X-ray scattering techniques were used to physically examine a sample of dye. However, it remains true that the expertise, the *discipline*, of studying ancient texts was required in addition to the scientific disciplines; and it was required that the two be linked together in order to form one justification for the conclusion.

Furthermore, it should be recognised that even if the relevant connections did turn out to be few, those connections will be with cognitive communities who themselves have *a different set* of relevant cross participations with other fields; rather like Haack's crossword puzzle (see 2.4, above). To see how this works, imagine the following situation. Subject A has interdisciplinary links with Subject B which itself has interdisciplinary links with Subject C. Practitioners of Subject B borrow techniques from Subject A to use in a particular investigation. The conclusions of that investigation are then used by practitioners of Subject C, because they offer additional support to some conclusions of their own which they have investigated independently.

Now, the fact that practitioners of Subject B were able, independently, to make use of the techniques of Subject A offers additional evidence for the efficacy of those techniques. The fact that the conclusions made by Subject B offer additional support for conclusions *independently reached* by Subject C, increases the likelihood that both Subject C's and Subject B's conclusions are correct; just like two intersecting entries in a crossword puzzle. This in turn offers still more evidence for the efficacy of Subject A's technique, even though Subject A and Subject C may have no direct interdisciplinary links at all. From this, we can see that even if a subject has only one

or two intersecting points with other areas of investigation, it does not diminish the importance of those intersecting points for the development of objectivity.

§4.6: Conclusion

Will's account has successfully explained and overcome the Relativist and Coherence Illusions. This is vital to our project, since someone operating under either of these Illusions would fail in undertaking the Linkmaking project. The Relativist Illusion prevents connections being made between links of type 1 and links of type 3; whereas the Coherence Illusion claims the precise opposite of Linkmaking: that group practices (links of type 2 and 3) act as a barrier between the enquirer and the physical objects of investigation.

Will addresses these Illusions by demonstrating that the rational governance of group practice need not come from some purely individualistic reasoning power, but can be understood in terms of the plurality of accepted systems of practices used by the individuals in question. The interactions between these systems prevent any one system gaining any kind of totalitarian control over the thoughts and actions of individuals. Will has reconciled two apparently contradictory truths; that reason is dependent upon traditions, rather than being in principle an independent quality belonging to individuals; and that we are more than capable of criticising our traditions in productive and meaningful ways. Firstly, he establishes that no one set of practices can have monopolistic influence over the thoughts and actions of any individual, since every individual takes part in a wide variety of practices. Secondly, he recognises the extent to which practices are formed by the existential conditions of the aspiring knowers, including the objects of investigation themselves. Will's account goes some way to answering the remaining sceptical problems with Longino's conception of a cognitively optimal group; and when combined with Haack's founderentism, gives us a clearer understanding of the role of interdisciplinary practices in developing objectivity.

Thus far in this chapter we have examined and adopted Founderentism as our standard of justification, and have seen that cognitive groups which meet Longino's standards are the best suited to provide Founderentist justification for the beliefs of their members. We have used Will's account to address some of the key sceptical

issues relating to a realist and social account of knowledge. What remains is to relate these conceptions of knowledge and of cognitive optimality to educational theory.

If Linkmaking is the way to develop reasoning and objectivity, then we should wish to educate children, students, and ourselves to be Linkmakers. This requires that they are accustomed to justifying their beliefs in the Founderentist manner, that they are members of and able to contribute to cognitively optimal groups, and that they are not vulnerable to the Relativist or Coherentist Illusions. What form of education would this be? We have already seen part of the answer to this question in Chapter One, where we examined Locke's educational theory. What follows is an exploration of the educational theory of Thomas Green, who has concerned himself with similar sceptical concerns to those addressed by Frederick Will. His solutions to those concerns illustrate how we might hope to educate individuals to be objective, rational, and able to contribute to cognitive communities; in short, how to educate Linkmakers.

§5.1: Thomas Green: Normation and Group Membership

The previous section drew on Frederick Will's work, *Pragmatism and Realism,* to demonstrate that it is both possible and desirable to reconcile realism with a social account of knowledge. Will's concern was to alleviate the intellectual shock of epistemologists, whose oversimplified understanding of socialisation led to the sceptical fears that Will referred to as the Relativist and Coherence Illusions. This section will draw upon Thomas Green's work, 'Education and the Formation of Conscience', to examine the parallel concerns which arise for educators, whose similarly flawed understanding of socialisation has lead to fears about indoctrination. We shall see that, inspired by a Cartesian conception of reason opposed to tradition, educators often feel that they have to choose between inculcating young students into a particular set of norms, *or* encouraging them to be autonomous. We shall see that this is a false dichotomy (§5.2), thus undermining the individualist conception of what 'autonomy' means. We shall then examine Green's assessment of how norm acquisition (or 'normation') occurs (§5.3) and look in depth at the 'voices of conscience' that students develop (§5.4). In (§5.5) we shall learn the importance of group membership for the development of conscience, and in particular look at the epistemic responsibility of members to provide improvements to the norms and practices of the group. Finally, we shall conclude that Green's account gives us

insight into how the Cartesian epistemic concerns expressed by Will are reflected in pedagogy; and also, that Green's account clarifies the importance of group membership for the acquisition of reasoning skills.

§5.2: Pedagogy and Moral Education

It should be noted that whilst Green's account focuses on moral education, my own interest lies in education more generally. However, Green's conception of the development of governance is sufficiently broad to encompass awareness of one's epistemic responsibilities; both as an individual and as a group member. Therefore his conclusions are pertinent to educational theory as a whole, not merely to moral education. Furthermore, it is vital to the project of Linkmaking that the dichotomy perceived by educators between inculcation and autonomy is seen to be false. We are offered an apparent choice between inculcation and respect for autonomy.

Inculcation, in the language of Linkmaking, would mean that links of type 3 become the sole determinants of links of type 2; that is to say, the arrangement of links between ideas in my mind is solely determined by my social links. In such a situation, a student can only independently evaluate the propositions and practices she learns from her teachers in terms of their internal consistency. For example, if she is taught that mammals can't fly, bats can fly, and bats are mammals, she is in a legitimate position to challenge her teachers. But she cannot independently seek out links of type 1 in order to challenge her teachers.

The 'autonomy' presented in this dichotomy is of a distinctly Cartesian and individualist kind, according to which links of type 1 are the sole legitimate determinates of links of type 2. The only legitimate reason to have a certain connection between ideas in my mind is that it is in some way homomorphic with the objective physical world. This is, of course, a foundationalist view, according to which knowledge is founded on certain 'basic' ideas which are justified solely by experience, and not by the support of other ideas. According to this view, the only purpose of links of type 3 (and therefore the only role of the educator) is to reveal links of type 1 more efficiently that the student could have managed herself.

Neither of these presented choices would successfully provide what Linkmaking demands: individuals with the capacity to undertake a holistic examination of all four kinds of links. The 'inculcated' student has no access to links of type 1; which impedes her ability to evaluate her links of type 2, 3, and 4. The 'autonomous' student has artificially limited links of type 3, which affects her ability to evaluate her links of type 1, 2, and 4. If the dichotomy were genuine, then Linkmaking would be an unattainable goal. Fortunately, as we shall see, Green shows the dichotomy to be false.

Green argues that moral education includes the tasks of bringing about right beliefs, guiding and enforcing right behaviour, and educating a learner to make autonomous and principled decisions (Green 1999: 5). Each of these is a legitimate aspect of moral education, but if any one of them is advanced as if it were the whole of moral education, then problems begin. Each of these aspects, when taken standalone, threatens a kind of repression:

> The view that moral education is merely a matter of forming right belief offers grounds to fear that orthodoxy will become its end and indoctrination its means ... And if moral education be conceived simply as shaping right behaviour, then we have reason to believe that its goal will be conformity and the development of unthinking habits ... 'Education', it is feared, will become simply another name for oppression. (Green 1999: 6)

Individualist educators face a dilemma. Regarding, as they do, the acquisition of norms and practices as contrary to the application of individual reason, they feel torn between two responsibilities: 'moulding' children by inculcating them into the norms and practices of various traditions, and facilitating the natural blossoming of the child's independent reasoning skills.

Every teacher (which includes every parent) wishes to guide the moral education of the children in her care. Yet no responsible teacher would be content with the concept that she is indoctrinating children or encouraging unthinking conformity. Even the most apparently liberal conception, that of learning to make principled decisions, has the taint of indoctrination since 'fanatics act on principle, but their principles are always too few and their application of them too inflexible' (Green 1999: 6).

Educators are understandably concerned about the apparent tension between providing a moral education and respecting the autonomy of the pupils.

But their understanding of autonomy is unconsciously based on an acceptance of a Cartesian dichotomy between reason and tradition, and upon the idea that reason in the narrow sense can be a sufficient guide to conduct. Insofar as a teacher imposes upon the child an idea of what conduct is acceptable and what conduct is foolish, unwise, inefficient or immoral, they suspect that they are usurping the role of reason and limiting the child's autonomous choices:

> We must ask whether parents and teachers are to remain silent on which choices are efficient or prudent – much less morally superior – and which unwise, inefficient, or foolish. Must we eschew all comment on these matters in deference to the child, retreating altogether from such questions out of fear that comment will abridge someone's freedom of choice? ... Except for the sweet voice of reason, by such a view, interventions are to be forbidden.
>
> (Green 1999: 7)

We might add that, if one truly believes that reason exists independently of tradition, then one is committed to believing that the child in this situation is at a considerable advantage over the adult. She has reason, but is not, as yet, sufficiently exposed to any tradition for them to have stifled her thinking. There is something to this idea, insofar as children tend to ask simple and perplexing questions that adults would not think to ask; but nonetheless it is essentially the reverse of the truth. The advantage lies with adults because they have better developed reasoning skills *and* a familiarity with a variety of traditions. Therefore adults have the advantage when it comes to making choices: they are better informed and more able to understand the implications and consequences of their choices. 'Autonomous choice', however, is a misleading phrase because it implies the existence of an opposite, 'non-autonomous choice'; which is oxymoronic. Rather, we ought to think in terms of informed, well-thought-out choices; as opposed to ill-informed, haphazard choices. Moral education involves teaching children how to make informed, well-thought-out choices; and that requires real guidance. A child has to learn to be autonomous, and initially, this requires an adult taking control. Yet out of respect for the child's presumed form of autonomy, we are, all too often, reluctant to do this.

To illustrate how adult authority leads to children's eventual cognitive and moral autonomy, Green talks of moral education as the development of conscience. He deliberately appropriates the now little-used term 'conscience' in order to endow it with a meaning which goes beyond the modern, narrow conception of 'morality'. He is quick to insist that he does not mean that 'dour, forbidding, internal voice' which says '"No" to every pleasure' (Green 1999: 21). Instead, by 'conscience', Green means 'reflexive judgement about things that matter' (Green 1999: 21). Conscience is our ability to judge our own actions. This extents not only to moral actions, but to more or less skilful actions, such as how well we have managed to clean the kitchen or weed the garden.

Conscience is self-assessment, qualitatively different to our experience of assessment by others. Yet judgements of conscience feel authoritative; as though they come from a 'distant, impartial, disinterested perspective' (Green, 1999: 23). A judgement of conscience is not, exactly, a judgement given by the individual whose conscience it is; neither is it something artificially imposed by an external entity called 'The Group'. Instead, it is our awareness *as group members* that we have neglected one or more of the broad range of interests which Green terms the 'voices of conscience' (explored further in §3.4). Green grants the 'member' an ontological status as real as that of the individual or of the group; and it is the *member* that is the source of the authority of the conscience.

Our conscience includes a loyalty to the group, which would involve valuing the existing norms and practices of that group; recognising that they have been developed with skill and expertise. Therefore, it would involve a limited form of conservatism. However it would also involve a sense of responsibility for correcting the errors of that group. When those errors arise from sub-optimal norms and practices, our 'conscience of membership' would require us to question those norms and practices. The remainder of this section will clarify what the process of norm-acquisition (or 'normation') entails (§3.3), explain the nature of the 'voices of conscience' and their role in self-appraisal (§3.4), and show the importance of group membership for the development of judgement (§3.5).

§5.3: Norms and Normation

Green's definition of conscience as the acquisition of norms which govern behaviour is sufficiently broad to allow 'the notion of moral education to embrace excellence in the exercise of the intellectual virtues as well as the crafts and various practices of the professions' (Green 1999: x). Green quite deliberately refrains from drawing a line between moral norm-acquisition and cognitive norm-acquisition, since when it comes to recognising one's cognitive responsibilities, the line does not exist. One's conscience of membership, the determination that one has (or has not) met the standards of some cognitive group of which you are a member, is a clear case of moral norms and cognitive norms being one and the same.

Aware of this, Green actively uses examples of cognitive norm-acquisition to shed light on his theory. For example, he recalls a suggestion he made to a biology teacher that rather than trying to get his students to apply their knowledge of biology, he should 'aim [at] simply getting the students to "see" things biologically' (Green 1999: 49). In addition to shedding light on the educational process, this suggestion illuminates what is meant by 'normation'. One does not learn a set of norms and then apply them, rather, one 'comes to be normed' (Green 1999: 49) within a setting, as a member of a group. The acquisition of norms, whether moral or cognitive, involves group membership and an accompanying tradition.

It is clear then that by 'norms', Green does not mean statistical norms, or even 'the done thing'. Norms do not describe how people in fact behave, but how they think they ought to behave (Green 1999: 32). Norms are not learnt in the sense that a collection of facts can be learnt, but rather as grammar is learnt. In order to establish whether a person knows a grammatical rule, it is neither necessary nor sufficient for the person to be able to state what the grammatical rule is; even recording the degree to which the individual's speech accords with the rule is not sufficient to see that their behaviour is governed by the rule. Rather, the most appropriate test is to see whether the person corrects his or her speech in the face of departures from the rule (Green 1999: 37). Green stresses that this form of knowledge is neither 'knowing that' nor exactly 'know-how', but 'learning to be' (Green 1999: 37), in the sense that someone might 'learn to be' more considerate.

It would be easy to look at this process of normation with fear that it might limit an individual's reasoning capacity. It certainly is possible for a cognitive group to discourage any questioning of the norms that it promotes, and it is easy to find cases of this occurring. If we are to contribute to the rational governance and advancement of our traditions, it is vital that one's adherence to a set of norms is subject to rational questioning. However, as Green insists, it is a mistake to say that such rational questioning requires a divorce from the relevant tradition; since without undergoing the process of normation, we are in no position to offer rational governance in any case. Like Will, Green recognises that 'no tradition is without its rational resources, and no rational criticism is without its tradition' (Green 1999: 44).

Another way of understanding that norm-acquisition does not intrinsically equal 'brainwashing' is to reflect on the fact that any given individual will have several sets of norms, each acquired from a different social or cognitive group of which she is a member. In terms of 'moral' or behavioural norms, for example, children learn that certain actions which are appropriate at home are not appropriate at school and vice versa. One key example of cognitive norms is 'standards of evidence'. The standards of evidence required for a historical investigation are different to the standards required for a scientific investigation, which are different again from those required for mathematical research. Indeed, as we will later discover by examining Galison's account of high-energy physics, even the cognitive norms of relatively small sub-groups within a discipline may differ significantly from one another. The challenge of an enquirer is to work out, in any given circumstance, which set of norms is the most appropriate to use. In the process of making such choices, an enquirer is able to judge which norms ought to be used and how they ought to be used.

Green describes the process of assessing which norms are used and in what ways in individual circumstances as a process of listening to the different 'voices of conscience'. The following section will explain what the 'voices' are, their role in the decision-making process of individuals, and how they assist reasoning.

§5.4: The Voices of Conscience

Drawing upon 'the familiar fact that the conscience speaks' (Green 1999: 26), or rather our familiarity with the habit of referring to the experience of guilt or unease as

'listening to one's conscience', Green uses the term 'voices of conscience' to refer to the various dimensions of self-appraisal which draw our attention to some of the most relevant considerations in a given situation. The 'voices' have a bearing on how norms may be used in a particular circumstance.

Green offers the following list to sum up the various kinds of 'voice' we experience. Whilst he does not claim the list to be exhaustive, it does appear comprehensive:

> **Conscience of craft**: 'standards of excellence in the practice of any craft.'
>
> **Conscience as membership**: reflection on 'our behaviour viewed in the light of our attachments ... our membership in a group'.
>
> **Conscience of sacrifice**: 'the voice that stands against the inclinations of self-interest'.
>
> **Voice of memory**: 'recollection of ancestral promises and the like ... [bringing] objects of the past into the present to take their place in forming standards of reflexive judgment'.
>
> **Conscience of imagination**, 'asking that we measure the present against what imagination proclaims would be the character of our community if ... it were all it could be'.
>
> (Green 1999: 26-27)

Each of these 'voices' are the consciousness of a type of obligation. Each of the voices reflects a major interest of the group; and a major interest of well-trained individual members within that group. The conscience of craft leaves one aware of sub-standard work by oneself and by others. The conscience of membership is an awareness of one's obligations towards others in a group which one belongs to: in other words, loyalty. Conscience as sacrifice is the 'voice' which speaks out against self interest. At times, these moral obligations may conflict with each other. 'When the conscience of membership speaks, as it often will, in partisan ways…the conscience of sacrifice is likely to speak against it, advancing the claims of impartiality' (Green 1999: 27). The voice of memory reminds us of promises made, but also and more importantly 'brings objects of the past into the present to take their place in forming standards of reflexive judgment' (Green 1999: 27). In other words it reminds us of methods that have previously helped us, or failed to help us, in our investigations. The conscience of imagination asks 'that we measure the present

against what imagination proclaims would be the character of our community if our community were all that it could be' (Green 1999: 27).

These are the major aspects of conscience which come into play when we make a decision. It is important to note that Green is not offering us an automatic decision-making process. Voices do affect how the norms are used, but they do so by drawing our attention to such concerns as 'what does my membership of group X tell me about how to address this situation?' 'What does my imagination regarding the potential of my community tell me about how to address this situation'? It is also important to note that the voices of conscience have no hierarchy. Where two voices disagree, we have no pre-written understanding of which voice is more important. Rather, we develop what Will would call 'flexibility of response' (Will, 1997: 70). We gain a 'feel' for when certain 'voices' take precedence over others, and for when certain norms are more appropriate than others. We develop, in short, a sense of judgment:

> Anyone who has learned simply that one should tell the truth in all circumstances, and undertakes to do so 'by the book', will soon run afoul of the norms of courtesy. (Green, 1999: 46)

It is not useful, therefore, to think about this sense of judgment in terms of any formal list of exceptions to rules. Indeed, anyone who required such a list would lack judgment, lack fluency in their behaviour. They would not yet have developed an understanding of what the norms are for.

The 'voices' provide an answer to the stanch individualist, who differentiates between 'the production of knowledge' and the 'product'; arguing that whilst the former may be irreducibly social, the latter is not. Knowledge, the 'product', consists of true, justified beliefs. In order to justify our beliefs, we must undertake self-assessment; we must be able to say that we have met our cognitive responsibilities by doing all that we can to seek out appropriate evidence. However, as Green argues, our means of self-assessment are so intimately tied up with our group memberships that it becomes implausible to divide 'the production of knowledge' from 'the product'; both are irreducibly social.

Green's understanding of self-assessment relates interestingly to Nietzsche's account of objectivity. Far from endorsing the ideal of what Nietzsche scornfully referred to as a 'pure, will-less, painless, timeless subject of knowledge' (Nietzsche 1998:85), Green acknowledges that the process of acquiring knowledge as a group member requires *the formation* of will; namely, the will to maintain and improve the standards (including cognitive standards) of the group. The first step in improving our objectivity consists in forming our *values*. In a simpler sense, Locke was aware of this too:

> 'He that would seriously set upon the search of Truth, ought in the first place to prepare his Mind with a Love of it. For he that Loves it not, will not take much Pains to get it; nor be much concerned when he misses it. (*Essay,* IV, XIX, §1)

Cognitive investigations require, first and foremost, that the inquirer is genuinely interested in ensuring that her conclusions are justified and her practices wise. The 'voices' in Green's account are various reminders of how best to ensure this. Of course, the 'voices can conflict'. There, the wisest course of action is to listen to all the pertinent voices and to decide, on balance, which course of action is wisest. Just as objectivity requires us to 'move between' our various perspectives upon a matter, objective investigation requires that we move between the various 'voices' which advise us, in order to plot the best course of action.

§5.5: Memberships

We have seen the importance of normation and the voices of conscience for development of self-governance. It remains to clarify the role of group membership in normation and the development of the voices of conscience. In the process we shall explore and clarify Green's claim that 'no rational criticism is without its tradition'.

It is clear that norms are situationally located: we can speak, for example, of the norms of school, the norms of police work, and the norms of cookery. These 'situations' link to group memberships: if someone is inculcated into the norms of police work, it is because she is a member of the police force. The community of cooks is obviously a less tight-knit group; but insofar as someone is a practiced home cook, it is because he has learnt from other experienced cooks and responded to their

comments and criticisms. It is in this way that we acquire practices and become 'normed'. This is also how we develop the 'voices of conscience' that guide which norms are used in a given situation and how they are used.

Recall that Green spoke of our experience of conscience as being both self-assessment and authoritative. Because conscience is self-assessment, it is experienced as an immediate, internal judgement. But it is also authoritative, because we are judging ourselves by a set of standards which have their origin in a particular social group; an 'external' authority. However, it should be made quite clear that the authority isn't really external at all. Whilst we do not exactly set the standards for ourselves, neither is it quite true to say that they are they imposed upon us by an outside source. This is because *we, as individuals, are indistinguishable from the source*: we are members of the group that requires those standards. That is what Green means when he says that 'the proper unit of consideration in moral education is not the individual, not even the individual conscience, but the member' (Green 1999: 86).

To those of a certain philosophical temperament, this could still be read as a limitation of the cognitive capacities and the freedom of the individual. So let us make this perfectly clear. In acquiring the norms and practices of a group, we are not relinquishing epistemic responsibility to some identifiable external thing, 'The Group'. Instead we are taking on the epistemic responsibilities of a group member. Our self-assessment, and our assessment of the practices of the group, are therefore aspects of the same thing:

> By 'governance' must be meant the self-governance of persons. And thus we quickly enter into the domain of ethics, the effective regulation of conduct. By 'governance' must also be meant the governance of belief and, therefore, the governance of inquiry. By 'governance' is meant the governance of various practices among human beings. It extends, therefore, to the governance of conduct in the professions. In short, by 'self-governance' is here meant the regulation of conduct in the exercise of every craft and profession and in the maintenance of every civilising practice.
>
> (Green, 1999: 2-3)

Part of our epistemic responsibility as a group member is the duty to present possible improvements to the norms and practices of the group. To use Green's terms, our awareness of that duty takes the form of the 'conscience of craft', and the 'conscience of membership'.

As we have seen, the 'conscience of membership' is the voice 'remarking on our behaviour ... when viewed in the light of our attachments ... our membership in a group' (Green 1999: 27). The 'conscience of membership' therefore includes our feelings of loyalty. The 'conscience of craft' is our reflexive judgement upon our own work. In most cases, this amounts to an evaluation of whether it meets with the standards we have adopted. It is usually limited to this in the case of the novice. But the 'conscience of craft' of the expert is that voice which urges the production of new and better practices, and, if necessary, of new standards.

The 'conscience of membership' will involve some limited form of conservation. Our loyalty to the group, our respect for other members, involves a recognition that the practices and standards of that group were not plucked out of the air. They have (hopefully) been developed with skill and dedication. So the 'conscience of membership' would urge that that old techniques should not be revised until those revisions are properly tested and developed. But an expert, who is loyal to a group which is dedicated to advances and development, will demonstrate loyalty by suggesting changes, even major changes, to traditional practices and standards, when it becomes clear that improvements can be made:

> If it be asked whether x is a good thing for us to do, or a good way for us to do y, then it is never sufficient merely to answer no ... one must be prepared to go beyond and offer some proposal for improvement. One must always be prepared to entertain the prospect that though things are not as good as we can imagine, nonetheless, they may be as good as we can make them at the moment. (Green 1999: 80-81)

It is our responsibility, in short, to recognise flaws in existing practices and to actively seek improvements; with the recognition that there may not be improvements to be had; at least, not just yet. Particularly it is the role of experienced members to do this.

In this way, the 'voices' of membership and of craft would be harmonised quite easily. Only in a cognitive group which discourages change, one which is conservative to the point of stagnancy, will members feel an *irreconcilable* conflict of conscience. In a healthy cognitive group, longstanding conflicts between the conscience of membership and the conscience of craft will lead to alterations in practice, as we can witness with Galison's interdisciplinary research groups.

Green does not explicitly speak about the importance of multiple memberships for the development of good judgement. But it is worth noting that the conscience of craft and the conscience of membership are necessarily plural, since, as Will rightly emphasises, people inevitably belong to more than one group and are educated in more than one craft or field of enquiry. Individuals caught in a clash between research groups, such as those in Galison's account (see Chapter Four), would not only have multiple 'consciences of membership' but also multiple 'consciences of craft', each aligned to the craft as seen by the group in question. In such cases, the interaction between the 'voices' remains; they must be weighed in the balance and their import to the situation at hand must be compared. Additionally, someone who has developed the sense of 'propriety' which Green speaks of, the sense that practices can be acceptable in one setting and unacceptable in another, would also be able to sense when practices used in one area would be appropriate for use in another. Both the conscience of craft and the conscience of membership would urge the transfer of knowledge and methods in cases such as this. Green's account of normation and the development of the conscience of craft may say little directly about interdisciplinarity, but nonetheless, the account provides a useful basis for understanding how governance benefits from an interdisciplinary approach.

§5.6: Conclusion

Taken alongside the philosophical groundwork obtained from Will, Green's work establishes that reasoning and meaningful criticism of a tradition can only occur if an individual is familiarised with the norms of that tradition. But Green's contribution advances Will's in two important ways. Firstly, it shows how the Cartesian epistemic concerns we met in Will are reflected in pedagogy, and addresses those worries as they are experienced by educators. This makes clear to us that these issues are not the sole concern either of philosophers or of people engaged in advanced research; but are

instead the genuine concern of anyone involved in education. Secondly, Green's work clarifies the importance of group membership. 'The proper unit of consideration in ... education is not the individual ... but the member' (Green 1999: 86).

Green appears to address groups of all kinds, rather than specifically 'cognitive' groups. Yet we have seen that his account of self-governance within those groups is, necessarily, cognitive. Membership requires normation, self-evaluation and evaluation of others with regard to existing norms and practices, and finally, evaluating those norms and practices in terms of their effectiveness in meeting the aims of the group. This understanding of self-governance most certainly applies to specifically cognitive groups. As members of such groups, we have gained the skills to evaluate our own epistemic conduct and that of others. Only as group members could we hope to do this.

§6: Chapter Conclusion

This chapter has established the concept of Linkmaking, a holistic project to develop four kinds of links: links between objects in the physical world (link type 1), which we uncover; Lockean 'links between ideas' (link type 2), cognitively optimal social links between individuals and groups (link type 3), and appropriate links between disciplines or areas of investigation (link type 4). We have argued that it is not possible to develop any one of these without reference to all the others. Linkmaking requires an approach to epistemology which is both realist and social. We therefore began this chapter by placing Linkmaking within the context of modern social epistemology.

Firstly we saw that neither foundationalism nor coherentism is compatible with the project of Linkmaking. The 'one-directionalism' of foundationalism is at odds with the holistic approach of the Linkmaking project, according to which we cannot improve links of type 1 without reference to links of type 2, 3, and 4. This is, of course, precisely what foundationalism demands. Coherentism, on the other hand, focuses on links of type 2, and could, on a certain reading, include links of type 3 and even 4, but does not evaluate these with reference to links of type 1. Therefore, coherentism is not compatible with Linkmaking either. Haack's alternative, founderentism, retains he empirical element of foundationalism but escapes the one-

directionalism. It is therefore, unique of the three, compatible with the Linkmaking project.

Secondly we found, in Longino, a means of assessing cognitive optimality in groups. A consensus within a cognitive group can count as knowledge if and only if said group has public forums for the criticism of evidence, there is an uptake of such criticism, there are public standards by reference to which theories and hypotheses are assessed, and there is an equality of intellectual authority, which prevents the consensus being as a result of political power rather than as a result of critical debate. Longino's account is compatible with Linkmaking because it both offers a means of assessing the optimality of links of type 3, and is committed to a realist view. We can also see that Locke's tentative first steps towards understanding cognitive optimality, as expressed in his distinction between received opinion and testimonial knowledge (*Essay*, XV, §4), find their completion in Longino's account.

We then went on to address in further detail some of the most pressing sceptical concerns about a social account of knowledge. In responding to the Relativist and Coherence Illusions, Will reveals the importance of a plurality of systems of practices for rational governance. This avoids any one set of practices having a monopolistic influence over an individual's thoughts and actions. He also shows the extent to which practices arise from the existential conditions of enquirers, including the physical objects of investigation themselves. Will demonstrates that rational governance of group practices is thoroughly dependent on those practices, and that this is in no way a problem for realism. This demonstration is vital for the Linkmaking project, which demands that we undertake a holistic examination of all four kinds of Links.

We then learnt, from Green's account, how the sceptical concerns addressed by Will have affected educators. As a result of an implicit acceptance of a Cartesian conception of reason as opposed to tradition, many educators have considered that they face a choice between inculcating students into the norms and practices of their traditions, and respecting their students' status as autonomous enquirers. 'Inculcation', in Linkmaking terms, would involve social links (link type 3) becoming the sole determinate of link type 2, whereas the Cartesian conception of autonomy would require that links of type 1 become the sole determinate of links of type 2, severely limiting the legitimate role of the educator as a result. Green's account, which

examines norm acquisition and the formation of the 'voices of conscience', grants us a new conception of autonomy, according to which it is only the acquisition of norms and practices which enables students to act as autonomous enquirers.

In summary, we have introduced and developed the concept of Linkmaking, established the conception of justification that it requires, and the conditions a social group must meet in order for its members to achieve it. We have seen and addressed some of the most basic sceptical concerns with such an account, and seen how it is relevant to understanding autonomy as well as the governance of group practices. It remains for us to address the most powerful challenge to Linkmaking to arise from the philosophy of science; namely, Thomas Kuhn's incommensurability thesis. That is the task of the following chapter.

Chapter Three: Kuhnian Incommensurability and its Influences

§1: Introduction

In the previous chapter, I established the concept of Linkmaking, a realist social epistemology which requires a new conception of objectivity and reasoning. We saw that improving our Linkmaking is a holistic project, which involves not only improving all four kinds of Links, but also developing connections between the four kinds. To improve the number of relevant connections between our ideas (including our curricula and our scientific theories) requires empirical investigation *and* improving our social links *and* developing links between different areas of enquiry. We have seen, then, that Linkmaking requires commitments to the following:

1. Realism
2. A social account of knowledge
3. Founderentism, rather than foundationalism or coherentism
4. Social groups at least theoretically capable of meeting Longino's cognitive standards
5. Making links between different kinds of Links
6. A perspectivist account of objectivity
7. A Lockean-inspired conception of reasoning
8. A non-individualist account of autonomy

Kuhn's incommensurability thesis is a threat to Linkmaking for several reasons. Firstly, the strong version of incommensurability is non-realist. To the Linkmaker, this not only prevents us accessing links of type 1, but thereby also prevents us from assessing links of type 2, 3 or 4 with reference to links of type 1. Kuhn is strongly committed to coherentism; and therefore is incompatible with Founderentism. Furthermore, no Kuhnian, paradigm-bound scientific research group would be able to meet Longino's standards, in part because they would not be able to form the public forums she requires.

More insidiously, incommensurability threatens the viability of Linkmaking because it threatens our capacity to link together the different kinds of Links. Firstly, it severely limits the extent to which scientific theories are based on empirical evidence (Link 1 to Link 2). Secondly, it does not evaluate social constructions in terms of their capacity to give rise to Links of type 2 which are homomorphic with links of type 1. Finally, it severely limits the potential for interdisciplinary work or collaborations between different areas of enquiry, wherever researchers in these areas are committed to different paradigms (Link 3 to Link 4).

It would seem that it is in the interests of the Linkmaker to reject incommensurability entirely, if they can. However, the situation is more complicated than this. Whilst the incommensurability thesis contradicts many of the commitments made by the Linkmaker, it actually provides considerable support to others. Kuhn is, after all, concerned with attacking a naïve form of realism which is intrinsically committed to a God's-eye-view conception of objectivity. His claim that scientists cannot achieve *this form* of objectivity is entirely in line with the Linkmaker's adoption of an alternative understanding of objectivity; even if Kuhn doesn't adopt this alternative himself. He is undeniably offering a social account of knowledge, and he reveals, through practical examples in the history of science, the extent to which our understanding of the world is formulated by our social interactions and by the cognitive commitments of our peers. This is not something that the Linkmaker, with her commitments to a Lockean conception of reasoning, can afford to dismiss. In short, to reject the incommensurability thesis entirely would be to *lose* valuable support for Linkmaking.

This chapter, then, has a careful path to tread. Firstly, we shall examine the original, 'strong' version of incommensurability offered by Kuhn (§2). We shall take each of the major commitments of Linkmaking in turn, and examine the ways in which incommensurability would be incompatible with Linkmaking (§3). We shall also examine the ways in which it might offer support for Linkmaking. This shall be followed by a close textual analysis of Kuhn's archetypal case; namely, the supposed incommensurability of Newtonian and Einsteinian dynamics (§4). In the course of this analysis we shall see that there is a fallacy inherent in the strong account of Kuhnian incommensurability, which occurs due to Kuhn's unconscious acceptance of a

descriptions theory of reference. We shall use Keith Donnellan's article 'Reference and Definite Descriptions' to reveal the flaws of the descriptions theory of reference and in so doing, to uncover Kuhn's mistake (§5).

In order to reclaim the aspects of incommensurability which offer support to the Linkmaker, we shall then go on to examine the alternative interpretation of Kuhn offered by Gerald Doppelt (§6). In the process we shall see that his account escapes the criticisms we have levelled at strong incommensurability. We shall then review the major commitments of Linkmaking, in relation to Doppelt's interpretation of incommensurability; establishing that Doppelt's weakened form of incommensurability is wholly compatible with Linkmaking (§7).

In conclusion (§8), we shall note that a tamed version of incommensurability is not only compatible with Linkmaking but can actually enhance it. However, we shall have to note that it is the strong form of incommensurability which has become influential; not just among philosophers of science but among, for example, sociologists, archaeologists, and physicists. We shall argue in the next chapter that this influence has demonstrably limited intellectual progress in those subjects. Linkmaking practices in general and interdisciplinary behaviours in particular, are the best means of rekindling such progress.

§2.1: Kuhnian Incommensurability

History of science was long considered to have the purpose of shedding light on current practice by examining the development of various scientific theories and methods. This approach was backed by the assumption that science was necessarily cumulative; that every historical development was a mark of progress towards the current state of affairs, which is closer to the truth about nature than previous theories, although perhaps not as close as future ones might be. As a result, examinations of the past were heavily coloured by the views of the present. Kuhn argued that traditional history of science was simply bad history. If historians of science acted as historians would in other contexts, by describing historical developments with reference to the motivation and assumptions of the peoples of that time, we would end up with a very different picture of the way science develops. We would see that science does not progress in a linear fashion, but rather in sudden and violent theoretical leaps, or

scientific revolutions, which involve losses as well as gains in data and in explanatory power. *The Structure of Scientific Revolutions* was Kuhn's attempt to tell a more accurate, and hence messier, story of scientific development.

Kuhn's history of science depicts periods of relative calm, or 'normal science', interspersed with scientific revolutions. The periods of normal science are relatively uneventful; scientists get on with the everyday business of solving scientific puzzles. The process of solving these puzzles does not, in itself, offer any challenges to the paradigm that the puzzles are solved in, any more than a chess game tests the rules of chess. Therefore periods of normal science involve little in the way of reflection on the accepted scientific theories and practices. However, as experimental data increases, anomalies emerge, results which do not fit in with the theory. Whilst a single anomaly is not considered evidence of a problem with the theory, when sufficient anomalies emerge, there is a motivation to develop and adopt a new theory which produces fewer anomalies.

According to Kuhn, the move from one theory (or family of theories) to another involves a 'paradigm shift'. This a conceptual leap made by certain (often younger) scientists. After a paradigm shift, scientists see the world in a completely new way, a way which it is not possible to communicate fully to people who still work within the old paradigm. Whilst the two groups of scientists can still communicate to some limited extent, fully informed debate between the groups is impossible. The old and new paradigms are, supposedly, incommensurable with each other.

What, exactly, does this mean? There is no single answer in the *Structure* to the question of what incommensurability is. Rather, we can distinguish three main versions of, or perhaps aspects to, incommensurability. The first two problems are data loss and a shift in the problem set between theories. Paradigms may be incommensurable because they disagree on which scientific problems are of primary importance; or because they disagree about suitable standards of proof. In practice, these two aspects are closely connected. As an example, Kuhn regards the move from pre-Daltonian to Daltonian chemistry as being an archetypal example of scientific revolution. He argues that the phlogiston theory 'achieved reasonable answers to a whole set of questions' (Doppelt, 1978: 42), including the question of why metals had

so many more properties in common with each other than their ores did (Kuhn 1970: 99). These questions were abandoned by the new chemistry:

> Lavoisier's reform... ultimately did away with chemical "principles", and thus ended by depriving chemistry of some actual and much potential explanatory power. To compensate for this loss, a change in standards was required. During much of the nineteenth century failure to explain the qualities of compounds was no indictment of a chemical theory. (Kuhn 1970: 107)

The phlogiston theory was incommensurable with Daltonian chemistry because scientists committed to the first paradigm maintained that the ability to address questions about the qualities of compounds was a necessary condition for an acceptable chemical theory. Dalton and Lavoisier's reform required (temporarily at least) abandoning such questions. This means that comparing the two theories is not a straightforward matter of assessing them in terms of how well they describe the physical world. It also requires a value judgment – should we, or should we not, require of *any* chemical theory that it must be able to address the qualities of compounds? This, as we shall see later, is the form of incommensurability which Gerald Doppelt regards as the primary form.

The third main aspect of incommensurability is that a change of theory involves redefinitions of key terms and concepts. After a scientific revolution, many of the terms and concepts used by the previous theory remain, but they have radically different relationships with one another. For example, the terms 'gravity' and 'mass' have radically different meanings under the Newtonian and Einsteinian theories, not least because Einsteinian mass varies with velocity and is convertible with energy, neither of which is true of Newtonian mass. Whilst the reaction of many is to say that Newton's Laws are simply a limiting case of Einstein's and can be derived from Einstein's, Kuhn argues that this is a mistake. Whilst Einstein's theory can explain why Newton's appeared to work, Newton's Laws cannot be derived from Einstein's. Kuhn maintains that Newtonian and Einsteinian mass *are not the same*. Kuhn goes so far as to say that Newtonians and Einsteinians 'practice their trades in different worlds' (Kuhn 1970: 150); in one; mass varies with velocity and is convertible with energy, and in the other, it does and is not.

This form of incommensurability has generally been regarded as the primary form, particularly by positivist readers. It has also, understandably, come under much critical scrutiny by philosophers, as smacking of relativism and scepticism about the possibility of scientific knowledge. Kuhn's protest against such accusations is that whilst 'two groups of scientists see different things when they look from the same point in the same direction', it does not follow that they can see anything they choose, because they are still 'looking at the world, and the world has not changed' (Kuhn 1970: 150). But this seems immediately contradictory. How can the scientists practice their trades in different worlds, and yet be looking at the same unchanged world? It is clear from the frequency of and emphasis upon the phrase 'different worlds'[5] that Kuhn means by it something more than a metaphor, and yet his qualifications of the phrase suggest that he means something less than an ontological claim. Since it is the apparent commitment to relativism and non-realism which renders incommensurability a threat to Linkmaking, it is vital that we unravel this confusion.

§2.2: Working in Different Worlds

Our next task, then, is to make a detailed examination of what 'working in a different world' consists of. To achieve this, it is necessary to establish what allegedly occurs in a Kuhnian paradigm shift.

Kuhn compared the experience of a paradigm shift to that of a Gestalt switch (Kuhn 1970: 85); when looking at the famous duck-rabbit image it is possible to see the image of a duck flip to the image of a rabbit and back again. In the case of a paradigm shift, however, one cannot flip back again; and also, paradigm shifts do not simply affect the scientist's perception of a single object or experiment. Inevitably, the scientist who has moved from one paradigm to another must also change his view of many other familiar substances. For example, when Lavoisier learned to see oxygen, he also had to learn to see a compound ore where others saw an elementary earth. Kuhn claims that

[5] See Kuhn 1970: 111, 118, 120, 121, 135

> At the very least, as a result of discovering oxygen, Lavoisier saw the world differently. And in the absence of some recourse to that hypothetical fixed nature that he "saw differently", the principle of economy will urge us to say that after discovering oxygen Lavoisier worked in a different world.... (Kuhn 1970: 118)

Many readers would want to say here that what changes with a paradigm is simply the interpretation of observations, but that the observations themselves remain unchanged; that 'Priestley and Lavoisier both saw oxygen, but they interpreted their observations differently' (Kuhn 1970: 120). Kuhn says that this view 'can be neither all wrong nor a mere mistake' (Kuhn 1970: 121). Nonetheless, he maintains that 'what occurs during a scientific revolution is not fully reducible to a reinterpretation of individual and stable data' (Kuhn 1970: 121). This is in part because the data is not unequivocally stable. The data, he argues, are a product of the interaction between a paradigm and the environment. Therefore when the paradigm changes, the data change too. Kuhn expands upon this claim with reference to the example of Aristotle, Galileo, and the pendulum:

> Seeing constrained fall, the Aristotelian would measure (or at least discuss – the Aristotelian seldom measured) the weight of the stone, the vertical height to which it had been raised, and the time required for it to achieve rest. Together with the resistance of the medium, these were the conceptual categories deployed by Aristotelian science when dealing with a falling body ...Galileo saw the swinging stone quite differently. Archimedes' work on floating bodies made the medium non-essential; the impetus theory rendered the motion symmetrical and enduring; and Neoplatonism directed Galileo's attention to the motion's circular form. He therefore measured only weight, radius, angular displacement, and time per swing, which were precisely the data that could be interpreted to yield Galileo's laws for the pendulum. (Kuhn 1970: 123-4)

To the Aristotelian, the weight, height, and time elapsed were the only relevant data, because they were measuring a stone falling, albeit falling under a constraint. To Galileo, weight was relevant, but so were radius, angular displacement and time per

swing; because he was measuring a pendulum. Kuhn maintains that when the swinging stone is seen through the Aristotelian paradigm, 'it exhibits its governing laws almost on inspection' (Kuhn 1970: 125). They see constrained fall, and the judgment of which data is relevant is made *because* they see constrained fall. The same is true of Galileo. In short, Kuhn claims that the immediate content of the Aristotelian's experience of swinging stones is not the same as the immediate content of Galileo's experience. Whilst Galileo, living in his own new paradigm, could successfully explain why the Aristotelians saw the stone as they did, he cannot experience the swinging stone as they did.

Kuhn therefore argues that the operations and measurements that a scientist undertakes are not "the given" of experience but rather "the collected with difficulty" (Kuhn 1970: 126). Which measurements are taken depends on the paradigm to a greater extent than it depends on direct experience:

> As a result, scientists with different paradigms engage in different concrete laboratory manipulations. The measurements to be performed on a pendulum are not the ones relevant to a case of constrained fall. (Kuhn 1970: 126)

Kuhn's key claim here is that neither seeing the pendulum nor seeing the constrained fall is properly an interpretation of data. Rather, seeing the pendulum or the constrained fall *is* the data. Interpretation of data can only occur *after* a paradigm has already been chosen. Whilst scientists do constantly interpret observations and data, Kuhn maintains that this interpretive enterprise can only articulate a paradigm, not correct it. Therefore, whilst differing *interpretations* within a given paradigm can be meaningfully discussed with reference to common data, differing *paradigms* cannot. This is what Kuhn means when he says that scientists committed to different paradigms 'live in different worlds'; and this is the strongest version of incommensurability.

§3.1: Kuhnian Incommensurability and Linkmaking

We must now consider the following question: in what ways, and to what extent, would incommensurability be incompatible with Linkmaking? Owing to the various commitments made by the Linkmaker, incommensurability would prove a threat if it does any of the following:

1. Has non-realist commitments
2. Equates a social account of knowledge with non-realism
3. Contradicts founderentism
4. Produces groups in principle incapable of meeting Longino's standards
5. Prevents links being formed between any of the four kinds of Links, e.g.:
    a. Severely limits the extent to which scientific theories are based on empirical evidence (Link 1 to Link 2)
    b. Does not evaluate social constructions in terms of their capacity to give rise to Links of type 2 which are homomorphic with Links of type 1
    c. Severely limits the potential for interdisciplinary work or collaborations between different areas of enquiry (Link 3 to 4)
6. Presupposes a 'God's eye view' account of objectivity
7. Presupposes an individualist account of reasoning
8. Presupposes an individualist account of autonomy

The business of this section is to work through the above list, in each case establishing whether incommensurability would count as a threat, offer support, or remain neutral with regard to each particular commitment made by the Linkmaker. We shall discover that whilst incommensurability is incompatible with Linkmaking as a whole, it offers support to certain Linkmaking commitments: in particular, in its opposition to the God's eye-view account of objectivity, and to individualist accounts of reasoning and autonomy. The task of the rest of this chapter will be, firstly, to demonstrate that the strong version of incommensurability is internally inconsistent, and then to establish that Doppelt's revised interpretation of incommensurability can offer the

Linkmaker a means to retain the more desirable aspects of the incommensurability thesis.

§3.2: Realism and a Social Account of Knowledge

The first two concerns can be addressed together: namely, the concern that incommensurability is non-realist, and the concern that Kuhn regards social accounts of knowledge as *necessarily* non-realist. Realism, as we have defended it in this thesis, is the belief that there is a physical world and that we can know a great deal about that physical world. Realism has, then, both an ontological demand and an epistemological demand. It requires that a physical world exists, and also that our empirical experiences serve to give us access to this physical world.

Kuhn's relationship with realism is complex, because he is *both* at pains to show that his account is not a threat to the ontological claims of realism; *and* openly threatens the epistemological demands of realism. As we have seen, he unites the claim that 'two groups of scientists see different things when they look from the same direction' with the claim that they are still 'looking at the world, and the world has not changed' (Kuhn 1970: 150)[6]. The main threat to realism from incommensurability, then, is this 'different worlds' thesis. According to this thesis, paradigms are not merely ways of interpreting neutral empirical data. Instead, the paradigm which a scientist is committed to shapes the data that they perceive.

Since the data themselves are formed by the paradigm, it is not possible for two competing paradigms from being directly compared in terms of how well they explain the available data. Therefore, an individual scientist's move from one paradigm to another is not a rational choice, but an irrational 'conversion' which, for most, will be brought about by the weight of popular support for the new paradigm. (In Locke's terms, the conversion is due to 'received opinion' rather than 'testimonial knowledge'.) Here, social influence upon scientific beliefs is *contrasted* with rationality, which can apparently only occur within the narrow confines of normal science. Only once a paradigm is in place, it is possible to assess the cognitive optimality of that paradigm; and then only in terms of how well it answers its self-set

[6] Particularly sympathetic readers of Kuhn have tried to interpret this as a Kantian distinction between noumena and phenomena, but Kuhn does not openly distinguish the two within the *Structure*.

questions. For the Linkmaker, who requires both cross-paradigm communication and a broader conception of rationality than the one suggested here, this simply will not do.

§3.3: Founderentism

In our examination of Haack's founderentism, we established that neither foundationalism nor coherentism is compatible with the Linkmaking project. Linkmakers maintain that we cannot *just* improve our capacity to make Links of type 1 without simultaneously addressing our Links of type 2, 3, and 4. This is at odds with the 'one-directionalism' of foundationalism. The coherentist view is also incompatible with the Linkmaking, since it cannot define the optimality of Links of type 3 and 4 in terms of their capacity to give rise to Links of type 1. Founderentism retains the empirical element of foundationalism whilst avoiding the one-directionalism; and successfully combines this with the coherentist virtue of allowing mutual support among beliefs. Founderentism, then, is the theory of justification adopted by Linkmakers.

Therefore, if incommensurability were (a) committed to foundationalism, (b) committed to coherentism, or (c) is committed to a *disjunction between* coherentism and foundationalism such that founderentism is not a viable alternative, it would be incompatible with Linkmaking.

Of course, Kuhn can hardly be accused of (a). According to incommensurability, the very data gathered by scientists is formed by their commitment to a paradigm. There can, on this account, be no such thing as 'basic' beliefs, and hence, no foundationalism. Instead, the Kuhnian account lends itself to (b), a coherentist view of justification.

Let us consider these points a little further. In order for data to serve as a test on a theory or hypothesis, it is necessary that the data is recognised to be relevant to that theory or hypothesis. Kuhn argues that in a pre-paradigm state, 'all of the facts that could possibly pertain to the development of a given science are likely to seem equally relevant' (Kuhn 1970: 15). Conversion to a paradigm means accepting an (at least partially arbitrary) consensus as to which facts are, and are not, relevant to the

development of a given science. Since only relevant facts can be taken into consideration when evaluating how justified we are in believing a particular conclusion, justification can only occur within a paradigm. This necessarily means that conversion to a paradigm is not *itself* justified.

What, then, of (c)? Well, we can explain the issue with reference to Haack's crossword puzzle metaphor. Suppose that we wish to assess the likelihood of a particular solution being accurate. We have two lines of investigation open to us. The first is to evaluate that solution with reference to its clue. Could we have misinterpreted the clue, or missed part of it? Is there another possible solution to the same clue? The second is to evaluate the intersecting entries. Could we be mistaken in any of those? We have said before that if there are only one or two intersecting entries, then we are as likely to pursue the second method of investigation as the first; but if there are several intersecting entries each of which itself has several intersecting entries, we are much more likely to focus on the first line of investigation.

The question that arises here is, within Haack's fine metaphor, where exactly is the 'paradigm'? Kuhnian incommensurability suggests that the paradigm would *both* form the data, *and* decide which questions are worth asking (therefore, which solutions are considered to 'intersect' with the one at hand). Haack's two forms of investigation would become, simply, two means of investigating the paradigm, and would therefore collapse back into coherentism.

§3.4: Longino's standards

Let us first review the standards put forward by Longino, and then assess the extent to which a Kuhnian research group could meet those standards:

1. There must be publicly recognized forums for the criticism of evidence, of methods, of assumptions and reasoning.
2. There must be an uptake of criticism. The community must not merely tolerate dissent; its beliefs and theories must change over time in response to the critical discourse taking place within it.
3. There must be publicly recognized standards by reference to which theories, hypotheses, and observational practices are evaluated and by

appeal to which criticism is made relevant to the goals of the inquiring community…

4. Finally, communities must be characterized by equality of intellectual authority. What consensus exists must be the result not of exercise of political or economic power, or of the exclusion of dissenting perspectives, but as a result of critical dialogue in which all relevant perspectives are represented. This criterion is meant to impose duties of inclusion: it does not require that each individual, no matter what her or his past record or state of training, should be granted equal authority on every matter (Longino 1994: 145).

The first thing to note is that Longino calls for 'publicly recognised forums' for criticising, among other things, evidence. Since, according to incommensurability, the data or evidence available to the scientist depends on the acceptance of a particular paradigm, the forum in question could not be so public as to include any scientist not already committed to the same paradigm. 'Uptake of criticism' would therefore be limited to criticism within the paradigm as to how well the paradigm was being implemented or how well it is succeeding at meeting its self-defined goals. Criticism from outside the paradigm would be incommensurable with the epistemic commitments of those within the paradigm.

Longino also asks that there be 'publicly recognised standards by reference to which theories… are evaluated' (Longino 1994: 144). She grants that there isn't a set of standards in common for all cognitive communities, but does insist that locally adopted sets of standards should be drawn from such 'cognitive virtues' as 'accuracy, coherence, and breadth of scope' (Longino 1994: 144). In the case of Kuhn's research groups, the theory-ladenness of data makes it difficult to answer the question of what counts as 'accurate'. One paradigm could, indeed, have a broader scope than another, but (as supposedly in the case of Lavoisier's revolution in chemistry) a scientist within the new paradigm could simply deny that previously central questions were relevant, or even that they were scientific questions at all. It does, admittedly, seem possible that one paradigm might be demonstrably more coherent than another, but since this cannot be the reason for adopting the paradigm in the first place, it would be difficult to maintain that even this is a '*publicly* recognised standard'.

Lastly Longino demands that cognitive communities be characterised by equality of intellectual authority. Consensus, in order to qualify as knowledge, must not result from non-rational influences such as political power, and dissenting voices must be encouraged to contribute. But if conversion to another paradigm is itself a non-rational act, and the majority of scientists who convert to a new paradigm do so as a result of an eventually overwhelming peer pressure, then the consensus of a paradigm group is necessarily as a result of political power.

We are left here with a perplexing question. Kuhn's research groups cannot meet Longino's standards for cognitively responsible groups whose consensus may be regarded as knowledge; so, in what sense could Kuhn's scientists show themselves to be cognitively responsible, or *more* cognitively responsible than adherents to an alternative paradigm? The fact that they meet the epistemic requirements delineated by their own paradigm would not be sufficient to show this, since those epistemic requirements are not exposed to public scrutiny. Instead, perhaps we could say that paradigm groups demonstrate that they are cognitively responsible simply by developing better ways of solving scientific problems, thus contributing to the progress of science (as Kuhn understands such progress). However, as we have already seen, each paradigm group decides for itself what *counts* as a scientific puzzle. Therefore, it is unclear how later paradigms could be deemed 'more successful' than earlier ones at solving puzzles: particularly if the new paradigm simply rejects certain puzzles as being unimportant or irrelevant, as was apparently the case with the archetypal case of Lavoisier. It would seem, then, that without some form of assessment akin to Longino's, scientific progress (even in the limited sense that Kuhn allows) would be under threat; yet the Kuhnian groups necessarily fail Longino's assessment, and we have been offered nothing in its stead.

§3.5: Making links between Links

Linkmaking not only encompasses the four kinds of links (links between objects in the physical world, Lockean links between ideas, social links, and interdisciplinary links) but also maintains that the project of improving these Links is necessarily holistic. For example, we cannot improve the number of relevant connections between our ideas (Link type 2) without reference to our empirical evidence (Link type 1) our

social epistemic links, and our interdisciplinary links.[7] It is necessary therefore to make links between Links. One of the major threats to Linkmaking from incommensurability is that it would severely limit our capacity to do this.

Firstly, according to Kuhnian incommensurability, data is theory-laden. This is not, as we have seen, merely the claim that one's preconceived theory is likely to affect one's interpretation of the data. Rather, as a result of their commitment to their respective paradigms, Aristotle saw constrained fall where Galileo saw a pendulum. It was not merely that the two had different interpretations of the same data; but rather, that they did not have the same data to interpret. This theory ladenness (which ultimately leads to Kuhn's insistence that the two scientists 'lived in different worlds') severely limits the extent to which scientific theories are based on empirical evidence, thus weakening the link from Link 1 to Link 2.

Secondly, the Linkmaker is committed to evaluating social constructions in terms of their capacity to give rise to Links of type 2 which are homomorphic with Links of type 1. Incommensurability would make this problematic for several reasons. Firstly, theory-ladenness weakens the link between Links of type 1 and Links of type 2. Secondly, the Kuhnian view does not, in any case, equate scientific progress with progress towards the truth about nature; and therefore, cannot evaluate social constructions in this way. Instead the Kuhnian prefers to evaluate cognitive groups in terms of their ability to solve scientific puzzles more effectively than their competitors (although, as we have seen, this is itself a problematic requirement).

Thirdly and finally, the Linkmaker emphasises the importance of developing links between disciplines and areas of investigation in order to improve our reasoning skills and our objectivity. Our social links must therefore be arranged such that we are capable of accessing as many relevant interdisciplinary links as possible. This is the link between Links of type 3 and Links of type 4. Incommensurability is problematic here for two reasons. The lack of communication across paradigms is in and of itself a limitation; since a paradigm can be loosely interpreted as an 'area of investigation', and the Linkmaker would therefore want as much communication between paradigms

[7] For a more detailed account of this see the introduction to Chapter Two.

as possible. Also, insofar as such cross-paradigm research is limited, we have reason to believe that interdisciplinary research would be limited as well. As we shall learn in the next chapter from Peter Galison, much interdisciplinary research within physics would also count as 'cross-paradigm' research under a Kuhnian view. Indeed, when it comes to sub-disciplines within particle physics, 'disciplines' and 'paradigms' seem to be interchangeable. In addition, the dispute between processual and post-processual archaeology can as convincingly be described as the result of a 'paradigm shift' as it can be understood as a disciplinary divide. In both cases, the issues seem to be the same: a major disagreement as to what methods of investigation and standards of proof are appropriate for archaeologists. If, as strong incommensurability suggests, there is no means to construct a rational debate across such a divide, then the Linkmaker's hopes for interdisciplinary research are severely curtailed.

§3.6: 'God's eye view' account of objectivity

The Kuhnian account does not claim that there is one 'God's eye view' of the universe which humans must strive to attain; very much the opposite. Kuhn's concern is to debunk a naïve form of realism which is committed to such a version of objectivity. In this much, the Kuhnian and the Linkmaker are agreed. However, rather than offering a *better* conception of objectivity, Kuhn leaves us without any version of objectivity whatsoever. Nor can we simply incorporate the Linkmaker's Nietzschean, perspectivist account of objectivity into an otherwise unaltered version of incommensurability. Perspectivism requires that we seek out and retain all perspectives which offer insight into the object of investigation. This stands in considerable tension with incommensurability, according to which there are non-rational conversions from one scientific paradigm to another which result in scientists who inhabit different worlds, and cannot fully communicate those worlds to each other. Kuhn's rejection of the traditional, simplistic, Gods-eye-view conception of objectivity is admirable, but without a new conception of objectivity, the incommensurability thesis necessarily collapses into a form of relativism that Kuhn himself would be reluctant to embrace; insisting as he does that scientists of opposing paradigms are 'still looking at the [same] world, and the world has not changed' (Kuhn 1970: 150).

§3.7: Individualist account of reasoning

We have learnt from Frederick Will that an all-too-common result of acknowledging that our cognitive practices are social is to conclude that they are therefore irrational:

> To the extent that the processes of observation, reasoning, criticism, judgement, evaluation, and so on, traditionally associated with rational governance, are recognised to be thoroughly determined by, thoroughly rooted in, social practice, it has seemed to many philosophically sensitive writers increasingly difficult to continue to maintain for them their claims to rationality. (Will 1997: 63)

It is true that the Kuhnian account does not do much to quash such fears; and yet, we still cannot accuse Kuhn of promoting an individualist account of reasoning. He insists that observation and judgement are paradigm bound, and hence necessarily social, and also maintains that science can progress (albeit towards improved puzzle-solving rather than towards the truth about nature). This combination implies a commitment to social account of reasoning, even if this commitment is not openly explored.

The problem that remains is not that incommensurable paradigm groups would be irrational, but rather, that they would only be rational within the context of their particular paradigm. Conversion to that paradigm is, in itself, a non-rational act. Kuhnian reasoning is, therefore, 'reasoning' in a very narrow sense. By this, I do not mean Frederick Will's 'narrow sense' of reasoning, which is limited to the formal logic, calculation, inference, deduction and so on that an individual mind is capable of. Rather, this reasoning is 'narrow' because it is limited to the confines of its paradigm. It is certainly social reasoning, but it is not social *enough* for the Linkmaker.

§3.8: Presupposes an individualist account of autonomy

Linkmakers are opposed to the individualist conception of autonomy which regards autonomy as opposed to social influence. Green has shown us that educators often feel torn between two apparently conflicting cognitive duties to their students; on the one hand, inculcating them into the norms and practices of their traditions, and on the other, encouraging them to be autonomous. Green's solution to this apparent dilemma is to demonstrate that autonomy, far from existing 'in opposition' to accepted norms

and practices, is developed by exposure to such practices. [8] Only once an individual has gone through the process of 'normation' are they capable of making autonomous decisions. They develop their self-assessment skills, or 'conscience' as a member of a group.

This understanding of autonomy or self-assessment as dependent upon group membership is not incompatible with incommensurability. The Kuhnian scientist's very *data* exists as a result of their group membership. Their self-assessment as to, for example, how well they have interpreted that data, is therefore necessarily dependent upon their 'normation' (or in other words, their adherence to the paradigm.) However, a major reason why normation does not amount to indoctrination on Green's account is that any given individual will have several sets of norms, each acquired from a different social or cognitive group. This allows them to assess which set of norms is appropriate to use in a given situation; and can include 'importing' norms from other areas (as is common in interdisciplinary research practices). Incommensurability would severely limit scientists' capacity to do this. Another problem arises from Kuhn's strict dichotomy between two distinct kinds of scientific practice: 'normal' and 'revolutionary'. Incommensurability only seems to allow for the version of 'conscience of craft' which is practiced by novices in the field; namely, the ability to evaluate whether we personally have managed to meet the standards we have adopted. The conscience of craft as practiced by experts is meant to urge the production of new and better practices and standards. According to the incommensurability thesis however, new standards means a new paradigm. Therefore, the more sophisticated and important role of the conscience of craft would only come to the fore in tearing down the craft, in order to build a new one, as occurs in scientific revolutions. However, the expert's conscience of craft has no place in normal science. This, the Linkmaker would maintain, grants it too limited a role.

§3.9: Conclusion

We have seen that incommensurability as Kuhn presents it is incompatible with Linkmaking, as it contradicts many of the philosophical commitments of the

[8] In fact, Green uses the phrase 'self-assessment', or 'conscience', rather than 'autonomy', to avoid confusion that may result from the latter's individualist connotations. I have chosen to retain the term 'autonomy' as I believe it deserves to be reclaimed.

Linkmaker. Notably it has non-realist commitments, maintaining that paradigms shape the data which scientists perceive. It also strongly implies that a commitment to a social account of knowledge is *necessarily* a commitment to a non-realist account of science. It is incompatible with the founderentist account of justification adopted by Linkmakers; and would fail to produce cognitive groups capable, even in principle, of meeting Longino's standards for cognitive optimality. Furthermore and most significantly, incommensurability would curtail the possibility of links being formed between any of the four kinds of Links; because it severely limits the extent to which scientific theories are based on empirical evidence (Link 1 to Link 2), fails to evaluate social constructions in terms of their capacity to give rise to links of type 2 which are homomorphic with links of type 1, and markedly reduces the potential for interdisciplinary work.

There are points, however, where the incommensurability thesis echoes and even strengthens the position of the Linkmaker. Notably, Kuhn's account does not presuppose a 'God's eye view' account of objectivity. On the contrary, it is concerned with eliminating the naïve form of realism which is committed to this view of objectivity, and his alternative account of scientific development beautifully illustrates its implausibility. In this, the Kuhnian and the Linkmaker are agreed: the 'God's-eye-view' account of objectivity is as meaningless as it is unobtainable. The difference, however, is that Kuhn does not offer any alterative conception of objectivity, as the Linkmaker does. Furthermore, we cannot simply import the Linkmaker's perspectivist conception of objectivity into the Kuhnian account, as it stands very uneasily with the lack of alternative perspectives that results from incommensurability between paradigms.

We have also seen that the Kuhnian account is compatible with a social rather than an individualist account of reasoning. His insistence that observation and judgement are paradigm-bound, combined with his commitment to the existence of progress in science (albeit progress towards improved puzzle-solving) implies a commitment to a social account of reasoning. The difficulty remains that a social account of reasoning coupled with incommensurability necessarily means that reasoning can only be said to occur within a paradigm. A Kuhnian account of reasoning may be social, but it is also relativistic. Similarly, a Kuhnian account is compatible with a non-individualistic

account of autonomy, since it recognises that a scientist's self-assessment is necessarily dependent upon her membership of a cognitive group. Unfortunately, the cognitive group in question is bound by their commitment to a paradigm, incommensurable with competing paradigms. As we have seen, a major reason why normation does not amount to indoctrination on Green's account is that any given individual has the capacity to 'import' norms from elsewhere; something impossible for the Kuhnian scientist.

We can therefore see that in order to defend Linkmaking, it is necessary to demonstrate that incommensurability as Kuhn describes it does not occur. We shall take a two-pronged approach to this.

Firstly we shall reveal a fundamental flaw in Kuhn's argument. We shall undertake a close-text analysis of Kuhn's archetypal and strongest argument in favour of strong incommensurability, namely the apparent impossibility of deriving Newtonian dynamics from Einsteinian dynamics, and reveal that Kuhn's argument rests on in implicit acceptance of the descriptions theory of reference. Using Keith S. Donnellan's article *Reference and Definite Descriptions* we shall demonstrate that the descriptions theory of reference is flawed; and thus, so is Kuhn's theory.

Secondly, we shall examine an alternative interpretation of the incommensurability thesis, as presented by Gerald Doppelt. Doppelt argues that the common interpretation of incommensurability, which he terms the 'positivist' interpretation, is neither charitable nor convincing. Instead, he suggests that Kuhn's argument is more plausibly interpreted (and more plausible if we interpret it) as based on an incommensurability of scientific problems, rather than scientific meanings. This interpretation escapes the 'definite descriptions' objection; but happily, also turns out to be compatible with Linkmaking.

§4.1: Deriving Newtonian Dynamics from Einsteinian Dynamics

Kuhn argues that Newtonian dynamics cannot be derived from Einsteinian dynamics. He begins his argument by setting out what a derivation of Newtonian dynamics from

relativistic dynamics would look like. He then attempts to demonstrate that such derivation is not possible. Firstly he asks us to imagine a set of statements, E1, E2, and so on, 'which together embody the laws of relativity theory'(Kuhn 1970: 101). These statements contain variables and parameters, including the Einsteinian conception of mass. These statements, along with a judicious application of mathematics and logic, can be used to make predictions. In order to demonstrate that Newtonian dynamics is a special case within relativistic mechanics, we would have to restrict the range of parameters and variables to relatively low velocities, which do not approach light speed. In this way we can produce what Kuhn refers to as Ni statements. These Ni statements are a limited case of relativistic mechanics, and are consistent with Newtonian dynamics. In other words, the predictions made by relativistic dynamics are consistant with the predictions made by Newtonian dynamics, provided that velocity is not approaching light speed. Therefore, 'apparently Newtonian dynamics has been derived from Einsteinian dynamics, subject to a few limiting conditions' (Kuhn 1970: 101). Kuhn then details his reasons for rejecting this conclusion:

> ...the derivation is spurious, at least to this point. Though the Ni's are a special case of the laws of relativistic mechanics, they are not Newton's Laws. Or at least they are not unless those laws are interpreted in a way that would have been impossible until after Einstein's work. The variables and parameters that in Einsteinian Ei's represented spatial position, mass, etc, still occur in the Ni's, and they there still represent Einsteinian space, time, and mass. But the physical referents of these Einsteinian concepts are by no means identical with those of the Newtonian concepts that bear the same name. (Newtonian mass is conserved, Einsteinian is convertible with energy. Only at low relative velocities may the two be measured in the same way, and even then they must not be conceived to be the same.) Unless we change the definitions of the variables in the Ni's, the statements we have derived are not Newtonian. If we do change them, we cannot properly be said to have derived Newton's Laws, at least not in any sense of 'derive' now generally recognized. Our argument has, of course, explained why Newton's Laws ever seemed to work. In doing so it has justified, say, an automobile driver in acting as though he lived in a Newtonian universe. An argument of the same type is used to justify teaching earth-centred

> astronomy to surveyors. But the argument has still not done what it purported to do. It has not, that is, shown Newton's Laws to be a limiting case of Einstein's. For in the passage to the limit it is not only the forms of the laws that have changed. Simultaneously we have has to alter the fundamental structural elements of which the universe to which they apply is composed.
>
> (Kuhn 1970: 101-102)

The Ni statements, then, are not strictly speaking Newton's Laws, because they have been interpreted in a way which would not have been possible before Einstein. Both Ei and Ni statements use the same parameters: they both use the same (Einsteinian) concept of mass. Since the Newtonian and Einsteinian concepts of mass are not the same, the Ni statements cannot properly be said to be Newtonian unless we change the variables; but if we do change the variables, then they are not derived from Einsteinian dynamics. Therefore, we cannot derive Newtonian dynamics from Einsteinian dynamics, and the two are incommensurable with one another.

In order to assess Kuhn's argument, I will first put it into the form of a deductive proof, as follows. Several implied but missing premises are presented in italics. I will then list the problems inherent in the argument.

1) The variables in both Ei and Ni are Einsteinian, including the Einsteinian version of mass. (Kuhn 1970: 101)

2) The Einsteinian concept of mass is different to the Newtonian concept of mass. On Einstein's account, mass is a function of velocity, and is convertible with energy. On Newton's account, mass is fixed. (Kuhn 1970: 102)

3) *A description denotes an entity by accurately describing it.*

4) *If the concept of mass is different, the description of mass is also different.*

5) The Einsteinian and Newtonian concepts *(and descriptions)* are incompatible . (Kuhn 1970: 102)

Therefore,

6) *They do not describe, or denote, the same entity.*

Therefore,

7) The physical referent of Einsteinian mass is not the same as the physical referent of Newtonian mass. (Kuhn 1970: 102)

8) Unless we change the definition of the variables, then the Ni statements are not Newtonian. (Kuhn 1970: 102)

9) If we do change the definition of the variables, then the Ni statements are no longer derived from Einsteinian dynamics. (Kuhn 1970: 102)

Therefore,

10) Newtonian dynamics cannot be derived from Einsteinian dynamics. (Kuhn 1970: 102)

Therefore

11) *Newtonian and Einsteinian dynamics are incommensurable with one another.*

Presenting the argument in logical form immediately brings a paradox to the reader's attention. If the Newtonian and Einsteinian theories do not have the same physical referent, then they are referring to different things, and therefore they are not incommensurable at all (or even incompatible). However if we reject the idea that there are two separate physical entities for them to refer to, namely Newtonian mass and Einsteinian mass, and insist that they are talking about the same thing, then premise 7 is false. In this case, whilst the two theories are incompatible, we have no reason to suppose that they are incommensurable. This paradox has developed due to Kuhn's implicit acceptance of the Descriptions Theory of Reference. The following

section (§5) will further explain Kuhn's dependence upon the Descriptions Theory of Reference. We shall then see why the Descriptions Theory of Reference, and therefore incommensurability, is founded on a mistake.

§5.1: The Descriptions Theory of Reference: Problems for Kuhn

In the previous section, I demonstrated that Kuhn's key argument depends implicitly upon the descriptions theory of reference. Kuhn claims that:

> The variables and parameters that ... occur in the Ni's ... still represent Einsteinian space, time, and mass. But the physical *referents* of these Einsteinian concepts are by no means identical with those of the Newtonian concepts that bear the same name. (Kuhn 1962: 102, italics mine)

Kuhn goes on to argue that the referents are not identical *because* whilst Newtonian mass is conserved, Einsteinian mass is convertible with energy. At relatively low velocities (that is to say, most velocities that non-scientists deal with) mass could be measured by both an Einsteinian and a Newtonian and we would find the same result. This explains why the Newtonian conception of mass was plausible and useful. But Einsteinian mass and Newtonian mass are not, in fact, *the same*, even at relatively low velocities.

It is certainly true that Einstein's theory of mass applies equally to velocities great and small, and that therefore it isn't quite accurate to say that Newton's Laws are a 'special case' of the laws of relativistic mechanics, where the normal rules don't apply due to the relatively low velocities involved. Rather we ought to say that Newton's Laws are sufficiently accurate at low velocities for them to be of use, but Newton's proposition that mass is conserved becomes more obviously problematic at higher velocities.

The difficulty is that Kuhn won't say that Newton *did not know* that mass is convertible with energy. Rather, he says that Newtonian mass *is* conserved, whilst Einsteinian mass *is* convertible with energy. At first reading, we might think that this

is not an ontological claim but merely a turn of phrase, intended as shorthand for 'according to Newton's theory, mass is conserved' whilst 'according to Einstein's theory, mass is convertible with energy'. This would make sense: the older theory was developed and improved, and rendered more accurate. Unfortunately, whilst Kuhn would acknowledge that Einstein's theory improved upon Newton's (in its puzzle-solving capacity), he also specifically says that the two theories have different *physical referents*. That is to say, because they give contradictory accounts, they cannot be referring to the same physical property. Kuhn's account would require the following relationship between the term 'mass', the respective theories, and the physical referents:

TABLE I

Term	**Description**	**Referent**
'Mass'	(a) Aspects of Newtonian theory pertaining to mass, including the proposition that mass is conserved (b) Aspects of Einsteinian theory pertaining to mass, including the proposition that mass is convertible with energy	(a) The physical property of Newtonian mass (b) The physical property of Einsteinian mass

The conceptual content, or meaning, of the term 'mass' is provided by the theories of mass in Einsteinian and Newtonian dynamics respectively. Those theories provide mutually incompatible descriptions. Kuhn draws the conclusion that the two uses of the term must have different referents, because of his commitment to the missing premise that a description can only denote a property by accurately describing it.

This is known as the 'Descriptions Theory of Reference', and was originally put forward by Bertrand Russell. In short, the Descriptions Theory of Reference claims that a description can denote an object only if the description is accurate of that object. If the description is accurate of one *and only one* object then the description can stand in the place of a proper name. Take the following description: 'The present Queen of

England is a Protestant'. According to the Descriptions Theory of Reference, this statement is logically equivalent to the following:

i) There exists a present Queen of England,
ii) There is at most one present Queen of England,
iii) That person is a Protestant.

Therefore, the statement 'the present Queen of England is a Protestant' fails to denote a particular person if there is no present Queen of England, if there is more than one present Queen of England, or if it is false that the present Queen of England is a Protestant. An inaccurate description cannot successfully refer. Were there no such person as the current Queen of England, the above would be an *empty description.* If there were more than one Queen, the phrase 'the present Queen of England' would be an *indefinite description* as it would pick out more than one individual. As it is, the description picks out one and only one individual, and is therefore a *definite description*, the logical equivalent of the proper name 'Elizabeth II'.

The Descriptions Theory of Reference expressly concerns the names of individual objects, and descriptions which denote individual objects by describing them correctly. Nonetheless, the same basic kind of referring can be extended to the use of descriptions to refer to properties. Take the example above. Note that 'the Queen of England' is an *office.* The property of 'being the Queen of England' has belonged to multiple individuals. Aspects of that property include: being a monarch, a female, the head of the Church of England, and so on. A list of those aspects could be used as a denoting description, which denotes the office of Queen of England: or in other words, the property of being the Queen of England. So too, other properties have aspects which can be listed in the form of a description. In the case of the property of mass, those lists can be found in Newtonian and Einsteinian theories (see the table above).

It is true that the distinction made between 'definite' and 'indefinite' descriptions does not hold in the case of properties. Mass is *a* property with *many* instantiations; so the use of quantifiers is inappropriate here . But the issue of *empty* descriptions does hold. According to the Descriptions Theory of Reference, the Newtonian description of mass can only successfully refer to mass if it provides an accurate description of the

property, mass. However, Newton's description and Einstein's description are mutually inconsistent. The descriptions theory of reference would allow for only two possibilities here. The first is that Einsteinian term 'mass' and Newtonian term 'mass' have different 'physical referents' in 'different worlds'. The second is that at least one of the terms fail to denote anything at all.

The aim of this section is to demonstrate the greater plausibility of the following relationship between the term 'mass', the respective theories, and the physical referents:

TABLE II

Term	**Description**	**Referent**
'Mass'	(a) Aspects of Newtonian theory pertaining to mass, including the proposition that mass is conserved (b) Aspects of Einsteinian theory pertaining to mass, including the proposition that mass is convertible with energy	The property of mass in all physical objects

To achieve this, the section will draw upon Donnellan's article 'Reference and Definite Descriptions'. One point of clarification should be made in advance. I am applying Donnellan's conclusions to the relationship between a referent, a description *and a term*, and therefore the role of *terms* must be made plain. The term 'mass' is, obviously, a noun rather than a description. However, as I mentioned above, our understanding of mass, the property, is derived from the theories of Newton and Einstein. Therefore the conceptual content of the term 'mass' is derived from those theories. Descriptions (a) and (b) in the table above therefore retain a referential role even when they are not literally vocalized by the speaker; because the conceptual content of their speech act remains the same as if they had used the description directly. The mutually inconsistent descriptions can be used to denote the same object; we can infer that mutually inconsistent descriptions can also be used to denote the same property (in this instance, the property of mass) despite the fact that they describe the property differently.

It is important, at this point, to clarify the following. I am *not* claiming that Kuhn adopted, or was even aware of, the finer technicalities of Russell's theory of definite descriptions. Instead, Kuhn simply presupposes the 'descriptions theory of reference', according to which the referent of the description is 'the object uniquely satisfying the conditions in the definite description' (Kripke 1980: 26). The finer details of Russell's particular version of the descriptions theory of reference would not have entered Kuhn's consideration. Nonetheless, we shall examine Russell's account in some depth, as this is our best means of fully understanding Donnellan's reanalysis of definite descriptions. We only need one key feature of Donnellan's reanalysis: his demonstration that definite descriptions need not be attributive. This is sufficient to reveal Kuhn's mistake.

I will begin by briefly presenting Russell's theory of descriptions. I will then draw upon Donnellan's article *Reference and Definite Descriptions*, to demonstrate that the descriptions theory of reference is flawed. It is possible for an inaccurate description to successfully denote an entity (or property); therefore, it is possible for two incompatible descriptions to be used to denote the same entity (or property). One, or both of the descriptions would have to be inaccurate to some extent, but it does not follow that they are being used to refer to different entities (or properties).

Russell divides denoting descriptions into three categories: empty descriptions, indefinite descriptions, and definite descriptions. An empty description is one which describes nothing: Russell's famous example is 'the present king of France' (Russell, 1905). Since France is now a republic, and has no king, the description fails to denote any entity. An indefinite description is one which could apply to more than one entity, for example the phrase 'a man'. Because the description applies to many individuals, it cannot pick out one in particular. A definite description is one which describes one entity and one entity only: for example, 'the present queen of England'. Definite descriptions successfully denote individual entities:

> If 'C' is a denoting phrase [as definite descriptions are by definition], it may happen that there is one entity x (there cannot be more than one) for which the proposition 'x is identical with C' is true...We may then say that the entity x is the denotation of the phrase 'C'. (Donnellan 1966: 281)

According to Russell, then, definite descriptions successfully denote individual entities iff they describe one and only one entity. If they are inaccurate, they fail to denote. If they are insufficiently specific, then they denote multiple individuals rather than one individual in particular. Therefore according to Russell, all definite descriptions are *attributive*: you describe attributes, and the definite description successfully denotes something if there is one (and only one) entity with those attributes. Since a speaker can use a definite description to draw attention to an entity, Russell considers that his account allows 'referring'.[9] However Donnellan, following Strawson, distinguishes between two kinds of definite descriptions; the attributive form known to Russell, and the referential form. In an attributive use of a definite description, the attribute of being the 'so-and-so' is all-important. If nothing fits the description in question, then the sentence fails to refer to anything. However in the use of a referential definite description, the main purpose of the description is to point out a particular; and it can succeed in doing so even if the description is inaccurate. What is more, the same sentence can be used in both ways in different situations. The sentence can remain the same, whilst the statement alters.

Suppose for example, that I ask someone to 'take the book from the table.' It is possible to use the phrase 'take the book from the table' as an attributive definite description. The attribute is 'being on the table'. This might occur if I was asking someone to clear and tidy the table, and was indifferent to which book happened to be on the table. If it turned out that there was no book on the table, then my statement would fail to refer to anything.

The same phrase, 'take the book from the table', can be used in a referential sense. This might occur if I was recommending a particular book for a friend to read, and was under the impression that I had left that book on the table. Now suppose that the book in question had been moved to a nearby shelf. The description I gave would be inaccurate, because the book was not, in fact, on the table. Nonetheless, my statement referred to that particular book, and once I have discovered my mistake, I could point

[9] Donnellan argues that many of the things said by Russell about proper names can be justly transferred to the referential use of definite descriptions (Donnellan, 1966, 282).

out which book I was referring to.

It is important for our purposes to note that whilst Donnellan is here using the example of definite descriptions, his point still applies to descriptions of properties, for which a quantifier is inappropriate. Therefore, it can be applied to Kuhn's argument regarding mass. To look once more at the tables from earlier:

TABLE I

Term	**Description**	**Referent**
'Mass'	(a) Aspects of Newtonian theory pertaining to mass, including the proposition that mass is conserved (b) Aspects of Einsteinian theory pertaining to mass, including the proposition that mass is convertible with energy	(a) The physical property of Newtonian mass (b) The physical property of Einsteinian mass

TABLE II

Term	**Description**	**Referent**
'Mass'	(a) Aspects of Newtonian theory pertaining to mass, including the proposition that mass is conserved (b) Aspects of Einsteinian theory pertaining to mass, including the proposition that mass is convertible with energy	The physical property of *mass* in all physical objects

Table I shows an *attributive* use of descriptions. The descriptions (a) and (b) refer if and only if they accurately describe their referents. The equivalent in the examples above is 'the book on the table' when I want the table cleared. If there is no book on the table, then I have not referred to anything. If there is no physical property which exactly corresponds to the Newtonian (or Einsteinian) theory of mass, then there too, the speaker fails to refer.

Table II shows a *referential* use of *the same descriptions*. The description is used to

refer to a physical property (the referent). It may be a more or less accurate description, but the presence of inaccuracies does not prevent successful reference to the referent. Therefore, more than one description can successfully refer to the same referent. The equivalent in the examples above is 'the book on the table' when I want to lend the book to my friend. If I were later to say 'the book on the shelf' both descriptions would successfully refer to the same item, although one description was more accurate than the other. Of course, in the example of the book, the referent is an object, whereas in this case the referent is a property.

Again, we should note that in these cases, the speaker does not (or need not) literally vocalize the description. She does not, generally, provide a full verbal account of all aspects of Newtonian (or Einsteinian) theory pertaining to mass, including the proposition that mass is conserved, when she seeks to refer to the physical property of mass. Instead she simply uses the term 'mass'. However; as mentioned previously, the conceptual content of the term 'mass' is provided by the theories of mass in Einsteinian and Newtonian dynamics respectively; and therefore the description would retain its role of referring to the property.

Kuhn presumes, as Russell did, that all denoting descriptions are attributive: they can only successfully denote a referent by accurately describing that referent. Russell restricted his focus to descriptions denoting entities, and did not discuss denoting properties. However, there is no reason why the Descriptions Theory of Reference would not apply to cases where the description denotes a property. Therefore the implications of the Descriptions Theory of Reference for Kuhn's account of mass are as follows. If the Newtonian description of mass were an attributive description, then it could not successfully refer to the same thing as the Einsteinian description of mass, because the two descriptions they provide have *incompatible attributes*. According to Newton, mass has the attribute of being conserved, according to Einstein, mass has the attribute of being convertible with energy.

Kuhn is left with three possible options. The first option is that only the Einsteinian description of mass (and hence use of the term) successfully refers to the physical property 'mass'; the Newtonian description fails to refer due to inaccuracy in its list of attributes. But this interpretation cannot explain why the Newtonian theory was ever

useful, which to a historian of science is vital, so Kuhn doesn't take this route. The second option is outright skepticism: neither of them refer. Einstein's theory may well be replaced at some point (perhaps after the development of a Unified Theory) and would then face the same difficulties that Newton's theory faces now, so it is as inaccurate to say that Einstein's description refers as to say that Newton's does. Kuhn's view that scientific progress is merely progress towards better puzzle-solving techniques, rather than some objective truth about the world (Kuhn, 1962, 35-42), hints towards accepting this second option. Yet Kuhn's approach to this issue is both hesitant and inconsistent: he falls short of *openly* embracing anti-realism. We see this most clearly in his ambiguous use of the phrase 'different worlds', in which it is never quite apparent whether his intended meaning is ontological or epistemic[10]. The third option is to claim that both 'Einsteinian mass' and 'Newtonian mass' successfully refer. Clearly this is a desirable option, as it allows us to explain why Newtonian mass was useful, whilst acknowledging the superiority of Einstein's theory. But to take this option whilst accepting the Descriptions Theory of Reference commits Kuhn to something very odd indeed. He is forced to claim that both Einstein and Newton successfully refer, but to different entities, hence his talk of "different worlds".

Donnellan has shown a way out of this apparent paradox. He has demonstrated that not all denoting descriptions are attributive. The function of a referential description is to draw attention to a particular entity (or in this case, property). The two different descriptions can successfully draw attention to the same property, even if the two descriptions are incompatible with each other. If we conceive of scientific theories as referential descriptions, then both Newtonian and Einsteinian descriptions of mass have the same referent. Therefore they are not incommensurable, though they are incompatible. (Incompatible theories can be meaningfully compared with one another; incommensurable theories, by definition, cannot.) If asked to explain the term 'mass', a Newtonian and an Einsteinian would offer different accounts, or *descriptions.* In each case, the conceptual content of the term is different, but the referent of the term is not; because the referent of the description (which makes up the conceptual content of the term) is not. This is why I can coherently say that the Einsteinian *means something different* by the term than a Newtonian, but does not *mean some different*

[10] See Kuhn 1970: 111, 118, 120, 121, 135, 150.

thing. The science developed and so the description changed, but in each case it was a description of the same property. This is, of course, precisely the account of scientific development which most researchers would instinctively defend: that where one theory replaces another, the two theories may be incompatible, but are commensurable; that both theories successfully refer to regular patterns of natural events, but that the better theory provides a more accurate description of those events. By this account, we can take scientists seriously on their own terms.

This section has refuted Kuhn's case for strong incommensurability. However, there is an alternative interpretation of incommensurability presented by Gerald Doppelt. It is necessary, therefore, to address this alternative form of incommensurability it terms of its plausibility and in order to establish whether it is compatible with Linkmaking. This shall be the task of the following section.

§6.1: Doppelt's Challenge: A New Incommensurability?

We began this chapter with the concern that incommensurability was incompatible with Linkmaking. Having given an account of the strong version of incommensurability and established that it would in fact be incompatible with any Linkmaking project; we have gone on to undermine the strong incommensurability thesis, demonstrating that it depends upon an implicit acceptance of the (flawed) descriptions theory of reference. However, the objections we have raised do not apply to every interpretation of Kuhnian incommensurability, but simply to what Doppelt refers to as the 'positivist interpretation' of incommensurability (Doppelt 1978: 36). According to the positivist interpretation, the most basic form of incommensurability is that of scientific meanings. Doppelt convincingly argues that Kuhn's relativist argument is far more plausible if we take it to be based upon the incommensurability of scientific problems, data and standards, rather than upon scientific meanings. In so doing, Doppelt achieves an 'incommensurability' thesis which is not vulnerable to the objections raised above concerning the descriptions theory of reference.

We must firstly, establish whether this account escapes the criticisms levelled at 'strong incommensurability', and secondly, whether or not it is compatible with Linkmaking.

We shall begin by examining Doppelt's account of incommensurability, and contrasting it with the strong version of incommensurability, which Doppelt calls the 'positivist account'. We shall see that a far more plausible version of 'relativism' emerges if we allow that it is primarily grounded in scientific problems, rather than in scientific language. Therefore it is far more plausible to regard Kuhnian relativism as a short-term rather than a long term phenomenon; but we shall question whether this still counts as relativism at all.

We shall then go on to assess whether the surviving 'incommensurability' remains a threat to Linkmaking; examining in turn the issues of realism, founderentism, Longino's cognitive standards, making links between Links, Lockean reasoning, and perspectivist objectivity. We shall see in each case that Doppelt's interpretation is compatible with the commitments of Linkmaking, and indeed that it can provide support to the Linkmaking account.

At various points in *The Structure of Scientific Revolutions*, Kuhn gives several different definitions of incommensurability. Rival paradigms can be incommensurable because:

1. they do not speak the same scientific language,
2. they do not address, acknowledge, or perceive the same observational data,
3. they are not concerned to answer the same questions,
4. they do not construe what counts as an adequate, or even legitimate, explanation in the same way. (Doppelt 1978: 35)

These various definitions are closely interrelated. Nonetheless, there are far-reaching implications which depend on which form of incommensurability is taken to be the most basic to explaining and justifying the others. Doppelt addresses and criticizes the line of interpretation drawn from Scheffler and Shapere, according to which (1) is basic to explaining the others; and therefore:

> ...every paradigm is essentially imprisoned within...its own unique and untranslatable language, or conceptual framework... (Doppelt 1978: 35)

Doppelt dubs Scheffler's and Shapere's line of argument the 'neo-positivist interpretation' of incommensurability. According to this reading, Kuhnian relativism depends upon 'thoroughgoing conceptual relativism and related holistic doctrine of scientific meaning' (Doppelt 1978: 36). Scheffler and Shapere use this interpretation to defend their objections regarding the plausibility of Kuhnian relativism. Their interpretation must conclude that incommensurability between two paradigms consists of an absolute epistemic break between them (Doppelt 1978: 38). This epistemic break would prevent the two paradigms being meaningfully compared. It would also prevent them being rivals in any meaningful sense at all. This would mean that the move from one paradigm to another is never compelled for scientific reasons; and therefore, that Kuhn must explain paradigm shifts in terms of non-scientific or irrational motivations. On this view Kuhnian relativism would be incompatible with scientific development as a rational process.

Doppelt argues that Kuhn's argument for relativism is primarily grounded in the incommensurability of scientific problems. This 'relativism' is compatible with a significant overlap in language, data, and standards between paradigms, and avoids the total epistemological break implied (and criticized) by the positivists. There may even be an overlap of problems, though there would be disagreements as to which problems were the most central. This overlap is sufficient to provide a sense in which rational scientific development occurs. It remains a relativist view, Doppelt insists, because the development in question is not progress towards 'the truth concerning nature' (Doppelt 1978: 39). Rather it is progress towards better ways of solving scientific puzzles. Doppelt bases his argument on the following claim by Kuhn:

> ...paradigms...are directed not only to nature but also back upon the science that produced them...As a result, the reception of a new paradigm often necessitates *a redefinition of the corresponding science. Some old problems may be relegated to another science, or declared entirely 'unscientific'.* Others that were previously non-existent or trivial may...become the very archetypes of significant achievement. And *as the problems change, so often, does the standard that distinguishes a real scientific solution from mere metaphysical speculation, word game, or mathematical play.* (Kuhn 1962: 103, Doppelt's italics)

This extract implies that the root of incommensurability lies in the disagreement between two paradigms regarding what counts as a problem and what counts as a solution. On this view, a paradigm shift does not primarily consist in the introduction of new and novel scientific concepts, but in 'a shift of commitment to a new set of theoretical problems as the 'core' of the discipline' (Doppelt 1978: 41). Therefore, cross-paradigm debates among scientists are at cross-purposes, not because they lack a common language in which to communicate, but because they lack a common concept of the core problems of the discipline, and therefore lack common criteria of adequacy for the solving of those problems[11]. The differences between the paradigms are embedded into their standards of explanatory adequacy; and it is for this reason paradigms are incommensurable with one another. Therefore incommensurability of problems leads to incommensurability of explanatory adequacy.

The incommensurability of problems leads to incommensurability of data. In part, this is because what data is considered to be significant alters with which problems are taken to be central to the discipline. To demonstrate this point, Doppelt draws on Kuhn's example of the transition from the pre-Daltonian to the Daltonian paradigm of chemistry:

> His account of the transition stresses the alleged fact that the pre-Daltonian chemistry of the phlogiston theory and the theory of elective affinity achieved reasonable answers to a whole set of questions effectively abandoned by Dalton's new chemistry. The old chemistry was able to explain the observable qualities of chemical substances...For example it explained the common properties of the metals as due to their possession of phlogiston, lacking in their ores. In effect, the new 'quantitative' chemistry of Lavoisier and Dalton abandoned any concern for these questions and these observational data – whose treatment constituted the main achievement of the earlier model of chemistry. (Doppelt 1978: 43)

[11] This version of 'incommensurability' is the one observable in Peter Galison's account of the Image and Logic groups, which is examined in Chapter Four.

Scientific progress, according to the positivist interpretation, requires that a new theory is superior to the one it replaces if and only if it can handle a greater range of data and problems than the old theory, and if it can explain the data and problems of the old theory in a simpler way; and a more accurate way, one which is most consistent with the observable phenomena. The above example from Kuhn is a challenge to the positivist criteria for scientific progress. Kuhn's account shows that paradigm shifts can (often) involve losses of data as well as gains. This is not because the data can no longer be accessed, but because the shift in problem-set changes what data is seen as relevant to the enquiry. Rival paradigms do have continuity of common observational data but they can nonetheless be incommensurable regarding 'criteria of observational adequacy' (Doppelt 1978: 45).

Doppelt's version of incommensurability also includes an 'incommensurability of scientific languages', but he means by it something much milder than the positivist. According to the positivist account, the primary form of incommensurability is an incommensurability of scientific languages, or 'conceptual schemes'. As we have established, Kuhn's original account of incommensurability presupposes the descriptions theory of reference. Since scientists belonging to different paradigms do not speak the same scientific language, what they mean by a given term is not the same; for example, Einsteinian mass has properties which Newtonian mass does not. Therefore, according to the descriptions theory of reference, the term 'mass' as used by the Newtonian and the term 'mass' as used by the Einsteinian cannot have the same referent. This form of incommensurability requires relativism.

By contrast, consider Doppelt's suggestion that we regard the incommensurability of *scientific problems* to be the most basic form. Incommensurability of explanatory adequacy follows, because there is a new consensus regarding what we ought to be explaining; and incommensurability of data follows, simply because what data is regarded as significant depends upon what problems are being addressed. In addition, there is incommensurability of scientific concepts or languages. But on Doppelt's account, 'incommensurability of scientific language' need not mean that the two paradigms cannot refer to the same referent. Instead, it is 'best understood in terms of the incommensurabilities of standards, problems and data already explicated'

(Doppelt 1978: 45). In order to clarify this claim, Doppelt uses Kuhn's example of Copernicus:

> Copernicus' innovation was not merely to move the earth. Rather *it was a whole new way of regarding the problems of physics and astronomy, one that necessarily changed the meaning of both 'earth' and 'motion'. Without those changes the concept of a moving earth was mad.* (Doppelt 1978: 46, original italics)

Indeed, Copernicus used the term 'earth' to mean something different to his predecessors. However, the issue is not that Copernicus was referring to some different referent, but rather that he was bringing forward a conception of *the problems of physics and astronomy* which was incommensurable with that of his predecessors. Incommensurable scientific languages and concepts may follow, but this is neither so primary nor so total as the positivist interpretation would imply. Doppelt offers a much milder form of incommensurability of concepts than that arrived at via the 'neo-positivist' interpretation, according to which shared observational data between paradigms is logically ruled out. Doppelt acknowledges that Kuhn

> may give the impression that any common observational content between rival paradigms entails the existence of a neutral 'empiricist' language…But a more consistent reading suggests that he rather maintains that the unavailability of such a language makes the task of eliciting the common observational content of rival paradigms more difficult than the positivist program implies. (Doppelt 1978: 47)

In order to clarify this, Doppelt offers Kuhn's example of Galileo's law of pendulum motion. Kuhn argues that perception of the observational data which validates Galileo's law depends upon the acceptance of Galileo's law; but there are two possible ways of interpreting this claim. The first is that it is impossible to perceive the data without an initial commitment to Galileo's law. The second, weaker version of incommensurability of concepts is that the data in question (weight, radius, angular displacement) will not be perceived as interesting or relevant without a commitment

to Galileo's method of analysis. Doppelt's interpretation of Kuhn suggests that the second, weaker version of incommensurability of concepts is more consistent with Kuhn's analysis as a whole and is also far more plausible. However, if Doppelt's version of incommensurability can be said to require relativism at all, it is in a drastically truncated form; both in terms of severity, and in terms of duration.

Doppelt argues that in order to provide a fair evaluation of Kuhnian relativism it is necessary to distinguish between 'long-run' and 'short-run' relativism, and to evaluate them separately. In the process, we shall discover that long-term relativism is implausible whilst short-run relativism is defensible.

Kuhn provides two main arguments in defence of long-run relativism, which Doppelt calls the 'loss-of-data' argument and the 'shift in standards' argument. According to the loss-of-data argument, most paradigm shifts involve losses as well as gains. However, Kuhn acknowledges that some of these losses are temporary. For example, Lavoisier's reformation of chemistry resulted in a change of standards such that, '*during much of the nineteenth century* failure to explain the qualities of compounds was no indictment of a chemical theory' (Kuhn 1970:107, my italics). This is, of course, not a permanent loss. Furthermore, the 'loss of data' argument cannot ever establish the impossibility of science recouping those losses in future paradigms. In order to do this, it would have to examine scientific development as a whole. Yet one of the most illuminating features of Kuhnian relativism is to demonstrate that 'scientific progress as a whole' is merely scientific progress as viewed through current scientific paradigms. Therefore, at best, Kuhn might be able to establish (with a lot of empirical leg-work) that

> contemporary physical theory at this point in its development exhibits losses with respect to the genuine observational data and problems explained by its predecessors. (Doppelt 1978: 61)

However, this is not 'long-run' relativism anymore. It is now merely an interesting short-run (or perhaps medium-run) question about the current status of physical theory. Kuhn's theory, on its own principles, cannot establish the impossibility of cumulative progress in science, towards a better representation of nature. All Kuhn's

theory can successfully do is to disrupt the common assumption that scientific progress is 'unambiguously actualized in contemporary physical theory' (Doppelt 1978: 63).

The second argument which Kuhn advances in defence of long-run relativism is the 'shift-in-standards' argument. This argument maintains

> (1) that any physical theory can only be evaluated relative to its own standards of adequacy, and (2) that in fact successive physical theories in the development of science embody different, indeed *incompatible* standards of scientific valuation. (Doppelt 1978: 63)

The only terms on which a physical theory can be evaluated and judged are the standards which it sets for itself (and all theory). The theory may or may not reach its own standards, and another theory may come closer to reaching them. However, there are no other terms on which the theories can be evaluated, and therefore the result is relativism. The plausibility of this line of argument depends on the following issues:

> (1) Do physical theories in the history of science in fact exhibit 'incompatible' standards of theoretical adequacy?

and

> (2) Is Kuhn's implicit relativistic criterion of scientific knowledge and progress philosophically acceptable? (Doppelt 1978: 64)

Issue (1) would take a vast empirical undertaking to resolve. It is enormously problematic either to demonstrate or to disprove. Kuhn's treatment of scientific development cannot provide sufficient support for the long-run shift-of-standards argument. Again, the shift-in-standards argument is only defensible in its short-term version: for shifts in standards surely do occur. Doppelt maintains, however, that short-term shifts in standards still provide a challenge to positivism, since positivists would want either to deny that shifts in standards occur or to claim that they have been in some sense 'cumulative' (Doppelt 1978: 66).

Issue (2) is equally problematic. As Doppelt argues, Kuhn presents a variety of strong criticisms of the positivist criterion of scientific development. Kuhn considers that the acceptance of his criticisms levelled at positivism lead naturally to the acceptance of his own relativistic view. He does not explore the possibility that the positivist view and his own relativist view do not exhaust the epistemological possibilities available. Doppelt hints at a third, intermediate position which is 'neither positivist nor relativist, neither static and ahistorical nor a throughgoing historicism' (Doppelt 1978: 67). We may say that such a position would closely resemble the requirements of the Linkmaker.

We should note, however, that the form of 'relativism' that Doppelt ends up defending is very much weakened. It is short-run, not long-run. It is compatible with a great deal of common ground and communication between paradigms. It remains relativism, Doppelt insists, because there are never compelling reasons for converting from one paradigm to another. This is because what counts as a 'compelling reason' is relative to the set of standards established within particular paradigms. Kuhn does acknowledge the existence of external standards, but they consist of such things as 'problem solving ability, fruitfulness, accuracy, fidelity to data, simplicity' (Doppelt 1978: 55) and so on. These external criteria are better thought of as 'scientific values' rather than as 'rules of choice' (ibid.) which could reliably lead to a specific rational outcome. In this sense they are rather like Green's 'voices of conscience': it is necessary for the epistemic virtue of the inquirer that they are taken into account, but the 'voices' themselves do not dictate *how* they are taken into account. The values provide sufficient common ground to enable significant communication between paradigms. However, the way in which these values are interpreted depends on which problems define the core of the discipline, 'with respect to which it is most important to be faithful, accurate, fruitful, etc' (Doppelt 1978: 55). Again, the incommensurability of problems is the key issue. But the incommensurability which Doppelt addresses is concerned with ongoing arguments as to the core of a discipline. It does not constitute a challenge to realism. What remains to be shown is whether it constitutes a challenge to Linkmaking. That will be the purpose of the following section.

§7.1: Doppeltian Incommensurability and Linkmaking

It remains for us to establish the following: in what ways, and to what extent, would a Doppeltian version of incommensurability be incompatible with Linkmaking? Let us begin by reiterating what the philosophical commitments of a Linkmaker are. Doppeltian incommensurability would remain a threat to Linkmaking insofar as it does any of the following:

1. Has non-realist commitments
2. Equates a social account of knowledge with non-realism
3. Contradicts founderentism
4. Produces groups in principle incapable of meeting Longino's standards
5. Prevents links being formed between any of the four kinds of Links, e.g.:
6. Severely limits the extent to which scientific theories are based on empirical evidence (Link 1 to Link 2)
7. Does not evaluate social constructions in terms of their capacity to give rise to Links of type 2 which are homomorphic with Links of type 1
8. Severely limits the potential for interdisciplinary work or collaborations between different areas of enquiry (Link 3 to 4)
9. Presupposes a 'God's eye view' account of objectivity
10. Presupposes an individualist account of reasoning
11. Presupposes an individualist account of autonomy

We shall address now address each of these possible in turn. In so doing, we shall establish that Doppelt's interpretation of incommensurability does not pose a threat to Linkmaking.

§7.2: Realism and a Social Account of Knowledge

We have said that Kuhn's relationship with realism is complex, because he both wishes to show that his account is not a threat to the ontological requirements of realism and openly threatens the epistemological requirements of realism in his 'different worlds' thesis. Doppelt's rejection of the 'positivist' interpretation of incommensurability and substitution of an incommensurability which is based on scientific problems and which (crucially) is *short term*, amounts to a rejection of the 'different worlds' thesis. Since Doppelt's version of incommensurability allows for a

significant overlap in language, data, standards, and even problems between alternative paradigms, it is no longer accurate to regard the choice between paradigms as fundamentally irrational. Therefore Doppelt offers us an 'incommensurability' which is compatible with realism, and by extension, he regards a social account of knowledge as compatible with realism.

§7.3: Founderentism

We have established that Linkmakers are committed to founderentism, rather than coherentism of foundationalism. Therefore, were incommensurability committed to foundationalism (it isn't), committed to coherentism, or committed to a disjunction between foundationalism and coherentism which would exclude the possibility of founderentism, then incommensurability would contradict Linkmaking.

We have seen that the Kuhnian account lends itself to coherentism, since justification can occur only within a paradigm. This is for two reasons. Firstly, conversion to a paradigm is itself an irrational act. Secondly, it is not possible to evaluate the paradigm by comparison to empirical data, because the paradigm *forms* the empirical data. Under this view, Haack's two-fold means of investigation as expressed by the crossword puzzle metaphor would collapse back into only one form: coherentism.

Doppelt regards the incommensurability of problems as the most basic form. This allows a far more significant level of cross-paradigm communication than would be possible under strong incommensurability; sufficient for us to regard the conversion from one paradigm to another could be a reasoned and rational choice (though never a *compelling* one). Furthermore, such access to alternative paradigms limits the extent to which one's empirical evidence can be said to be formulated by one's paradigm, since one could access data formulated by an alternative paradigm. Such a version of incommensurability would be compatible with founderentism.

§7.4: Longino's standards

As we have established, Longino requires the following standards of a cognitive group before we ought to regard a consensus in that group as knowledge:

1. There must be publicly recognized forums for the criticism of evidence, of methods, of assumptions and reasoning.
2. There must be an uptake of criticism. The community must not merely tolerate dissent; its beliefs and theories must change over time in response to the critical discourse taking place within it.
3. There must be publicly recognized standards by reference to which theories, hypotheses, and observational practices are evaluated and by appeal to which criticism is made relevant to the goals of the inquiring community…
4. Finally, communities must be characterized by equality of intellectual authority. What consensus exists must be the result not of exercise of political or economic power, or of the exclusion of dissenting perspectives, but as a result of critical dialogue in which all relevant perspectives are represented. This criterion is meant to impose duties of inclusion: it does not require that each individual, no matter what her or his past record or state of training, should be granted equal authority on every matter. (Longino 1994: 145)

We have seen that Kuhnian incommensurability would not be compatible with the creation of 'publicly recognised forums' for the criticism of evidence. The data or evidence available to the scientists depends on the adoption of a particular paradigm, and therefore, the forum for the evaluation of that evidence could not be available to a broader 'public' than the scientists already committed to that paradigm. This further limits the 'uptake of criticism'. Doppeltian incommensurability escapes this problem. Since it allows a significant overlap in language, data, and standards between rival paradigms, it also allows for public forums available to persons outside of the paradigm.

It is similarly difficult for Kuhn's cognitive groups to meet Longino's requirement for 'publicly recognised standards by reference to which theories… are evaluated' with reference to cognitive virtues like 'accuracy' and 'breadth of scope' (Longino 1994: 144) since, according to strong incommensurability, the theory-ladenness of data leaves us with a relativist's 'accuracy'; and one paradigm's 'breadth of scope' can be another paradigm's 'inclusion of irrelevant data'. Since Doppeltian

incommensurability allows for the public forums described above, it offers potential for cross-paradigm debate regarding a desirable breadth of scope. Whilst Doppelt makes no move to deny the theory-ladenness of data, the public forums allow individual scientists to access data laden with several *different* theories about the same area of investigation. This enables them to develop their (perspectivist) objectivity regarding that area of investigation. Thus armed, they can again speak of the *accuracy* of a particular theory.

Longino also requires an equality of intellectual authority, such that dissenting voices are encouraged and consensus is not as the result of non-rational influences. Dissenting voices *within* a paradigm, concerning the evaluation of data, may or may not be encouraged depending on the cognitive culture of the group. But, dissenting voices from outside the paradigm are necessarily excluded by incommensurability; and if conversion to a new paradigm is a non-rational act, then the consensus *that the paradigm works* is necessarily as the result of non-rational influences. Again, the presence of a public forum for debate between paradigms is the key to Doppelt's compatibility with Longino's standards. Unlike strong incommensurability (or 'positivist' incommensurability), Doppeltian incommensurability allows for the inclusion of dissenting voices from outside of the paradigm.

§7.5: Making links between Links

Strong incommensurability, then, would severely limit our capacity to make links between the four different kinds of Links. The theory-ladenness of data limits our capacity to form links of type 2 which are homomorphic with Links of type 1. Also, incommensurability would affect our capacity to evaluate social constructions in terms of their capacity to give rise to Links of type 2 which are homomorphic with Links of type 1, since the Kuhnian view does not equate scientific progress with progress towards the truth about nature, but with progress towards solving scientific puzzles. The most fundamental difficulty however is the lack of links between Links of type 3 and Links of type 4; namely, the lack of communication across paradigms, and the limitations placed on interdisciplinary research. Doppelt's version of incommensurability lacks this fundamental difficulty; and this in itself goes a long way towards addressing the other difficulties.

Doppeltian incommensurability still regards data as theory-laden. However, the fact that scientists can potentially access more than one set of theory-laden data pertaining to the same area of investigation means that they have the potential to *compare* such sets of data. This is compatible with the aim of developing links of type 1 which are homomorphic with Links of type 2, and therefore enables us to evaluate social constructions in terms of their capacity to give rise to links between Links of types 1 and 2. Furthermore, Doppelt's improved communication across paradigms translates to improved communication across disciplinary boundaries, and thus would not give rise to difficulties for interdisciplinary research.

§7.6: 'God's eye view' account of objectivity

We have said that the original Kuhnian account of incommensurability does not fall prey to the 'Gods-eye-view' account of objectivity; instead it is committed to debunking naïve realism, and this conception of objectivity along with it. Yet Kuhn does not offer us any alternative conception of objectivity; and it is not possible for the Linkmaker simply to incorporate a perspectivist account of objectivity, since incommensurability would severely limit the perspectives available. However, Doppeltian incommensurability is compatible with a high degree of communication between paradigms, and thus retains the perspectives of alternative paradigms. It is therefore far easier to combine a perspectivist conception of objectivity with a Doppeltian account than with strong incommensurability.

§7.7: Individualist account of reasoning

Kuhn is by no means guilty of promoting an individualist account of reasoning. His claim that observation and judgment are paradigm-bound, coupled with his insistence that science does progress (albeit towards improved puzzle-solving ability) commits him to a social account of reasoning. However, as we discussed earlier, his account of reasoning could not be social enough for the Linkmaker, since it would be limited to the confines of a paradigm. Again, the fact that Doppeltian incommensurability is compatible with a higher degree of cross-paradigm communication renders it compatible with Linkmaking.

§7.8: Individualist account of autonomy

We saw that whilst an understanding of self-assessment as dependent upon group membership is not incompatible with incommensurability, there are two difficulties Kuhn raises for the Linkmaker on this issue. Firstly, one of the reasons that normation does not amount to indoctrination, on Green's account and on Will's, is that each individual will be a member of several cognitive groups, and will therefore have acquired several sets of norms. This enables them to evaluate which sets of norms are applicable in a given situation. Strong incommensurability would reduce scientist's capacity to do this. However, Doppeltian incommensurability allows individuals to possess multiple sets of norms pertaining to the same scientific investigation; and would therefore retain the capacity to evaluate which set of norms are applicable.

We also expressed the concern that Kuhnian incommensurability would severely limit the role of the conscience of craft as practiced by experts; since the production of new and better practices or standards would seem to be an activity necessarily limited to revolutionary, rather than normal, science. This is, perhaps, symptomatic of too strict a distinction between normal and revolutionary science. However, Doppelt's account allows for continued interaction between paradigms; and would therefore enable experts to evaluate their current practices in relation to those present in alternate paradigms. The conscience of craft as practiced by the expert would therefore remain.

§8: Conclusion

In this chapter, we have seen that the strong version of Kuhnian incommensurability would be incompatible with the Linkmaking project. Whilst Kuhn's thesis offers a social account of knowledge and reasoning, both valuable to the Linkmaker, he is also committed to non-realism and coherentism, and would produce cognitive groups incapable of meeting Longino's standards. Most importantly, the strong version of incommensurability would prevent links being made between the different kinds of Links. We have seen, in some detail, how incommensurability would contradict many of the philosophical commitments of the Linkmaker. Therefore, it has been necessary to demonstrate that the strong version of incommensurability is flawed; whilst, if possible, preserving some of the elements of incommensurability that are more appealing to the Linkmaker. Our assessment of Kuhn's original 'strong' version of incommensurability revealed that the most problematic aspect, his commitment to the

'different worlds' thesis, rested upon an unconscious acceptance of the descriptions theory of reference. We have used the example of Bertrand Russell's account, as assessed by Keith Donnellan, to demonstrate one key issue: that not all denoting descriptions are attributive, and therefore it is possible for two contradictory descriptions to denote the same referent. Therefore, whilst Einsteinian and Newtonian uses of the term 'mass' correspond to different descriptions, they can nonetheless denote the same property. Therefore, the scientists do not practice in 'different worlds' in the ontological sense which Kuhn sometimes suggests that they do.

Having dispelled the stronger version of Kuhnian incommensurability, it remained to address a newer, weaker, more plausible version of incommensurability presented by Doppelt. We saw that incommensurability has several aspects; and it makes a difference which of these aspects we regard as primary. Positivist interpretations of incommensurability (otherwise known as 'strong' incommensurability) regard incommensurability of scientific languages and concepts to be the primary form. The result is 'thoroughgoing conceptual relativism' (Doppelt 1978:36). However, if we instead take the incommensurability of *scientific problems* to be the primary, or most basic, form of incommensurability, then the result is an incommensurability which is compatible with a considerable overlap of scientific languages, concepts, questions, and standards of empirical adequacy. It would even be possible for different paradigms to have an overlap of scientific problems, though they will not agree on the relative importance of those problems. Doppeltian incommensurability, as we have established, is fully compatible with the Linkmaking project, and indeed may even serve to enhance it. Unfortunately, however, it is not the Doppeltian version of incommensurability which has proven the most influential. The following chapter shall show the impact that the *idea* of Kuhnian incommensurability has had upon certain groups particularly vulnerable to its influence; including sociology of science and archaeology. We shall address how that influence may be reversed, and the cognitive optimality of those groups improved, with the application of the Linkmaking project. We shall also see a practical case study in the development of particle physics, which demonstrates the implausibility of the strong version of incommensurability, reveals in practical terms the importance of interdisciplinary practices for the Linkmaking project, and reveals to us more clearly what research communities require for such practices to thrive.

Chapter Four: Interdisciplinarity and Linkmaking

§1: Introduction

The previous chapter has demonstrated that the strong version of Kuhnian incommensurability is flawed. The weaker version presented by Doppelt is insightful and plausible, but it is also fully compatible with realism and with the Linkmaking project. Unfortunately, the 'strong constructivist' version of Kuhn is the one which has had the most influence in fields outside the philosophy of science. It is important to address this, since sociology of knowledge provides a further line of reasoning (or at any rate a methodology) which favours strong, anti-realist incommensurability. According to strong constructivism:

> Not only scientific concepts, but the objects to which they apply – not only the concept of a gene or electron, but actual genes and electrons – are brought into being by scientists' intellectual activities. (Haack 2003: 192)

The strong, relativistic interpretation of Kuhn's arguments has been used to defend this position, which has become extremely influential among sociologists and indeed among the social sciences generally. If, as I have argued, the strong version of Kuhnian incommensurability is fallacious, the extent of its influence is a cause for concern. Sociologists of science who adopt the Strong Programme are liable to underestimate the potential of scientists to provide objective information on an objective world; and their potential to provide meaningful criticisms of one another's scientific approaches. Social scientists influenced by the Strong Programme are liable to underestimate their own potential to provide objectivity and meaningful criticisms, an attitude which can only serve to stunt their progress. In short, it is not merely incommensurability that poses a threat to Linkmaking: *the belief in* incommensurability is sufficient.

The tasks of this chapter, then, are as follows. Firstly we shall show how the 'strong constructivist' version of Kuhn has influenced the Strong Programme in the sociology of science, and offer a brief summation of the goals of the Strong Programme (§2).

We shall then delineate and defend Haack's alternative approach to the sociology of science, wryly named the Sensible Programme, explaining why it works and why it is essential for Linkmaking (§3). This will be followed by a presentation of Robert Pahre's article '*Positivist Discourse and Social Scientific Communities: towards an epistemological sociology of science*'. Despite some confusion within the text, Pahre's article serves to demonstrate that sociologists who study 'border crossing' in the sciences are less likely to adopt the Strong Programme, and more likely to adopt (something akin to) the Sensible Programme. This is because exposure to the transformative processes that occur during such 'border crossings' reveal the transformative potential which exists within the (sub)disciplines themselves (§4).

We shall then back up Pahre's hypothesis with a case study drawn from Peter Galison's work *Image and Logic*(§5). Galison's account of a cognitive divide within microphysics serves to show that, indeed, sociologists who focused purely on the behaviour of either one of these subgroups would be likely to reach strong constructivist (or Strong Programme) conclusions, whilst those who took inter-sub-disciplinary behaviours into account would be far more likely to adopt the Sensible Programme. Furthermore, we shall see that the conflicts facing the Image and Logic groups are recognizably problems of incommensurability, and that 'paradigms' and 'sub-disciplines' are in many respects indistinguishable. However we shall see that the problems faced by the Image and Logic groups are best understood according to the weaker Doppeltian sense incommensurability. This enables us to further develop our understanding of incommensurability and the processes by which it is, routinely, overcome.

Emboldened by this new understanding, we shall then proceed to our second case study, in which we examine developments in archaeological theory (§6). We shall see that as a discipline, archaeology is particularly vulnerable to the Strong Programme in the sociology of knowledge, owing to a long-standing tension between the disciplines links with the physical sciences on the one hand and the humanities on the other. We shall examine Robert W. Preucel's account of the debate between processual and post-processual archaeology (the latter subdivided into hermeneutic and critical theory approaches). Like the Image and Logic groups, the processual/post-processual divide can be understood as a paradigm divide, with each

tradition ascribing different cognitive requirements to archaeological investigation. Unlike the Image and Logic groups, the debate has had no clear resolution. Following Preucel's claim that 'resolution of the competing interests is... the wrong goal' (Preucel 1993: 27), we shall offer a Linkmaker's advice on how archaeological theory can best achieve objectivity.

We shall then briefly examine the extent to which each of the case studies undermines the incommensurability thesis and the Strong Programme, and how they are in accordance with the various requirements of Linkmaking. Finally we shall draw our conclusions (§7): that indeed, the Strong Programme loses plausibility as soon as we start examining boundary-crossing or interdisciplinary behaviour; that such examination reveals that communication across 'sub-disciplines' faces the same problems as communication across 'paradigms' and that both can be successful; and that close examinations of boundary-crossing science reveals a correlation between Linkmaking practices and scientific advancement; whatever form that advancement takes.

§2: The Strong Programme in the Sociology of Science

Sociology of science is concerned with offering a social account of scientific development. Bernard Barber understood the purpose of sociology of science as threefold. Firstly, by examining science as a social activity we can see the connections that it has with other parts of society; for example, political structures (Barber 1952: 26). Secondly, it allows us to compare how scientific structures in our own society differ from scientific structures in other societies. Whilst in modern Western society, science takes place largely in universities, industries, and government research groups; it had a very different 'social locus' in ancient Greek society (Barber 1952: 27). Finally, an examination of science as a social activity allows us to see more clearly the social consequences of scientific research, which are not necessarily positive. For example, scientific developments can lead to a greater level of unemployment, or the development of powerful weapons (Barber 1952: 27). This aspect of sociology of science raises the question: to what extent can scientific development be *planned* such that it can best benefit society? Sociology of science is also concerned with identifying the characteristics of societies in which scientific practices thrive. For example, in his article *The Republic of Science: Its Political and*

Economic Theory, Polanyi argues that 'any attempt to guide science towards a purpose other than its own is an attempt to deflect it from the advancement of science' (Polanyi 1962: 62). Polanyi, then, would be opposed to any attempt to 'plan' scientific development from above. This would affect, for example, how governments ought to fund scientific research.

Early approaches to the sociology of science addressed the impact that science has on society, and the impact that society has on science. However, later sociologists of science have gone further, attempting to explain the scientific process itself in sociological terms. Those who embraced what would become known as the Strong Programme in the sociology of science, including Barry Barnes and David Bloor, advocated regarding sociological factors as a complete explanation for successful and unsuccessful scientific enquiries alike. The Strong Programme drew heavily on the work of Thomas Kuhn; something that is most obvious in Barry Barnes' work, *T.S. Kuhn and Social Science.* Barnes regards Kuhn's *Structure* to be a profoundly useful gift to sociology. Kuhn reveals and avoids the 'Whig historian' habits retained by historians of science, and therefore, in Barnes' view, provides a means to avoid scientism. Indeed, his response to Kuhn's lack of customary respect for the 'rationality' of science (he uses the scare quotes) is positively eager (Barnes 1982:13). Barnes presents sociology of science as exclusively a descriptive, not a normative project (Barnes 1982: 5), concerned with describing how scientists in fact behave, rather than evaluating how they should behave. Barnes reveals that Kuhn's work, used as a descriptive model, has been highly influential in sociology; in particular, 'current sociological work relies heavily on Kuhn's account of normal science, and takes much of it for granted' (Barnes 1982: 51). In contrast, any normative evaluation of science undertaken by philosophers, whether in accordance with or in opposition to Kuhn's account, meets with a deep and abiding cynicism from Barnes.

Barnes seems committed to the view that normative evaluation of scientific practices is not appropriate. Yet surely this is to suggest that there is no difference between cognitively optimal scientific practices and cognitively suboptimal ones. Furthermore, whether Barnes recognizes it or not, it is not possible to provide a descriptive account of the development of science without some reasonably clear normative implications arising. For example, if Kuhn is correct that there is a marked distinction between

normal and revolutionary science, and that scientists spend the majority, if not all, of their lives undertaking the former, then students of science *ought* to be trained up as normal scientists, which requires focusing on different skills than would be the case if the boundaries between normal and revolutionary science were more fluid. We can, therefore, demand a sociology of science which makes normative judgments; and one which will not tip so easily into relativism as the Strong Programme does. The strongest candidate we have for this is Susan Haack's Sensible Programme.

§3.1: The Sensible Programme in the Sociology of Science

In Defending Science – within Reason: between Scientism and Cynicism, Susan Haack expresses concern with the Strong Programme in the sociology of science, arguing that it produces two equally undesirable approaches to analyzing scientific communities. The first, which she labels 'neutralism', refuses to make any epistemological judgements whatever about the scientist's behaviour, instead seeing the role of sociology simply as recording how they do in fact behave and how their beliefs are in fact formed. The result is that 'neutralists' are unwilling to make normative judgments about the scientific communities they examine. This approach can easily slip into what Haack calls 'cynicism', where no distinction is made between what the scientists *take to be* evidence, or knowledge, and what *actually is* evidence or knowledge. As a result, the 'cynics' come to regard all scientific beliefs as equally non-rational.

Haack argues that in order for sociology of science to be useful to epistemologists and to scientists, it must be willing to make normative judgments, and to take scientists seriously on their own terms. In short, it must neither be neutral nor cynical. Haack argues that the flaws of both ideologies can be traced to a common cause: their mutual assumption that the social contrasts with the rational, and that insofar as scientific practices are socially motivated they are not rational. This, Haack insists, is a false contrast, since science is a social institution, and social and evidential elements interlock (Haack 2003: 196). Therefore, she seeks to offer an alternative approach, the Sensible Programme, which can enable sociologists of science to avoid Kuhnian relativism and to make a genuine epistemic contribution to scientific inquiry.

The purpose of this section is to examine the 'neutral' and 'cynical' approaches to sociology of science respectively, and to present Haack's alternative of the Sensible Programme. We shall see that the Sensible Programme is the only one of the three which enables sociologists of science to contribute to the Linkmaking process.

§3.2: Relativism in the Sociology of Science

There are two main differences which render historians less susceptible to the impact of relativism. The first is that historians of science have the benefit of hindsight (Haack 2003: 183). They are aware of which theories survived in the long term and were the most productive. The second is that history of science has an inevitable normative element. Even under Kuhn's account, where theories are supposedly judged solely on their problem-solving abilities rather than their 'accuracy', history of science describes scientific progress of a kind. Inevitably part of the history of science is explaining how that progress occurs. Kuhn is well aware of these factors; and in fact defends himself against the charge of relativism with reference to them (Kuhn 1970: 205). The situation of sociologists of science is quite different to that of historians of science in these respects. Sociologists study current and developing practices. They do not know whether the theories and practices they are observing will survive in the long-term, or how productive they will be. Partly as a result of this, sociologists actively avoid taking any normative stance towards the experiments they are observing. Haack argues that:

> Because from a sociological perspective there is no way to distinguish knowledge, the genuine article, from what is only taken for knowledge, sociologists often write, not of knowledge, evidence, or rationality, but of "knowledge", "evidence", "rationality"; meaning, whatever is *taken to be* known fact, relevant evidence, rational procedure. Shapin is helpfully explicit: in a footnote to a paper presented as launching a radically new social epistemology of science, he writes that he uses "epistemology" in an entirely non-evaluative, purely sociological, sense – not theory of knowledge, but theory of "knowledge" (Haack 2003:180)

If we are being charitable to the sociologist, we might accept that the refusal to make normative evaluations can be simple neutrality, or appropriate caution. We could

argue that it isn't the task of the sociologist to decide what ought to count as evidence, or which of the scientists' procedures are rational. The sociologist may well lack the necessary scientific expertise. In fact, they are not even trying to decide how scientists *ought* to reason, but rather attempting to establish and describe how they in fact *do* reason, and how theory choices are made. In such circumstances, the use of scare quotes may be considered appropriate; if it is not within the sociologists' remit to speak of knowledge, let them speak of 'knowledge'. However, the sociologists' professional detachment from the status of the scientists' beliefs makes them far more easily influenced by strong constructivism and relativism than the historian of science would be. As Susan Haack observes, the sociologists' neutrality can easily slip into cynicism; in which 'these content-stripped, covertly cynical senses of "knowledge," etc., are the only legitimate senses' (Haack 2003: 183). Cynical sociologists of science not only guard their references to truth, rationality and reality with scare quotes, but also denounce philosophical or 'rationalist' approaches to science as 'bankrupt' (Haack 2003: 180).

Neutralism, which arose out of the sociologist's caution, 'flipped' into cynicism due to the influence of relativism; despite the fact that neutralism and cynicism are incompatible with one another. Neutralism requires the sociologist to refrain from normative judgements about scientific beliefs; hence the use of scare quotes around 'knowledge', and 'rationality'. Cynicism, on the other hand, evaluates all scientific beliefs as equally non-rational. Yet in denying that there is any such thing as objectively better or worse evidence, cynical sociology of science surely undermines its own attempts to make objective claims about science; since presumably their own theories would be subject to the same claim. Meanwhile, neutral sociology of science is 'epistemologically inert', unable to contribute anything to the debate regarding how scientific research groups ought to be structured or funded (Haack 2003: 183).

Dissatisfied with both the 'neutral' and the 'cynical' approaches to the sociology of science, Haack proposes a third way, which she wryly labels the 'Sensible Programme'. The aim is to produce a sociology of science capable of identifying and analyzing social elements that can get in the way of good research; developing our understanding of issues such as fraud, plagiarism and the politics of research funding (Haack 2003: 195). If it fulfils this role, sociology of science can be

epistemologically relevant. However, this can only occur in conjunction with an understanding of the epistemology of scientific evidence; only if sociology of science is neither deferential, nor cynical, but *critical* regarding scientific practices.

§3.3: What does the Sensible Programme require?

Neutral sociology of science, by refusing to make any kind of epistemic judgment at all, leaves sociology of science with an artificially restricted role, whilst cynical sociology of science overestimates its own importance, since it regards all scientific behaviour as explicable in sociological terms (Haack 2003: 194). The 'Common-Sensist', as Haack terms them, would accept neither view. Instead they would adopt an intermediate view, regarding sociology of science as a means to reveal how sociological factors help or hinder scientific progress. As an example of this, Haack explores how the 'Common-Sensist' would address the issue of scientific fraud:

> As a rough first stab, he might characterize fraud as a kind of scientific misconduct; but also recognize it as entirely different from, say ...failing to get proper consent from subjects – as specifically intellectual dishonesty. (Haack 2003: 196)

Scientific fraud requires assessing the scientist's *intent*: namely, to knowingly deceive other scientists, and perhaps the general public, into believing claims about her results that she herself knows to be false.[12] Haack shows that the Common-Sensist is at an advantage over the cynic, since in order to make sense of cases of fraud, it is necessary to distinguish between 'truth and fabrication' (Haack 2003: 198). Haack uses the example of Jan Sapp's *Where The Truth Lies*, in which Sapp's examination of a case of scientific fraud perpetuated by Franz Moewus is coloured by Sapp's deeply held conviction that 'all knowledge about nature is "fabricated"' (Supp 1990: 20). Sapp thus fails to distinguish between two profoundly different conceptions of 'fabrication'; on the one hand, the acknowledgement that scientific knowledge involves 'devising and constructing models of the world', and on the other, the claim that 'scientific knowledge is all a sham' (Haack 2003: 199). Only by distinguishing

[12] It is thus distinguishable from the other common intellectual fraud of plagiarism, where the deceit is to falsely claim authorship of another's work.

between these two versions of 'fabrication' can the sociologist of science address certain practical questions about fraud. For example:

> The sensible sociologist of science... might try to test some of Broad and Wade's conjectures: that fraud has become commoner as scientists come under increasing career pressures: that it is commonest in biomedical fields, where precise repeatability is not to be expected; and that it has been made easier by an explosion of publications that has put the refereeing system under severe strain. (Haack 2003: 201)

These questions reveal that sociology of science has far more potential to contribute to epistemology than neutralism would suggest. Potentially, sociological studies of science can be of great assistance to epistemologists and indeed to scientists themselves; they can reveal 'what encourages, and what discourages, good, honest, thorough inquiry' (Haack 2003: 181). A *useful* sociology of science, then, has two major requirements: the first is that the sociologists are willing to look critically at the organization of science, not merely document it. The second is an acceptance that 'good, honest, thorough inquiry' can exist; and therefore that social factors can encourage or discourage such inquiry. It is clear that neither neutral nor cynical sociology can successfully perform this role. Neutral sociologists cannot succeed because they restrict the role of sociology to an artificial degree; refusing to make any normative judgments whatsoever. Cynical sociologists cannot succeed either; because they exaggerate the importance of sociological factors in scientific progress, to the point where they treat scientific inquiry as 'nothing but a form of social negotiation in which scientists bargain their theoretical loyalties for prestige' (Haack 2003: 194). The sensible programme avoids both of these flaws, because it does not *contrast* the rational and the social (Haack 2003: 194).

§3.4: The Sensible Programme and Linkmaking

It is worth taking a few moments here to establish exactly why the Sensible Programme is such a vital tool for the Linkmaker. Firstly, the Sensible Programme clearly provides a social account of scientific knowledge which is compatible with scientific realism, and indeed encourages a realist view. It therefore provides the Linkmaker with another way of addressing Kuhnian incommensurability. Secondly

and crucially, it not only allows for the existence of social groups that meet the standards set by Longino, but also provides a means via which such groups might be cognitively assessed by sociologists of science: something that neither the neutral nor the cynical sociologists of science can offer. Furthermore Linkmaking can, in its turn, provide support for the Sensible Programme. In the next section I will draw on Robert Pahre's article 'Positivist Discourse and Social Scientific Communities', in order to demonstrate that sociologists who focus on interdisciplinary research are more likely to embark on the Sensible Programme in the sociology of science. Linkmakers, then, are more likely to adopt the Sensible Programme; and the acceptance of the Sensible Programme is likely to produce Linkmaking researchers.

§4.1: Parhe on Strong Constructivism and Border Crossing

Robert Parhe, in his article '*Positivist Discourse and Social Scientific Communities*', offers an account of current sociological practice which seeks to explain and decry the pervasiveness of strong constructivism among sociologists of science. Pahre's aim is to persuade sociologists to

> move inside scientist's epistemological beliefs in order to understand on intellectual grounds why they think border-crossing is intellectually justifiable...a stronger statement of my [Pahre's] thesis suggests that often we should agree with the scientist's epistemological beliefs (Parhe 1995: 237)

In order to achieve this aim he presents the two main research methods used by sociologists of science: the study of one subgroup in isolation, and the study of border-crossing. He illuminates why the former method tends to lead them to strong constructivist conclusions, and why the latter method does not. Parhe argues that the study of boundary crossing research reveals to the sociologist the transformative potential that exists within existing structures (in a way unavailable to sociologists who research within one scientific field).

Most sociologists of science examine the development of scientific fields, the continued production of knowledge within them, or their destruction, such as the end of Newtonian physics. The majority of these sociologists, therefore, focus their

research upon a particular subgroup within science, paying close attention to the existing administration, professional associations and professional incentives which influence the process of knowledge production in that field. It becomes clear to them that 'one would not create just this discipline with just these boundaries if one were to consider the problem as a benevolent philosopher-king' (Parhe 1995: 234). This realization has a powerful influence on the sociologist's attitude towards those scientists who defend the boundaries of a discipline against the encroachment of other subgroups, or against non-science. The scientists come to be regarded as 'either myopic prisoners of disciplinary socialization or scientific empire builders beholden to powerful social interests outside the narrow world of science' (Parhe 1995: 234). Parhe argues that those sociologists who focus on the growth of science within one particular field are likely to pay a good deal of attention to non-scientific influences and motivations, and much less attention to the influence of scientific progress in forming the boundaries of the field. As a result, they are more likely to come to strong constructivist or anti-realist conclusions about scientific progress as a whole.

§4.2: The exception - sociologists studying boundary crossing

Whilst the majority of sociology of science addresses itself to disciplines, either the process of their construction or the knowledge (or 'knowledge') produced by them, a minority of sociologists address various kinds of interdisciplinary research. This includes 'boundary crossing, the migration of subfields, the reorganisation of knowledge, or the parital destruction of two fields that later merge and become a new hybrid body of knowledge (Pahre 1995: 233). Sociologists who study the migration of scholars across boundaries are, Parhe argues, less likely to be sceptical about the cognitive effectiveness of the scholars they study, than sociologists who work primarily within a particular discipine or sub-discipline. Pahre argues that existing power structures and disciplinary socialization have less influence on scientists producing work in fields that were not originally their own; and the conclusions of the sociologists change accordingly:

> 'To look at the migration of scholars across boundaries is to see the creative and transformative potential latent in existing structures' (Parhe 1995: 234)

These sociologists will witness the scientist's ability to transfer knowledge and practices between different areas of enquiry. This, as Frederick Will has established, provides an escape from the Relativist Illusion; no longer can these sociologists believe that disciplinary systems of practices adopted by scientists 'determine the main features of a participant's thought and action... completely' (Will 1997: 94). Therefore, Parhe is right to maintain that they are likely to be less sceptical about the possibility of scientific knowledge than their colleagues who focus on one scientific discipline alone.

It is important to be careful here. The only creativity and transformation Parhe describes is that which occurs when two existing structures are brought together. This would suggest that the transformative potential of existing structures remains latent until two or more structures meet. It may be justifiable to interpret Parhe as claiming that migrating scholars have succeeded in breaking free of traditions which previously restricted them, and that therefore to study them is to see the true potential of scientists. Indeed, in arguing that disciplinary socialization has less influence over those scientists who produce in new fields, Parhe invites this interpretation. However such an account is indefensible. It is far from accurate to claim that a scientist who has crossed over boundaries is less influenced by his original tradition than a scientist who stayed within boundaries. Indeed, the influence of former traditions often enable scientists to contribute to innovations in that new field in ways which would not otherwise be possible.

For example, let us reconsider Will's example of the discovery of nuclear fission (Will 1997: 102). Hann and Strassmann could be said to have crossed boundaries between chemistry and physics; but they cannot be said to have 'broken free' of the cognitive requirements of their original tradition. Instead, they adopted an additional set of cogntive requirements, that of nuclear physics. It was their awareness of the clash between these two sets of cognitive requirements which enabled them to contribute towards the discovery of nuclear fission. They did not 'break free' of the tradition of chemistry, nor would it have been cognitively desirable for them to do so.

Parhe is right to isolate the importance of studying research which crosses boundaries, and to show that sociologists are more likely to recognise transformative potential in

such cases. But we must not conclude that transformative potential is in fact restricted to, or even that it is more prevelent in, such cases. It is neither necessary nor helpful to claim that the boundary-sociologists are optimistic simply because they have the privilege of studying the best and the brightest. Rather, the key issue is that examining cross-boundary research reveals to the sociologist in the clearest possible way that there is transformative potential *in existing structures*. Whether Parhe recognises it or not, this requires that existing traditions must carry the seeds of their own transformation.

In order to demonstrate what is meant by 'transformative potential in existing structures', we shall now turn to the first of our two case studies: Peter Galison's historical account of a cognitive divide within microphysics. We shall see that, indeed, it seems plausible that sociologists who paid attention only to the behaviours of one of these two subgroups would be far more likely to adopt the Strong Programme than those who study the interactions between the two subgroups. The latter is far more likely to result in the adoption of something akin to the Sensible Programme. This is because the value of studying interdisciplinary behaviours is greater than merely discovering the effect of those behaviours themselves. It reveals the cognitive processes that occur *throughout* the subdiscipline, and not merely within what Galison would call the 'trading zones' between subdisciplines. It reveals a scientific community of Linkmakers, whose success (or otherwise) is best evaluated by the sociologist who has adopted the Sensible Programme.

§5.1: Image and Logic Physicists

In *Image and Logic: A Material Culture of Microphysics*, Peter Galison gives a historical account of two traditions of instrument-making within particle physics[13], each with conflicting sets of cognitive norms, and shows how the interactions between these two research traditions eventually led to the development of a hybrid Image/Logic machine, a precursor of the particle accelerator. Galison presents two competing traditions of instrument making within microphysics, with different cognitive values, which he labels the Image and Logic traditions. 'Image' machines

[13] Galison uses the term 'microphysics' rather than 'particle physics' in his title, because he considers that the latter term is only appropriate when referring to post war physics, whilst the former is sufficiently general for his purposes (Galison 1997: xviii)

include cloud chambers, nuclear emulsions, and bubble chambers. Each of these machines produce a visual representation of natural processes. As these aim at the preservation of natural structure, Galison calls them 'homomorphic' or 'mimetic'. 'Logic' machines include Gieger-Müller counters, spark chambers, and wire chambers. The aim of these machines is to provide statistical information, gathering data about a larger number of events than the 'Image' machines can. This approach 'consciously sacrifice[s] the detail of the one for the stability of the many' (Galison 1997: 20).

Within each tradition, there are pedagogical, technical, and epistemic continuities (the latter amounting to continuities in the characteristic form of argumentation). In the Image tradition, the search for proof involves a commitment to the production of the 'golden event'; a 'single picture of such clarity and distinctness that it commands acceptance' (Galison 1997: 22). The Logic tradition relies fundamentally on statistical demonstrations, taking the focus away from any one individual event. The techniques of Image experimenters were comparatively passive, which they took to be a virtue guarding them against theoretical presuppositions which would otherwise affect their results. The Logic physicists, by contrast, regarded this passivity as unscientific. They argued that by removing themselves from 'real-time manipulation of the apparatus' (Galison 1997:25), the image physicists were no longer truly experimenting at all. The cognitive ideals of the two traditions could not be more clearly opposed (Galison 1997: 25):

IMAGE	**LOGIC**
Golden Event (focus in detail upon one incident)	Statistical demonstration (less detail, great number of incidents)
Passive registration	Experimental control
Vision	Numbers
Photography	Electronics

Having begun a career in the one of these two traditions, a scientist rarely adopts the opposing approach. But notably for us, we find that in the rare cases where they have

done so, the scientists in question retain the argumentative strategies of their original tradition. Galison offers the example of bubble chamber physicists who, having adopted the use of electronic detectors, still continued to look for 'golden events' (Galison 1997:30). Although initially this may simply seem inappropriate, in fact, as we shall discover, this 'epistemic hybridization' produced some stunning results.

Galison offers a historical tracing of the conflicts between the Image and Logic traditions, and of the first hesitant attempts to bring the two traditions together. This historical process culminates not in the melding of communities per se, but rather in the production of an Image/Logic hybrid machine, namely the Mark I, which satisfies the epistemic requirements of both communities. (The Mark I was a precursor of modern particle accelerators). Galison then relates – or rather, pointedly fails to relate – this process to the Kuhnian incommensurability thesis.

The epistemic commitments of the Image tradition are illustrated by the example of Louis Leprince-Ringuet. An archetypal Image physicist, he worked throughout his life within that tradition and considered that 'one never knew what was going on with counters' (Galison, 1997: 433). Leprince-Ringuet regarded visual evidence as the archetype of proof and the best guard against scepticism. Galison sums up the plausibility of this view as follows:

> Indeed, when it comes time to point to the most secure of evidence, physicists, historians, philosophers and sociologists can rally round the microphysical image of gently arcing tracks to say that this is as persuasive as evidence can get. How can one argue for scepticism against the extraordinary detail of these frozen interactions? As Wittgenstein once said in a different context, "Everything speaks for it and nothing against". (Galison, 1997: 433)

Louis Leprince finds perfect opposition in the Logic physicist Curry Street, who regards images as statistically insignificant, remarking of the golden event that 'anything can happen once' (Galison 1997: 434). Logic experimenters wanted large, statistically significant numbers of events; and they wanted those events 'on demand'. This meant altering the experiment whilst it was in progress. The Logic experimenters had the following epistemic requirements:

> To marshal statistically powerful numbers of events on demand, to alter the apparatus and see the effect vanish – then return the apparatus to its original state and find the phenomenon again. (Galison 1997: 434)

The first of these requirements, the large quality of events, could not be achieved by Image physicists due to physical limitations with the equipment; they could only photograph one event at a time. The second of the requirements, altering the apparatus in the process of experimentation, was *both* impossible for the Image physicists to deliver given the limitations of their equipment *and* contrary to their epistemic commitments. Since the use of images machines required that photographs be taken, processed externally in a photo laboratory, returned by post, and then analyzed by a research team, the results may not have appeared until months after the experiment was completed. In these circumstances, it wasn't appropriate to alter the equipment whilst the experiment was in progress. But further than this, the Image physicists considered their role to be recording events rather than manipulating them. This reflects both their concerns about scepticism, since images are regarded as the most valuable guard against it, and their concerns about theory-ladenness, which the Image physicists were concerned would creep in through manipulation of equipment during the experimental process.

§5.2: Epistemic Disputes and Instrument-Making

For decades, the development of scientific instruments in the Image and Logic traditions remained separate. But in 1944, Erich Bagge began working on parallel-plate spark counters, bringing to the research his prior experience working with cloud chambers. His prior commitments as an Image physicist led him to press the possibility of track localization in spark counters (Galison 1997: 467). In short, he continued to look for Golden Events in the logic machines. Bagge's contributions led to the development of spark counters which could produce images. As this new form of logic machine grew in sophistication, producing more and more pictures, it also grew in popularity, leading to the unexpected situation of logic physicists 'clamouring for better optics' (Galison 1997: 488). The development of these new instruments inspired Samuel J. Lindenbaum, a quintessential logic physicist. In the 1950s, Lindenbaum began to see the need for a 'new departure' in particle physics, which

would overcome what he saw as the weaknesses of both the Image and Logic traditions. He identified the following key shortcomings:

> Spark counters could not detect in all directions at once and could not handle multiple production of particles; visual methods, by contrast, could not handle large numbers of events (Galison 1997: 493).

The ideal solution, according to Lindenbaum, would be to develop a machine which could achieve both of these criteria. In principle, this could be achieved simply by multiplying the number of electronic counters used; but in practice, the resulting hardware would overwhelm any laboratory and the resulting data any physicist (Galison 1997: 493). Lindenbaum's solution was to use on-line computers:

> Because of the almost immediate on-line computer data processing feature, this complicated counter system which would normally be inherently blind is given a remarkable degree of vision. One can now see almost instantaneously the progress of the experiment. (Galison 1997: 493)

By 'vision' Lindenbaum means two things; firstly, that the computers make it possible to turn data from counters into a visual record; but secondly and more subtly, the use of the computer changes the experience of the physicist performing the experiment. As the Image physicists demanded, the computers could produce Golden Events; literal insights into the natural processes; but they escaped the criticism made by Curry Street that 'anything can happen once'; since the images in question were the product of not one event but many.

The process of developing an instrument capable of meeting the cognitive demands of both traditions found its full expression in the 1980s. Three teams joined the collaboration to create a new hybrid machine. One group brought with it expertise on bubble chamber analysis, whilst the other two were drawn from a tradition of electronic experimentation (Galison 1997: 519). The result was the SLAC-LBL Solenoidal Magnetic Detector or the Mark I, which Galison describes as 'arguably the single most important instrument that high energy physics ever produced' (Galison 1997: 517). The Mark I was a beam collider, which used a huge number of detectors

around the site of the collision. Whilst the information was gleaned from spark counters, the design enabled the information from those counters to be used to form an image via a computer screen. Thus the epistemic commitments of both groups were satisfied.

§5.3: Sub-disciplines, Paradigms, and Scientific Development

Galison's account of scientific development is immediately different to Kuhn's in one key respect. Galison does not examine conflicting scientific *theories*. Instead, he focuses on the epistemic commitments which are inherent in differing scientific *practices*. Insodoing, Galison brings the use and development of scientific instruments to the foreground of his historical account. Disputes about the comparative appropriateness or usefulness of alternative scientific instruments are regarded as central to scientific progress, rather than theoretical disputes. Yet, just as with theoretical disputes, we find embedded within these different traditions of instrument making epistemic commitments which we would be hard pressed to avoid calling 'paradigms'. Having put forward Galison's account of this cognitive battle and eventual peace between the Image and Logic traditions in particle physics, we can now ask: has Galison given us an account of a scientific revolution, comparable to those in Kuhn's account? And if so, how does it differ from what the Kuhnian account would lead us to expect from a scientific revolution? To answer this question, let us compare Galison's account with the case put forward by Kuhn as the archetypal example of a scientific revolution, namely, Lavoisier's reform of chemistry. The phlogiston theory was incommensurable with Daltonian chemistry, because phlogiston scientists maintained that *any* chemical theory ought to be able to address questions about the qualities of compounds (Kuhn 1970:107). Lavoisier's reform required setting these questions aside. Kuhn's point was that comparing the two theories was not simply a matter of assessing them in terms of how well they represented the physical world. There was also a value judgment involved, as to whether we should consider the qualities of compounds to be sufficiently central to chemistry that any chemical theory whatever ought to be able to address them.

The Image and Logic groups described in Galison's account are not separated by commitments to incommensurable scientific theories; yet, they face the same conflict. The question of which approach is preferable is not simply a matter of assessing how

well their results represent the physical world. Both groups would accept that there were advantages to their opponent's methods and disadvantages to their own in terms of their ability to 'represent' the world. Therefore, there is a value judgment involved, just as with the Lavoisier case. We must ask; is it of central importance to research in particle physics that our methods produce a clear visual image, a Golden Event? Or, is it centrally important to assess a statistically significant number of events, at the cost of insight into any individual event? This decision, like that of the phlogiston chemists and Lavoisier, cannot be justified in purely empirical terms. It seems that the Image and Logic groups are as much 'incommensurable' as the phlogiston and Lavoisierian chemists are; and that therefore it would be artificial to restrict the concept of incommensurability to theory changes. Instead, we should acknowledge that the same phenomenon is present in inter-sub-disciplinary exchanges such as the ones presented by Galison (and of course, such exchanges are far more commonplace than large-scale revolutions in scientific theory).

It might be objected that in the case of the Image and Logic groups, their differing value judgments end up being of limited importance. After all, it is merely a constraint of the early Image machines and Logic machines respectively that they could not meet the epistemic criteria of both cognitive traditions. The arrival of the Mark I would not necessarily dissolve the cognitive disagreements between the Image and Logic traditions, but it could render those disagreements irrelevant to research decisions, since the Mark I manages to meet both sets of cognitive standards. Surely this isn't a true example of incommensurability? But in response to this objection, we can only say, tu quoque. The phlogiston theory was incommensurable with Lavoisier's reformed chemistry because the latter failed to address questions about the properties of chemical compounds – *temporarily:*

> *During much of the nineteenth century* failure to explain the qualities of compounds was no indictment of a chemical theory. (Kuhn 1970: 107, my italics)

The movement to Lavoisier's chemistry involved neglecting questions that the phlogiston theorists regarded as central; until such a time as Lavoisier's new chemistry become sufficiently well-developed to begin asking such questions again.

This is one reason why a Doppeltian conception of incommensurability as involving 'short term relativism' is much more plausible than strong incommensurability. Similarly, commitment to the Image tradition or the Logic tradition involves neglecting the cognitive requirements of the other group; until the point where the equipment becomes sufficiently well-developed to meet both sets of requirements at once.

Galison's account, like Doppelt's, shows that commitment to incommensurable paradigms is consistent with an extremely high degree of communication; which for Galison includes not merely linguistic terms but 'communication' of techniques and technology across what he calls a 'trading zone' (Galison 1997: 781). From the interactions between the Image and Logic traditions, we can observe that neither tradition entirely dominated the other at any point. Instead, the traditions developed side-by-side, eventually producing technology which satisfied the cognitive requirements of both. Contrary to the account of scientific development we gain from Kuhn, it does not appear to be necessary for one paradigm to 'win' in order for scientific development to thrive. Furthermore, at no point in the development of the Image and Logic traditions do we find a clear melding of the two traditions into some new tradition with a single consensus on the epistemic issues which might be regarded as a new, replacement 'paradigm'. Instead, we find the development of the Mark I renders those disagreements largely irrelevant to the practice of research. These disagreements may therefore fade away, to be replaced with a clearer consensus among researchers in particle physics as to what their epistemic criteria ought to be. But we can note that the scientific development preceded, rather than followed, the creation of any such consensus. A premature focus on arriving at a consensus would, in fact, have stifled the scientific progress that Galison describes. This is not in accord with marked division between 'normal' and 'revolutionary' science involved in accepting the Kuhnian incommensurability thesis (and by implication, in accepting the Strong Programme in the sociology of science).

§5.4: Image, Logic, and the Sensible Programme

We began this section with the claim that Galison's account can be used to demonstrate the plausibility of our argument, drawn from Haack and Parhe, that sociologists examining interdisciplinary behaviours are more likely to adopt the

Sensible Programme in the sociology of science. We are now in a position to substantiate this claim. Let us begin with Parhe's observations regarding sociologists of science who research within a particular discipline or sub-discipline. They pay close attention to the existing administration, professional associations, and any professional incentives within the discipline which would influence knowledge production. Parhe has expressed concern that sociologists studying scientists acting within disciplinary boundaries will become aware that the boundaries defended are never quite those which would have been put in place by a 'benevolent philosopher-king'(Parhe 1995: 234).. Therefore, the fact that the scientists in question *nonetheless defend those boundaries* will raise sceptical concerns that they must be either 'prisoners of disciplinary socialization' or else 'scientific empire-builders' (Parhe 1995: 234).

Let us consider for example how a sociologist of science might interpret the behaviour of, for example, the Image group, if they examined behaviour *within* that group rather than across group boundaries. The Image group defended their sub-discipline against the encroachment of an alternative, even though that alternative met certain cognitive demands that their approach failed to meet. They defended their conception of proof as more important, more basic to research in physics, than that of their opponents, despite the fact that this is a matter of value-judgment rather than a claim defensible through empirical evidence.

This account of the behaviour of the Image group is ripe for interpretation along Strong Programme lines. Moreover, the account is not false. It is simply incomplete. For an objective (dare I say, a Linkmaker's) understanding of the cognitive behaviour of the Image group, it is necessary to place the behaviours described above in the context of the interdisciplinary practices that occur between the Image group and the Logic group.

The Image and Logic groups *did* defend their sub-disciplines against each other, in a sense. The Image group, regarding the Golden Event as the archetype of proof, mistrusted the Logic group's focus on counters; the Logic group in turn mistrusted the Image group's focus on individual events. But the defence did not take the form of erecting boundaries that kept them safe from intruders. Instead it consisted in

articulating what the cognitive demands of the sub-discipline were, why they were considered to be important, and why the competing group was failing to meet them. Thus the 'defence' opened, rather than closed, communication across boundaries. Furthermore, each group *did* regard its own conception of proof as more basic and more important than that of their opponents. Quite possibly, they still do. They did not, however, regard the conception of proof put forward by their opponents as irrelevant to the inquiry. This was why Erich Bagge's development of track localization in spark counters (seeking a Golden Event in an electronic counter) was welcomed rather than dismissed, and why Lindenbaum was ultimately able to join the Image and Logic approaches in order to develop the Mark I.

This account of the behaviour of the Image and Logic groups lends itself naturally to a Sensible Programme interpretation. Recall that Haack defines a useful sociology of science as one which is willing to critically evaluate, rather than merely document, the organization of science, *and* one which acknowledges the possibility of 'good, honest, thorough inquiry' (Haack 2003: 181). The behaviour of the Image and Logic groups encourages sociological questions like: exactly *what* social factors had to be in place in order to enable Erich Bagge to bring the epistemological requirements of the Image tradition into the Logic tradition, and later to enable Lindenbaum to merge the material culture of the two completely? Why did the merging of equipment happen when it did? How did the way in which the research was funded affect the relationship between the two research traditions? What about the peer review process: were there journals which favoured one tradition above another? Could certain scientific developments, *valued by the scientists themselves*, have happened earlier had the sociological conditions been different? These questions, which neither underestimate nor over-blow the importance of sociology to science, are best answered by Haack's Common-Sensist.

§6.1: Archaeological Theorists

In the previous section we saw that the cognitive behaviour of the Image and Logic groups remains ripe for interpretation along Strong Programme lines only if the sociologist focuses on one group or the other. Those who observe boundary crossings between the two groups are far more likely to adopt the Sensible Programme. We also saw that the sub-disciplines of Image and Logic are, in effect, 'paradigms', insofar as

they present incompatible accounts as to what the central questions of microphysics ought to be, and therefore what counts as suitable evidence within an investigation in microphysics. They are recognizable as *Doppeltian* paradigms, but not as the paradigms that one would expect from strong incommensurability. Anticipating the objection that the Image and Logic case would not count as 'real' incommensurability in any case because it was temporary in scope, we countered that the same is true even of Kuhn's archetypal case, that of Lavoisier's revolution in chemistry. Close observation of boundary crossings, then, lends support both to the Sensible Programme and to the mild, Doppeltian conception of incommensurability; all in all, it grants the observer greater confidence in the objectivity of the observed.

We shall bear this discovery in mind as we investigate our second case study; which is based on Preucel's account of developments in archaeological theory. Archaeology, as we shall see, is particularly susceptible to the acceptance of strong Kuhnian incommensurability. This is, in part, due to the fact that archaeological interpretation genuinely is vulnerable to theory-ladenness, arguably to a far greater extent than is the case with the 'hard science' examples originally presented by Kuhn. Their vulnerability also stems from the cognitive split between the processual and post-processual traditions in archaeology. As with the Image and Logic groups, we find incompatible accounts of what the epistemic goals of archaeology are and therefore how they ought to be achieved; unlike the Image and Logic groups, there has thus far been no resolution to the debate. In this section, we shall firstly examine the history of the debate, including an explanation of the cognitive goals of processual and post-processual archaeology respectively. The latter we will examine in its two main forms; the hermeneutic archaeology of Ian Hodder, and the critical theory approach of Mark Leone. As we outline the epistemic goals of each approach, we shall see how the gentle, Doppeltian conception of incommensurability is helpful. The archaeological groups in question have talked at cross-purposes, so passionately and so long, not because they are living in different archaeological worlds, but because they have different conceptions of what the key questions of archaeology are. We shall see that the Linkmaker's account is fully in accordance with Preucel's belief that the best solution for archaeology lies in retaining all of these questions (Preucel 1993: 27).

§6.2: Processual and Post-Processual Archaeological Theory

Archaeologists face unique epistemic challenges. Like anthropologists, their concern is the examination of a culture, but unlike anthropologists, they rarely have direct access to a living cultural group capable of answering their questions. Like historians, archaeologists are interested in reconstructing the past; but unlike historians, they do not (or rarely) have written records to act as testimony about the times they are examining. Like scientists, they rely on experiments, such as carbon dating, to reach (some of) their conclusions; but those conclusions rely on a further layer of historical interpretation, which must itself be justified. It is difficult, then, to say what standards of proof we can reasonably demand of archaeologists.

Archaeologists themselves have long been aware of these issues. In 1913, Roland B. Dixon accused his fellows of showing 'too little indication of a reasoned formulation of definite problems', and of neglecting 'saner and more truly scientific methods' (Dixon 1913: 577, quoted in Wylie 1993: 20). This view met opposition; but notably, this was not because Dixon's opponents viewed archaeology as something in principle divorced from scientific methods. Instead, Dixon met the objection that in order to implement those methods, archaeology required considerably more raw data than was available at that time. Indeed, Berthold Laufer was concerned that a focus on theory rather than data-gathering might hold back archaeological research:

> We should all be more enthusiastic about new facts than about methods; for the constant brooding over the applicability of methods and the questioning of their correctness may lead one into a Hamletic state of mind not wholesome in pushing on active research work. In this sense allow me to conclude with the words of Carlyle: 'Produce! Produce! Were it but the pitifullest infinitesimal fraction of a produce, produce it in God's name! 'Tis the utmost thou hast in thee: out with it then! (Dixon 1913: 577 quoted in Wylie 1993: 20)

Formalized scientific methods in archaeology (and the accompanying high academic status) would, it was thought, become feasible once there was sufficient data available for them to be developed. With these new methods, archaeologists would be better equipped to construct agreed standards of evidence. Solving the epistemological dilemmas of the subject was regarded as only a matter of time. Yet, in fact, there was

a dramatic increase in the availability of data over the course of the twentieth century, and it did not lead to any clear agreement as to what the standards of proof in archaeology ought to be.

On the contrary, the 1960s saw the rise of a 'New Archaeology', now known as processual archaeology, which was concerned with introducing a new scientific rigor to archaeological practices. Adherents to the view, including Lewis Binford, argued that traditional approaches to archaeology produced arguments which 'could only be evaluated on the basis of the authority of their proponents' (Preucel 1991: 19). As Helen Longino would put it, in lacking a firm theoretical framework, the community also lacked publicly accessible standards of proof, which necessarily limited the degree to which meaningful criticism could be achieved (let alone responded to). The solution, processualists argued, was to introduce scientific method to archaeological practices.

Processional archaeology was heavily influenced by positivism, and most especially by the logical positivism of the 1940s and 1950s. Lewis Binford developed and popularized a version of logical positivism within archaeology, with the aim of producing a set of appropriate 'general laws' of cultural functioning (Preucel 1991: 19) which could then be used to justify archaeological claims. In practice, this involved observing the archaeological record, forming hypotheses to explain observed patterns, and 'testing those hypotheses against independent empirical data' (Preucel 1991: 20). The 'independent data' in question was to be ethnographic, not archaeological. Binford acknowledged the difficulty of ascribing meaning to artefacts in the absence of direct empirical observation of the communities concerned, but he believed that this difficulty could be overcome to a great extent by making observations about currently existing communities and how they interact with material culture:

> Fieldwork ... should focus on those contexts in where the production, use, and discard of material culture is empirically observable. *Only by making observations in the present will archaeologists be able to construct a 'Rosetta-stone' with which past material culture can be translated.* Binford's approach has come to be known as 'middle-range theory'. (Preucel 1991: 20, italics mine)

The aim of middle-range theory was to use current-day ethnographic observations to produce a general set of laws which describe how humans produce, use and discard material culture. This general set of laws could then be used to interpret archaeological data. Middle-range theory became extremely productive within archaeology, developing the subfields of 'ethnoarchaeology, modern material culture studies, and experimental archaeology' (Preucel 1991: 20). However, the emergence of post-positivist thought in philosophy was to have an impact upon archaeological theory. Archaeological theorists were influenced by Kuhnian concerns about theory-ladenness of data, and about sociological factors in research, which convinced them that the goals of processual archaeology were unattainable. One of the most notable proponents of this objection to the processual programme was Ian Hodder, to whom we will now turn.

§6.3: Post-Processual Archaeology – Ian Hodder

Hodder presents his case against processual archaeology as follows. The central difficulty in archaeology is generally acknowledged to be uncovering the relationship between behaviour and material culture. Early archaeologists treated this as a one-way causal relationship; behaviour gave rise to material culture. Later archaeologists realized that the relationship between behaviour and material culture is a two-way causal relationship. Behaviour gives rise to material culture, which affects behaviour, which gives rise to new material culture, and so on. Hodder adds to this by saying that 'the relationship between behaviour and material culture [further] depends on the actions of individuals within particular cultural-historical contexts' (Hodder 1986: 13) and that, therefore, there is 'no direct, cross-cultural relationship between behaviour and material culture' (Hodder 1986: 14).

Hodder then argues that the early, flawed view of the relationship between behaviour and material culture is reflected in the processual archaeologists' view of the relationship between fact and theory, which is equally flawed. Early archaeologists considered that the archaeological facts could speak for themselves. 'In general, inference could be seen as following the design: data → theory' (Hodder 1986: 16). Hodder considers that to interpret the causal relationship between data and theory as one-way is as naive as interpreting the relationship between behaviour and material

culture as one-way, and for a similar reason. Data does not straightforwardly produce theory:

> Renfrew (1982) has depicted the relationship between theory and data as data ↔ theory. Fact and theory confront each other but each changes in relation to the other. Binford and Sabloff (1982) have in fact suggested that the relationship between theory and data is so close that the data are observed within theory, and that therefore observational data are really theories (in Binford and Sabloff's terms the observational data are paradigm dependent) . (Hodder 1986: 16)

Hodder argues that it is a simple, unavoidable fact about archaeological research that there is no possibility of testing theory against data. This, he believes, must be faced if archaeology is to thrive as a discipline. Whilst he understands the desire to achieve positivist objectivity, he maintains that this aim must be curtailed in favour developing other aims that archaeologists would have a more realistic hope of achieving. The key point that we must accept, he argues, is that 'the data themselves are part of the same argument as the theories' (Hodder 1984: 28), and therefore, interpretations of archaeological data cannot be tested.

Hodder's key claim, that data is so permeated with theory as to be indistinguishable from it, gains a level of plausibility in archaeology that it may never have had in chemistry or physics. After all, archaeologists ask questions not solely about the material culture they have direct access to, but about the human culture that they do not. The data they seek, therefore, is data about that vanished human culture. It *is* genuinely difficult to distinguish this kind of data from interpretation. But Hodder's conclusion is that there can be 'no testing of theory against data, no independent measuring devices and no secure knowledge of the past' (Hodder 1986:16) is the incommensurability thesis at its strongest, and is incompatible with the Linkmaking project. If his conclusion is correct, it is difficult to see how archaeological research could be objective in any sense.

§6.4.1: Preucel and the Epistemic Goals of Archaeology

What, then, would count as objectivity within archaeology? The Linkmaker will find her best answer from Robert W. Preucel. In his introduction *to Processual and Post-Processual Archaeologies: Multiple Ways of Knowing the Past*, Preucel sets out three supposedly competing schools within archaeology. The first is the positivist approach, which aims to introduce scientific rigor to archaeological research via the use of middle-range theory. In addition to this he sets out two main versions of post-processual archaeology, namely, the hermeneutic approach characterized by Ian Hodder, and the critical theory approach typified by Mark Leone. Preucel's suggestion is that the epistemic goals of all three approaches ought to be retained, as they can be achieved simultaneously. Therefore, he argues, they should not properly be considered rivals at all. Indeed, as we shall see, the achievement of each set of cognitive goals depends upon the achievement of each of the others.

In this section, we shall present the hermeneutic and critical theory approaches to archaeology in turn, establishing the epistemic goals of each. We shall see that these approaches are 'incommensurable' only in the Doppeltian sense that they are concerned with answering different questions. Furthermore, we shall see that each approach to archaeology relies, for the justification of its claims, upon each of the others; a finding which links beautifully to the Linkmaker's perspectivist account of objectivity.

§6.4.2 Hodder's Hermeneutics

As we have seen, Hodder rejects the cognitive goals of processual archaeology as unattainable, since, he believes, middle-range theory relies on an overly simplistic understanding of the relationship between behaviour and material culture. Hodder is therefore concerned with reforming the epistemic goals of archaeology, specifically, with providing 'an adequate answer to the problem of how we infer past cultural meanings' (Hodder 1986: 121). For this purpose, Hodder imports to philosophy an approach more usually applied to the interpretation of texts; namely, hermeneutics.

Hermeneutics originated within theology; in which the primary text to be interpreted was the Bible. Within this context hermeneutics tended to be optimistic about uncovering meaning. Therefore, its practitioners conceived their task simply as a

methodological aid for the study of (primarily) religious texts (Bjørn et al, 2009: 1). However, with the influence of thinkers such as Marx, Freud and Nietzsche, hermeneutics ceased to be regarded as the relatively straightforward task of providing rules for interpretation (Hoy 1978: 2). Instead, it began to deal with the philosophical question of 'explaining the conditions for the very possibility of understanding' (Hoy 1978: 2). Heidegger offers his explanation in the form of the apparently paradoxical 'hermeneutic circle', in which he claims:

> Any interpretation which is to contribute understanding, must already have understood what is to be interpreted... In a scientific proof, we may not presuppose what it is our task to provide grounds for. But if the interpretation must in any case already operate in that which is understood, and if it must draw its nurture from this, how is it to bring any scientific results to maturity without moving in a circle, especially if, moreover, the understanding which is presupposed still operates within our common information about man and the world? Yet according to most elementary forms of logic, this circle is a *circulus vitiosus*. (BT 194; SZ 152, in Hoy 1978:2)

Understanding a text, according to Heidegger, is not a linear process but a circular one. Specifically, we can only understand any part of a text in relation to the whole of that text, and simultaneously, we can only understand the whole of the text by understanding each of its component parts. Heidegger is aware that the hermeneutic circle initially appears to be a vicious one; yet, interestingly for us, he maintains that it is only vicious if one accepts the ideal of objectivity presented by the Cartesian tradition (Hoy 1978: 3). Hodder's rejection of processual archaeology arose from his awareness of the dilemma that faces archaeologists regarding the attainment of such objectivity:

> The dilemma apparent for archaeologists is that there is a widespread desire for science and objective tests, a fear of speculation and the subjective, and yet we want to say something about the past... Yet to say anything about the past, and past ideas, involved moving beyond the data to interpret them, and there can be no testing of these interpretations because the data themselves are part of the same argument as the theories. (Hodder 1984a: 28)

Acutely aware of the limitations of what he saw as the 'positivist testing programme' offered by the processualist approach to archaeology (Preucel 1991: 22), Hodder sought to replace it with a hermeneutic approach to interpretation. Just as the interpreter of a text understands individual parts of that text in relation to the whole, and the whole in relation to the individual parts, the archaeologist interprets the meaning of a particular artefact in relation to its context; and simultaneously comes to understand the context by interpreting particular artefacts. Hodder claims that 'archaeology is defined by its concern with context' and that studying artefacts outside of any context whatever is not archaeology at all, but rather, antiquarianism (Hodder 1986: 123). Only within context is it possible to uncover the meaning of artefacts. Hodder distinguishes between two types of meaning that it is possible to ascribe to artefacts. The first is its function, and the 'functional interrelationships' that it might have with other aspects of material culture (Hodder 1986: 124). When seeking out the first 'meaning' of an artefact, archaeologists might ask questions concerning:

> The human and physical environment, the depositional processes, the organization of labour, the size of the settlement, [and] the exchanges of matter, energy and information (Hodder 1986: 124)

The second meaning of the artefact is 'the structured content of ideas and symbols'. Hodder offers as an example of this the case of discovering fibulas (a type of brooch used to fasten clothing) in the graves of women, and concluding that fibulas were used to symbolize women, whilst swords was used to symbolize men. Questions about symbolism can go deeper than this, asking for example what the use of a fibula to symbolize women says about attitudes to womanhood in the society examined (Hodder 1986: 124). Of course, the two meanings, functional and symbolic, will be interconnected: for example, the symbols used to represent men and women respectively may suggest a certain division of labour between the genders, which might serve to answer questions about the organization of labour in that society. Hodder's concern is that whilst the first kind of 'meaning' of artefacts has been examined by a wide variety of (processual) archaeological approaches including palaeo-economics, optimal foraging theory, and social action theory (Hodder 1986:

125), these same approaches have failed to engage with the symbolic meanings ascribable to those artefacts. Since these two forms of meaning are necessarily connected to each other, Hodder argues that all such theories are severely flawed. Hodder is therefore concerned to develop an archaeological approach which uses contextual relationships to uncover meaning of both kinds. In order to achieve this, we must 'consider the archaeological record as a text to be read' (Hodder 1986: 125). Yet in order for this to be possible, the 'language' of material culture examined must have at least some common features with our own:

> Clearly, if the past material culture language had no common features, words, grammar or structure with our contemporary verbal language, then any such reading would be difficult if not impossible, especially since the surviving text is partial and fragmentary in addition to being simply different. However, I wish to argue that there are some very simple rules underlying all languages – or at least underlying the ways in which homo sapiens at all times and in all places gives meaning to things. (Hodder 1986: 126)

Hodder's suggestion is that there is a 'universal grammar' to material culture which archaeologists can come to comprehend; an insight he ascribes to Collingwood, who argues in *The Idea of History* (1946) that 'each unique event has a significance which can be comprehended by all people at all times' (Hodder 1986: 127). Indeed, Hodder argues that the reason that archaeologists have success in reading material culture is that they assume the existence of such a grammar, even if they do not do so explicitly:

> In beginning to systematize the methodology for interpreting past meaning content from material culture, it seems that archaeologists work by identifying various types of relevant similarities and differences, and that these are built up into various types of contextual associations. Abstractions are then made from contexts and associations and differences in order to arrive at meaning in terms of function and content. (Hodder 1986: 128)

Archaeologists identify the meaning of the artefacts by 'reading' them in context. In the case of a written text, 'context' would mean the passages preceding and following a particular passage (Hodder 1986: 128). In the case of archaeological investigation,

'context' can mean spatial location; temporal location; depositional unit, meaning 'closed layers of soil, pits, graves, ditches, and the like' (Hodder 1986:135) and typology, meaning material culture than has 'similar arrangements or forms in space' (Hodder 1986: 135). The context of an artefact is, in short:

> The totality of the relevant environment, where 'relevant' refers to a significant relationship to the object – that is, a relationship necessary for discerning the object's meaning (Hodder 1986: 143)

Hodder followed Collingwood in the belief that 'the only way the past can be known is through re-living it' (Preucel 1993: 22). Hodder regarded this as an achievable goal since archaeologists can use hermeneutic methods to uncover the meaning of artefacts; by which Hodder means, what the artefacts meant to individuals living in the society studied. In short, Hodder brought to archaeology a *new goal*; that archaeologists, using hermeneutic methods should relive the past, experience the past, and as far as possible become a part of the culture studied.

§6.4.3: Leone's Critical Theory

Hodder's epistemological goal is not the only alternative offered to processual archaeology. Mark Leone promotes a critical theory approach, in which he addresses how an archaeologist's 'received views structure their reconstructions of past cultures' (Preucel 1993: 24). According to this approach, how archaeologists interpret the past depends upon the ideologies of the present. Leone does not expose this in the hope of overcoming it; but rather to enable archaeologists to analyze their own social and political context (Preucel 1993: 25). His goal is the formation of a 'self-conscious archaeology… a study of the history of ideology' (Preucel 1993: 25)

Critical theory, which has its roots in Marx and German idealism, began with the Frankfurt school, the most prominent members of which were Max Horkheimer, Theodor Wiesengrund Adorno and Herbert Marcuse (Guess 1998: 1). Concerned that Western societies were becoming 'closed, totalitarian systems in which all individual autonomy was eliminated', the Frankfurt School aimed to develop a social theory 'guided by an interest in the normative goal of human emancipation'. The school's early writings focused on the social impact of capitalist modes of production, which

they believed could result in a 'tendency towards totalitarianism' Later works, however, focused their criticism on the general acceptance of a purely 'instrumental' conception of reason. Their response to this was to argue that:

> Every society... must be seen as making a tacit claim to substantive (and not merely instrumental) rationality; that is, making the claim that it allows its members to live a good life. (Guess 1998: 1).

Therefore, societies and the institutions within them can be evaluated in terms of their capacity to achieve their own ideals. This formed the basis for the technique of internal or 'immanent' criticism, which works by articulating the 'concept of a given institution in a given society' and comparing this ideal with the institution as it actually is. This technique allows critics to evaluate that institution without necessarily adopting or justifying the institution's standards. It enables them to reveal and critique the ideologies inherent in a society:

> through a dialectical process of analysis and reanalysis. The conceptual foundations of an object are first examined and their implications revealed, and then the object is re-examined in the light of those implications to transcend the object and achieve a new understanding. (Preucel 1991: 24)

Mark Leone saw the potential for adopting this dialectical process in archaeology. He rejected processual archaeology as a result of his concern that archaeologists do not pay sufficient attention to how their received views affect their reconstructions of past cultures For Leone, doing archaeology has to include uncovering the ideologies present within archaeology itself. For example, he aims to expose the ideology inherent in 'the modern tripartite division of time into past, present and future' (Preucel 1993: 24). Eventually this work developed into Leone's thesis that archaeology is 'produced in the present for the present and only acquires meaning when it is given a public performance' (Preucel 1991: 24). Critical archaeology, Leone argues, requires 'the analysis of one's own social and political context through a process of self-reflection' (Preucel 1991: 25). This enables the archaeologist to undertake a history of ideology, understanding both the ideologies of the present and those of the past.

This approach is particularly clear in Leone's article *The Relationship between Artefacts and the Public in Outdoor History Museums*. The article is based on a visit by Leon to the outdoor museum of Shakertown in Kentucky. Visitors purchase guidebooks and explore the museum's buildings and artefacts, asking questions of staff dressed in period clothing. Leone notes that the information available to the visitors focuses on two main areas: the religious life of the Shakers, and their agriculture (Leone 1981: 302):

> One is told that the Shakers lived in three large families, that they were celibate, which children came in only through conversion, that men and women lived separately, were not to touch each other, and worked apart at sex-specific duties... All along one hears of the sobriety, solemnity and deliberateness of their way of life. (Leone 1981: 302)

The visitor learns that the Shakers were the first to package and export garden seeds in packets, shipping half a ton annually (Leone 1981: 302). The museum makes use of each of the houses, outbuildings and workshops to display a particular aspect of Shaker life. Visitors will encounter information on 'sleeping and cooking, worship and furniture, a bit of history, some dates, laundry and seeds, water works and bathing' (Leone 1981: 302). Leone argues that the overall impression one gets is that whilst the Shakers were admirable, their beliefs were 'peculiar'. However, 'this minor conflict is resolved in favour of Shaker efficiency' (Leone 1981: 303). The efficiency of the Shakers is depicted in several ways, the clearest of which is the presentation of the 'Herb House'. This is a small wooden building in which hang a collection of dried herbs. A costumed worker talks the visitors through the collection, identifying their medicinal and other useful properties. This lends heavily to an overall picture of how 'practical, inventive and industrious' the Shakers were (Leone 1981: 303). Leone concludes that the presentation of Shaker life is fragmented to the point that we can no longer gain an impression of Shaker culture as a coherent whole. Furthermore, the aspects of Shaker life focused upon; 'efficiency, rationality, innovation... export, profit, philanthropy and... logic' (Leone 1981: 305) reflect modern American ideology rather than Shaker ideology. Not only will visitors misunderstand Shaker culture, but they shall 'misunderstand it in terms idiosyncratic

to our own' (Leone 1981: 305). Whilst all the information presented at the museum is accurate, the focus on certain aspects of Shaker life to the exclusion of others has the effect of hiding Shaker history, and imposing modern meanings onto Shaker material. Leone argues that 'this dual process is what Marx called ideology' (Leone 1981: 305), and that the imposition of modern American ideology onto Shaker materials creates 'ideotechnic artefacts'; in which the ideology conveyed is that of modern America rather than the Shakers:

> Once outdoor presentations that use archaeological knowledge and collections are seen as ideotechnic, the way to treat them becomes substantially more clear. For instead of being warehouses of artefacts needing further analysis of as neutral masses of potential information, such museum presentations can be seen as fully operating parts of modern American culture... It can be a clue to the ideological part of our own society, in this case our conception and use of the past and its relationship to the present (Leone 1981: 306)

The first task of the responsible archaeologist is to recognize that an ideological interpretation has occurred. Then, through recognizing the ways in which (in this case Shaker) culture has been distorted, it becomes possible to 'achieve some sense of the differences of thought and action between the two societies' (Leone 1981: 312). The archaeologist's investigation will naturally raise questions about 'how our own (and perhaps Shaker) thought came to be constructed as they are' (Leone 1981: 312). For Leone, then, archaeological investigation is as much about coming to understand the ideology of our own culture as it is about revealing the ideology of another. Leone's suggestion is that the museum itself ought to be regarded as an ideotechnic artefact, reflecting the ideology of the archaeologist's own culture. By 'ideology', Leone means 'the givens, the taken-for-granteds, and the obviousnesses of a culture' (Leone 1981: 309). He maintains that once the process of creating this ideotechnic artefact is visible, it is possible for us to control the composition. In addition to presenting Shaker culture in as complete and non-fragmented a manner as possible, the museum should 'illustrate the misinterpretive or masking process, the ideological process' (Leone 1981: 309).

Leone's concern is to 'demystify the way in which various pasts have been constructed' (Preucel 1993: 25). This is important for several reasons. Firstly, it is easy for archaeology to be appropriated for political aims, and a critical theory approach can reveal when this is occurring. For example, 'archaeology was used to serve colonial interests in South Africa by denying the time depth of black history' (Preucel 1993: 25). Secondly, since ideologies existed in the past, critical theories capable of identifying past ideologies and contrasting them with our own offer a valuable insight into past cultures. Finally, critical theory promotes self-consciousness and self-criticism in archaeologists. Leone does not demonize ideology, nor does he think it is possible or necessary to 'overcome' it, but he does argue that if archaeologists become aware that their research and presentation of artefacts can be coloured by (conscious or unconscious) ideological loyalties, then the past culture is less likely to be masked.

§6.5: The Epistemic Goals of Archaeology, and the Sensible Programme

We began this section with an acknowledgement that archaeologists face sceptical questions, as a result of the genuine epistemic limitations of their inquiries. Given that they cannot easily check the accuracy of their interpretations of material culture, what are they doing but 'creating alternative pasts' (Preucel 1993: 17)?

This scepticism comes in two forms. First is the unavoidable fact that archaeologists *always* lack data that it would be beneficial for them to have. They have access to material culture and they wish to find out about the society that produced that material culture: but no members of those societies are present to evaluate the archaeologist's interpretations. So shortcomings in those interpretations are both inevitable and difficult to pick up on. This form of 'scepticism' amounts to a healthy awareness of the limitations of the subject; and it is in fact shared by processual and post-processual archaeologists alike. It amounts to concerns about whether archaeology can, in practice, achieve objectivity given its limitations. The main difference is that processualists are confident that middle-range theory largely solves the problem, whereas post-processualists are not.

The second, harsher form of scepticism questions whether archaeology can achieve objectivity *in principle*; whether interpretation of archaeological data is so dependent on theoretical assumptions that we can say 'no fact has meaning except in the context of a conceptual scheme' (Kluckholn 1940: 47). This is the form of archaeological scepticism which has toppled over into cynicism. We should note that post-processual archaeology provides *both kinds* of scepticism. It is more aware of the limitations of the subject than processual archaeology (and so arguably is better equipped to address them), but it also tends to overestimate those limitations, to the point that they are not so much *addressed* as considered immovable obstacles to achieving objective knowledge about the past.

To consider how archaeological theory can advance from this point, it is useful to consider Haack's Sensible Programme in the sociology of science. Sensible sociology of science had two major requirements: sociologists ought to be willing to look critically at the organization of science, and they ought to accept that 'good, honest, thorough enquiry' can exist (Haack 2003: 180). Similarly, a 'sensible programme' for archaeology would be one which acknowledges the genuine limitations of the evidence gathered whilst recognizing the potential for objectivity within the subject. In the case of sociology of science, Haack says that neither neutral sociologists nor 'New Cynics' can successfully contribute to a Sensible Programme. Instead, a new conception of sociology of science, and an altogether new approach, is needed. We are not exactly in the same situation here. Neither camp in archaeology is unaware of the limitations of the data-gathering process. Rather, the processual archaeologists have offered a possible solution; and the post-processualists are sceptical about the efficacy of that solution. If anything, we might wish to say that the processual archaeologists are best placed to follow a Sensible Programme, since they are neither deferential nor cynical in their approach. But this is to miss a vital point. The post-processual archaeologists are not merely characterized by their scepticism. They have, in reaction to their scepticism, developed entirely new ideas about what the epistemological aims of archaeology ought to be. In Hodder's hermeneutic archaeology, the proper goal of archaeology is to experience the past. In Leone's critical theory approach, the proper goal is ideological self-consciousness. These are both distinct from Binford's 'middle range theory', which aims to develop a 'Rosetta-stone' for interpreting artefacts (Preucel 1991: 20). The challenge currently facing

archaeological theorists is how each of these goals can be accommodated within archaeology. Yet, according to Preucel, this is often seen as a question of how the goals can be in some way unified. He maintains that 'the resolution of these competing interests is... the wrong goal for both the social sciences in general and archaeology in particular' (Preucel 1991: 28). The approaches do not need to be reconciled, because they do not compete. Processual archaeology aims at understanding cause and effect relationships between societies and their material culture; hermeneutics aims at the development of empathy with another culture; whilst critical theory aims at developing archaeologists' critical self-awareness, in order that they might avoid their conclusions being coloured by some ideology. Not only are these goals not mutually exclusive, they may well assist each other:

> There are three mutually reinforcing projects that lie ahead. First, there is the project of explaining the past in terms of patterns and processes. In order to achieve this, archaeology must adopt a subject-object relationship with the archaeological record. This is best accomplished through a form of neo-positivism that makes use of statistical rather than universal laws. Second, there is the project of understanding what happened in terms of meanings and subjective intentions. This involves developing a subject-co-subject relationship between the interpreter and past actors, which only seems to be possible through a hermeneutic exercise. Third, there is the project of interacting with various interest groups to evaluate these explanations and interpretations. To do this, archaeology must adopt a subject-co-subject relationship between practitioners of archaeology and the general public. This is effected by adopting a historically self-conscious, critical framework. Each of these three projects must be pursued simultaneously if archaeology is to continue to grow and develop as a social science. (Preucel, 1991, 28-29)

Not only does the future of archaeology depend on all three of these projects; but each of the individual projects *depends on the other projects*. Without a sufficiently well-developed concept of cause and effect, it would not be possible to empathize with the life-experiences of someone from an ancient culture, because you would have no idea what those life-experiences were like. Without the ability to empathize with the life-experiences of someone from, to pick Leone's example, Shaker society, you would

have no means of assessing whether the archaeological interpretation of that life was accurate or not. Finally, by taking care to ensure that your judgment is not being coloured by some ideology, you are in a better position to assess cause-and-effect relationships. Progress in archaeological research, then, depends upon us retaining all three sets of epistemic goals, and understanding how those goals influence each other.

Preucel's recognition that these goals firstly, do not conflict and secondly, are 'mutually reinforcing projects' (Preucel 1991: 29), offers a prompt for the kinds of inter-sub-disciplinary behaviours that we observe in the Image and Logic groups. It calls upon the divisions within archaeology to articulate exactly what their cognitive requirements are and why they hold them, and to open a debate. This is not to say that the goal of such a debate would be the reconciliation of the groups on the same lines as the Image and Logic groups. It may be entirely appropriate to maintain the different cognitive requirements within archaeology as separate entities. But the debate would be a cognitive good in and of itself. It would enable archaeology to be united as a discipline, without being homogenized, and enable relevant connections to be made: between pieces of material culture, between archaeological ideas, between individuals and research groups and, of course, between sub-disciplines. The debate, in and of itself, would develop the kind of perspectivist objectivity embraced by the Linkmaking project.

§7: Conclusion

In this chapter, we have seen that the Strong Programme in the sociology of knowledge, inspired by the strong, relativistic version of the Kuhnian incommensurability thesis, has held sway among sociologists because they do not focus their attentions on cases of boundary-crossing. Close examinations of boundary-crossing science, such as that between the Image and Logic research traditions in microphysics, are likely to result in sociologists adopting the Sensible Programme, because they reveal cognitive behaviours easily understood by Haack's Common-Sensist.

Furthermore, such examinations also reveal that conflicts between sub-disciplines can be indistinguishable from conflicts between paradigms. Whilst the latter divide is one of theory, rather than method, we nonetheless find the conflicts that one would expect

in a case of incommensurability: research groups have incompatible conceptions of what the central questions of the investigation or the discipline are and therefore, what counts as suitable evidence. This reveals several things. Firstly, if such conflicts as these resulted in the strong form of incommensurability that we discussed in Chapter 3, then the debate between the Image and Logic groups could not have been resolved as it was. Secondly, as inter-sub-disciplinary disagreements of this sort are remarkably commonplace, so too would cases of strong incommensurability be; fortunately, they are not. Finally, we see that, in contrast, the weak Doppeltian form of incommensurability give us insight into the conflict as it did occur; as well as providing a superior explanation even for Kuhn's supposedly archetypal case of Lavoisier's revolution in chemistry.

We have also seen a correlation between and adoption of Linkmaking practices and scientific advancement. Galison's account is very much consistent with the Linkmaker's commitment to a perspectivist account of objectivity. At first glance, we might be tempted to claim that the Logic group, who focus on the production of statistically significant data, have a better claim on achieving perspectivist objectivity than their opponents. After all, they are attempting to understand the phenomenon with reference to as many individual events as possible. In contrast, the Image group focuses on a single, Golden Event. However, the real achievement as far as the Linkmaker is concerned is the ability of both research groups to recognize the cognitive advantages of the alternative approach, to communicate across the cognitive divide not merely in language but in techniques and technology. This was, as Galison shows, made possible largely by a small minority of particle physicists who began in one tradition, but moved into the other. This is as clear a demonstration as we might wish to find that it is impossible to address Links of type 4 (interdisciplinary links) without improving Links of type 3 (social links).

We have seen that the advantage of retaining membership of more than one tradition is that we can evaluate each tradition with reference to the norms and practices of others. In this case, Galison's groups managed to achieve technological advances which effectively rendered the differences in their cognitive norms irrelevant for all practical purposes. As a result, we might say that they end by agreeing on a single set of cognitive norms, or that the cognitive groups have melded. However, we should

note that there is still dissidence between 'Image' and 'Logic' physicists in terms of which form of evidence they consider primary, even if the same equipment is capable of producing both forms of evidence. Therefore, there is not necessarily a 'merging' of epistemic norms, but only of scientific instruments.

Also, even if such a 'merging' was the result in this particular case; it should not be regarded as the goal of all cognitive groups. Instead, the development of perspectivist objectivity simply demands that the epistemic norms of different paradigms, disciplines, or sub-disciplines are articulated, and that cross-paradigm or interdisciplinary communication is encouraged and developed. In short, Linkmaking encourages scientific advancement, but we cannot predict what form that advancement takes. In the case of Kuhn's archetypal scientific revolution, Lavoisierian chemistry, we find that the earlier paradigm (associated with phlogiston theory) demands that any chemical theory must be able to address questions about the qualities of compounds (Kuhn 1970: 107). Kuhn pointed out, correctly, that the adoption of Daltonian chemistry required setting this cognitive requirement aside; so we might be tempted to say that the cognitive demands of the latter paradigm 'won out'. Yet, these questions were only dismissed temporarily, for 'much of the nineteenth century' (Kuhn 1970: 107). It was necessary to pursue a line of research which failed to meet the epistemic requirements of the further paradigm; but those requirements were neither dismissed nor forgotten, and were eventually readopted – when it was possible to meet both sets of requirements. In the case of particle physics, neither set of cognitive requirements were ever even temporarily put aside. Instead the creation of material culture capable of satisfying both sets of epistemic requirements made that disagreement fade away.

In archaeology, it seems likely that pursuing several epistemic goals simultaneously *is* the long-term epistemic solution, and neither setting certain goals aside (as with chemistry) nor merging them into a single set of cognitive requirements with a single solution (as with the particle accelerator) looks likely, given the nature of the archaeologist's materials. None of these results is intrinsically more desirable than any other. But in all of these cases we find that advancement is achieved because the cognitive requirements of the different paradigms or sub-disciplines are retained. To the Linkmaker, the fact that these concerns were retained even when they could not

currently be addressed is a sign of cognitive optimality, of retaining a connection to the previous paradigm which allowed a later generation of scientists to reclaim its strengths.

As Helen Longino rightly tells us, we cannot delineate out of context what the cognitive standards of a particular research group ought to be; but we can articulate context-independent conditions. I echo Longino here with my acknowledgement that the results of interdisciplinary practices vary; they can mean that subfields merge, or that they spilt, or even (as with phlogiston theory) that they die, albeit with a legacy. Any of these results are to be welcomed, if they arise from cognitive communities whose practices encourage Linkmaking.

Thesis Conclusion

I began this thesis with the sceptical concern that accepting a social account of epistemology requires us to relinquish realism; to relinquish our belief that our cognitive traditions can provide us with objective knowledge about an objective world. This fear, I established, is due to the fact that an individualist conception of reason, and a corresponding 'Gods-eye-view' conception of objectivity, has been regarded as key to defending a realist account of the physical world in general, and of the objects of scientific enquiry in particular. The message from the individualists seems to be: if we cannot understand the world as cognitively independent individuals, then we cannot understand it at all; if we cannot view the world without the taint of personal perspective (or 'bias'), then we can make no claim to objectivity.

It is not possible to respond to this challenge with the claim that, after all, we *can* understand the world independent of our social structures, or by claiming that we can overcome our personal 'biases'. All the sociological and pedagogical evidence points in the opposite direction: we are fundamentally dependent upon our social structures to enable us to know anything at all. As we saw at the beginning of the very first chapter of this thesis, this includes not only testimonial knowledge (which is socially dependent for obvious reasons) but also any knowledge gained as a result of socially inculcated norms and practices. Even propositional knowledge, such as 'I am holding a glass of blackcurrant cordial', is sufficiently rich in content that no one individual could possibly verify it. The appropriate response to the individualist, therefore, is to transform their distrust of our cognitive traditions into a set of epistemic requirements which can enable us to evaluate those traditions. Thus armed, we may disregard the general sceptical project of tearing down our cognitive traditions in general, and instead focus on the specific virtues and flaws of specific cognitive traditions: a moderate and constructive task far more appropriate for social epistemology.

I began building this new set of requirements by establishing an alternative to the individualist conceptions of objectivity and of reasoning. We saw that the classic conception of objectivity may be defined as obtaining a 'God's eye-view' of the object of enquiry, which is uncoloured by personal perspective or bias. In saying that

there is *only* perspectival knowing, that there is no alternative, even theoretically, for us to aspire to, Nietzsche is not asking us to succumb to scepticism or to 'give up' on the hope of achieving objectivity. Rather, he is presenting us with an alternative account of what objectivity means. If knowledge is necessarily obtained from a particular perspective, then our attainment of objectivity depends upon the acquisition of as many perspectives as possible. This 'perspectivism', rather than entailing scepticism, can be our means to overcome it.

I then married this conception of objectivity to the unlikely partner of Lockean educational theory. The introduction of Locke, or at any rate the *welcoming* of Locke, into a thesis which embraces a social account of knowledge is likely to be met with surprise. Locke is so far from being regarded as an asset to social epistemology that he has become the whipping boy of the literature, with writers such as Michael Welbourne arguing that Locke 'has no understanding' of 'the dynamics of a community of knowledge' (Welbourne 1981: 303). Yet, as we have seen, the Lockean distinction between received opinions on the one hand and testimonial knowledge on the other is not only compatible with a social account of knowledge, it requires such an account. It also contributes to social epistemology by giving us the (preliminary) means to assess the ability of our cognitive communities to provide us with testimonial knowledge rather than (mere) received opinion.

Locke applies this social epistemic claim in the *Conduct*, with his recommendations that in order to conduct our understandings well, we must actively seek out the testimony of others; and not just other people in general, but specifically the testimony of people who have different 'notions' to ourselves (*Conduct*, § III). The term 'notions' is ambiguous, it is clear that Locke means both 'opinions' and 'areas of expertise'. His example of the country gentleman illustrates that a cognitively isolated individual is liable to reason badly, both because he is less able to access new information, and because his ideas are not critically evaluated by others who have differing opinions. As, later in the thesis, we go on to examine the cognitive interactions between Galison's Image and Logic traditions in physics, or the processual and post-processual traditions in archaeology, we find that the ambiguity of Locke's phrase 'different notions' rings true. The Image and Logic groups have different opinions of what counts as suitable evidence for their enquiry, and they have

different areas of expertise as a result. Their interactions are well described as an exchange of 'notions'.

It is in this context that we can now better understand the purpose of a Lockean curriculum, which seeks to maximise the number of connections between subject areas. We can of course relate Locke's curriculum to his epistemology, in that he is encouraging the formation of 'links between ideas'; links which will improve the learner's insights regarding the relations between objects in the world (Link type 1). But we cannot fully understand it without reference to Locke's *Conduct* and the social recommendations he gives to the adult. The young person, who is accustomed to make links between different areas of enquiry during the course of private study, will naturally continue this habit as an adult. Such an adult is well suited to make the kind of social links that Locke requires: links which grant them access to the 'notions' of others. Access to such notions increases their objectivity by granting them, in Nietzsche' terms, new perspectives on the object of investigation; or in Locke's terms, new 'positions' to it (*Conduct*, §III). So we find that the Nietzschean conception of objectivity and the Lockean conception of reasoning marry surprisingly well, support each other, and grant us an effective alternative to the individualist conception of knowledge; one which, as we saw, does not require us to abandon realism.

This combination of Lockean reasoning and Nietzschean perspectivist objectivity forms the basis on which I developed my own understanding of cognitive optimality, which I named Linkmaking. Just as Locke is (perhaps deliberately) ambiguous in his use of the term 'notions', I have coined a term which could conceivably mean four different cognitive processes. Firstly, we can talk about 'making links' in the ordinary English sense of noticing similarities or causal connections between objects or events in the physical world (Link type 1). Secondly, we can make links in the creative sense of forming what Locke would call 'connections between ideas', as we do when we formulate curricula and scientific theories (Link type 2). Thirdly, we make social connections, as individuals seeking out the company of persons with different notions to ourselves, and as cognitive groups developing relationships with other cognitive groups (Link type 3). Finally, we can make links between disciplines, sub-disciplines, and different areas of enquiry (Link type 4). The most fundamental claims of Linkmaking are as follows: each of these four cognitive processes relates to each of

the others so closely that improving them ought to be regarded as a single, unified project; that our objectivity and reasoning skills are best understood as 'Linkmaking' skills, and that the optimality of our cognitive groups is best assessed in terms of how well that group enables Linkmaking amongst its members.

The epistemological implications of this claim are wide-reaching. In placing Linkmaking in the context of current social epistemology, we discovered firstly that it was incompatible with *both* of the predominant conceptions of justification. Neither foundationalism nor coherentism can support Linkmaking. The only option for the Linkmaker is to embrace Haack's founderentism, which retains the empirical element of foundationalism whilst avoiding the one-directionalism. Importantly, though, the founderentist can also benefit from Linkmaking. The crossword puzzle metaphor has one important shortcoming. When we fill out a crossword puzzle, it is very clear to us which of the answers intersect and which do not. The metaphor would suggest, then, that it is also clear to us as individuals when our beliefs intersect: when they support each other; when they contradict each other; or simply when they relate to each other at all. But this is not the case. Establishing how our beliefs relate to each other is an epistemological project in its own right: the 'light, which the scattered parts of truth will give one another' (*Conduct*, §3) is not in full view. Therefore our justification is not merely a matter of how our beliefs relate to each other, but how aware we are of those relationships. That is what a Linkmaking, or Lockean, conception of reasoning can bring to founderentism.

Linkmaking also has strong implications for what counts as an optimal cognitive group. When in Chapter Two, I examined the cognitive requirements advanced by Helen Longino, we discovered two things of importance to Linkmaking. Firstly, any community which met Longino's standards would also be able to meet Locke's requirements for assessing testimonial knowledge. They would be able to assess the integrity and the skill of the individual witnesses, with reference to the publicly accessible standards which Longino's groups would provide, and they would be well equipped to recognise the 'Design of the Author' (*Essay*, XV, §4) when assessing testimony from books, since if that author failed to address criticisms made by others, their reputation would be damaged. Members of Longino's groups would have access to multiple alternative testimonies, via the 'public forums' she requires. Therefore,

such a group would meet Locke's requirements for testimonial knowledge. But there is a second reason why Longino is important for this thesis; and that is, simply: Longino's cognitively optimal community is a community of Linkmakers. Not only are they staunch realists, whose social structure is set up to maximise their potential to form Links of type 1 and 2, but also, the focus on publicly accessible standards for the evaluation of evidence places them in a very strong position for developing Links of type 4. 'Publicly accessible' would, after all, necessitate availability to members of different cognitive groups, including different disciplines.

In order to defend Linkmaking fully it was necessary to see how well placed Linkmakers are to respond to the classic sceptical concerns which arise for social epistemologists. Chief among these was the following question: once we acknowledge that our cognitive traditions are governed by norms and practices which arise from those traditions, can we defend realism? We turned to Frederick Will's work Pragmatism and Realism in answering this question. His response to the problem was to identify two main Illusions which individualists are afflicted with, and to address each of them in turn. The Coherence Illusion is the belief that cognitive systems act as a barrier between enquirers and the objects of their enquiry. Will's solution to this is to illustrate the extent to which practices arise from the entire existential situation of the enquirer, including the objects in the physical world which are being investigated; a response which is valuable to realist Linkmakers, who need to respond to Kuhnian concerns about theory-ladenness. The Relativist Illusion is the belief that systems of practices are incapable of radical self-criticism. Will's solution is to point out that every individual enquirer has a more than one set of practices, and belongs to more than one tradition. Therefore no one tradition or system of practices could ever have a monopoly on their reasoning processes. There is a twofold implication: firstly, an individual with several sets of practices, *and the ability to recognise significant links between them* (in short, a Linkmaker) will not fall into the Relativist Illusion; and secondly, such an individual would be better able to critically evaluate their own traditions. The two implications mentioned here are, of course, connected, since anyone who regards themselves as incapable of critically evaluating their tradition is putting forward a self-fulfilling prophecy.

Drawing on Thomas Green's account in Voices: Education and the Formation of Conscience, we discovered how the sceptical concerns raised by Will have affected pedagogy. The individualist conception of reason implies that educators can either educate students by inculcating them into the various norms and practices of their cognitive traditions or they can respect the students autonomy; but they cannot do both. Since the epistemological implications of Linkmaking straddle the boundary between philosophy of science and philosophy of education, the very fact that there is such a clear parallel between Will's epistemological concerns and Green's supports Linkmaking. Specifically it supports the Linkmaker's belief that this boundary-crossing between philosophy of science and philosophy of education is something we ought to require of social epistemic theories. But we gained more from Green than this. He shows us that the development of self-assessment (or 'conscience' as he calls it) it dependent upon group membership. Only as members of some tradition, can we evaluate ourselves and others. Green therefore grants us a new understanding of autonomy, one which melds far more effectively with Linkmaking than the individualistic alternative.

Having established the concept of Linkmaking, placed it within the context of current epistemology, and offered some defences against sceptical gainsayers, it was necessary to address the biggest challenge to Linkmaking; namely, the Kuhnian incommensurability thesis. As we saw, the relationship between Linkmaking and incommensurability is a complex one. Strong Kuhnian incommensurability contradicts many of the epistemic commitments of the Linkmaker. Firstly, it denies realism, and furthermore regards any social account of knowledge as necessarily non-realist. It holds a coherentist conception of justification, incompatible with the Linkmakers founderentism. Kuhnian research groups would necessarily fail to meet the standards set by Helen Longino, standards which the Linkmaker has adopted. Kuhnian incommensurability also threatens our potential to make links between the different kinds of Links, largely due to the supposed theory-ladenness of data. However, Kuhn undermines the God's-eye-view conception of objectivity; and the individualist account of reasoning, and (to a limited degree at least) embraces a social conception of autonomy. So it is not in the interests of the Linkmaker simply to reject incommensurability. Instead, we examined Kuhn's classic example of incommensurability in depth, revealing that the supposed incommensurability

between the Newtonian and Einsteinian conceptions of mass presupposed the descriptions theory of reference. Using Donnellan's *Reference and Definite Descriptions*, we demonstrated that the descriptions theory of reference (and therefore the strong version of Kuhnian incommensurability) is flawed. The 'referent' of Einsteinian mass is the same as the referent of Newtonian mass, even if the descriptions provided by each theory are mutually incompatible. Therefore, the scientists are not living in 'different worlds' in any sense but the metaphorical.

The objection offered to incommensurability does not apply to every form, only to the strong form of incommensurability originally presented by Kuhn. So I then presented an alternative conception of incommensurability put forward by Gerald Doppelt. Doppelt begins by presenting what he calls the 'positivist' interpretation of incommensurability, according to which the most basic form of incommensurability is that of scientific meanings. Doppelt posits that instead, it is more charitable and more convincing to regard the most basic form of incommensurability to be that of scientific problems. In short, scientists working in different paradigms are at 'cross-purposes' not because they speak different scientific languages, but because they disagree as to which scientific problems should be regarded as central to the enquiry. This form of incommensurability escapes the objections we found to the strong Kuhnian conception of incommensurability. However, Doppeltian incommensurability is compatible with every single one of the Linkmakers commitments; and furthermore, gives us valuable insight into the difficulties that actually do occur when different paradigms interact. The Image and Logic case study we later examined in Chapter Four is far better understood with reference to Doppelt than strong incommensurability; suggesting that 'different paradigms' are often better identified as different disciplines or sub-disciplines.

Since it is the strong version of incommensurability which has had the greatest influence outside philosophy of science, I then examined how the acceptance of Kuhnian incommensurability has impacted the sociology of science and the social science generally. Strong constructivism, and the Strong Programme in the sociology of science, is committed to the view that sociological factors ought to be regarded as a complete explanation for scientific enquiry. As Susan Haack shows us, this view can lead to two, equally undesirable approaches to analysing scientific communities:

neutralism, and cynicism. The neutral approach regards the role of the sociologist as proving a purely descriptive rather than evaluative account. Therefore, the sociologist refrains from making any normative judgements whatever about the behaviour of the scientists they examine. As a result, such sociologists often use terms like "knowledge", "rationality", and "evidence" in scare quotes, intending to mean "what is taken to be knowledge" or "what is taken to be rationality". The cynical sociologist comes to believe that these scare-quoted versions are the only legitimate senses; that scientists produce not knowledge, but "knowledge". Haack showed us that neither the neutral nor the cynical sociologist is equipped to provide a useful sociology of science, one capable of identifying and analysing social factors that can get in the way of good research. Haack's alternative is the Sensible Programme, in which sociologists are willing to critically assess the organisation of science rather than merely documenting it. In order to do so, they must accept that 'good, honest, thorough enquiry' (Haack 2003: 181) can exist. Haack's Sensible Programme is important to Linkmakers for two reasons. Firstly, by providing us with a social account of scientific knowledge that is compatible with scientific realism, Haack supports the Linkmaker's goal of addressing the strong form of Kuhnian incommensurability. Secondly, it not only allows for the existence of scientific groups capable of meeting Longino's standards, but offers the possibility of such groups being cognitively assessed by sociologists and thereby improved.

Not only is the Sensible Programme valuable to Linkmaking; Linkmaking is invaluable to the Sensible Programme. As we learnt from Robert Parhe, those sociologists who study boundary-crossing are far less likely to reach strong-constructivist conclusions then their counterparts who study one research group in isolation. To focus on one sub-group, paying close attention to the administration, professional associations, and professional incentives which influence the production of knowledge in that field, is to see clearly that 'one would not create just this discipline with just these boundaries if one were to consider the problem as a benevolent philosopher-king' (Parhe 1995: 234). Therefore, such focus is likely to lead to constructivist conclusions. Parhe contrasts this with sociologists who study scholars migrating across disciplinary boundaries. These sociologists witness the ability of scientists to transfer knowledge and practices between different areas of enquiry. Therefore, they are less likely to embrace constructivism and the Strong

Programme. It is important to note, however, that study of interdisciplinary behaviours reveals the cognitive processes that occur *throughout* disciplines, and not merely at boundary-crossings; although, such boundary-crossings are vital for the cognitive optimality of each discipline as a whole.

Parhe's account gains some valuable empirical backing from Peter Galison's *Image and Logic, a Material Culture of Microphysics*. Galison depicted the interactions between two research traditions with conflicting sets of cognitive norms. Image physicists depended on visual information, gained via cloud chambers, nuclear emulsions, and bubble chambers; whilst Logic physicists depended upon Gieger-Müller counters, spark chambers, and wire chambers. Correspondingly, their cognitive requirements were different. Image physicists aimed to focus in detail on a smaller selection of incidents, seeking the 'Golden Event' which would give them literal 'insight'. The scale, both physical and temporal, of their equipment and processes demanded that the Image physicists were relatively passive during their experiments; which they took to be a virtue guarding them against theoretical presuppositions, and the Logic physicists regarded as unscientific. The Logic physicists sought statistical demonstration, at the expense of detailed understanding of any particular event; and they required a high level of experimental control. The gradual epistemic hybridisation of the two groups is interesting to the Linkmaker for several reasons. Firstly, it demonstrates Frederick Will's claim that practices are formed in response to the entire existential situation as a whole, including the object of investigation. Only if this is the case could it make sense for such varying epistemic traditions to cooperate as effectively as the Image and Logic groups have: they were studying the same physical events, and this was sufficient to enable them to cooperate. Secondly, it is quite clear the the Image and Logic groups are dealing with an incommensurabilty problem. It is also quite clear that this incommensurability is of the kind presented by Doppelt: the groups have (slightly) different conceptions of the scientific problem and therefore different conceptions of the appropriate standards of evidence. It is *not* an example of strong incommensurability; and if strong incommensurability existed, we should surely see it here. Finally, we can use it to support Parhe's claim that sociologists studying boundary-crossing are more likely to adopt the Sensible Programme. If a sociologist were to study the behaviour of either of these groups without referencing their interactions, they would be likely to

conclude that each group defended itself against the encrouchment of an alternative approach and regarded their own cognitive norms as superior, despite the fact that they could not offer empirical evidence to support this claim. This is likely to lead to the adoption of the Strong Programme. Alternatively, they could see that the 'defence' was a public forum of discussion between the two traditions, that each side borrowed ideas and techniques from the other. This is likely to lead to the adoption of the Sensible Programme.

Having established how the acceptence of strong Kuhnian incommensurabilty has affected sociology of science, and how the study of interdisciplinary practices can draw sociologists of science towards the Sensible Programme, it remained for me to address the impact that Kuhnian relativism (or to be precise, the *acceptance* of Kuhnian relativism) has had on the social sciences more generally. The case study chosen is archaeological theory. Archaeology faces some very genuine sceptical issues. Archeologists *always* lack data that it would be beneficial for them to have, and it is especially difficult in archaeology to distinguish data from interpretation, to a far greater extent than we find in the natural sciences. It is therefore to be expected that there is a strong disagreement within archaeology as to what cognitive standards ought to require. Processual archeologists, such as Lewis Binford, maintain that middle-range theory grants an objective means to interpret material culture. Post-processualists such as Ian Hodder, influenced by Kuhnian concerns about theory-ladenness, were convinced that the goals of processualist archaeology were unattainable. Some went so far as to claim that archaeological data is sufficiently dependend upon theoretical assumptions that 'no fact has meaning except in the context of a conceptual scheme' (Kluckholn 1940: 47).

However, post-processual archaeologists cannot merely be characterised by this scepticism. In response to their scepticism about the epistemic goals of processual archaeology, post-processualists have arrived at alternative epistemic requirements for archaeology. Hodder's hermeneutic archaeology seeks to experience the past. Leone's critical theory approach aims at ideological self-consciousness. And meanwhile processualists such as Binford aim to understand the cause and effect relationships between societies and material culture. The great challenge for archaeological theory is supposed to be how we can reconcile these approaches. Yet, as Preucel says, there

is no need to reconcile them because they do not compete. Indeed, they assist each other, they are essential for each other. Progress in archaeological research is best achieved by retaining all three sets of goals, and understanding the relevance of each project for the others. We can see, then, that the Linkmaker's advice is to call on the divisions of archaeology to articulate their cognitive requirements and explain why they hold them, opening a debate such as that we can observe in Galison's Image and Logic groups. That is not to say that we can expect, or desire, the same melding of approach that we found with Galison's case study. Rather, the debate is, in and of itself, an epistemic good; because it would improve the Linkmaking abilities of the archeologists concerned. They would be better placed to create links: between pieces of material culture, between archaeological ideas, between research groups, and between sub-disciplines. In the process, we should not be surprised if the deeper sceptical questions of the post-processualists are answered.

In many of the examples I have drawn upon in the development of this thesis, I have found that the accounts of the philosophers in question contributed towards Linkmaking, and that in turn, Linkmaking could be used to support or develop their accounts. For a time, I was concerned that this proved only that these accounts were mutually compatible with my own; itself interesting, but surely not sufficient to defend Linkmaking. However, with the case studies drawn from the cognitive communities within physics and archaeology, I believe that I have added another element to my justification. When the Image and Logic groups approached the forms of cognitive behaviour endorsed by Linkmaking, they achieved significant advances as a result. By including this empirical evidence in favour of Linkmaking, in addition to defending the internal coherence of my thesis (and its coherence with certain other accounts), I hope that I have met the founderentist requirements for justification which I have accepted from Haack. I also hope that I have made clear the connections between that empirical evidence (Galison); the theories which contradict, or cohere with, that evidence (Kuhn, Doppelt); the social means by which we can make that contradiction or coherence clear (Locke, Longino, Will, Green) and the extent to which making all these connections in the first place depends upon recognising the importance of interdisciplinary practices (Locke and Parhe). In this thesis, I have defended Linkmaking. It is my sincere hope that I have also practiced it.

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