SITUATING OBJECTIVITY: A FEMINIST CONCEPTUALIZATION

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Abstract

This dissertation focuses on the ideal of objectivity in science. My aim is to understand and situate how objectivity has been conceptualized in the philosophy of science, and to question whether these conceptualizations are consistent with the actual ways in which objectivity has been sought in scientific practice. I examine the dominant views of objectivity in mainstream philosophy of science and feminist reactions to them. Ultimately, I argue that Helen Longino's understanding of objectivity, complemented by some aspects of Sandra Harding's "strong objectivity", provides a more comprehensive and practical ideal to guide scientific practice than the received view's conception where objectivity is sought by adopting an impersonal methodology.

One of the main criticisms against feminist epistemologies, which argue for the gender specificity of knowledge, is that they lead to epistemic relativism. And hence it is argued that feminist epistemologies undermine "scientific objectivity". In arguing for the fruitfulness and consistency of a feminist account of objectivity, I examine in what ways claims about the gender specificity of knowledge could be understood without rendering the notion of objectivity redundant.

Dedication

For my parents,
İsmail & Gül Erciyes

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Introduction

In 1995 Karen Keegan, mother of three, was told that she needed an immediate kidney transplant. (RadioLab, 2008) In the search for a suitable donor, her husband and her two sons underwent DNA testing. The results were shocking: her two sons' DNA did not match hers. From a medical point of view this meant that she was not their mother. She insisted that the test be repeated since they were "obviously" wrong. *She* had given birth to her babies and had experienced the pain of delivery. However, the new tests confirmed the previous results. Clearly there had been no laboratory mistake. This was quite a puzzling situation, for although a mismatch between a father's DNA and a child's DNA is not unheard of, there had never been a case where a mother's DNA did not match her children's. Doctors initially thought that there was some sort of a mix up, such as switching of babies at the hospital, but then dismissed this possibility because the father's DNA matched his sons'. Since for medical doctors "DNA is never wrong" they suspected that Karen was hiding something. Some of the doctors even went as far as to suggest that she had implanted her womb with another woman's baby and lied about it. After investigating hospital records and so forth, the doctors decided to test some other tissues in her body, such as her thyroid, bladder and skin. The results of these tests were perplexing for they identified two sets of DNA, and two sets of DNA meant the existence of two different people. In other words, the tests indicated that there was another person

inside Karen. Furthermore, this other person was the mother of her two boys. Eventually, the doctors concluded that Karen *is* a twin.[1]

This remarkable story provides us philosophers with a broad range of material for thinking about science, how it works and how it affects our daily lives. This story can be read as an exemplary case of science as a puzzle-solving activity at work. It can also be read as supporting the idea that focusing on rare events in nature helps in revealing the structure of the world. Yet another way of approaching this story is to take it as a story about the authority of science, the ways in which scientific authority is at play in real life situations, and how its impacts can be devastating and unjustified. Perhaps Karen's case is not strong enough to make this last point. After all, although she had gone through stressful times, in the end science succeeded in giving them a reasonable explanation of the whole situation. This is not always so.

The case of Lydia Fairchild is illuminating in a different and more troubling way. It begins when Fairchild, a single mother, applied for welfare support for her children. She and her family were tested in order to prove that they were all related. (ABC News, 2006) Fairchild received a similar call from the authorities as did Karen, but this time from The Department of Social Services. The Department told her that she was not the

[1] This strange case is an outcome of a very rare event in nature. The scientific explanation is as follows: in Karen's mother's womb initially there were two fertilized eggs (twin girls) developing in their separate sacs. Then an anomaly occurred and somehow two embryos bumped into each other, merged and formed Karen. However, the embryos did not blend, they claimed different tissues from Karen. One of the twin's DNA formed her blood and the other her thyroids, bladder and so on. The scientific term for this rare situation is "chimerism". In Greek mythology "chimera" means a monster: part goat, part lion and part snake.

mother of her three children. Like Karen, Fairchild *knew* that *she* had carried the babies and delivered them at birth. Yet, she was being treated as a criminal suspect. When she resisted, she was told by a social worker that "DNA is 100 percent foolproof and it doesn't lie." Now Fairchild was not only denied public assistance but she was also "suspected of possibly acting as a paid surrogate mother and committing welfare fraud." State authorities told Fairchild that the children could be taken away from her permanently. In her defense, Fairchild had to fight the charges in court "to prove the children born from her own body were her own." (ABC News, 2006) But because DNA tests were considered a gold standard in court, and infallible, the attorneys Fairchild approached did not want to fight DNA evidence.

Fairchild's story provides a compelling illustration of the extensive authority "scientific evidence" claims. This case also shows us how dogmatically clinging to this authority could lead to unreasonable and unjustified practices. Faith in DNA testing was so firm and deemed so credible that even when Fairchild gave birth to another baby under the surveillance of a court officer (who immediately collected DNA samples from the newborn), authorities still did not believe her. These tests also revealed that there was a mismatch between the mother Fairchild and her newborn. "Even though they'd witnessed the birth, officials believed she was acting as a surrogate, possibly bearing a child for money." (ABC News, 2006) Eventually, the story of Karen Keegan provided the key to solving the mystery in Fairchild's case. Further tests proved that Fairchild was also her own twin, leading to the dismissal of her case. Yet the despair, fear and panic she

suffered through during her struggle to prove that she was not a fraud reveals to us how maintaining an unwavering faith in science can become unruly and irresponsible.

Perhaps these contemporary individual cases are not of the same scale of harm as those produced by the unquestioned authority of science in the past. Some systemic cases that come to mind include "establishing" the inferiority of certain groups of people to the white population in order to "justify" their inhumane treatment. Nonetheless, these contemporary cases are good indicators that dogmatically clinging to "established scientific facts" still persists. These cases also suggest that similar historical cases, often thought to be merely examples of bad science, cannot be lightly dismissed as representative of an immature stage in the development of science.

The existence and persistence of such injustice urges us to question the (authoritative) grounds on which science continues to rest.[2] There is no straightforward answer to this question. Constructing an answer demands both an analysis of the characteristics of science, which have shifted and evolved throughout history, and an examination of the societal features within which Western science has emerged. Therein, one of the key features of Western science, buttressing its privileged and protected status, is its acclaimed adherence to "objectivity".

This dissertation focuses on the ideal of objectivity in science.[3] My aim is to understand and situate how objectivity has been conceptualized in the philosophy of

[2] Admittedly, not all scientific disciplines have the same extent of authority. The authoritative status of science varies across different domains.

[3] In this dissertation I focus largely on the natural sciences. However, my discussion of objectivity is not limited to the natural sciences. In the end I argue for a feminist

science. Ultimately, I argue that a feminist conceptualization of objectivity provides a more comprehensive and practical ideal to guide scientific practice than the received view's conception, where objectivity is sought by adopting an impersonal methodology. This methodology is believed to insure the removal of subjective elements, which are seen as impediments to neutrality, in scientific thinking. Nevertheless different schools of thought, feminist among them, successfully show that this so-called impersonal methodology does not succeed in completely eliminating bias and prejudice in scientific thinking.

Objectivity is one of the chief regulatory epistemic notions that vindicates inquiry, observation or method, and which provides authority to whoever can claim it. As such the notion of objectivity has been at the center of discussion for different groups of philosophers of science. Specifically, in the early twentieth century a vast amount of literature on the nature of scientific observation, explanation and method was produced in accounting for scientific objectivity. In this period, the methodology of the natural sciences was regarded as providing the model for "objectivity" by the predominant Western worldview of the time, namely positivism. However, objectivity itself is a complicated and a contested notion. Objectivity has been variously attributed to descriptions or judgments where ontological considerations come into play, and/or to methods of inquiry where epistemological considerations come into play. Furthermore,

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understanding of objectivity where which norms to follow in achieving objectivity is influenced by the local, pragmatic and material circumstances of the inquiry at hand. Hence, this new conception of objectivity is applicable to any inquiry regardless of its content.

these two attributions are often conjoined by the belief that objective descriptions of the world are attained through a unique methodology. Here, the gap between an inquiring—yet fallible—mind and an independent world is filled by the assumption that following an impersonal and value-free methodology will yield "objective" truths about the world. Hence, the objectivity of scientific methodology means that the results of scientific inquiry provide the last word about matters of fact. The authority of science, then, is closely related to claims about the integrity of science. That is, attributing objectivity to scientific method means that the results of scientific inquiry are not tainted by any subjective elements—be they individual prejudices or social and political values and situations. Throughout this dissertation, I will examine how this belief in the integrity of science has been maintained by those who adhere to the received view of objectivity, and will question whether this view is consistent with the actual ways in which objectivity has been sought in scientific practice.

In questioning the authority of science, perhaps the first thing to note is that scientific authority has historically been and continues to be reinforced by the linguistic style of scientific discourse. According to Ruth Hubbard, "[t]he way language is used in scientific writing implicitly denies the relevance of time, place, social context, authorship, and personal responsibility." (Hubbard 1979, xv) For instance, expressing scientific writing in the passive voice, that is, in the form of 'it has been observed that', deletes the scientist as an agent and his/her *activity*. This makes it possible for science to be treated as though it were as real as nature. For Hubbard "a further degree of reification, and consequently alienation, is introduced by removing *all* verb forms, even

passive ones." Hence, even the impersonal agent of the passive voice is erased. Hubbard explains the process of reification with an example taken from the book *Sociobiology: The New Synthesis* written by E. O Wilson, wherein Wilson claims, "Human beings are absurdly easy to indoctrinate."[4] For Hubbard what is important in this claim is that "the activity of one person, who has the power and desire to indoctrinate others, who presumably are not in positions to reciprocate, is turned into a pseudo-objective statement—a fictitious description of 'human nature'." Further, Hubbard warns us about the consequences of such grammatical tricks: different programmes with drastically different effects on the natural and social world can be followed, depending on how reality is described.[5]

This im-personalization is also at work when a "philosophical" examination of science is restricted to an inquiry into the logic of scientific knowledge, such as was carried out by the logical positivists. Here scientific knowledge is identified with theoretical knowledge comprised of propositional, that is, impersonal statements. In this understanding the practical knowledge, or, know-how, of the majority of practicing scientists (such as how to sketch phenomena or what to do in a laboratory) is overlooked.

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[4] The quotation is taken from Hubbard (1979, xv).

[5] Hubbard writes, "if our description of reality is that 'human beings are absurdly easy to indoctrinate,' then it seems entirely proper to ask what makes them so and to look for reasons why they (of course, meaning "we") might have evolved genes for 'indoctrinability,' which is what Wilson goes on to do. However, if we described the situation as one in which some people are in position to indoctrinate others, we might, think about how to change that. A very different program!" And she goes on to note that "the grammar of active participation is conducive to action, whereas the grammar of depersonalized description tricks one into submission to "facts of nature" or at best encourages their further exploration." (1979, xv)

This approach to science is fed by a certain prior understanding of science. In truth, what constitutes science remains an open question, and how we define science has direct effects on how we conceptualize or pursue its fundamental norms. If we define science strictly as "a logically coherent body of knowledge deduced from a limited number of principles"[6], then the work of scientists indeed becomes invisible. We end up with a certain conceptualization of objectivity that identifies it with impersonality and neutrality. However, reducing science to scientific knowledge—if it is not wrong—at best will provide an inadequate account of science and its governing ideals. It will also leave important philosophical questions regarding science unanswered.

Another way of defining science is to take it as "the system of behavior by which man [*sic*] acquires mastery of his environment."[7] Sexist terminology aside, this understanding conceptualizes science as a practice. However, some would be disturbed by the implication of the manipulation of nature, for it reflects a masculine aspiration.[8]

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[6] This definition belongs to French historian of science Arnold Reymond. The quotation is taken from Farrington (1944, 11).

[7] This definition belongs to James Gerald Crowther, a scientific journalist and a pioneering advocate of the social relations of science. The quotation is taken from Farrington (1944, 14).

[8] In her book *Reflections on Gender and Science* (1985) Evelyn Fox Keller explains how masculinity is attributed to science. I have a detailed discussion of her position in Chapter Four. Broadly put, according the object relations theory in psychoanalysis, boys and girls go through different sexual and emotional development that results in different cognitive styles. In forming their sense of self boys have to separate themselves from (disidentify with) their mothers (who are the primary objects for their infants.) Keller claims that scientific ideology rests on a rigid distinction between the knower (mind) and the known (nature). In this ideology the knower should have autonomy, separation and distance from his/her subject matter in order to have a grasp on nature. Furthermore, here the relationship between the knower and the known is one of control and dominance rather than an emphatic understanding which, in her view, is typically marked as a feminine

Yet, I believe this somewhat crude understanding is a good reminder of the sacrifices made in order to control the world, and the price we human beings, or other beings in nature, pay for the success of science. If this characterization of science is accepted openly, it is more likely that the methods of manipulation, as well as its consequences, will become more visible. It is also more likely that there will be more initiatives to take action against, and responsibility for, the wrong doings of science. Of course not all science is harmful; nor am I suggesting that it does not have any positive impact in our lives. In truth, we human beings need science and its achievements in order to survive. That is why it is of great importance to cultivate more responsible science. If it is not hidden that science comes with a price, then there can be room for negotiation to stop certain harmful research programmes and commence with certain others.

These two definitions of science are by no means the only way to understand science. For instance, we can argue for the intrinsic value of science, and define science as an activity of seeking knowledge for its own sake. The opening sentence of Aristotle's *Metaphysics*, namely "All men by nature desire to know" may confer an intrinsic value on science. But could the struggle to understand and explain phenomena so far be accounted for by appealing to curiosity alone? The way knowledge is pursued has changed from the times of Plato and Aristotle. In today's neo-liberal world, I think it would be naïve to think that the foundations of big-scale laboratories, formations of complex organizations, training of millions of people and so forth are due to the intrinsic

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trait. Autonomy and separation, on the other hand, reflect a masculine cognitive style. As a result, scientific thought, for Keller, is identified with male thought.

value of scientific knowledge. Having said this, I do not contest the claim that science has intrinsic value. My claim is that defining science merely in terms of its intrinsic value neither helps us to understand what science amounts to today nor helps us to cope with its problems and successes.

One point that needs to be stressed is that defining science as an activity devoted to controlling our environment goes hand in hand with emphasizing its use-value. In other words, the knowledge that science produces should be relevant to human needs and values. Although the question of needs and values itself is controversial (for example, defining whose needs and which values), given the circumstances in which scientific knowledge is produced it is urgent to underline the role of science in our lives. In today's neo-liberal societies, scientists, located both in universities and private research institutions, increasingly produce science for purposes of accumulation and exchange.[9] The process of creating exchange value for scientific knowledge results in alienation from and the mystification of science in people's lives.[10] Insisting on the use-value of

[9] "Its value is *not* in the *use* of the knowledge that is generated, but in its power to realize exchange value in the form of fellowships, publications, jobs, research funds, positions on the committees that allocate funds, scientific honors, prized, etc." (Hubbard 1979, xxi)

[10] I should emphasize that my claim is not that today's scientific environment reflects an intrinsic character of scientific practice. Historically, there have been and still are scientists who approach their subject matter out of sheer curiosity or amazement. However, the current structure of the scientific industry is such that these idealists, so to speak, are marginalized. Almost all research programmes, to a lesser or greater extent, need funds to be carried out. Broadly put, today which programmes get funded and which are set aside is often based on decisions that follow a vicious cycle: publications help scientists to become known and attract more funds and job offers such as building new laboratories that carry out more research and produce new publications and attract even more funds. This cycle pushes more and more scientists into focusing on subject matters

science, on the other hand, may make it easy for "people to involve themselves in the decision-making process because the connections between "scientific decisions" and our daily lives would be obvious." (Hubbard 1979, xxii) Such transparency may also help generate discussions about for what and for whom science is or should be useful.

It should be admitted, however, that defining science as a practice is not sufficient to demarcate it from other forms of behavior intended to cope with the world.[11] Yet, the main point in this view is to emphasize that, for many theorists and practitioners, science has its origin in technique. According to Farrington "Science arises in contact with things, it is dependent on the evidence of the senses, and however far it seems to move from them, must always come back to them. It requires logic and the elaboration of theory, but its strictest logic and choicest theory must be proven in practice. Science in the practical sense is the necessary basis for abstract and speculative science." (Farrington 1944, 14) It is important to note that this understanding reinstalls agency in science, and hence reveals the problems and limitations of an impersonal method. It also makes space for questions about responsibility in scientific practice.

Many empirical and historical studies of science support a conceptualization of science as practice. These studies reveal that since its early beginnings in the Western

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that could potentially generate more interest and publications and eventually bring fame. Hence scientists increasingly move toward profit making research topics rather than what they are genuinely interested in. For more information on this cycle see Hubbard (1979).

[11] In Israel Scheffler's (1982) terms, what demarcates science from other forms of knowledge acquisition is that (i) it rests on empirical data acquired by observation of phenomena; (ii) it relies on rational evaluations by appealing to general principles; and (iii) it is open to criticism. Note that set of criteria—even if incomplete—is not incompatible with an understanding of science as practice.

world science has rarely been an entirely solitary activity. Rather, science has evolved as an essentially social practice that both affects and is affected by nature as well as by social institutions such as politics, economics, the academy, the military and other such public institutions and practices. Internally, science also has a complex structure with its different compartments and divisions of labour, each of which stands in different and dependent relations with one another. Thus, we need to reconceptualize objectivity in science today in a way that is compatible with and applicable to this practical understanding. Yet, it is important to keep in mind that one cannot effectively reconceptualize one of the main regulatory notions in science without shifting the other major ideals that constitute it. Any such shift would likely result in a new epistemology and philosophy of science.

Nevertheless, the value and use of empirical studies in the philosophy of science has been contested by such philosophers as Philip Kitcher, on the grounds that the actual (or historical) structure of science and how it works (or has worked) is a separate issue from how it should work. Hence, a strict distinction has been drawn between a descriptive and a normative understanding of science. Similarly, according to this line of thought, inquiring how scientific norms are actually pursued is not relevant to how they should be conceptualized and prescribed. However, the question of how the norms that guide scientific practice can be determined, without taking into consideration how these norms work and to what use they are put, remains open. I think our lack of evidence about the nature of concepts suggests that faith in an *a priori* understanding of a concept is unjustified. Admittedly, the grounds on which concepts stand are big questions, and I

do not tackle this issue in this dissertation. But my conviction is that an *a priori* analysis of a concept is often inadequate in capturing the subtleties and complexities in the actual usage of the term which, in my Wittgensteinian opinion, is the seat of the meaning of a term. Hence looking at actual practices of objectivity helps us understand and appreciate the intricate applications of the term.

Following this contention, I begin my examination in the first chapter by looking at how objectivity has been historically understood and pursued within scientific practice. Since such inquiry requires considering historical cases, I appeal to the works of prominent historians of science such as Peter Dear, Lorraine Daston and Peter Galison. These historians demonstrate that "objectivity" in science has been historically conceived in various ways. Objectivity has taken different forms depending on the currently dominant projects in science and depending on predominant ontologies and assumptions. Hence, my discussion in the first chapter aims to establish the components and ideals of objectivity and to show how different aspects of scientific practice focus on different components in their search for objectivity. Throughout the thesis I point out the continuities as well as the discrepancies between the historical practices of objectivity and its philosophical conceptualizations. I argue that some problems within certain understandings of objectivity in philosophy result from a dismissal of or confusion over the multiple meanings embedded in the notion of objectivity itself.

In the second chapter I turn my attention to the historically dominant schools of thought in the philosophy of science. I examine their most common principles in an attempt to understand and formalize their understandings of objectivity. The comparison

between different schools of thought shows that each school emphasizes distinct components of objectivity, leading to conflicting prescriptions regarding how to achieve scientific objectivity. Apart from disagreements over specific scientific principles, one of the main reasons behind these discrepancies is the difference in their understandings of what science amounts to. In mainstream Western philosophy of science, for example among logical empiricists and scientific realists, the dominant approach to science as a subject of philosophical investigation is to focus on the logic of scientific knowledge (while dismissing other aspects of scientific practice as philosophically irrelevant). I believe this is a narrow and mistaken approach. As I discuss above, science is neither a monolithic enterprise nor a solitary activity. I find the resources for a more comprehensive account of science and its ideals in feminist literature on science and objectivity.

Hence, in the third and fourth chapters, I examine feminist epistemology and philosophy of science. The third chapter focuses on feminist standpoint theory and particularly on Sandra Harding's conception of "strong objectivity". I examine its strengths and weaknesses. In the fourth chapter, I focus on Helen Longino's understanding of objectivity. I examine her belief in the sociality of knowledge (and science), which stems from her engagement with contextual empiricism, and I seek to defend her position against certain criticisms. I argue that while Longino's conception of objectivity has certain advantages over Harding's conception (such as an ability i) to maintain a dialog with "the mainstream" and ii) to account for the normative aspects of

objectivity), her conception should be complemented by some features of Harding's "strong objectivity."

The fifth chapter focuses specifically on charges of relativism against feminist philosophy of science. I examine two specific charges. The first criticism I discuss is the general claim that feminists endorse the/a gender specificity of knowledge, which leads to a form of relativism that undermines knowledge. I examine in what ways the gender specificity of knowledge could be understood, and address the forms of relativism that would follow from them. I argue that feminists need not endorse a *universal* claim about the gender specificity of knowledge. The second criticism I discuss is Sharon Crasnow's criticism brought specifically against Longino's account of objectivity. Briefly, Crasnow claims that Longino's position does not go any further than affirming intersubjectivity and hence does not provide a firm ground from which to constrain our beliefs. My response to this criticism involves an opposition to a clear-cut distinction between ontological and epistemological objectivity, which Crasnow presupposes. I argue that although distinguishing things that are represented (which fall under ontological objectivity) and the processes used to represent them (which fall under epistemological objectivity) may be conceptually helpful, in practicing science, it is unintelligible to talk of the things that are represented apart from the representation process itself.

I suggest that adopting a naturalistic approach to objectivity enables us, who seek a comprehensive understanding of scientific objectivity, to situate conceptions of objectivity in specific times and circumstances. In this understanding there can be no absolute conception of objectivity, and no claims of objectivity can be absolute. Hence, I

do not argue that a feminist conception of objectivity is conclusively better than any other conception. Rather, I argue that a feminist conception of scientific objectivity is more directly applicable (and morally responsible) than the received view's conception. The historical fluidity of objectivity means that my analysis in this dissertation is also not conclusive. It should be expanded as new conceptualizations of objectivity emerge as a result of historical contingencies.

Chapter One

Scientific Objectivity: Historical Transformations and Philosophical Distinctions

'Objectivity' is an ambiguous term. It is associated with distinct yet often complementary ideas such as truth, rationality, empirical reliability, procedural correctness, emotional detachment and so forth. The confusion around objectivity is often a result of neglecting its diverse and layered character. The works of historians of science have shown that the meaning and value attributed to objectivity have transformed as the concerns, the ideals and practices of western societies in general and science in particular have changed in response to the ethos of the time. In order to understand today's claims of objectivity, authority attributed to it and criticisms against those claims, we need to have a clear grasp of the diverse components of objectivity. In the following, I will briefly explain how these components have emerged and merged historically. I will also examine how they relate to one another and form specific conceptions of objectivity. As such this chapter will constitute a base for the discussions of the different conceptualizations of objectivity operative within different schools of thought that I will examine in the coming chapters.

One of the remarkable indicators of the non-monolithic character of objectivity is its changing meaning throughout history. The *Oxford Dictionary* defines 'objective' as what is external to the mind; actually existing; real. 'Subjective', on the other hand, is defined as belonging to the individual consciousness or perception; imaginary.[1] In contrast to the modern opposition between 'objective' and 'subjective', in scholastic

[1] *Concise Oxford English Dictionary*, ed. Della Thompson, 1995.

philosophy the word 'objective' referred to the existence or nature of a thing as an object of consciousness while 'subjective' referred to the existing of a thing as 'in itself'.[2] Accordingly, what belonged to things subjectively were things as they are 'in themselves' where there is no reference to mind or consciousness, whereas, what belonged to things objectively pointed to the way they are presented to consciousness. It is curious how this striking contrast between the scholastic usage and the modern usage of 'objective' and 'subjective' took place. Yet tracking this change lies beyond the purpose of the present thesis. Instead, I want to explore the philosophical and historical elements behind the modern association of objectivity with ideals such as impersonality, impartiality, disinterestedness or neutrality through examining some of the points of view in the history of philosophy and the transformations that took place within scientific practice.

1.1 Ideals of Objectivity and the External World

Let me start by examining how ideals of impersonality, disinterestedness and impartiality relate to what is external to mind. In what ways, for instance, does speaking of what is external to mind make our judgments impartial or disinterested? A close consideration would reveal that what is common to all of these ideals is their separation from some form of subjectivity. This separation, for some historical reasons, is believed to make these ideals positive attributes. However, 'subjective' is also a layered concept. That is why it is important to ask which forms of subjectivity these ideals deny. Does each deny a different

[2] *The Oxford English Dictionary*, (2nd ed.) prepared by J. A. Simpson and E.S.C. Weiner, Volume X, 1989.

form of subjectivity, or do all deny the same kind of subjectivity? Before answering these questions we need to discern different usages of 'subjective'.

1.1.1 Subjectivity

The denunciation of subjectivity can be traced as far back as to Plato for whom anything bodily is a hindrance in acquiring knowledge. According to him the real world is the world of Ideas and true knowledge is knowledge of these Ideas. The world we live in, in contrast, is the world of appearances where knowledge is not possible. The only way for us human beings to acquire knowledge is by escaping our bodies, i.e. through death.[3] What is important here is the hierarchical dualism between the real world and the world of appearances. To put it another way, it is between the objective world and the subjective world in the modern sense, where the latter is an obstacle to knowledge. As such, this dualism, which is a function of a peculiar ontology, may explain the negativity that attaches to what is subjective.

However, the persistent denouncing of subjectivity even after Platonism signals that there are other components to subjectivity than ontology. In Descartes's philosophy, for instance, the senses are also an obstacle to knowledge as they are deceptive. Although the dualism between reality and appearance prevails in Descartes, the emphasis is not on ontology but on methodology, i.e. not on what exists, but on the way we come to know things. Accordingly, what is problematic about the subjectivity of the senses is that they

[3] This is Plato's view in the dialogue *Phaedo*. In the *Republic* Plato seems to allow for some sort of knowledge in this world, but this controversy is not central to my discussion.

block knowledge. It should be noted, however, that although underlying Descartes's methodology there is a concern that everything we perceive may be in our minds, the problem of subjectivity has yet to be construed in terms of the modern coupling between subjectivity and mind-dependence. Both in Plato's and in Descartes's philosophy the proper way to truth is through rational deliberation, which requires detachment from the body and senses (and in Descartes's case also a detachment from custom and what has been taught to us) as they obstruct knowledge. It is reasonable to claim that for both philosophers dependence on mind was not a hindrance to objectivity; hence subjectivity was not construed as mind-dependence. How then should we understand the relation between objectivity as detachment and ideals such as impersonality, impartiality and disinterestedness? Since impersonality requires not being influenced by personal or individual feelings, and disinterestedness requires bypassing considerations of personal advantage, it might be relatively easy to associate them with detachment, as desires of the body (and inclinations attained from previous experiences) might blur one's judgments. Yet, where does impartiality, which involves a reference to equality and fairness, stand in this relation? It can be claimed that once one's judgments are not blurred by senses and desires, one can have a more fair judgment about a given topic. This means that one can approach the topic from different angles and perspectives. But surely this does not apply to all cases, as not all perspectives start off being investigated at the same distance from the investigator. In other words, some perspectives might not be heard or understood properly by the investigator for reasons other than confusions brought about by the weaknesses of the body and senses. Social and cultural factors, for instance, not only

affect personal feelings but also personal (or collective) thinking.[4] One might insist that detachment must include detachment from the effects of social and cultural factors in our thinking, as they are examples of inclinations learned from previous experiences. Yet, since we human beings are social beings, it is clearly very difficult (if ever possible) to achieve such detachment. Perhaps a philosophically more challenging problem here is that the relationship between objectivity as detachment and impersonality, disinterestedness and impartiality is premised on the idea that if, say, a judgment is personal (subjective), partial or reflects someone's interest, then it suggests that judgment is less than rational, and is influenced by the body and senses. However, this is a problematic premise. First, there is the problem that in certain cases pertaining to subjective conditions, in order to have a fair judgment the "rational" thing to do is to include the related subjective perspective.[5] Second, the aforementioned premise implies that if a judgment is not blurred by the workings of the body and senses, i.e. (given dualism) rationally achieved, then it is not prone to partiality or personal advantages. If this is so, what do we do with cases where I attempt to rationally demonstrate, say, the existence of God because I have a vested interest in it? Would my judgment about the existence of God be disinterested? This question is ultimately a question about the disinterestedness and impartiality of the insistence on rational deliberation in acquiring objective knowledge. In short, it is a question about where reason gets its privileged status.

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[4] This understanding is more or less the main line of thinking that many feminists adopt. There will be further discussions on this issue in the coming chapters.

[5] This point will be discussed further later in this chapter in relation to the ideal of aperspectivity.

So far I have examined objectivity as opposed to the subjectivity of the body and the senses where the relationship between subjectivity and mind-dependence has not come up. The dualism between objectivity as referring to *external reality* and subjectivity as referring to *the workings of the mind* is mainly derived from Kant's philosophy. His dualism between noumena and phenomena is not so much about the real world versus deceptive appearances registered by unreliable senses, but more about the raw materials that are not intelligible to us as human beings before they are processed by forms of intuition and categories of the mind which make perception and understanding possible. Accordingly, our judgments about the world are mediated by our faculties and to claim otherwise would be implausible. Here modern concerns regarding the mediation of the mind and its categories in understanding the world as an *obstacle* to objectivity have not yet been conceived. In Kant's philosophy the mind-independent world (the noumenon) is not accessible to us, but our judgments can be about things that exist independently of our thinking about them. (Kant 1998) For Kant, although what we perceive or think depends on subjective conditions, our judgments may still count as "objective" because the categories of the mind (and forms of intuition) are universal. It is important to note, however, that this later form of 'objectivity' is not directly derived from objective reality. It could be speculated that the objectivity of judgments in Kant is related to their being impersonal rather than to objective (external) reality. This is because the universality of the categories of the mind (and forms of intuition) assures that judgments are subject to intersubjective assessments. The emphasis on universality here signals that this impersonality does not deny the subjectivity of, say, a judgment because it refers to

something in the mind, but it denies any claims of randomness or contingency attached to the influences of personal (subjective) feelings and thinking.

Those who identify objective reality with external reality can argue that impersonality is linked to objective reality, for what impersonal knowledge represents is external reality. Yet, this cannot be true in Kant's philosophy, because if we agree that noumena are inaccessible, then we cannot intelligibly claim that impersonal knowledge represents the external world. In order to confirm or deny that a representation relation holds between our knowledge and the external world, we have to assume that we have some access to the external world.

On what grounds, then, can the link be established between impersonal knowledge, i.e. knowledge achieved through impersonal means, and objective reality. There are many interesting and complex questions that could be asked here: What does "external world" amount to?; if objective reality merely means external reality, what is the use of invoking the term 'objective'?; how could we tell that our impersonal method indeed guides us in achieving knowledge of the external world? Answers to these questions depend on certain epistemological and ontological considerations. Discussion of objectivity, then, revolves around issues about the ways we achieve knowledge and issues about the object (or nature of the object) of our knowledge. I think a good deal of confusion about objectivity will be resolved if we examine how these two aspects of objectivity are connected. I will have detailed discussion on the relationship between epistemological objectivity and ontological objectivity in Chapter Five.

So far I have discussed the claim that in the history of philosophy understandings

of objectivity varied depending on the forms of subjectivity that were believed to obstruct knowledge. Yet, my examination did not include an empiricist uptake of objectivity. This is because, first, I will have a whole chapter (second chapter) on positivist understandings of objectivity and its critiques. And second, keeping in mind the empiricist contention that all knowledge (including any knowledge about objectivity) stems from experience, I believe any empiricist examination of objectivity should start from an inquiry into the actual pursuit of objectivity. Since I am interested in the notion of objectivity in the sciences, in order to have a consistent empiricist understanding of objectivity, I shall proceed with examining the historical pursuit of objectivity in the sciences.

1.2 The Historical Pursuit of Objectivity in the Sciences

Looking at how objectivity was pursued historically in the sciences helps us understand the links between different components of objectivity as well as the reasons why certain components were more salient in certain periods of time than others. This inquiry shows us that there has not been a uniform conceptualization of objectivity throughout the history of science. There are important implications of this non-uniformity. First, the authority attributed to science (and its claim to objectivity) becomes questionable. This is because if there are many forms of achieving objectivity—not all of which are consistent with one another—due to which form(s) science attains its authority becomes problematic. Second, the historical variety in achieving different forms of objectivity shows us that any *a priori* conceptualization of objectivity will be doomed to fail in explaining and understanding how objectivity is sought and how claims of objectivity

operate. An empirical investigation, on the other hand, will not result in a uniform theory of objectivity. Yet, it will provide a more comprehensive understanding of objectivity. Let's then start our investigation with Peter Dear's account of the shift in the meaning of objectivity that occurred in the seventeenth century.

1.2.1 Peter Dear: From Truth to Disinterestedness

In tracing the shifts in the meanings of 'objective', Peter Dear, a prominent historian of science, argues in "From Truth to Disinterestedness", that construing objectivity as disinterestedness was due to a shift in the understanding of 'objective knowledge' in the seventeenth century with the rise of the scientific revolution. Until then criteria for knowledge claims were tied to 'objective truth'. Dear claims that, in the seventeenth century, following transformations in the ways of making knowledge, the criteria for knowledge claims shifted from 'objective truth' to 'trustworthiness' and 'disinterestedness'. In other words, justification for knowledge-claims shifted from the truth of the propositions to the trustworthiness and disinterestedness of the claimant. What this meant was that in assessing and accepting knowledge claims certain *characteristics* of natural philosophers began to be taken into consideration, in ways that they were not before. The significance of this shift for our discussion, I believe, is that it expands the possibility of objective knowledge beyond mere correspondence between judgments and external reality (or the holy scripture for the scholastics). Now the assessment of objective knowledge involves considering how concrete specificities of the knower and the known (as well as the general environment) affect knowledge relations. If Dear's account is plausible, it is

reasonable to claim that current theories in epistemology such as contextualism or theories emphasizing the situatedness of knowledge relations found their practical applications well before twentieth century. One important question that demands an answer is why mainstream epistemology neglected this line of thinking for so long. A reasonable response to this question would lend support to the idea that prevailing commitments (philosophical or otherwise) do influence our conceptualizations.

Before reflecting on the implications this shift may convey for the contrast between objective and subjective that I have been discussing, it is worth looking at Dear's historical explanation of how disinterestedness arose as an ideal. What initially brought about the change from truth to disinterestedness according to Dear is a focus on degrees of certainty in the seventeenth century, which had never appeared before. In fact, it is interesting to track how the talk of certainty among natural philosophers of the time gradually shifts. (Dear 1992, 622) In the first half of the century, Roderigo de Arriaga, for instance, distinguishes three kinds of certainty: moral, physical and metaphysical. According to him, moral certainty is the kind of certainty we rely on for practical purposes. Arriaga's example that Dear mentions is that his belief in the existence of Naples rests on the testimony of 'prudent and truthful men'. That is, "although it is physically possible that these witnesses are lying, the existence of Naples is still *morally* certain—that is, it is safe to *act* on the assumption that Naples exists." (622) Physical certainty, on the other hand, rests on physical principles of cause and effect. Arriaga could be physically certain that Peter is running when he sees Peter is running. This form of certainty does not "rely on the testimony but on the regular physical behavior of things, in

this case related to vision." (622) Since, however, we could be deceived by God and perceive an object as moving even though it stands still, physical certainty is not the highest degree of certainty. It is metaphysical certainty that ranks highest. This is because it applies to cases where it is not possible in any ordinary way for something to be otherwise, e.g. axioms such as 'Everything either is or is not'.[6] (622) Arriaga's contemporary, Honoré Fabri makes a further distinction between objective certainty and formal certainty where the former depends on the nature of the thing in question and the latter depends on the act of the intellect by which it assents to a proposition. A similar dualism was defended by Rudolph Goclenius who distinguished between the certainty of the object and the certainty of the subject. According to his distinction, unlike objective certainty which is derived from the object itself, subjective (formal) certainty is possessed by the knower. (622-623) However, what is important for Dear's argument and our discussion is the shift that happened in the latter half of the seventeenth century: certainty possessed by the subject started to be compared with the *objectivity of evidence* rather than objective certainty.

Although Dear does not elaborate on this issue, my understanding is that the certainty a subject has regarding a judgment about the world began to be assessed against the evidence he or she had rather than the certainty attributed to the part of the world that the judgment was about. To clarify we can think of approaches to certainty in talk of

[6] A similar distinction seems to be implied in Descartes' *Meditations.* He clearly states in *Seventh Set of Objections with the Author's Replies* that he is dealing with "metaphysical doubt" to reach highest certainty, but not doubts about practical life. (Descartes 1984, 306-310)

primary and secondary qualities of objects. For example, certainty about "primary qualities" of an object such as its size, shape, motion and so forth was due to the belief that these qualities are present in things themselves. In contrast, "secondary qualities" which are believed to be effects of objects on people did not carry such certainty. Now the switch Dear points out presumably entails, for instance, that one's certainty about primary qualities of an object started to be assessed against the evidence, that is, the reasons why the subject came to think that the object has such and such primary qualities rather than a reference to things in themselves. Once this switch happened, it is no surprise that questions regarding the nature and reliability of evidence became a matter of assessing objectivity. According to Dear, this switch signals that from then on certainty was placed in the realm of the mind. He notes that during scholasticism 'subjective certainty', which came from the Holy Ghost, overrode objective evidence. But the problem of how human knowers could attain reliable knowledge became a pressing issue in late seventeenth century English philosophy. (625) It is in this context that disinterestedness as a value began to be praised especially in the areas of inquiry where individual testimony mattered.

Dear illustrates this shift by a striking example of Galileo's book *Sidereal Messenger* where Galileo reports his discovery of four moons circling Jupiter. His book reported observations that no one at the time was able to confirm, as nobody had as good a telescope as Galileo. After the publication of the book, Kepler, who did not have the proper equipment to repeat Galileo's observations, stated his reasons for believing Galileo's claims: Galileo's occupation, his character, and his social relations all validate the judgments in the book. Moreover Galileo's very style in expressing his judgments, i.e.

publishing a book, and inviting others to use his equipment for testing, proves his disinterestedness. Dear takes this event as an important indication of the shift from truth to disinterestedness, because in this case, the thing itself did not have any role in the evidence upon which certainty was built. Accordingly, Dear claims "the 'objective' as a dimension of knowledge rooted in things and their knowability thus came to be replaced in the seventeenth century by a negative category characterized by the absence of features deemed to be inappropriate to valid knowledge." (627) In short, reliability became an important condition for knowledge (based on testimony) which paved the way for an understanding that takes into consideration the characteristics and limitations of human knowers.

If objective knowledge rests on objective evidence, then we need to ask what the criteria for objective evidence are. In cases where testing a judgment empirically is not possible, validity is sought in the reliability of the scientist. The indicators of such reliability are found in the scientist's personal traits and social relations. While some characteristics and virtues that scientists possess (as well as prevailing social values) mark the objectivity of a judgment, some others undermine it. As such, the objectivity of evidence here is not tied to neutrality or to not being influenced by personal feelings or values.[7] The importance of this discussion is that it indicates that in this frame of thought impersonality and disinterestedness are not inextricably connected to one another. If disinterestedness is the governing ideal for objectivity, and if it is measured against the

[7] It is important to note that emphasis on values (social, political, contextual) as a detriment to scientific objectivity is a post-Marxian conceptualization.

characteristics and virtues that a scientist possesses, then the objectivity of a judgment is not secured by impersonality.

The connection between the personal traits of scientists and objectivity is also picked up by Lorraine Daston who does extensive work on the history of objectivity. But before examining her views let me note that we should approach Dear's claim about truth to disinterestedness with caution. For one thing, he does not claim that disinterestedness entails truth. Surely, observations (and /or predictions) of "disinterested" scientists can (and do) sometimes turn out to be false. Dear's example is about assessing the objectivity of evidence in a particular case where further observation is not possible at the time. This limitation, however, does not indicate that disinterestedness is a false ideal or that it is not important. What it shows is that there is more to objectivity than disinterestedness.

In this section I have argued that different ideals of objectivity are not intrinsically related. If we consider objectivity as a mixed bundle of ideals without acknowledging the subtleties that each ideal contributes to objectivity, then we face the following complication. The judgment about the disinterestedness of Galileo's observations discussed above will be deemed objective even though it fails to be impersonal. Hence we will have a judgment that is both objective and not objective depending on which criteria we favor. This complication can easily be overcome by an understanding of objectivity where it is accepted that standards of objectivity emerge out of diverse epistemic practices and concerns. Daston's and Galison's discussion in the next section will illuminate this point further.

1.2.2 Lorraine Daston and Peter Galison: Ontological, Mechanical and Aperspectival Objectivity

Daston distinguishes three forms of objectivity each of which reflects distinct social and historical ideals and concerns. While *ontological objectivity* seeks the ultimate structure of reality, *mechanical objectivity* prescribes avoidance of judgment and interpretation in reporting and illustrating scientific results. *Aperspectival objectivity,* on the other hand, is about eliminating individual and group idiosyncrasies.[8] (Daston 1992, 599) At first glance, the avoidance of intervention and judgment might seem to entail the avoidance of individual idiosyncrasies. Yet a careful investigation shows that although they might coexist, they are conceptually distinct, for they attack different subjectivities. This difference becomes more apparent in "The Image of Objectivity" (1992) which Daston co-authored with Peter Galison. Their central claim is that gradual mechanization in scientific practice brings about a novel approach to objectivity in the nineteenth and early twentieth centuries which has led to the moralization of objectivity by means of a new ethos –the ethos of self-denial. We shall see in detail below that this ethos prescribes the control of certain characteristics of researchers that would possibly distort the results of the representation process.

In tracing the emergence of this ethos, Daston and Galison focus on atlas making from the eighteenth to the twentieth century, where changes in the conceptions of objectivity can be clearly followed as new techniques of mapping nature have developed.

[8] Rebecca Kukla in her article "Naturalizing Objectivity" (2008) reports that Daston and Galison modify this distinction in their book *Objectivity* (2007). My discussion here is based on the aforementioned articles in the text.

Scientific atlases of diverse fields aim not only to document but also to organize and stabilize the aspects of nature that they focus on, providing the working objects that scientists base their inquiry on. To that end, scientific image making lies at the heart of scientific practice.

Central to scientific image making is the issue of human intervention in representing nature. Yet, the idea of representation is understood rather differently throughout the history of objectivity as the governing norms that dominated the representation process changed. Each representation project demanded different kinds of personalities from their practitioners, with specific ethical virtues. While seventeenth century scientists were concerned primarily about a mismatch between nature and representations, for nineteenth century scientists the primary concern was the inner temptations of individual scientists. In other words, while seventeenth century scientists sought objectivity in the products of representation projects, nineteenth century scientists sought objectivity in the process of representation itself by controlling and limiting the involvement of scientists in it. With this shift, skeptical attitudes towards attaining objective knowledge in the seventeenth century were abandoned in favor of optimism about the possibility of securing objectivity in the nineteenth century, as it was believed that scientists could be objective as long as they carried certain traits. Here we again see the abandonment of the idea that "truth" (in the sense of correspondence) is the only governing ideal in the search for objectivity. What is also important to realize I think is that these developments brought about significant changes in *underlying assumptions* about the *assessments* of objective knowledge. When the concern is a mismatch between

nature and representation, the assessment of objectivity requires a special point of view which already has a grasp of how nature is so that whether the representation fits nature or not can be evaluated. Hence, the assessor presupposes the possibility of attaining such a viewpoint, i.e. a God's eye point of view. Whereas, when the focus is not on the end result of representation, but on the process itself, no such God's eye point of view is necessary. Here what the assessor needs is an apprehension of the governing norms for representation projects. Since different aspects of nature can be represented in different ways, the assessor has to specify which norms are used in the assessment of each representation project. This last point will become clearer as we follow Daston and Galison's account of how the governing norms have changed in the history of atlas making.

Daston and Galison note that from the sixteenth century on the dominant ideal in science has been to be "true to nature". In order to illustrate "what truly is" atlas makers were compelled to make ontological and aesthetic judgments. (Daston and Galison 1992, 84) The primary aim of the atlas maker was to reduce the variety and multiplicity of nature into manageable pieces (e.g. specimens) which required decisions about the selection of what phenomena to observe, and from which point of view to observe them. In doing so, both the observing subjects and the observed objects were to be standardized in order to preclude any idiosyncrasies. (84-5) From today's point of view such standardization and the ideal of representing things as they truly are might seem to be in tension, but Daston and Galison argue that "truth to nature" in standardization was construed in different ways. Some atlas makers focused on illustrating "types" and

"ideals" while others on "characteristic individuals" or "individuals". Daston and Galison have an elaborate discussion of the differences of these interpretations in representing nature, which there is no room to discuss here. The important thing to note is that, according to the advocates of illustrating ideals, "what truly is" could be captured by depicting the ideal type as the best pattern or model of nature. The task of the image-maker is to find the essential properties that underlie and unify contingent particulars. Accordingly, monstrous or idiosyncratic particulars that deviate from "normal" instances were left outside science. Daston and Galison note that in addition to reducing the plurality of nature to standard types, idealization also provided greater precision, since the process of idealization required intense study of nature from all view points as well as the critical expertise of scientists in selective judgment. As such, the ideal image was not necessarily restricted to a specific viewpoint. In the process of idealization there was room for the competent "interpretation" of the scientist, which was not as a hindrance to objectivity. In fact, interpretive depictions of phenomena were something to be proud of, for the ontological and aesthetic judgments involved in creating working objects were essential to the merit of atlases. (87) The main reason why judgments in attaining ideals were no obstacle to objectivity was related to the metaphysics of the day where it was believed that universals exist just as particulars do. Therefore, although they are not actually embodied in a specific particular, i.e. no particular is ideal, they can be represented. (91) This view contrasted with late nineteenth century metaphysics which attributed existence only to particulars.

According to scientists who believed that "true" images consist in representing

particulars with their differences, such images were true to nature because they were almost exact copies of particulars as they are seen. As imaging techniques progressed, the practice of imaging individuals rather than ideal types has gradually prevailed in science, and for Daston and Galison this progression marked the transition to mechanical objectivity which I will discuss shortly. Underlying the insistence on illustrating particulars was the contention that idealizations produce distortions, because they serve the interests of theories. (91) In other words, the talk of ideals rests on decisions as to what feature of a particular constitutes its essential property. Such decisions, on the other hand, are products of previous systems of ideas. Hence, they are not "neutral" decisions. A well-known proponent of this line of thought is Francis Bacon. Although he did not specifically write on image making, his views might be helpful in understanding how idealization can yield distortion.

1.2.2.1 Francis Bacon and the Emphasis on Differences

In the philosophical background of the insistence on illustrating particulars with all their imperfections was a shift of focus from knowledge derived by argument to knowledge of matters of fact. While the knowledge of matters of fact is gained by observation and employing induction, the knowledge derived from arguments is achieved by deducing conclusions from premises (regardless of their content) by following logical principles. Following the Aristotelian tradition in philosophy, which had prevailed well into the seventeenth century, the primary concern had been that of attaining universal truths and discerning regularities in nature. Hence, mere collections of observational reports of

various phenomena were not regarded as scientific knowledge. In the Aristotelian school of thought, scientific knowledge rests on logical demonstrations (syllogisms). In the *Posterior Analytics*, Aristotle states, "the premises of demonstrated knowledge must be true, primary, immediate, better known than and prior to the conclusion, which is further related to them as effect to cause." (Aristotle 1947, 11) So we need to "know" the premises of the demonstration that scientific knowledge rests on, but this knowledge cannot itself be demonstrative knowledge. According to Aristotle, our knowledge of the premises primarily rests on sense-perception. In order for sense-perception to transform into knowledge, a *sense-impression* has to be retained in the soul. Some animals develop a power of systematizing sense-impressions when these impressions persist. As a result of this process *memories* are formed. An *experience* is developed when the memory of the same thing is frequently repeated. (107) In other words, when we perceive a phenomenon over and over again, we form an experience of it. Aristotle states "From experience again—i.e. from the universal now stabilized in its entirety within the soul, the one beside the many which is a single identity within them all—originate the skill of the craftsman and the knowledge of the man of science…" (107-108). In sum, for Aristotle we get to know the primary premises by induction. According to him, however, although "the act of sense-perception is of the particular, its content is universal." (108) That is, from singular experiences, we arrive at universals. When the idea is applied to image making, it can be said that these experiences of regularities were illustrated in ideal images which manifested the essential natural form of each particular. However, philosophers such as Robert Boyle and Francis Bacon opposed the Aristotelian contention that experience

which comes directly from the senses, automatically represents things as they are. Hence, they rejected the claims to scientific status of Aristotelian generalizations achieved through the experience of regularities. They insisted on the importance of paying attention to particular matters of fact, specifically rare events, such as monstrous births, in understanding the regularities of nature. This approach shifted the focus from the general to the particular and singular. (Daston 1994, 40-41) Bacon, specifically, fiercely opposed claims for the importance and immutability of knowledge that relied on logical demonstrations. Pointing to the inadequacy of abstracting from a limited number of particulars in order to arrive at universals, he rejected the Aristotelian conception of "experience". In contrast, he advised not only compiling accounts of "normal" phenomena but also of the errors and monstrosities of nature. (44) This insistence coincides with the contentions of those image-makers who advocated illustrating individuals with all their imperfections on the assumption that their images reveal the true nature of what is being depicted.[9]

Now, what does this debate about illustrating ideals and particulars indicate for our discussion of objectivity and its relation to impersonality, disinterestedness or impartiality? Since for illustrating ideals the selective judgment of the scientist is essential, we can reasonably claim that it does not rest on the ideal of impersonality. What about the belief that proper idealization requires a careful study of nature from all viewpoints? Could this belief be led by the ideal of impersonality? A close examination

[9] Note that Bacon was writing in the seventeenth century when the practice of idealization was still prominent. This situation is a good indication of the fact that the transitions in ideals were never clear-cut, and that they often co-existed.

reveals that the faith in such idealization is premised on the experiences and capacities of scientists. Obviously, the premise that scientists can (or should) study nature from *all* points of views is problematic. But the very fact of demanding this implausible capacity from scientists gives us important clues about the conception of objectivity underlying the contention about illustrating ideals (i.e. essential properties common to all members of a species) as representing the true patterns of nature. For one thing, the demand for a comprehensive study of nature brings to mind the ideal of impartiality which asks for equal treatment of all perspectives. In this understanding, true patterns of nature can be captured only if all perspectives are taken into account. Yet it is quite doubtful that this requirement demands an impersonal method. It is important to note here again that the ideal of impersonality is premised on interchangeable subjects (or in this case perspectives). In this context, there seems to be a tension between the ideal of impartiality and the ideal of impersonality. This is because the very idea about interchangeable perspectives renders redundant the call for a study of all perspectives, which implies that there are fundamentally different, perspectives.

The relationship between the ideal of impartiality and the ideal of disinterestedness is also interesting. I have mentioned above that the demand about the impartial study of *all* perspectives is problematic. Bacon's objections point out how an Aristotelian treatment of phenomena is unequal, as it leaves aside what is abnormal. Idealization not only favors what is "normal" or "usual", but also favors the ontological and aesthetic commitments of the scientists. Presumably, it is because of the influences of these commitments that idealization serves the interest of theories (insofar as they

organize and explain phenomena). In this sense, the process of imaging ideals is not a disinterested enterprise.

To summarize, in the pursuit of capturing the true patterns of nature the project of illustrating "ideals" cannot accommodate the ideals of impersonality, impartiality and disinterestedness all at once. Should we then simply conclude that those advocating the project of illustrating "ideals" failed to achieve objectivity? I think it would be too strong (or even an impossible) a condition for objectivity to demand conformity to all these ideals. As we shall see, the project of representing nature –even when the focus is shifted from ideals to particulars— has to overlook some aspects of phenomena in order to manage the multiplicity of nature. This necessary practice of overlooking, however, is in certain ways in tension with the ideals of disinterestedness and impartiality, for it involves a decision as to which aspects of a phenomenon to depict or from which perspective to depict it. In other words, it is a decision about which aspects or perspectives should be favored over others, which in turn fails to pay due respect to all aspects or viewpoints of nature. This dilemma is likely to occur regardless of how representation is pursued as long as it rests on the project of representing nature in its totality. Accordingly, we are faced with two options: (i) we can accept that disinterestedness, neutrality and impartiality are not suitable norms for achieving objective representations of nature (in its totality), or (ii) we can accept that representation of nature is bound to be incomplete (because we human beings can only have partial access to nature) but nonetheless strive for meaningful applications of norms of objectivity that may vary across different practices and contexts in science. The aim of this thesis is to explore the second option.

1.2.2.2 Mechanical Objectivity

In order to understand the different ways in which the norms of objectivity have been pursued, let's turn back to the opposition between the two groups of atlas makers. The clash between them can be construed as a dispute about which form of subjectivity is dangerous and which form is to be tolerated. [10] For the advocates of the "ideal" image, the judgments of expertise and the control of the scientist were necessary for preventing distortions that an incompetent observer (the illustrator) might carelessly have brought about. As scientists started to realize that *their* tendencies (in addition to the illustrators') might also cause distortions, and as imaging technologies improved, the once-praised insights of scientists became an obstacle to achieving precise images. With the gradual arrival of a new form of objectivity, namely mechanical objectivity, the characteristics expected of scientists if they were to achieve objectivity also changed. Daston and Galison distinguish positive and negative characteristics that were required for mechanical objectivity, both of which put intense pressure on the individual scientist. Acute care, exactitude, patience, perseverance and endless appetite for work were required as positive traits. Negative traits, on the other hand, consisted in eliminating the mediating presence of the observer. This latter ideal could take different forms such as the elimination of judgments in the selection of phenomena, the replacement of sense perceptions with machines, and the elimination of theories and hypotheses that organize (and distort) the

[10] It should be noted that tendencies in atlas making were not limited to these two groups. Nor were the boundaries of groups as sharp as I make them sound for the purposes of clarification. For a detailed explanation see Daston and Galison (1992).

phenomena. (Daston and Galison, 1992, 82-3) All aspects of mechanical objectivity supported the morality of self-discipline that is built upon self-restraint and self-abnegation. Now the accuracy of images was secured by mechanical means such as photographic depictions, x-rays, lithographs, photo-engravings, camera obscura drawings, ground glass tracings. As new instruments and machines were built to aid in producing accurate visual representations, it became clearer that what was required by the morality of self-discipline was better accomplished by machines than by scientists and illustrators. Hence, selfless, soulless, steadily working, mechanical machines gradually became the symbols of objectivity.

Although in the pursuit of being 'true to nature' photographic reproductions were praised for copying phenomena precisely, not all mechanical techniques supplied the perfect similarity that was sought. According to Daston and Galison “What the photograph (along with tracings, smoked glass, camera lucida, and other mechanical devices) offered was a path to truthful depiction of a different sort, one that led not by precision but by automation—by the exclusion of the scientist's will from the field of discourse.” (117) Hence, the primary concern in mechanical objectivity was not accuracy or resemblance but nonintervention. Nevertheless, Daston and Galison note that even after the invention of photography at the beginning of nineteenth century, which came to be the emblem of mechanical objectivity, the debate surrounding objectivity was not over. Mechanical reproduction, although it increased accuracy by automatizing the representation process, failed to “fully” remove human intervention between object and representation. (98) The accountability of photographic evidence—both in science and in

the court room—for instance was brought into question since photographic images are susceptible to the locations of the instruments as well as the duration of exposures, and therefore subject to distortion. (110) Moreover, the issue of demarcating the normal from the pathological phenomena in nature was a big challenge for those who advocated the mechanical registration of images of individuals. The question was "how would one distinguish between variations within the bounds of the 'normal' and variations that transgressed normalcy and entered into the territory of the pathological." (107) For some, the solution to this problem was to emphasize the importance of rare deviations in delineating what is normal. Hence it was argued that phenomena should be illustrated with all their imperfections. Yet, many atlas makers, such as Rudolf Grashey, argued that a single representation could not depict the great variations in nature. They suggested the need for illustrating multiple instances of the same phenomenon. Each individual instance, it was hoped, "would evoke a class of patterns in the mind of the reader". For instance, Grashey published a series of skull X-rays that illustrate "the far reaches of the normal and thereby demarcate the normal from the pathological." (105) By doing so, he shifted the responsibility of classification from the author to the audience. Daston and Galison write "while in the early nineteenth century, the burden of representation was supposed to lie in the picture itself, now it fell to the audience. The psychology of pattern recognition in the audience had replaced the metaphysical claims of the author. Mistrusting themselves, they assuaged their fear of subjectivity by transferring the necessity of judgment to the audience." (107) But this move merely replaced the subjectivity of the scientists with the subjectivity of the audience. It could not do away

with subjectivity all together. This point is important for our discussion as it constitutes an early example of an attempt to include the audience as an active participant in both making and assessing knowledge, which has often been overlooked by mainstream epistemological projects, yet is underlined by many feminist theorists. I shall revisit this issue in the following chapters.

Although shifting the responsibility to the audience in depicting phenomena was a novel idea, it was ineffective in the face of mechanical reproductions such as x-rays, apprehension of which required the trained eye of an expert to detect *relevant* information. It was also true of the mechanical production of mathematical pictures such as graphs which came to be widely celebrated in the late nineteenth century in the name of objectivity. Graphical expression was judged universal as it transcended the limits of natural language and it could "cut across disciplinary boundaries to capture phenomena as diverse as the pulse of a hearth and the downturn of an economy." (116) Yet reading such graphs also demanded trained judgment.[11] The emphasis on the universal applicability of these representations also signaled the emergence of a new ideal, the ideal of aperspectivity, in achieving objectivity.

In distinguishing mechanical objectivity from aperspectival objectivity, photography as an emblem of mechanical objectivity can help us appreciate the subtle

[11] Kukla notes that in *Objectivity* Daston and Galison distinguish "trained judgment" as the governing norm that dominated the project of representation in the twentieth century. Yet this can be seen as a recurrence of the ideal of "truth to nature" sought in idealization where insights of scientists were crucial. However, as Kukla quotes from *Objectivity*, "the sage revealed the true image of nature, and the trained expert possessed and conveyed to apprentices the means to classify and manipulate." (Daston and Galison 2007, 332)

differences between them. In aperspectival objectivity, not only the inner temptations of individual scientists but also the physical, social, biological and cognitive structure and limitations are seen as impediments to objectivity. A photograph, on the other hand, although it produces almost an exact copy of what is captured is not without perspective. In fact, perspective is vital for photography. Hence, the sort of non-intervention achieved by mechanization does not guarantee aperspectivity, which would demand transcendence of all idiosyncrasies and locations of subjects.

Daston and Galison are careful to note that the aim of acquiring universal knowledge through such de-individualization and emotional distance in the nineteenth century was not a novel attempt; it was also pursued in the eighteenth century. But, according to Daston and Galison the objects of concern were different in each century. In the eighteenth century, discussions about the transcendence of individual perspectives were not about understanding or describing the natural world, but occurred mostly in moral philosophy and aesthetics. "Subjective" (meaning private) perspectives started to be denounced in these realms, not in the ontological realm. Moreover, they claim that it was in these domains that values such as detachment, impartiality, disinterestedness, self-effacement made their way into our conceptualization of objectivity (Daston 1992, 603). Eighteenth century moralists deemed transcendence of individual viewpoints necessary for a *just society*, whereas such transcendence was deemed necessary for a *coherent scientific community* in the nineteenth century. The existence of a coherent scientific community, for some philosophers of the period, was a precondition for attaining truth. (607) Now, if this historical account is plausible, it constitutes a problem for the modern

coupling of aperspectivity with the natural sciences. That is, if aperspectival objectivity, which is the prevailing understanding of objectivity today, first appeared in moral philosophy and aesthetics, only then to be transferred to the natural sciences, then the authoritative status attributed to the natural sciences on the grounds that they are the only domains which are (or which sought to be) governed by the ideals of disinterestedness, detachment or impartiality is highly questionable.[12] In other words, it is a significant challenge for the worldview in which the natural sciences constitute the paradigm of knowledge.

1.2.2.3 Aperspectivity and Communicability

Daston and Galison's discussions of aperspectivity as a governing norm in scientific practice does not address philosophical questions regarding aperspectivity as a view from nowhere which is believed to yield absolute objectivity. Their inquiry into aperspectivity is not concerned with such questions as whether transcending all perspectives is possible. Yet they take *communicability* as a condition for achieving aperspectivity. According to Daston and Galison, since one of the essential requisites for a coherent scientific community is communicability, what the ideal of aperspectivity in science serves is primarily to maintain this communicability. Communicability became a primary concern in science in the nineteenth century as society in general and scientific practice in

[12] It should be noted that the challenges from historicists, sociologists of knowledge, feminists and post-colonial theorists started to shake this understanding of objectivity which confines it to aperspecitivity.

particular went through rapid changes.[13] For example, changes due to technological innovations led to refined divisions of labor within science and to technologies such as postal systems and railways which helped ideas to travel across national boundaries. Communicability became an issue as scientists from different parts of the world and disciplines started to share their views more and more widely. Hence, in Daston's words aperspectivity "became a scientific value when science came to consist in large part of communications that crossed boundaries of nationality, training and skill. Indeed, the essence of aperspectival objectivity is communicability, narrowing the range of genuine knowledge to coincide with that of public knowledge." (Daston 1992, 600) This relationship between communicability and public knowledge is quite important for our discussion of objectivity and deserves further deliberation. In so far as public knowledge is knowledge that is open to comprehension or examination by *all,* it seems to convey a claim to universality. For instance, in my earlier discussion of Kant I have claimed that his conceptualization of objectivity rested on impersonal knowledge secured by the universal capacities of the mind. Yet here, although communicability requires certain capacities, there is no reference to categories of the mind. Instead, there is reference to *impersonal communication* which is presumably secured by following the established procedures or standards in testing, measuring, reading graphs and so forth. In other words, it rests on the standardization of data. Hence, it is reasonable to claim that the impersonality attached to public knowledge here is not a function of the inner capacities

[13] This view is challenged by Jennifer Tannoch-Bland (1997) who claims that the ideal of communicability was important since the time of Bacon. I will examine this objection in detail in Chapter Four.

of subjects, which according to Kant, are universal, but a function of the universal application of procedures shaped through social and historical developments. The idea about the universal application of procedures that yield knowledge has important consequences for the status of scientists. What the impersonalization of communication enables is that any observer can reach the same results by following the universally accepted procedures which render the individual qualities of scientists redundant. Hence Daston writes "aperspectival objectivity was the ethos of the interchangeable and therefore featureless observer—unmarked by nationality, by sensory dullness or acuity, by training or tradition; by quirky apparatus, by colorful writing style, or by any other idiosyncrasies that might interfere with the communication, comparison and accumulation of results." (609) Note how the idea of interchangeable observers contrasts sharply with the earlier practices where observational reports were evaluated in terms of the skill and integrity of the observer. As we saw in Dear's discussion, the skill and character (trustworthiness) of the individual scientist or reporter (as well as their social status) provided warrant for the content of the report. After the appearance of the ideal of interchangeable observers, disinterestedness secured by the personal traits of scientists was no longer a sufficient measure to vindicate the content of an observation. Moreover, although perspectival distortions prompted by self-interest might be prevented by, say, the application of the ideal of disinterestedness, it still does not secure aperspectivity, for there might be distortion with no self-gain, say because of mere prejudices.

The emphasis on interchangeable subjects also gives us clues about the insistence of avoiding subjectivity in the mainstream epistemological project, where necessary and

sufficient conditions for knowledge are sought by abstracting subjects from the analysis.[14] However, this historical account should remind us that the ideal of aperspectivity that is premised on interchangeable subjects is just one form of objectivity which came about as a result of particular social and historical developments and needs. It is also worth noting that adopting this aperspectivity was not without cost. For instance, although the ideal of aperspectivity contributed to the accessibility to and mobility of knowledge, it also led to the loss of practices that yield valuable information. Practices that were once an integral part of observation reports but could not be communicated were dismissed, as they did not conform to the ideal of aperspectivity. Daston's example of the favoring of sphygmometer over the human pulse reader is a good indicator of a case where an old practice, i.e. human pulse reading, is abandoned in favor of a new practice which exhibits results that the human pulse reader cannot communicate. Another similar example would be replacing midwives with doctors and technological apparatuses. Vrinda Dalmiya and Linda Alcoff in their article "Are 'Old Wives' Tales' Justified?" (1993) have an elaborate account of how (practical) "knowledge" of midwives was left outside the domain of "proper" knowledge which is "propositional". Modern instruments display the uterus in detail and detect any (possible or actual) problems, yet midwives could spot these problems and fix most of them without these tools. For example, they could turn the baby in the womb to avoid breached deliveries; they could perform abortions; hasten prolonged labor; reduce the pain of childbirth and cure breast infections. (Alcoff and Potter 1993, 222) Midwives'

[14] For a detailed analysis of the mainstream epistemological project and abstract subjects see Code (1993).

knowledge rested mainly on their *direct personal experience* of childbirth as well as their experience of delivering babies from a young age on. Another important source for their knowledge is the shared information and stories of difficult births among themselves. In other words, "hearsay" played a crucial role in midwives' knowledge accumulation. This factor indicates that midwives often communicated with each other, but this form of communication did not fulfill the standards for the modern ideal of "communicability" which rested on the impersonality of data. The very factors, such as being oral, practical and experiential, which made midwives' knowledge possible were also the reasons why their knowledge *could not* be communicated (or could not be communicated in the form of propositional knowledge). However, one might argue that the practical knowledge of midwives could be transferred into propositional knowledge as instructions provided in manuals of certain instruments. Yet this argument can be dismissed simply by pointing out the difference between knowing how, say, a bicycle works and how to ride a bike. Dalmiya and Alcoff are careful to note that, this claim about the incommunicability of midwives' knowledge is not merely a report of the illiteracy of most midwives and their failure to codify their skills. (224) They write: "a crucial aspect of a midwife's skill was her capacity to be empathetic and sensitive to the situation of her patients as well as to allay their fears and inspire them to have forbearance and hope." (225) In other words, midwives could identify with the expectant mother, and this experiential knowledge could not be transmitted through impersonal propositions. (225) Dalmiya and Alcoff argue that because midwives' knowledge cannot be forced into "the S knows p schema", it was dismissed from "real" epistemology. Hence, if we turn back to Daston's and Galison's

historical account, in the ethos of aperspectivity (which is prominent in mainstream epistemology), mechanically produced statistical reports that are immediately accessible to others replaced skills, intuitions, inspirations which are intangible, hence could not be totally explained and communicated. (Daston 1992, 612)

In sum, according to Daston's and Galison's historical account, aperspectivity in science as an ideal for achieving objectivity was sought by impersonalizing the procedures and standards for establishing communicability in order to preserve continuous dialogue among scientists. It should be noted however that here there is no direct reference to truth in the sense of correspondence to nature. In a-historical philosophical discussions, on the other hand, it is often assumed that the impersonalization of scientific method (including the procedures and standards that are followed) leads to true representations of phenomena. We shall see examples of such philosophical positions in detail in the next chapter. Moreover, there is a line of thought which associates aperspectivity with a view from nowhere that demands impersonalization through self-effacement to attain absolute objectivity where the world is represented as it is in its totality. Whether and how such impersonalization could be achieved prompted lively discussions among philosophers in the twentieth century. One of the leading contributors to these discussions, who made famous the phrase "the view from nowhere", is Thomas Nagel.

1.3 Thomas Nagel and the View from Nowhere

Although Nagel clearly rejects the possibility of attaining absolute objectivity, he retains the idea that objectivity is a method for understanding the world that involves self-effacement. His main idea is that an objective view of the world has to include everything about the world including the perspective of the viewer. Nagel explains this method of objectivity as follows:

> To acquire a more objective understanding of some aspect of life or the world, we step back from our initial view of it and form a new conception which has that view and its relation to the world as its object. In other words, we place ourselves in the world that is to be understood. The old view then comes to be regarded as an appearance, more subjective than the new view, and correctable or confirmable by reference to it. The process can be repeated, yielding a still more objective conception. (Nagel 1986, 4)

In this picture objectivity comes in degrees, because the objectivity of a view is achieved through the gradual elimination of subjective specificities such as an individual's character or position in the world. In other words, the more impersonal a view is, the more objective it becomes. Nagel acknowledges that we cannot have an absolute picture of the world since we are limited beings. Moreover, he accepts that there are certain aspects of the world of which a subjective view provides better understanding than an objective view. I suppose what he means is that since appearances and perspectives are also part of the world and since an objective view is attained by detaching oneself from one's specificities as much as one could, in trying to understand the part of the world that is about our appearances and perspectives themselves, we are better off with a subjective view than an objective one.

However, there is no reference to the possibility or the extent of such detachment, in Nagel's discussion. We supposedly acquire a more objective view when we reflect on our perspectives and their relation to the world. But surely, mere reflection does not automatically yield objectivity. One needs to have a certain disposition for a "fair" inquiry into one's views. That is, one needs to have the patience to examine one's view from every possible angle to the extent that one can. Moreover, one needs to have the courage to follow up the consequences of one's views as well as an openness to modify them if need be, and the appropriate training to do it well. Nagel might respond that all of these requirements are in principle possible within the method of objectivity he is defending. It might be plausible, in fact, to think that one can transcend one's specifities and come to understand the world which includes other perspectives and the relationship between one's views and the world within a rationalistic tradition which is reinforced by a commitment to autonomous individual reasoning. But how could we make sense of this method within an empirical tradition where not only the limitation of human faculties but also the fallibility of reasoning, sense perceptions and so forth are pressing issues? Nagel claims that our new conception of the world (or an aspect of it), which also includes our old conception, is more objective. But he does not discuss how reliable this method is. What reason do we have to believe that our (individual) reasoning, i.e. stepping back (detachment) and thinking about our own view, will yield an objective view? Surely our faculties are limited, hence we cannot get the whole picture, but accepting this fact is not the same as accepting that our reasoning might be faulty. In the case of faulty reasoning which leads not just to a limited conception of the world but to a distorted one, what is it

in this method that would correct our conception? In the case of distorted conceptions or representations, could we still talk about the objectivity of our view of the world? Nagel could claim that even in the case of distorted views we can talk about objectivity since it is the method that counts, not what it produces. Yet, the idea of an objective method that could yield a distorted conception of the world contradicts "objectivity" as an ideal. A method is basically a means to acquire a certain end. If the end that the method produces is undesirable, the method is discarded as either faulty or useless. A distorted conception of the world is obviously not a desirable end. Hence, a method that could produce a distorted conception of the world is far from being an ideal. Reducing objectivity to a methodology which rests on an ungrounded optimism about (individual) reasoning results in such contradictions.[15]

Furthermore, in presenting this method as *the* method of objectivity, Nagel overlooks the different uses of ideals of objectivity that I have been discussing. However, I do not wish to claim that this method of self-effacement is futile altogether, as it would also be overlooking important tasks that are carried out in scientific practice by following it. It is not contradictory to maintain that certain ideals might contribute to the objectivity of science when applied to certain aspects of scientific practice while they are not sufficient to secure objectivity when applied throughout the practice. In fact, they might even hinder objectivity when they are not applied in appropriate places. For instance, as I discuss above, self-effacement is the guiding principle in achieving mechanical

[15] I will revisit Nagel's views when discussing feminist attacks on individualist epistemologies in Chapter Four.

objectivity. In "The Scope and Limits of Scientific Objectivity" (2004) Joseph F. Hanna argues "[s]cience makes objective progress when decision procedures requiring subjective human input are replaced by decision procedures that are automatic or mechanical." (Hanna 2004, 341) However, he adds that this understanding of objectivity does not imply that human reason or human communicative discourse have no role in objective science. According to him communicative discourse involves conscious reflection, application of epistemic and social values, creative elaboration of analogies and metaphors, hence it reflects human interests, values, perceptions and intuitions. Hanna argues that scientific progress is not simply about the elimination of communicative discourse, but in raising the level of that discourse. (341) It is in this higher level that judgments of i) whether artefacts (products of automation such as meter readings, digital outputs of recording instruments, photographic plates) count as evidence for or against a given theory; ii) whether or how the experimental context that has produced those artefacts ought to be modified is discussed. In this understanding the automation (objectification) of lower level decision processes makes possible more complex communicative discourse at a higher level. (342) Obviously we can question the "objectivity" of higher-level discourse: to what extent the assessments of evidence are (can be/ ought to be) disinterested or impartial? The point is, though, although we can claim objectivity in lower level decision processes by invoking self-effacement guaranteed by automation, this lower level objectivity is not sufficient to generate objectivity throughout scientific practice where higher-level communicative discourse is an essential part. In other words, specific ideals

that guide lower level discussion processes are often inappropriate and/or not efficient in guiding higher-level communicative discourse.

1.4 Conclusion

In this chapter my aim has been to understand how distinct governing norms of scientific practice, which have changed throughout history, accommodate different ideals of objectivity such as impersonality, disinterestedness and impartiality. We have seen that as the needs, the focus of concerns, and the means of research changed in science, these ideals took on different meanings and were directed at different aspects of scientific practice. Moreover, objectivity has been discussed in terms of the ways in which objective judgments are reached as well as what those judgments are about. Hence objectivity has been attributed sometimes to the methodology and sometimes to the object of a study. As a result, different forms of objectivity occurred. However, the relationship between different forms of objectivity has often been left obscure and/or rested on unexamined assumptions. I believe a comprehensive account of objectivity should not leave these assumptions unexamined. Moreover, such an account should take the diverse applications of ideals of objectivity throughout science. Perhaps this approach is easier to follow in a position where science is viewed as a practice rather than interrelated cluster of theories. In the next chapter we will see how some of the dominant schools of thought in the philosophy of science have ended up with limited conceptions of objectivity because they have viewed science in a certain way which caused them to disregard the diverse character of objectivity. As a result their theories have failed properly to address the real

questions that occur in actual scientific practice, and concerns regarding the relationship between science and everyday life. Ultimately, in this dissertation, I will argue that a feminist understanding of objectivity (and science) can both accommodate the diverse character of objectivity and provide substantial guidance for scientific community.

Chapter Two

The Received View of Scientific Objectivity

The examination of the historical development of ideals such as impersonality, disinterestedness and impartiality in the previous chapter led us to the conclusion that these ideals were pursued and manifested in different ways throughout the history of science depending on the prevailing concerns and projects in representing and understanding nature at the time. In this chapter I turn to the philosophy of science to look at how these ideals operate in the philosophies of the dominant schools of thought. I want, for instance, to examine what elements in their philosophy have generated the ideal objectivity which in post-positivist times has come to mean a detached, neutral and disinterested approach to a subject matter that exists in a publicly observable space, separate from knowers/observers and making no personal claims on them. As we shall see, these schools of thought often overlooked the layered character of objectivity and thus failed to provide a sufficiently comprehensive account of it. It is important to note that this shortcoming is not merely a conceptual failure but has important consequences in our daily life since scientific conceptualizations inform future research programs that form public opinion.

The philosophy of science as a distinct sub-discipline of philosophy emerged in the nineteenth century, mainly with the rise of positivism.[1] Among other schools of

[1] This claim, however, needs to be qualified. Historically, philosophy and science have gone hand in hand, at least up to the eighteenth century. Many of the major figures in

thought with positivist tendencies, logical positivists and logical empiricists stand out as major sources of influence in the philosophy of science. The boundaries of the discipline, that is, the concerns and questions about science that are worthy of a "philosophical" investigation were delineated mostly by members of this school of thought. However, it is hard to talk of positivism as a doctrine with strict boundaries and set principles. It can be better understood as a frame of mind or spirit with a certain humanistic turn away from the dogmatism of preceding theology and metaphysics. In fact, as a legacy of the Enlightenment, positivism was a progressive movement. The key idea of positivism can be summarized very broadly as 'seeing is believing'. That is, we can only know or claim to know things that we can observe. It is this key idea that underlies the positivistic attitude towards epistemology, metaphysics and semantics. Yet, this attitude is manifested in diverse ways within different schools of thought.

Initially, "the philosophy of science" as a distinct discipline was not comprised of a comprehensive critical examination of "science", as its name would suggest. Rather it was an investigation of "scientific knowledge". Establishing the philosophy of science as a study of scientific knowledge reflects the positivistic tendency of reducing science to its product, i.e. to scientific knowledge, where the focus is on the method of validating such knowledge. This tendency is clearly manifested in the distinction between the context of

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philosophy, such as Descartes, Pascal and most of the Enlightenment thinkers were also scientists. Those ancient Greeks who inquired into nature were also called natural philosophers. Moreover, concerns about the aims and methods of inquiry into nature go as far back as Aristotle's *Posterior Analytics*. In the eighteenth century science and philosophy started to move apart, and came to be regarded as distinct enterprises.

discovery and the context of justification.[2] Although it has often been ambiguous which features of scientific practice constitute the context of discovery and which features constitute the context of justification, it can be claimed that the main aim of this distinction was to separate practices that involve subjective elements such as personal backgrounds, characteristics, decisions, values and inspirations in constructing the theories from the objective relations between hypotheses and evidence that validate them. It was argued that a philosophical inquiry into science should be concerned with the context of justification since the context of discovery was not an appropriate subject for a logical investigation. One consequence of this approach was to reinforce the view that the only knowledge worthy of the name is knowledge achieved by scientific methods: a view which narrowed the scope of epistemological investigations to a great extent.

The general purpose of this chapter is to show how different approaches to science inform i) subject matters and concerns of "the philosophy of science", and ii) the meanings attributed to fundamental scientific concepts, especially 'objectivity'. I will examine the basic ideas of certain different traditions in the philosophy of science in an attempt to understand how these ideas affect (or could affect) their conceptualizations of objectivity in science. I will start off with what I perceive to be the received view and its understanding of objectivity, and then will proceed to its critique. [3]

In order to have a clear grasp of some of the ideas that have a bearing on objectivity, a chronological exploration is useful. Yet the development of the ideas that I

[2] This distinction was first introduced by Hans Reichenbach.
[3] I will focus on feminist critiques in Chapter Three and Chapter Four.

shall examine should not be seen as linear. Although the importance attributed to specific themes changes from time to time, the ongoing literature continues to contribute to the development of these themes. Since thematic discussions in the philosophy of science (like many other philosophical inquiries) are not conclusive, it is often difficult to set sharp boundaries among various philosophical positions.[4] That is why it would be misleading to talk of a static received view established by a certain philosophical school. Rather, I shall try to formulate the received view in a dialogical fashion, where I will examine one philosophical position in comparison to another. I shall start off with logical positivism and logical empiricism.[5]

2.1 Logical Positivism, Logical Empiricism and Objectivity

Logical positivism, which dominated philosophy of science in the early decades of the twentieth century, was initially founded by members of the Vienna Circle, who included Moritz Schlick, Rudolf Carnap, and Otto Neurath, as a reaction to the unsolvable riddles and metaphysical excesses of traditional philosophy.[6] It quickly drew the attention of philosophers from other parts of Europe (and later from the USA) such as, Hans

[4] Even if the basic principles of a position can be distinguished for practical purposes, labeling one philosopher as an advocate or an opponent of a certain position is not easy, as thinkers rarely subscribe to a position in its entirety, i.e. they agree with some aspects of a position while rejecting some other aspects of it. Or they may not subscribe officially or consciously to any position at all, but are assigned to such and such a position by their readers/colleagues/other associates.

[5] In my discussion of logical positivism and logical empiricism I focus on a standard history of these positions. I do not consider the more recent distinction between "The First Vienna Circle" and the "Left Vienna Circle."

[6] Other members of the circle included Hans Hahn, Karl Menger, Frederich Waismann, Kurt Godel and Herbert Feigl.

Reichenbach, Alfred Jules Ayer, Carl G. Hempel and Karl Popper, who became involved in the critical exchanges the circle had generated.

Before proceeding further, let me clarify an important point in delineating this influential philosophy. Logical positivism and logical empiricism are often treated as alternative names for a single position. However, for some philosophers logical empiricism marks a distinct turning point in the logical positivist programme.[7] For instance, Gary Hardcastle argues that logical empiricism was developed out of critical discussions of many logical positivist themes, leading to the rejection and dismissal of some principles while promoting the refinement of others, as in the transition from the verifiability criterion of cognitive meaning to the confirmability criterion.[8] (Hardcastle 2006) Constant exchanges and adjustments of ideas sometimes resulted in new approaches to the same problems and sometimes to a complete shift of focus onto different problems. Accordingly, it is difficult to pinpoint when exactly logical positivism ceased and logical empiricism started.

In their search for clarity, logical positivists argued for a scientific worldview that should prevail in every area of philosophy.[9] Their insistence on such a scientific worldview enabled them to take a dismissive stance towards any subject matter that could not be subjected to observation and verification. The clarification of traditional

[7] There is also a distinction between early and late logical empiricism in this literature.
[8] Another philosopher who marks the beginning of logical empiricism with the inquiry on confirmation theories is Wesley Salmon.
[9] They also had a social and political agenda in their appraisal of the scientific worldview, which is often overlooked. Hence they did not merely pursue a negative task of dismissing metaphysics.

philosophical problems was pursued either by attempts to transform them into empirical problems or, when such transformation was not possible, by discarding them as pseudo-problems. As a result, metaphysics and its problems were brushed aside.

In following this programme, logical positivists were motivated by developments in both science and logic. Positivist convictions grew stronger, as earlier metaphysical speculations about empirical matters were gradually proved wrong by physics and biology. The birth of theories of relativity and quantum mechanics, for instance, called Newtonian mechanics and its metaphysics into question. Yet, as science became more and more abstract, a formal systematization came to be required in order to clarify theoretical concepts and to establish their connection with observation. Some positivists found the resources for this project in the quantificational logic developed by Gottlob Frege, Alfred NorthWhitehead and Bertrand Russell.[10]

Logical positivists were committed to the empiricist conviction that experience and observation are the only ways to learn anything about the world. Yet, unlike classical empiricists, they were not interested in the questions of how we come to know things. They were rather concerned with the formal processes by which knowledge claims are validated. Hence, they combined the mathematical method of demonstration championed by rationalists and the experimental method supported by empiricists. While the mathematical method of demonstration (i.e. logical deduction) provided the *form* of reasoning, experimental investigations provided the *content*. In the following we will see

[10] They were also greatly influenced by Ludwig Wittgenstein's *Tractatus Logico-Philosophicus*.

that these two methods presupposed different forms of objectivity. The method of demonstration aimed at a form of objectivity that is secured by an impersonal method. The objectivity of the content, on the other hand, was assessed with respect to truth, relevance and testability. As we shall see, the ideal of impersonality that assists the method of demonstration failed to guide assessments of truth, relevance and testability which required experimental investigations. I will argue that overlooking this point leads to a misleading conception of objectivity which ties it to impersonality, disinterestedness, neutrality and impartiality all at once. As I have discussed in the previous chapter, these are distinct ideals and their simultaneous application sometimes yields contradictions. The best way to elaborate on the two forms of objectivity guided by different ideals is to examine the main areas of concern for logical positivists such as the principle of verification, scientific explanations and confirmation.

2.1.1 The Verifiability Theory of Meaning [11]

In distinguishing science from pseudo-science and discarding metaphysics from philosophical inquiry, logical positivists sought a formal criterion to distinguish cognitively significant statements from meaningless ones. To that end they appealed to the idea of empirical significance as well as the idea of analyticity that separated analytic statements from synthetic ones. Although there are different definitions of analyticity, basically analytic statements are statements that are true by virtue of the meaning of the

[11] In the literature, this theory is also referred as the "verification theory of meaning".

words contained in them.[12] Hence, they can be known a priori. Understood as such, adopting analyticity enabled logical positivists to make room for the knowledge of certain truths such as mathematical truths or logical theorems in an empiricist tradition. The empirical significance of non-analytic statements, on the other hand, was determined by verification.[13]

Although the idea of verifiability can be found in Hume's philosophy, the search for a *formal* criterion was one of the major concerns for logical positivists. There have been different formulations of the verifiability principle of meaning, all of which have run into various problems.[14] However, the basic idea underlying the principle was to confine cognitive meaning to the statements which can be empirically verified, thereby marking every other statement as literally meaningless.

Among the problems with this principle were questions about the status of the principle itself (i.e. whether it is an empirical claim or not), and the adequacy of the principle as a means of distinguishing meaningful statements from meaningless ones. Specifically, when statements of general laws are considered, the principle was doomed to fail. Statements of generalizations were not verifiable because it was impossible to observe phenomena in their generality. In other words, one cannot observe each and every instance of any phenomenon. Some philosophers, such as Ayer, tried to overcome this

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[12] Immanuel Kant, Bernard Bolzano (1973), Gottlob Frege (1974) and Rudolph Carnap (1937, 1956) provided different definitions for 'analyticity'.
[13] Analytical propositions are devoid of content and they owe their validity to linguistic conventions associated with the meaning ascribed to the terms involved in them.
[14] Carnap (1967), Schlick (1936), Ayer (1936), Carl Hempel (1950) devoted attention to developing a criterion of cognitive significance.

problem by distinguishing ‘strong’ and ‘weak’ senses of ‘verifiable’. According to this distinction, a proposition is verifiable in the strong sense if and only if it could be conclusively shown to be true by experience. Whereas, a proposition is verifiable in the weak sense if experience could render it probable. Ayer argues that conclusive verifiability would set too strong a standard for empirical significance. (Ayer 1990, 18) It was adopting this strong sense of verifiability that rendered the statements of general laws (as well as statements about the past) meaningless, as their truth could not be established conclusively. In contrast, Ayer was convinced that a weaker principle should be adopted. Yet, formulating this weak sense proved to be difficult. For instance, one formulation Ayer suggested for a “weak” verifiability principle was that a statement is verifiable if an observation-statement, which expresses an actual or possible experience, “can be deduced from it in conjunction with certain other premises, without being deducible from those other premises alone.” (179) Here, a statement itself need not be an observational statement in order to be meaningful or empirically significant. Under this version of the principle neither statements of general laws nor statements about the past are denied significance. For example, a general statement such as ‘All men are mortal’ can be shown to be meaningful, because an observation statement such as ‘Socrates is mortal’ can be deduced from it together with ‘Socrates is a man’.[15] However, Ayer admitted that this weak formulation of the principle is too flexible since it could be applied to any statement

[15] Here of course we assume that being mortal is not contained in the concept of a man. If it were, ‘Socrates is mortal’ could be deduced from ‘Socrates is a man’ by itself, which would reduce the application of the verifiability principle to the general statement ‘All men are mortal’.

whatsoever, thereby rendering the principle redundant. For instance, according to this formulation a general statement such as 'All beings have souls' should be granted empirical significance since an observation statement such as 'It is sunny' can be derived from 'All beings have souls' in conjunction with the premise that 'If all beings have souls, then it is sunny'. The difficulty in formulating a verifiability principle which is neither too strong to undermine the meaning of general statements that are significant in science, nor too loose to grant meaning to any metaphysical statement, is one of the reasons why the logical positivists turned away from this project.[16]

Other important factors that undermined the verification criterion of meaning were Willard Van Orman Quine's attack on the analytic-synthetic distinction, and Hempel's challenges to the sharp distinction between cognitively significant sentences and those that are not significant in this sense. Following these developments, both the rigid verification principle of cognitive meaning and the positivists' strict opposition to metaphysics were gradually abandoned. The positivist programme took a different turn and the topics of concern changed. For instance, philosophers such as Hempel (1965), Ernest Nagel (1961) and Richard B. Braithwaite (1953) turned to concentrate on the logic of scientific explanation. The status of theoretical entities and the question about the aims of theories later became central issues.

[16] For other difficulties Ayer mentions about formulations of verifiability principle see Ayer (1990, 176-185). Also for detailed discussion of these formulations and criticisms, see Carnap (1967), Schlick (1936), Hempel (1950), Reichenbach (1938), Alonzo Church (1949), Isaiah Berlin (1939).

2.1.2 Scientific Explanations and Objectivity

The earlier positivists had observed that in the earlier philosophical literature, exploring 'why' a phenomenon occurs required a metaphysical or theological explanation. Such explanations, however, rested on assumptions about vital forces and purposes embedded in objects, or divine will in occurrences of events, none of which can be investigated by empirical means. They endorsed Hume's rejection of causality where he claims that when we assign a causal relation between two events we do not see causality, what we actually observe is one event following another. Since the idea of causality rests on the existence of some unobservable occult powers, the rejection of causality by positivists followed from the verification principle. Hence, in the positivist tradition statements about causes of events came to express *observed regularities* that took place in the world rather than metaphysical relations. Accordingly, the laws of nature, which were previously believed to reveal the *causes* of events, became mere *descriptions* of events. However, since not all descriptions of phenomena constituted scientific explanations, an inquiry into the characteristics of acceptable scientific explanation increasingly occupied such philosophers as Popper (1965), Hempel (1948), Nagel (1961) and Braithwaite (1953).

The logical empiricist approach to scientific explanations was to regard them as arguments. Before examining the relationship between this approach to scientific explanations and objectivity, I should note that in their discussions of scientific thinking, the logical empiricists do not explicitly state how they conceptualize objectivity. Given their rejection of talking about the world as it really is independent of human observation, it is fair to claim that they did not adopt an understanding of objectivity where scientific

statements represent the world as it is. Their emphasis on methodology, on the other hand, is an indication that objectivity, in their view, is mainly a matter of method. But it is often left vague what features of the method secure or yield objectivity. Moreover, it is not always clear which form of objectivity they sought.

Typically an explanation consists of two parts: the explanandum and the explanans. The explanandum is a particular (an event) or a general fact (a law of nature) that is to be explained. The explanans, on the other hand, is or are particular or general facts that do the explaining. In the terminology of logic, while the explanans constitutes the premises, the explanandum is the conclusion. For instance, Hempel's and Paul Oppenheim's (1948) once highly influential model of scientific explanation, namely the deductive-nomological (D-N) model, suggested that explanations of phenomena are possible when the facts to be explained can be subsumed under a universal law. According to this model, a particular fact is explained when it is logically deduced from a universal law.[17] If, on the other hand, the explanandum is an observed regularity in nature (or a law of nature), then it is deduced from laws of a broader scope. The D-N model, however, is neither necessary nor sufficient to determine whether an explanation is scientific or not, as there are other forms of law such as probabilistic laws that many scientific explanations rest on. In an explanation that makes use of a probabilistic law, the explanans does not deductively imply an explanandum statement; hence an explanans

[17] It should be noted that not all statements of universal form are universal laws. Among other characteristics, for a statement to be a universal law it should be true (or there should be good reasons to believe that it is true), it should support counterfactual conditionals, and so forth. For a detailed discussion of this issue see Hempel (1966).

does not yield "deductive certainty" but only high probability. In other words, in the case of probabilistic laws, contrary to the D-N model, if the explanans is true, the explanandum is not invariably true.[18] However, the matter of what constitutes a probabilistic law is complicated. Simply put, while a statement of a universal law expresses that "in *all* cases where conditions of kind F are realized, conditions of kind G are realized as well"; a law of probabilistic form asserts, basically that "under certain conditions, constituting the performance of a random experiment R, a certain outcome will occur in a specific percentage of cases." (Hempel 1966, 66) Hence, testing a statement that expresses a probability becomes a matter of examining the "relative frequency" of the expected outcome in long series of repetitions. As mentioned above probabilistic explanations are not logically conclusive. That is to say, although the explanans is true, the explandum might still be false. Now, if observed frequency is crucial for accepting or rejecting a probabilistic statement, then the question of the criteria on which such decisions are based becomes a pressing issue. In fact, Hempel stresses that the following questions should be answered in assessing statements of probability: a) what deviations of observed frequencies from the probability stated by a hypothesis are to count as grounds for rejecting the hypothesis? b) how close an agreement between observed frequencies and hypothetical probability is to be required as a condition for accepting the hypothesis? Hempel claims that the answers to these questions will be of a contextual sort, that is, they will depend on what is at stake. (65) This view undoubtedly has important implications

[18] For a detailed discussion of scientific explanations see Wesley C. Salmon (1990).

for our discussion of objectivity and I will return to it in my discussion of the requirement of relevance for scientific explanations.

Now let me remark on the relationship between the positivist emphasis on the *form* of scientific explanations and objectivity. In the history of science, mathematical and logical thinking have long constituted the domain of impersonal and disinterested knowledge. Hence, it can be claimed that approaching scientific explanations as arguments is closely related to the objectivity of explanations. However, the so-called disinterestedness of logicians that is being aspired to is essentially a result of the subject matter that they deal with, which is the *form* of thinking. Scientific explanations however are intrinsically related to content and should therefore go beyond mere form. After all, not all deductively valid arguments provide scientific explanations. Furthermore, some biased conclusions can be validly deduced from biased premises. The logical form of a scientific explanation by itself is not sufficient to yield objectivity (at least the kind of objectivity we seek in science). That is to say, we need to look beyond the impersonality of logic for the objectivity of explanations. If the empirical content of the explanans has an important bearing on objectivity, then we are urged to ask what guarantees the objectivity of the content of scientific explanations (which is a question of semantics but not syntax). The empirical (semantic) aspect of the D-N model, however, is confined to the truth of the premises. In other words, in order for the D-N model to be an adequate explanation, among other things, the sentences constituting the explanans must be true (or highly probable). In thinking about objectivity, since the *form* of explanations is

insufficient to guarantee objectivity, this condition of truth might be the vehicle for arriving at the objectivity of scientific explanations.

This interpretation, however, is problematic. For one thing, true premises of a valid argument do not suffice to provide a scientific explanation (let alone an objective one). For instance, the conclusion that John does not get pregnant can be deduced from the premises that i) he has regularly been taking birth control pills, and ii) anyone who regularly takes oral contraceptives will avoid getting pregnant. Yet this derivation—although it is valid—does not constitute a scientific explanation.[19] But more importantly, there might be cases where true premises could entail some widely accepted (or even "true") conclusions, yet the resulting explanation can hardly be accepted as "objective". For instance, the failure of a certain group of people to accomplish a task might be explained by appealing to a statistical regularity between a certain biological constitution common to that group of people and certain behaviors. Sometimes in such cases even if the numbers are correct, it is hard to claim that they provide an "objective" explanation for the incompetency of the group of people under investigation. This is because, it is always possible that some information may have been neglected either because of mere ignorance or because of some bias i) for a worldview or ii) against the group of people being investigated. For instance, the relevant effects of social circumstances on one's biological constitution might be overlooked because of a commitment to biological determinism and/or liberalism coupled with underlying racist, ethnicist or sexist assumptions.

[19] This example is taken from Salmon (1999).

Hempel distinguishes two requirements which are significant for our discussion of the objectivity of scientific explanations. They are the requirement of *explanatory relevance* and the requirement of *testability for scientific explanations*. The requirement of explanatory relevance is meant to assure that the explanatory information offered is relevant to the phenomenon under consideration, so that it could provide good reasons for expecting that the phenomenon will actually occur. The requirement of testability, on the other hand, assures that the information provided has objective explanatory power. An explanation can have no explanatory power if the information provided could not be subjected to empirical testing, i.e. if it is devoid of empirical content. (Hempel, 1966, 48-49) Hempel's emphasis on "objective" explanatory power is interesting. Here we can ask whether possessing objective explanatory power is enough for attaining objective explanations. If it is enough, what role, if any, does the requirement of relevance play in the objectivity of scientific explanations? According to Hempel, the two requirements are interrelated in the following way: if an explanation fulfills the criterion of relevance it also fulfills the criterion of testability, but not vice versa (49). He argues that the logical form of an explanation secures this interrelation. Now, if it is true that fulfilling the requirement of relevance always entails testability, then any explanation that conveys relevant information will have objective explanatory power. But recall the previous explanation which relates the biological makeup of a group of people to their failure in accomplishing a certain task. In such cases the data provided might be relevant and in some ways testable, yet might fail to yield an objective explanation as a result of ignoring some other relevant data which might have a bearing on the explanation. Clearly, then, having

"objective" explanatory power does not guarantee an objective explanation. We can argue that 'objective' is understood in a specific way when it is attached to testability, where it is about bringing the information to the public sphere so that it can be confirmed or disconfirmed by others. In other words, it is about dismissing what is personal (or private), as it is not testable. With this understanding, testability is associated with impersonality. On the other hand, when we are bothered by the idea that any relevant information could yield objectivity, we are attributing a different meaning to 'objective' than mere testability.[20] This difference indicates that the relationship between relevant information and objective explanations goes far beyond the criterion of public testability.

We are left with the question of what ideals assist us in fulfilling the requirement of relevance. A scientific explanation cannot (and should not) include all possible facts. It necessarily ignores some facts and focuses on others. In some cases, it is easy to draw a line between relevant and irrelevant facts. But sometimes it is not. The case of John's failure to get pregnant is comparatively easy to respond to: it is not a scientific explanation because the explanans is not relevant to the explanandum. Even if John had not taken pills, he would not have become pregnant. Yet, the second case, which is about the failure of a certain group to accomplish a task, is more challenging. As a matter of fact, the question of what constitutes relevant information is always at the heart of the

[20] Here one might argue that testability is a necessary but not a sufficient condition for objectivity. Yet even the necessity of testability for objectivity can become obscure in cases where an explanation requires reports of subjective experiences which are not always testable. In saying this I do not mean to claim that testability (or relevance) has nothing to do with objectivity. But I believe they are not applicable for assessing the objectivity of explanations regarding certain phenomena. Which is to say, they cannot be accepted as universal criteria for objectivity of explanations.

problem in such cases. What is included in or excluded from the explanans has a direct impact on the objectivity of the explanation. Recall that Hempel appealed to context in deciding on the standards that would determine which observations to include and which ones to set aside. In his words, "The stringency of the chosen standards will normally vary with the context and the objectives of the research in question. Broadly speaking, it will depend on the importance that is attached, in a given context, to avoiding two kinds of error that might be made: rejecting the hypothesis under test although it is true, and accepting it although it is false. The importance of this point is particularly clear when acceptance or rejection of the hypothesis is to serve as a basis for practical action." (1966, 65) In short, the decision about which standards to employ will depend on what is at stake in accepting or rejecting a hypothesis. However, taking the context into account contradicts certain positivist principles. For one, it clashes with the ideal of disinterestedness that is praised by positivists, because the determination of what is at stake will inevitably favor one perspective or set of values over others. What this implies is that if the importance of the context is acknowledged, then we are compelled to admit that the ideal of disinterestedness is not always an appropriate guide to objectivity. Moreover, there seems to be a tension between consideration of features such as what is at stake in deciding on hypotheses where social values are likely to interfere, and emphasis on the logic of scientific knowledge which is supposed to erase the impact of social values in scientific practice. Yet without such considerations, logical empiricist formulations fall short of providing meaningful criteria for establishing relevance. This means that however

impersonal the validating processes become, in certain cases the positivist method cannot decisively sweep away biases or interests in the content of scientific explanations.

2.1.3 Confirmation and Objectivity

As I have discussed, establishing the meaning and truth of generalizations was problematic for the logical positivists because the verification principle denied cognitive meaning to general statements. As each version of the verifiability criterion failed in certain respects, philosophers such as Carnap, Neurath and Hahn came to recognize the need for a weaker criterion of meaning. An important shift occurred for the logical positivist programme when Carnap turned his attention away from verifiability to confirmability in establishing cognitive significance.[21] Unlike verifiability where the focus is on whether what a statement expresses can be verified by observation or not, in the confirmation relation the focus is on the observational consequences of statements. This shift enabled logical empiricists to account for statements about future occurrences as well as statements about the past.[22] It also helped them to account for the epistemological status of general laws. In this section my task is to understand how the issue of confirmation is related to objectivity, and which forms of objectivity can be captured by the confirmation theories of logical empiricists.

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[21] This shift, for Salmon, marks the transition from logical positivism to a more ramified logical empiricism.

[22] One objection to verifiability was that it rendered statements expressing events in the past meaningless, as we cannot observe the past. This objection was responded to by the claim that verification is not about actual experience but possible experience. But this response also raised questions about the nature of this possibility, that is, about whether it is a logical possibility or an empirical one.

The logical empiricists' approach to confirmation was along the same lines as their approach to scientific explanation. The confirmation relation was also a matter of the logical relations between sentences, where the focus was on the status of the evidential support that observations provide for hypotheses (either about a particular event or a general law). Logical empiricists believed that there is a single relationship between evidence and hypotheses that applies to all sciences regardless of their subject matter.[23] Hence, philosophers such as Hempel (1965) and Popper devoted their attention to inquiring into the logic of confirmation in science.

According to Hempel's formulation of the hypothetico-deductive (H-D) method of confirmation, for instance, if the observational consequences drawn from a scientific statement (hypothesis) together with the initial conditions and auxiliary hypotheses that inform it fit our observations, then the scientific statement (hypothesis) is confirmed. Such confirmation, however, does not prove the truth of the hypothesis. What the occurrence of observational consequences at best yields is inductive support. Similarly, the failure of observational consequences does not necessarily refute the hypothesis. In the case of false auxiliary hypotheses, the observational consequence would also be false even if the hypothesis tested were true. For instance, testing certain hypotheses requires the use of complex instruments. In using these instruments scientists tacitly adhere to auxiliary

[23] This conviction was a result of their commitment to *the ideal of the unity of science*. This ideal guided logical empiricists in their project of demarcating science from pseudo-science. Logical empiricists sought this ideal in various ways. Among others, unity in method across all of the sciences (social and natural) was argued for. Perhaps, a stronger form of this ideal concerned the language of sciences. Unity of language in sciences was sought by appealing to logical constructions of scientific statements out of basic concepts. For more on this issue see Carnap (1928).

assumptions regarding the reliability of these instruments (i.e. readings of the instruments are precise or the principles upon which these instruments are constructed are accurate, etc.). However these assumptions might be wrong. That is to say the failure of the main hypothesis that is being tested might be due to instrumental error. Accordingly, the failure of observational consequences cannot conclusively refute a hypothesis.

One complication for this model is that the same observational consequence, such as the prediction of a future occurrence, can be drawn from two different hypotheses. If the prediction were observed to be true, it would support both hypotheses. For example, suppose that in predicting the occurrence of an earthquake hypothesis A states 'When there is a sudden release of energy in the earth's crust an earthquake occurs', and hypothesis B states 'Anytime certain specific behaviors of ants are observed right before the earthquake an earthquake occurs'. Now, a statement that expresses an observational consequence such as 'There will be an earthquake' can be derived both from hypothesis A and hypothesis B, in conjunction with the premises 'There is a sudden release of energy in the earth's crust' and 'Ants are moving in a specific way' respectively. In this case, the occurrence of an earthquake would confirm both hypothesis A and hypothesis B. This situation has been referred as the underdetermination of theory by evidence; and it constitutes a compelling problem in the philosophy of science, as it implies that the decision between two rival theories could not be empirically grounded. This consequence led philosophers such as Pierre Duhem, Jules Henri Poincaré and Édouard L. E. J. LeRoy to argue that our choice between two hypotheses, both supported by the same evidence, is a matter of convention. As we shall see below this conventionalist line of thought has

significant import for the logical empiricist programme and its conception of objectivity, but first let me elaborate on conventionalism.

There are various formulations of conventionalism that address different aspects of the scientific method, the details of which I will not discuss here. One formulation, which is important for our discussion of objectivity, relies on the idea that descriptions of the world require a suitable conceptual apparatus (e.g. a geometry or a metric). In some cases—both in empirical sciences such as physics and in non-empirical sciences such as mathematics and logic—the choice of which conceptual apparatus to adopt is the function of a convention. Consequently, a hypothesis' conformity to observable facts, by *itself*, does not provide warrant for accepting the hypothesis. That is to say, pragmatic or instrumental considerations may have a role in determining which conceptual apparatus will be employed. This line of thought, although it helped logical empiricists to deal with problems such as the synthetic a priori, also challenged some of their convictions. Following the development of non-Euclidean geometries, Poincaré argued that the axioms of geometry are conventional and no empirical testing can determine the selection of one geometry over the other. As a result, we cannot talk about one geometry being a better fit than another.[24] The discovery of new geometries undermined the Kantian idea that geometrical axioms are synthetic a priori, the very existence of which contradicted the anti-metaphysics of positivism. Hence, it was in conventionalism that the logical empiricists found the resources to dismiss synthetic a priori statements. They regarded all

[24] Poincaré maintains that the world described by scientific theories does not exist apart from the human mind and the term 'objective', if it is to be meaningful at all, stands for 'intersubjective'. (Leszek Kolakowski 1968, 172)

analytic statements as conveying logical or linguistic conventions that are established a priori through stipulation. Positivists such as Schlick, Hahn and Carnap applied Poincaré's views of geometry to mathematics and logic. Schlick, for instance, claimed that laws of logic such as the excluded middle or the principle of identity do not say anything about reality, but only help to regulate it. Similarly, Carnap maintained that the choice of a conceptual apparatus (a linguistic framework) in science is conventional, a claim which implied that there could be other legitimate linguistic frameworks.

While the possibility of accounting for logical truths by appealing to conventions benefited the logical empiricists, the same line of thought also created serious problems for the consistency of their position. The arguments drawn from the underdetermination of theories by evidence, which reduced theory choice to a convention, clashed with some of the basic principles of logical empiricism. For instance, one possible consequence of the problem of underdetermination was that certain theological accounts of the world could in principle be compatible with scientific theories. Such compatibility, however, contradicted the logical empiricist project of distinguishing science from pseudo-science. Moreover, since conventions involve pragmatic considerations, and pragmatic considerations are likely to be susceptible to personal, social and political values, this conventionalist approach to theory choice introduces an accidental feature into scientific practice which in turn undermines the positivist idea that there is a universal scientific method. In other words, the absence of a criterion other than utility for deciding between theories threatened the epistemological privilege that the logical empiricists had attributed to scientific knowledge by bringing the objectivity of theory choice into question. After

all, if theory choice were a matter of convention, it would no longer be guided by the ideal of disinterestedness since such decisions will inevitably favor one convention over another.

The question of which theory to accept when one is presented with two empirically equivalent theories in fact proved to be a compelling one in the philosophy of science. According to one line of thought a rational decision-making process would resolve the problem. To that end, formulating methodological (epistemic) principles[25] that would dictate rational decisions became a major concern. Principles most commonly proposed included testability, consistency, epistemic adequacy, simplicity and scope. Yet their effectiveness has always been open to debate. For example, one criterion that generated heated discussions was "simplicity". One formulation of the principle of simplicity recommended accepting those hypotheses with conservative ontologies.[26] However, it was hard to explain why simplicity should matter. After all, the simplest explanation for phenomena is that they occur according to God's will and this was not acceptable for science-oriented thinking.

One logical empiricist attempt to overcome the problem of the underdetermination of hypotheses by evidence was to deny that they count as rival hypotheses. Some logical empiricists such as Schlick and Reichenbach maintained that although the terms used in theories might be different, as long as they have the same

[25] In the literature these principles are sometimes referred to as virtues or constitutive values.
[26] For an elaborate examination of the cognitive (epistemic) status of these principles see Longino (1996).

observational consequences, i.e. as long as they are empirically equivalent, they say the same thing. This point of view was supported by the logical empiricist contention that unobservable components of hypotheses are merely linguistic entities. Since they function as shortcuts for otherwise detailed descriptions of experience to help us communicate and memorize experimental outcomes, theories should not be taken as literal descriptions of the world. Accordingly, there is no genuine rivalry between two empirically equivalent theories. This solution, however, was opposed by those who distinguished between the meaning of a statement and the occurrences that make it true. According to this latter point of view, two alternative theories might have different meanings even though they lead to the same observational consequence. Discussions about the ontological status of unobservable entities and their impact on the assigned role for theories dominated the philosophy of science literature in the late twentieth century. This change of focus from the logic of confirmation to the referential character of theories also signals an important shift in the conceptualization of objectivity.

Before exploring this shift, let me remark on how we should view the logical empiricist conceptualization of objectivity, given the conventional aspect of the confirmation relation that I have briefly explained. On the one hand, insistence on a single method of confirmation for all sciences (as in scientific explanation) can be seen as an extension of the logical empiricists' trust in the impersonal method of logic and the unity of science. But if logical principles are matters of convention as some logical empiricists argued, then even the impersonality of logical method is jeopardized. Of course it can be argued that the fact that the rules are man-made does not prove that the conclusions

reached will be distorted. After all the mechanical objectivity that is secured by automation, which I have discussed in the first chapter, is not affected by the fact that the machines that replace human beings in performing measurements, experiments and so forth are man-made artifacts. However, it is important to remember that mechanical objectivity is just one form of objectivity among others. Even if we grant this mechanical objectivity (in a limited sense of non-intervention) to logical empiricists, relying on so-called impersonal logical forms is still not sufficient for comprehending the different forms of objectivity embedded in scientific knowledge.

A similar point can be made by emphasizing an important feature of confirmation: since observations are not conclusively veridical, confirmation never yields absolute certainty. In other words, since experimental control is never perfect, scientific hypotheses are never absolutely infallible. The import of this acceptance of fallibility for understanding objectivity will depend on the relationship between impersonal truth and objectivity. If truth is a condition for objectivity, i.e. if false scientific statements cannot be objective, then we cannot rely merely on the confirmation relation as a firm base for attaining objectivity. However, the link between truth and objectivity in logical empiricism is not straightforward. An examination of logical empiricist approaches to theories will make this point more clearly.

2.1.4 The Status of Theories and Objectivity

In logical positivism scientific theories were viewed as sets of singular sentences which can be represented in a formal language. Descriptions of particular facts obtained by

observation were expressed in statements containing observational terms. These statements, in turn, presented evidence for generalizations about observable phenomena. That is to say, they yielded confirmation (to a certain degree) of general statements. However, science does not merely deal with observable phenomena. In the case of general statements that involve theoretical terms, the confirmation relation is not as straightforward as in the case of observational terms. The issue of how theoretical terms acquire their meanings needs to be settled. In the logical empiricist tradition it was argued that meaningful theoretical terms in a generalization (or hypothesis) could be logically constructed from descriptions of immediate experiences. These constructions involved conventions. Positivists such as Carnap offered elaborate explanations about how such rational constructions were possible, details of which I will not discuss here. What is important to note is that this attitude towards theoretical terms led positivists to the view that the task of theories is not to describe the world as it is, but to provide us with the cognitive tools to understand phenomena. In other words, theoretical terms help us understand the world as we experience it. With this understanding, positivists escape committing themselves to a metaphysical entity such as "the world as it is".

Now in light of their instrumentalist approach to scientific theories, we can trace the logical empiricists' conceptualization of objectivity by examining their responses (or possible responses, given the principles they adopted) to the following questions: In the case of two empirically equivalent theories, i) which one, if any, provides an objective understanding or description of the world? ii) is there an objective way of choosing one theory over the other? These questions, although closely related, are in principle

concerned with two separate issues. While the first question is about the representational character of theories, the second one is about rationality in science. In this section I will be concerned with the first question and will discuss the second one in the next section.

The logical empiricist response to the problem of underdetermination by identifying the empirical equivalence of two theories with their semantic equivalence was criticized for making a trivial semantic point about the referring expressions. But conventionalism can be construed as making a point about the world beyond referring expressions. Let me clarify. It is true that in measuring the length of a stick, whether we use the metric system or the imperial system as our unit of measurement does not have a bearing on the actual size of the stick. And at the most basic level, our talk of a 2 meter stick or a 6.5 foot stick does not create any significant problems for understanding or representing the aspect of the world relating to the size of the stick. However, in the case of unobservable phenomena things get more complicated, as we do not have the means to assess in what ways our conceptual framework affects our understanding of the world. Hence the question of what theoretical entities to posit and which theory to adopt becomes far more important in describing the world. That is why Pierre Duhem claims that, in the absence of empirical testing, propositions of physics are neither true nor false, but convenient or inconvenient. This line of thinking, in principle, allows for a theoretical system that may contain incompatible hypotheses when it is convenient: a conclusion that most logical empiricists would not readily accept. However, according to the logical empiricists, linguistic (or logical) constructions belong to the realm of analytic statements; they are not about matters of fact. In other words, the conventionality of the describing

apparatus did not have any import for the empirical content of statements. This separation between analytic and synthetic statements prevented a descent into relativism in such matters of facts as conventionalism might yield. Hence, it can be argued that for logical empiricists this separation played an important role in maintaining the objectivity of the content of theories. However, Quine's attacks on the analytic/synthetic distinction, and on reductionism in the empiricist programme showed that the conventionality of the truths of logic could extend beyond mere matters of naming. Very briefly, he argued that if no genuine distinction can be drawn between analytic and synthetic statements, any synthetic statement can be turned into an analytic one by supplementing ad hoc conventions.[27] This view implied that matters of facts would also become prey to the relativistic consequences of conventionalism. In other words, as Quine puts it, any claim—including inconsistent claims—can be held to be true come what may. This conclusion undoubtly impaired the logical positivist project of merging empirical knowledge with (so-called) firm grounds of logic.

In order to grasp what is at stake for objectivity in the midst of these relativistic consequences of conventionalism, and to explore a possible logical empiricist answer to the first question (i.e. which theory provides an objective understanding of the world?), we need to clarify what an objective understanding of the world would amount to. In the

[27] In his famous passage on this issue Quine writes "The totality of our so-called knowledge or beliefs, from the most casual matters of geography and history to the profoundest laws of atomic physics or even of pure mathematics and logic, is a man-made fabric which impinges on experience only along the edges…Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system [of our beliefs]." (Quine 1963, 42-43)

first chapter I note that one way of defining objectivity is with reference to an independent reality. If what 'the objective description of the world' refers to is 'describing the world as it really is', then logical empiricists do not have an answer to the first question. In fact, they would not even be concerned with it, because according to their verifiability principle we cannot intelligibly talk about "the world as it really is". If, on the other hand, an objective description of the world were a description that is arrived at through an objective method secured by the logic of confirmation, then the logical empiricist response to the first question would be along a line such as this: as long as two theories have the same evidential relations, then both theories would offer an objective understanding of the world. But this conclusion is highly controversial especially when the accounts provided by two theories contradict one another. Take for instance the following two theories estimating the age of the earth. One theory estimates the age of the earth at some billion years by appealing to the fossil evidence, while the other estimates it as being less than 6000 years, together with the assumption that God created the world complete with the fossils that indicate a longer life span for the earth. The same evidence, i.e. fossil samples, could be used to support both of these theories. As long as the confirmation relation holds there seems to be no difference between the two theories. So should we conclude that these two theories both offer an objective account of the age of the world? Of course the immediate positivist reaction to this conclusion would be to deny the objectivity and the scientific status of the second theory since one of its assumptions is not empirically verifiable. Yet their failure to formulate an adequate criterion for

verifiability makes the logical empiricists vulnerable to the counter-arguments of theists who claim that theological assumptions are in fact, in one way or another, observable.[28]

An alternative way to contest the objectivity of the second theory is to emphasize the ideal of disinterestedness. Accordingly, a logical empiricist could claim that the second theory is not objective because it is invested in religion. Yet since disinterestedness is not an empirical principle, for reasons similar to those declaring the inadequacy of the principle of simplicity in explaining why a conservative ontology is empirically relevant, it is difficult for the logical empiricist to account for why disinterestedness alone should yield objectivity. The narrow focus on the logical relations among scientific statements, taken together with empiricist convictions, renders the logical empiricist programme inadequate for providing satisfactory resolutions of such complications.

Another reason why it is controversial to attribute objectivity to both of the theories discussed above is the common presupposition that there can only be one objective description of the world just as there can only be one "true" description of the world. Here the objectivity of a theory is tied to its truth. But "truth" is a controversial concept and it is not always clear what it means for a theory to be true. Nonetheless, let me speculate on how positivists could make sense of the "truth" of a theory, given their view that theories are mere conceptual tools. Positivists are interested in the world as we

[28] For example, those who appeal to teleological arguments (among them William Paley) claim that the evident purpose, the ordered structure and complexity of the natural world suggest that there is a grand intentional designer behind it all which we call God. Then, the observed instances of such features in the natural world are accepted as evidence for the existence of God.

experience it, so it can be argued that the "truth" of a theory can be construed with reference to correspondence to our experiences. There are two problems that positivists face here. First, if we experience the world in different ways, then there will be multiple "true" descriptions of the world which correspond to different experiences of it. If objectivity is tied to truth, then it will be possible to talk of distinct "objective" understandings of the world. Accordingly, if positivists want to deny this conclusion, they have to show that we do not experience the world differently from one another. But what aspect of positivism could guarantee the uniformity of our experiences? The Kantian solution for establishing the uniformity of experiences was to universalize the preconditions for experience. That is, since the forms of intuition and the categories of understanding are the same in everybody, our experiences of the world cannot differ. However, the logical empiricists are not interested in how we come to know or understand the world. The proper role for a philosophy of science, for them, is to investigate the validation process of knowledge claims by working out formal representations of scientific expressions in general. Hence, they shift the focus in assessing the world of experience from psychological categories to the logic of science. Yet, nothing in their methodology seems to support the uniformity of experience other than an appeal to a realist assumption about the uniformity of the world which goes against the grain of their phenomenological stance.

Another problem with the positivists' phenomenological approach to truth has to do with their foundationalist convictions. According to them the positive content of our

descriptions comes from our immediate sense perception.[29] However, since positivists maintain that statements can only be justified by other statements, the attempts to link perceptions (mental entities) to statements of descriptions (linguistic entities) are problematic. On the basis of these difficulties Karl Popper, for example, claimed that "we must distinguish between, on the one hand, *our subjective experiences or feelings of conviction*, which can never justify any statement (though they can be made the subject of psychological investigation) and, on the other hand, the *objective logical relations* subsisting among the various systems of scientific statements, and within each of them."(Popper 1965, 44, italics in the original). Yet, the emphasis on objective relations fails to equip positivists with solutions to the pressing issues such as theory choice that scientists are constantly faced with. Because of these complications the phenomenological approach to scientific theories (and truth) was challenged by scientific realists.

2.2 Scientific Realism and Objectivity

As opposed to the logical empiricists' instrumental approach, scientific realists such as Wilfrid Sellars, Michael Dummett, and Richard Boyd maintain that scientific theories are descriptions of what there really is. The main realist conviction is that the universe has a structure independent of human minds. Accordingly, scientists *discover* rather than invent reality. What makes scientific statements true or false is the external world, i.e. not our sense data or the structure of our language. For example, a statement about atoms refers to external entities called atoms. For logical empiricists in general, on the other hand, 'atom'

[29] The "positive" content was also referred as the "objective" content.

does not refer to a real entity but it is a conceptual tool that helps us organize our experiences. In this aspect, logical empiricism is an anti-realist position.[30] Furthermore, unlike logical empiricists, scientific realists do not confine science merely to the description and prediction of phenomena. According to scientific realists, the ultimate aim of science is to reveal the true story of the world (or represent the underlying structure of the world). In this view, the acceptance of a theory is not a matter of convenience but a matter of belief in its truth.[31]

In arguing for this position, philosophers such as John C. Smart, Gilbert Harman, and Charles S. Peirce took the explanatory power of realism as a good indication that it is true. For them, the trajectory of progress in the history of science gives us good reason to believe that the world has a uniform structure. In *Meaning and the Moral Sciences*, Hilary Putnam argued that if realism is not accepted, then the systematically observed regularities in the world should be regarded as cosmic coincidences or miracles which are opposed to the scientific worldview.[32] Scientific realists maintain that the continuous predictive success of theories shows that these theories provide us with accurate pictures

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[30] There are anti-realist positions other than logical empiricism. Among those, Bas C. Van Fraasen's constructive empiricism is highly influential. According to him, scientific statements should be construed literally, i.e. atoms are real entities, but good theories need not be assessed with reference to truth. Constructive empiricism is basically the view that science aims at empirical adequacy (as opposed to truth) and acceptance of a theory is a matter of believing in its empirical adequacy (not its truth). An empirically adequate theory is a theory that fits the observable phenomena, i.e. that saves the phenomena.

[31] Scientific realism, however, should not be construed as a totally separate position from logical empiricism. Rather, it is a position that developed out of logical empiricism in reaction to some of its troublesome contentions while preserving most of its principles. Some construe it as simply a different strand—a realist strand—of logical empiricism.

[32] For a detailed discussion of inference to the best explanation, see Van Fraassen (1991).

of the world. It also indicates that the theoretical entities posited in these theories must exist. Given these convictions, in this view, an objective theory becomes a theory that describes objective reality, i.e. reality as independent of the human mind or conceptual intervention. This understanding overlaps with the metaphysical sense of objectivity that I have distinguished in the first chapter, which aims at revealing the ultimate structure of the world. However, as we shall see, this form of objectivity is not only conceptually difficult to maintain, but is also not an effective ideal to follow throughout complex scientific practices.

Now, given their commitment to metaphysical objectivity, scientific realists' response to our question about the objectivity of two empirically equivalent theories would be different from logical empiricists' responses. Above I have argued that if objective descriptions are confined to descriptions of the world as it really is, then the logical positivists do not have an answer to our question because their anti-metaphysical attitude forbids them to talk of "the world as it is". In fact, according to the logical positivists, any claim about the ultimate structure of the world is meaningless, as it is not verifiable. For a scientific realist, on the other hand, there is no inconsistency in claiming that an objective description of the world is a description that represents the world as it is. According to the conviction in the uniformity of the world, not only is any inconsistency within a theory unacceptable, but there cannot be two empirically equivalent theories both of which describe the world objectively.[33] Furthermore, on this view the problem of

[33] Note that realism as the view that we can have descriptions of the way(s) the world is does not necessarily lead to this conclusion. It is the assumption about the uniformity of

which theory to accept becomes comparatively easier to resolve: between the two theories, the one that is believed to be true (i.e. believed to correspond to reality as it is) will be accepted. Of course our beliefs are fallible, but as long as science aims at "truth", erroneous descriptions will eventually be corrected. Then the problem that remains to be solved is to establish the conditions under which our beliefs about the truth of theories are warranted. However, this proves to be a difficult problem to solve.

Although scientific realism resolves certain complexities that logical empiricism fails to overcome, and avoids the kind of relativism that a conventionalist position might yield to, its ontological commitments render it vulnerable to skeptical arguments about evil demons or brains in vats: given the possibility that we might be brains in vats in a crazy scientist's laboratory (or continuously being deceived by an evil demon) how can we ever be justified in believing that our theories in fact correspond to the reality as it is? Unless scientific realists provide satisfactory answers to such questions their conviction regarding the uniform structure of the world as well as the existence of theoretical entities upon which their conception of ontological objectivity rests will be unfounded. In the face of such strong skeptical challenges, the predictive success of theories that scientific realists appeal to in arguing for the truth of theories is too loose a criterion for grounding

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the world that undermines the possibility of multiple objective theories of the world. For instance, if we assume that some of the aspects of the world are so structured that they allow for different descriptions, realism and the possibility of multiple descriptions of the world would not be inconsistent. I will revisit this issue in Chapter Four with reference to Longino's views.

these assumptions.[34] After all, as Larry Laudan has pointed out there have been many theories in the history of science which have successfully predicted phenomena such as phlogiston theory and the caloric theory of heat, yet later proved to be false. (Laudan 1991)

One conclusion that we can draw from this discussion is that scientific realists fail to provide sufficient ground for the form of objectivity, i.e. ontological objectivity, that they rely on in theory choice. Yet a more charitable conclusion would be to claim that, although ontological objectivity is not attainable (or cannot be shown that it has been attained), it could still be sought as an ideal to be achieved through methodology. This is where the two questions I have raised about the objectivity of two empirically equivalent theories intersect. It is often believed that knowledge of the objective world is attainable by following an objective method. That is why the question about features that concede objectivity to a methodology becomes highly important.[35] Accordingly, many realists such as Popper, adhered to the logical empiricist emphasis on the validating processes in scientific knowledge and to the so-called distinction between the context of discovery and the context of justification.

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[34] It might be difficult to see how predictive success could be affected by brain in vat or evil demon hypotheses. The point is that we do not/cannot have good enough reason to distinguish whether the predictions that a theory provides are made true by the world out there, i.e. whether our theory correctly depicts the structure of the external world, or whether the predictions are realized by, say an evil demon. In short, since "we" cannot "know" the cause of "successful" predictions, "we" cannot tell that our theories indeed correspond to reality as it is.

[35] I will have an elaborate discussion on ontological objectivity and epistemological objectivity in Chapter Five.

2.2.1 Karl Popper and Objectivity

In *The Logic of Scientific Discovery*, Popper explains the distinction between the context of discovery and the context of justification with reference to the psychology of knowledge and the logic of knowledge. While the psychology of knowledge deals with empirical facts, the logic of knowledge focuses on logical relations. According to Popper, the task of the philosopher of science is to give a logical analysis of scientific discovery where scientists construct hypotheses or theories, and test them against observation and experiment. The process involved in the act of inventing or constructing a hypothesis, however, cannot be a topic for logical investigation. This is because this process reflects subjective preferences and values which cannot be logically traced. According to him, how an idea occurs in a scientist's mind, or an examination of the context that inspires the scientist to come up with a hypothesis would be of interest for an empirical psychologist or a sociologist. Yet, it is irrelevant to the logical analysis of scientific knowledge, which is concerned with the questions of the validity or justification of statements. Hence, the domain of interest for the philosopher of science is the context of justification. It is in this context that philosophers investigate the methods by which the validity of scientific statements is examined, regardless of their source. One important difference between the logical empiricists and Popper is the method they believe to be central in attaining scientific knowledge. As I have discussed earlier, the central issue in scientific method for the logical empiricists is the theory of confirmation, which rests on inductive logic, whereas Popper argues for a deductive method of testing. According to Popper, the confirmation relation between theories and evidence supported by the logical empiricists

can only *temporarily* support a theory. However, in the method of falsification, if an observational consequence is falsified, then the theory which it is derived from is also falsified. Hence, in contrast to confirmation, falsification yields definite results. Yet, it should be noted that Popper's method of falsification is also vulnerable to certain problems that the confirmation theory faces: similar to the method of confirmation, falsification also cannot yield absolute certainty as to whether the theory under investigation is false. That is, if each scientific theory belongs to an interconnected web of theories, and operates within a set of background assumptions, then even if the data does not agree with what the theory predicts, we cannot claim with certainty that our theory is false. This is because the cause of the problem might well lie in the background assumptions or in another theory that our theory is related to. I will discuss this issue further below, but now let me proceed with examining the forms of objectivity the method of falsification could possibly yield.

Popper's emphasis on the logic of scientific discovery might suggest that he shares the logical empiricist contention that objectivity is secured by the impersonal logical form of the method. Yet, in a realist position the inconclusiveness of Popper's method is an obstacle in attaining an objective description of the world: if we do not have a conclusive way to accept or dismiss a theory, then we do not have the means to determine whether a theory is actually getting us closer to the true story of the world or not. Similarly, in the case of two competing theories if we do not have conclusive reasons to discard one theory or the other, we cannot decide which theory actually describes the objective world and

which does not. In other words, the method of falsification, whether or not it is impersonal, fails fully to guide theory choice.

Yet, Popper's understanding of objectivity extends beyond the impersonal logical aspect of the method. For Popper, although linguistic analysis is important, philosophical problems are not mere linguistic puzzles; there are some real philosophical problems such as the problem of the growth of knowledge. Even though he thinks that there is no method peculiar to philosophy, there is a method that Popper praises for all rational discussions, including philosophy. This method involves adopting a critical discourse where examinations of clearly posed problems are carried out critically. A critical examination consists basically of attempts to refute proposed solutions to a problem. In cases where one fails to provide sufficient criticism to disprove one's solution, other people will supply more criticisms. Hence, a critical discourse requires a community. As opposed to the foundationalist project of the logical positivists where knowledge is built upon immediate sense perception, according to Popper, justification procedures cannot be confined to individual convictions (which belong to the realm of the psychological). Any scientific statement could, in principle, be tested and understood by anybody. Any experiment that yields a "scientific" finding could be "reproduced by anyone who carries out the appropriate experiment in the way prescribed." (45) Hence, the objectivity of scientific statements becomes a matter of intersubjective testing.[36]

[36] It should be noted that in a realist position this intersubjective testing is believed to pave the way to the "true" description of the world. However, from an anti-realist perspective it can be construed as a method of "consensus building".

In so far as critical discourse is crucial for the method of falsification, it plays a significant role in attaining objective knowledge. Since the possibility of a critical discourse rests on communicability, it is reasonable to conclude that an important part of Popper's conceptualization of objectivity is governed by the ideal of aperspectivity which, as Daston argues, is premised on communicability. By bringing the aspect of critical discourse into focus, Popper's understanding opens new avenues for discussing objectivity in science. For instance, if what is essential for critical discourse is to consider other people's views and objections, it encourages us to ask questions about the ways in which other people's criticisms matter, as well as questions about the characteristics (personal or social such as their sex, race, class and so forth) these other people should have in order to contribute well to the critical discourse. However, Popper's positivist tendency of abstracting subjectivities in order to attain a universal account about how science works (or should work) prevented him asking such questions. The emphasis on the importance of these questions for objectivity had to wait for the feminist critiques of science which I will discuss in the next chapter. The intersubjective exchange that Popper defends essentially serves to control the rational course of belief formation and assessment, the rules and ideals of which are predetermined. It does not deal with the ways in which personal or social differences among participating individuals could contribute to the critical exchange.

Another method for solving philosophical problems according to Popper rests on the historical aspect of science. Central to this method is a consideration of past approaches to and judgments of a specific problem. How the problem was formulated and

handled, as well as the situations that brought it about are all important for this approach. Although it is frequently overlooked by logical empiricists, this attempt to proceed by understanding the history of a problem is valuable for Popper, as it would constitute an essential part of critical discourse (or in Popper's terms, rational discussion). Popper's emphasis on the historical aspect of science is shared by the critics of positivist and scientific realist approaches to science whom I will discuss in the next section. What is interesting to note is that although for these critics a historical approach to science renders the strict distinction between the context of discovery and the context of justification redundant, for Popper, considering historical features does not undermine the distinction. I think it is an indication of the confusion regarding what exactly belongs to the logic of science and what does not.

Another attack, from a somewhat different direction, on the foundationalist ideas of logical empiricism came from Duhem and Quine. According to the Quine-Duhem thesis, the underdetermination of theories by evidence implied that no single statement could carry any evidence for a hypothesis by itself. Consequently, it was argued that theoretical systems should be tested as a whole. This thesis, together with a holist theory of meaning where it is claimed that a theoretical term gets its meaning from the entire body of scientific beliefs in which it is embedded, led Quine to conclude that any scientific claim might be used to justify any other accepted scientific claim. Hence, accounting for theory change, which is at the heart of the growth and development of science, continued to occupy philosophers of science. The issue of theory change is

closely tied to questions about objective (and rational) ways of choosing one theory over others.

2.3 Wholism, Theory Change and Objectivity

Following the challenges raised by such conventionalists as Quine and Duhem, and Popper, a new approach to the philosophy of science has emerged. Although proponents of this new understanding do not agree with one another in all points of criticism, they are united by their emphasis on the actual practice of science as opposed to the logical empiricist emphasis on the logic of science.[37] It is not possible to do justice to the intricate ideas put forward by adherents to this new school of thought here, but for my discussion of objectivity a general outline will be sufficient.

The general outlook of wholist theorists was in line with Popper's emphasis on the history of science, although the ultimate understandings of science and the philosophy of science differ. As opposed to philosophers in the positivist tradition, wholists maintained that an examination of the history of science has important philosophical import. Similarly to Popper, wholists such as Thomas Kuhn, Imre Lakatos, Larry Laudan, and Paul Feyerabend were concerned with problems relating to the growth of knowledge. Hence, their work is mainly directed at issues about theory change and development.

Philosophers who adopted this new approach took issue with the basic distinctions and principles of logical empiricism and scientific realism, thereby challenging previous

[37] The proponents of this new understanding have been referred as wholists, historicist or globalists. In discussing this new understanding I will focus on Kuhn's views and refer to it as wholist tradition.

conceptions of scientific objectivity. For example, they rejected any claims that there could be a distinction between theoretical sentences and observational sentences. They denied the possibility of pure observations and claimed that observation sentences are always infected by theory. This view implied that if observations are theory-laden, then immediate sense perceptions cannot provide objective content to scientific descriptions as positivists had argued. Hence, the logical empiricist project of founding science on pure sense data is undermined. Moreover, they opposed the scientific realist contention that there is one true theory which can represent the ultimate structure of the world. In other words, they dismissed metaphysical objectivity. Adherents to the wholist tradition also denied that there is a single logic to scientific discovery. Hence, they found both the method of confirmation and the method of falsification inadequate for assessing theories. In emphasizing the history of science and its importance for philosophy, they attacked the so-called distinction between the context of discovery and the context of justification. Following these views, they dismissed the idea of a cumulative science where the empirical content of the old theory as well as the meanings of the terms contained in it are preserved in the new theory. In the dominant view, the growth of knowledge and the development of science were taken as simple accumulations of observational statements and individual discoveries. Hence, a new theory was conceived as an advancement achieved by improving the old theory. That is why it was believed that the new theory was more objective than the old one. Yet, wholist attacks on the cumulative picture of science challenge this understanding of accelerating objectivity through improvements in

theories.[38] In thinking about scientific development, Kuhn points out an important dilemma about how to distinguish the "scientific" component of past theories in natural sciences which are discarded either as wrong or as superstitions. (*The Structure of Scientific Revolutions* [SSR] 1970, 2) The first horn of the dilemma is that if we reject past theories as "unscientific" as a whole, we will be compelled to accept that the method that yields those erroneous theories could now lead to scientific knowledge. This means that the method does not guarantee that today's "scientific knowledge" would not be tomorrow's superstition. The second horn of the dilemma, on the other hand, is that if past theories are accepted as 'scientific', we have to accept that science consists of incompatible theories, as some past theories are incompatible with the accepted theories of today. (1970, 2) According to Kuhn, we are better off with the second option than the first one. However, the second option requires a new approach to scientific development, one which does not view it as cumulative.

Kuhn's understanding of objectivity is closely related to his thoughts on how science proceeds. Although he has an elaborate account of the complex ways science proceeds, the key ideas for Kuhn's understanding of scientific development are "normal science" and "scientific revolutions". Very briefly, scientific revolutions trace the following general course: Normal science operates in a framework (paradigm) where the scientific community has firm beliefs about how the world is: the fundamental entities of

[38] These improvements might be construed as increases in truth as well as in scope or accuracy. Accordingly the greater the scope of the theory, i.e. the more phenomena it can explain, the more objective it becomes. It is similar to Thomas Nagel's understanding of objectivity that comes in degrees, which I discussed with its problems in the previous chapter.

which the universe is made, the relationships between these entities and us, the techniques that need to be employed or rules to be followed in inquiring into them are some of the basic questions, answers to which are agreed upon by the scientific community during the period of normal science. The mind-set of the scientific community is formed through the education that qualifies students for professional practice. (5) Accordingly, the world is confronted by the conceptual apparatus provided by professional education. In normal science any attempts to challenge these conceptual apparatuses are initially resisted. Yet, when scientists are faced with a problem that cannot be solved by the accepted rules and procedures of normal science, the earlier challenges can no longer be suppressed. If the problem (anomaly) persists, the old conceptual apparatus together with its commitments are gradually abandoned and a new basis for the practice of science is accepted. (6) In this conceptualization, scientific activity is viewed as a puzzle-solving activity.

One of the main points that Kuhn and Popper disagree on is this conceptualization of science as essentially a puzzle-solving activity. According to Popper, scientific activity is a critical discourse where proponents of rival theories are in constant exchange with one another.[39] And this exchange is fundamental for attaining objectivity. As I have argued, communicability is crucial for critical exchange, yet Kuhn's ideas about paradigms in SSR do not allow for a meaningful exchange among scientists who subscribe to different paradigms. In this work, Kuhn compares scientific revolutions to gestalt switches where one cannot see two distinct figures embedded in an image at the same time. Similarly, science cannot operate in two different paradigms at the same time. A new paradigm with

[39] For more detail on this disagreement see Kuhn (1970b).

its worldview is formed once the old paradigm is abandoned. After this transition, the world is never viewed in the same way as it had been viewed under the old paradigm. One reason for this incapacity is that scientific revolutions do not occur very often in the history of science. Except on rare occasions, most scientists work in one paradigm through all of their lives. According to Kuhn, those who witness such a transformation could not contemplate the two paradigms at the same time because the questions asked, the techniques and standards used to respond to those questions, as well as the meanings of the terms in the theories alter during paradigm change. Hence, even a careful historical examination would likely fail fully to reconstruct the old paradigm once the scientists (and the historians) are exposed to the new paradigm.

If this analogy between gestalt switch and theory change is accurate, then it has important implications regarding the rationality of science. In the famous figure which contains both a rabbit and a duck image, for instance, the change in perception is not guided by any rational principle; one sees the rabbit image at one time and the duck image at another time. If theory change is similar to a gestalt switch, it means that theory choices are not based on rational criteria. This is to say, there is no neutral ground on which to compare competing theories and choose one over another. This view leads to the criticism that Kuhn denies the possibility of any objective course of theory choice. He rejects this criticism on the grounds that it associates objectivity with neutrality. According to Kuhn, the absence of a neutral ground does not necessarily mean that theory choice is arbitrary. In his later work he argues for the effectiveness of certain principles (or virtues) such as accuracy, consistency, broadness of scope, simplicity and fruitfulness in the processes of

theory choice. (Kuhn 1977, 321-2) In the absence of *a priori* criteria, theory choice rests on the collective judgment of the scientific community, and objectivity becomes a matter of shared criteria. In other words, these principles are not universal, and different sets of principles might be emphasized in different paradigms. For Kuhn, although shared principles are the communal bases of theory choice, they are open to interpretation and they sometimes contradict one another. Accordingly, two scientists working from these principles might reach different conclusions, as they might interpret certain principles differently or disagree on their relative importance. (324) For such reasons, these principles cannot be taken as the fixed criteria for deciding among theories.

From Kuhn's point of view, in explaining why scientists have chosen the theories they are committed to, we need to look beyond the shared criteria. Among other factors, scientists' past experiences are influential in their choice. Personal values and inclinations also play a part in their decisions. For instance, someone who values originality might pick a theory accordingly. (325) According to Kuhn's opponents, however, these subjective factors belong to the context of discovery and they have no philosophical import in examining science. In contrast, Kuhn maintains that theory choice depends on a mixture of objective (shared criteria) and subjective factors. (325) His insistence on the effects of subjective factors in science has led Kuhn's critics to condemn him for relativising theory choice to subjective preferences, and thereby undermining the overall objectivity of science. In his response to the critics we find some clues about Kuhn's conceptualization of objectivity. Kuhn distinguishes two senses of 'subjective' where one is used as the opposite of 'objective' and the other is used as the opposite of 'judgmental'.

Kuhn claims that his critics are confused about these two uses when they imply that he reduces theory choice to a matter of taste. Matters of taste are 'subjective', and similar to reports of sensation they cannot be discussed. In contrast, judgments are claims that can be grounded and discussed. Take Kuhn's example: when I state "I like this potboiler film", my sensation of liking the film is not discussible, in the sense that someone who did not like the film cannot disagree with my report of my own sensation although they might think that I have bad taste. Yet my judgment that the film is a potboiler is open to discussion. If somebody disagrees, they can state their reasons as to why they disagree while "each revealing, implicitly or explicitly, something about how he *judges* cinematic merit", and they could try to convince one another by comparing the film to similar ones, and so forth. (336-337) Now, Kuhn emphasizes that when he claims that subjective preferences play a role in theory choice he does not mean that the decision is not discussible. In fact, he thinks it is very important for a scientist to exhibit the bases for his/her choices (337). As long as the bases of decision about theories are discussible they are not 'subjective' in the sense opposed to 'judgmental', hence it is not true that "anything goes". Accordingly, it can be claimed that for Kuhn the effects of subjective factors in scientific practice do not undermine the overall objectivity of science as long as they do not hinder communicability. Yet there seems to be a tension in Kuhn's position regarding the requirement of communicability.

Kuhn writes, "my report that I like the film is objective unless I have lied." (337) Here there is a sense of 'objective' which is identified or tied with the "truth" of a statement (where a matter of fact makes the statement true). It is in this sense that we can

understand why theory choice is not "objective". As discussed above, according to Kuhn, because observations are theory-laden, there is no neutral point of view where two theories can be compared. That is, there is no single fact of the matter that we could appeal to in making an absolute claim about the "truth" of theories. But this situation does not undermine the overall objectivity of science. Kuhn claims that the proponents of different theories can still discuss the results achieved by the theory they have adopted, and try to convince one another. Surely, such attempts rest on the possibility of communication between two parties. Yet, as I have mentioned, Kuhn's philosophical position in SSR regarding the incommensurability of different paradigms undermines the communicability that is required for a genuine discussion.

In his later work "Objectivity, Value Judgment, and Theory Choice" (1977) although he preserves most of his previous thoughts, Kuhn compares the situation that the proponents of different theories are in when faced with theory choice to native speakers of different languages. Similarly to the case of communication between native speakers of different languages which is possible through translation, proponents of different theories can achieve communication by means of translation. However, since translation is not problem free (both in the case of different languages and of different theories) there will be a limit to what is communicated. Moreover, in the case of different theories "the vocabulary of the two theories may be identical, and most words function in the same ways in both. But some words in the basic as well as in the theoretical vocabularies of the two theories—words like "star" and "planet," "mixture" and "compound," or "force" and "matter"—do function differently. Those differences are unexpected and will be

discovered and localized, if at all, only by repeated experience of communication breakdown." (338) Yet, despite the incompleteness of communication, proponents of different theories can show the concrete results that their theories achieve in application to certain phenomena, and these impressive results might convince a few of the proponents who would attempt to translate one theory into another by examining published papers, and talking to and observing the scientists who practice the theory. If the new theory survives, some of these proponents meanwhile might "find that at some point in the language-learning process they have ceased to translate and begun instead to speak the language like a native." (339) Kuhn emphasizes that in these cases no deliberate decision-making occurs. That is why he is ambivalent about calling this process 'theory choice', as it is more like a case of conversion.

According to Kuhn, one reason for regarding subjective factors as human weaknesses that would render science irrational rather than accepting them as natural parts of scientific inquiry is the incompleteness of the list of methodological virtues for theory choice. Faith in the possibility of a well-articulated list of criteria, which would dictate rational choice, reinforces the denunciation of subjective factors. According to this point of view, the objectivity of science will be secured in the context of justification by means of objective criteria valid throughout the scientific community. (327) However, in the face of many failed attempts, such as those I have discussed with respect to "simplicity" to formulate the perfect list, "objective criteria" came to be regarded as an ideal although it is not attainable. Yet, Kuhn claims that such idealization is a result of a misleading scientific pedagogy where crucial experiments in well-known problems in

science are presented as exemplary cases of how theory choice works. But these crucial experiments, where evidence attained prompted scientists to favor one theory over another, do not display the actual processes of decision-making. The crucial experiments that are illustrated as exemplary cases of theory choice are always about the theories that eventually triumphed. For instance, in the case of the decision between oxygen theory and phlogiston theory, we are presented with evidence of the explanatory power of oxygen theory for addressing certain phenomena, while nothing is noted about phlogiston theory's power of explaining other phenomena for which oxygen theory fails to account. The idealization of instances of theory choice which depend on these simplified examples masks the actual problems scientists face during processes of theory choice, and hence overlooks an essential part of decision making. In real situations, scientists are almost always faced with some good reasons for each possible choice, which is why it is especially important to understand the role of subjective factors in theory choice. Once it is understood, it becomes clear that "considerations relevant to the context of discovery are then relevant to justification as well; scientists who share the concerns and sensibilities of the individual who discovers a new theory are ipso facto likely to appear disproportionately frequently among that theory's first supporters." (328)

Consequently, although the five criteria mentioned for theory choice are not sufficient to *dictate* a rational decision, when treated as *guiding* values for theory choice they are useful tools. (330) According to Kuhn, as long as they are left vague in their specifications they could account for aspects of scientific practice which have been discarded as irrational by proponents of the dominant view. Since the application and

significance attributed to these values change in time and according to the topic of investigation, it is more plausible to regard them as guiding values that influence decisions rather than as strictly fixed rules which determine theory choice. (335)

In sum, Kuhn's naturalistic approach to examining science where the focus is not on *a priori* principles but on what scientists actually do when engaged in scientific practice led him to depart from the orthodox belief in a unique methodology that secures objectivity in science. According to him, subjective factors inevitably contribute to scientific practice (including methodology), yet we can still talk about objectivity in science, as there are some principles that assist theory evaluations. Although these principles are not universal, in so far as the scientific community agrees in endorsing them, they guide assessments and decision-making. Here what is achieved is not absolute objectivity but a provisional one which I believe is an expected outcome of a naturalistic approach to science.

2.4 Conclusion

In this chapter I have argued that different approaches to science have a significant bearing on understandings of objectivity. My brief discussion of central traditions in the philosophy of science shows that the aspects of objectivity that come under scrutiny have changed as the concerns and questions have shifted within different philosophical traditions. Yet I have recognized that some form of intersubjective examination has prevailed one way or another in conceptualizations of objectivity within different traditions. For instance, as long as conventions amount to agreements among participating

subjects, we can find an element of shared rules or criteria within the conventionalist strand of logical empiricism. In this tradition any idiosyncratic effects of conventions on the content of scientific theories were believed to be preventable (i) by confining their use to analytic statements; and (ii) by appealing to the impersonal method of logic. However, the first approach proved to be inapplicable after Quine's challenges to the distinction between analytic and synthetic statements. With respect to the second approach, on the other hand, the very idea that logical principles are conventions renders the impersonality of logic questionable. Yet even if impersonality is granted to logic, as I have discussed, it still fails to yield certain forms of objectivity which are sought after in scientific practice. Another aspect of positivism where we can trace a form of intersubjectivity is contained in the principle of testability. According to this principle, any scientific statement could, in principle, be tested and understood by anybody. In other words, scientific results should be reproducible by other people who follow the same methodology. What the principle of testability primarily aims to do is to exclude any arbitrary results obtained through ways that cannot be observed and controlled publicly. Although the applicability of testability requires a community of people, the principle itself is not concerned with the particular features of the participating subjects. As long as subjects are capable of following the prescribed method they should be able to reproduce the same result. Here it can be claimed that objectivity attained through this kind of intersubjectivity is premised on the interchangeability of assessing subjects. This is also true for Popper's emphasis on critical exchange in attaining objectivity, which requires interaction among multiple subjects. In both cases the differences among participating

subjects are at best taken as irrelevant for objectivity, or are seen as obstacles to arriving at objective descriptions of the world. In Kuhn's philosophy, however, this picture changes. Intersubjectivity is embedded in his view that the objective element in scientific practice mainly consists in following a set of shared rules adopted by the community of scientists for dealing with specific problems. Yet, according to Kuhn, subjective factors might and often do have an effect on why certain principles are adopted by certain scientists. However, this situation does not jeopardize objectivity as long as subjects can communicate their reasons as to why they prefer certain principles to others. So although the idea of public accountability still prevails in Kuhn's understanding of objectivity, it is important to note that in his picture, unlike those of the other philosophers I have discussed, subjects are not reduced to abstract place-holders; they are concrete individuals with specific features which inform their decisions. Yet, although Kuhn maintains that these subjective features do not threaten objectivity in science, he does not explicitly argue for the idea that they might in fact contribute to it. In the next chapter, we will see that feminist philosophers of science take this next step and claim that subjective values are not only present in every aspect of scientific practice, but also important in achieving objective understanding of certain phenomena. As we shall see, this approach requires certain modifications in our understanding of science and ideals of objectivity.

Chapter Three

Feminist Standpoint Theory and Objectivity

In Chapter Two I discussed how science has long been accepted as the paradigm of "proper knowledge". One of the most vigorous reactions to this contention came from feminist epistemologists and philosophers of science in the late twentieth century. In this chapter, I will start by briefly elucidating the main feminist concerns about the mainstream approach to science and its concepts, specifically the notion of objectivity. I suggest that feminist standpoint epistemology and contextual empiricism generate two noteworthy conceptualizations of objectivity that open up new venues in thinking about the interplay between science and values. My focus in this chapter is on feminist standpoint epistemology as advocated by Sandra Harding. I examine her notion of "strong objectivity" in an attempt to set out its strengths and shortcomings. I conclude that although Harding's conceptualization of objectivity is important, it is not adequate to compel those who subscribe to the "received view" to change their views.

Chapter Two illustrated that for most of the twentieth century the dominant schools of thought in the philosophy of science treated science *mainly* as a linguistic entity, and they focused on semantic analyses in tackling scientific problems. Scientific explanations were collections of statements of facts that were logically related to one another. Towards the end of the century, however, philosophical groups such as wholists, pragmatists, sociologists, feminists and postcolonial critics challenged this picture. Although these groups differed in their specific concerns and proposed methodologies,

they were united by an emphasis on the significance of the social circumstances in which actual science is pursued, in general, and the contexts within which a scientific explanation is put forward, in particular. These developments led some philosophers, among them those who adhered to the empiricist tradition such as Bas van Fraassen, to turn their attention to the study of the pragmatics of scientific explanation.[1] According to van Fraassen such a study starts first and foremost by clarifying the questions that are asked, as the content of a question depends on the context within which it is asked as well as on what is highlighted (sometimes tacitly) by the questioner. In order to have a genuine explanation, contextual features, such as the interests and background assumptions of the person or the group of people posing the question, need to be discerned. Another feature that needs attention is the cognitive status of the audience (i.e. receivers of the explanation). Accordingly, the background knowledge of the audience also has a bearing on what to include in an explanation (e.g. how detailed the explanation should be, what should be taken as common knowledge so that it could be excluded from the explanation and so forth). Other than background knowledge, the interests of the audience are also an important pragmatic feature of an explanation. As I have noted in Chapter Two, a scientific explanation is necessarily a selective enterprise. That is, in explaining a phenomenon we need to take into consideration not all facts, but only certain relevant facts. The question of how to discern which facts are relevant, however, is a challenging one. We have seen that the positivist programme does not have a satisfactory answer to

[1] It should be noted, however, that according to van Fraassen, explanation is neither an overriding virtue nor the end of scientific inquiry. He writes, explanation is "less a virtue than an anthropocentric pleasure." (1991b, 326)

this question. In fact, it is debatable whether there could ever be any formal satisfactory answer. Yet studies of the pragmatics of explanation offer some useful insights. In the absence of a definite formal criterion, considering the intentions behind asking certain questions and providing certain explanations, as well as the interests and the background of the audience, can (and do) play a significant role in deciding which facts are relevant and which are not.

This approach to science is drastically different from the positivist approach. Science is not confined to the logic of scientific knowledge but conceived more broadly. The study of science here encompasses the practices and the circumstances of a scientific community, which is constituted by scientists and practitioners, as well as by the receivers of scientific explanations. This conception of science, which acknowledges the dynamic character and the diverse features of scientific practice, has been central to feminist discussions of science. Various groups of feminists have developed diverse and original ideas in thinking about science and its workings. Before discussing two of the most prominent feminist theories of science let me first elaborate on some general problems with which feminists have taken issue.

3.1 Common Feminist Concerns Regarding Scientific Knowledge

Although feminist thought and projects were around much earlier, feminist work that specifically focused on the philosophy of science first came about in the late 20th century,

mainly inspired by the philosophies of Wittgenstein, Quine, and Kuhn.[2] Feminists, as in all other areas of thought, have not produced a uniform theory of science. Yet despite their differences, feminists typically share an epistemological project of situating knowledge within networks of social relations of power, specifically those relating to gender. In contrast to the mainstream epistemological project of finding universal necessary and sufficient conditions for knowledge where knowing subjects are abstracted from their specific circumstances, feminists argue for the significance of taking subjectivity into account.[3] The contention that the cognitive and the psychological status of subjects, as well as the social locations subjects occupy, have epistemological import for their knowledge claims (i.e. how those claims are made, assessed and received) are central to this feminist response. This conviction has led many feminists to focus on actual knowledge-making processes, and to analyse the specificities of the discourses that impact the assessments of knowledge claims. This investigation led to the argument that Western philosophy has systemically reproduced a discourse that serves male interests. Hence, in contrast to the positivist project of examining the universal features of a scientific method, feminists have focused on specific sciences, and have successfully demonstrated that the ways in which science is theorised and practiced are affected by Eurocentric, andocentric, class and race biased values.[4] Feminists have argued that ideals such as autonomy, objectivity, value-neutrality and universality upon which "proper

[2] I should note that my discussion of feminist philosophy is confined mostly to analytic feminism.
[3] See Code (1993).
[4] Biases against age, sexual orientation, and disabled bodies can also be added to this list.

knowledge" is built reflect the characteristics of Western, upper and/or middle class, white men. The extent to which this occurs has meant that the results and products of the scientific enterprise contribute to the interests of these privileged groups while subjugating those that fall outside of them.[5]

Androcentrism in science has been a specific target for feminists. The prevailing rigid dichotomies, such as objective/subjective, mind/body, universal/particular, and rational/social in Western philosophy are targeted. These dichotomies have contributed to the subordination of women both in everyday life and in the intellectual domain. Feminist works reveal how the first term in each of these dichotomies has traditionally been identified with male characteristics, and believed to constitute a feature of "proper knowledge", while the second part has typically been identified with female characteristics, and treated as an unreliable source of knowledge. As a result of this stigmatization women have often been left out of scientific practices. Feminists oppose this injustice by emphasizing the importance of body, emotions, particular experiences, and social values for knowledge production. They also question mainstream epistemologists' tendency to privilege first-hand experience of a small group of subjects at the expense of other ways of attaining knowledge, such as testimony.[6] Feminists such as Naomi Scheman have argued that such a dismissal is mainly a function of ignoring the

[5] For a detailed analysis see Lloyd (1984), Keller (1985), Bordo (1987).
[6] With the development of social epistemology and virtue epistemology there has been a growing interest in issues of testimony in mainstream epistemology. See Goldman (1999) and Goldman (2001). However, this take is still different from that of many feminists in important ways. For instance, it still lacks the ethical and political dimension of testimony that feminists are concerned with.

epistemic dependency of knowing subjects on one another and on the rest of society. This issue of epistemic dependency has raised questions regarding justificatory processes that the mainstream has overlooked. For instance, "trustworthiness" has become significant for knowledge acquisition.[7]

Apart from feminists' elaborate work in demonstrating inequalities in knowledge production, perhaps the most important feminist reaction to mainstream philosophy of science is the call for a more responsible epistemology. This call extends to science and to a philosophy of science that works within a theoretical framework capable of compensating for the hitherto neglected duties of knowers and scientists. Specifically, feminists argue for an extension of the responsibility of scientists beyond their liability for what counts as evidence in the context of justification to the explicit or implicit practices in the context of discovery as well as the technological consequences of the scientific enterprise.[8] Moreover, feminists emphasize the responsibility of the audience. Mainstream philosophy of science has ignored this responsibility, even though it has long been discussed among scientists themselves.[9]

While sharing these common concerns, nonetheless feminists have not produced a monolithic theory. In their struggle for sex and gender equality, different groups of feminists have generated diverse points of view depending on their priorities, subject

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[7] For an interesting account of epistemic dependency and trustworthiness see Scheman (2001).
[8] See Lambert (1987).
[9] Recall Daston's example cited in the first chapter. In pursuing objectivity, some scientists argued for leaving the decisions to viewers in depicting the diseases exhibited in an image.

matters, and the specificities of their social locations. Accordingly, we can talk of different feminist theories with different epistemological imports. Some aspects of these theories can be complementary to one another, while others are not in certain respects.[10] Two of the most prominent feminist theories requiring discussion are feminist standpoint theory and contextual empiricism.

3.2 Feminist Standpoint Theory

One of the first and perhaps most influential feminist reactions to mainstream philosophy of science came from feminist standpoint theory (FST). Notably, this theory is elaborated in the works of Nancy Hartsock, Sandra Harding, Patricia Hill Collins and Hilary Rose. To help understand and oppose women's subjugation, which extends to all areas of life including the intellectual domain, these works of FST drew inspiration from Marxist standpoint theory and Marxian epistemology. Nancy Hartsock, for instance, draws parallels between Marxian explanations of the differences between the ruling classes' (capitalists') vision of the world and the ruled (proletariat) vision, and the differences between male and female visions. Central to standpoint theories is the idea that society's material conditions (specifically, those material conditions pertaining to the social relations of production in Marxist standpoint theory) structure and limit everyone's vision of the world. Accordingly, the social positions and lived realities of the proletariat (and

[10] I will discuss this issue further in the following.

women) are structurally different from the ruling classes' (and men's) standpoints.[11] Standpoint theorists claim that these differences have important epistemological consequences. For instance, to explain the epistemic superiority of a proletariat vision, Marx's labour theory of value demonstrates the differences arising from the perspective of exchange and the perspective of production that reflect different social relations of power between the buyer and the seller of labour power. (Marx, 1891) From the perspective of exchange and the ruling capitalist class, the relation between the seller and the buyer of labour power is viewed as one of equals. The worker is seen as a free agent who sells his or her labour to the capitalist. However, such an explanation, according to Marx, is feasible only at the level of *appearance* (which is partial and perverse). From the perspective of production and labour, on the other hand, the relation between the worker and the capitalist is drastically different. The exchange is not one of equals, as all workers have to sell their labour power to survive. For Marx, the perspective of production reveals the structural inequalities embedded in capitalist social relations and class dynamics. Only by revealing the inner workings of capitalism can one understand the *real* or essential nature of the class structure that systematically exploits workers.[12] In similar ways, feminists have argued that in order to understand sex and gender inequalities embedded in social relations, a feminist standpoint is required. Here, a feminist vision does not merely reveal the reality of women's lives, which is often ignored or misrepresented by male

[11] Of course we should keep in mind that different categories of social identity cut across each other. For instance, the ruling classes also include women, to a greater or lesser extent.

[12] A detailed account of this analysis can be found in Hartsock (1987).

vision, but it also provides the correct vision for understanding social relations. In order to grasp how a specific vision could reveal the real nature of things we need to understand what a standpoint amounts to.

A standpoint is not a mere position or perspective. Any group of people who share a social location may also have a perspective in common without critically deliberating about it. A standpoint, on the other hand, "arises when people occupying a subordinate social location engage in political struggle to change the conditions of their lives and so engage in an analysis of these conditions in order to change them." (Potter 2006, 131-2) In order to *achieve* a standpoint people need to first become aware of their social location and its relation to the general social, political, economic, and gendered organization of society.[13] Because the ruling classes (dominant groups) disproportionately structure material relations, their collective vision of the world tends to dominate throughout society. To the extent that dominant social forces are successful, their social power often becomes less visible, more difficult to detect, and harder to oppose. The gendered production of scientific knowledge is one such case. As dominant groups tend to shape social life to their benefit, they also seek to regulate the production of knowledge. In turn, this regulation helps to reinforce the hegemony of the dominant group and its power to shape the social world. (Potter 2006, 133) That is why, in order to break or change this vicious circle, a standpoint requires perpetual reflection. It also requires vigorous self-

[13] The fact that a standpoint is an achieved position is very important. In the next chapter I will argue that Longino's account of objectivity should incorporate some of FST's ideas. There the idea that a standpoint should be achieved by the marginalized themselves will be significant for my argument.

reflection. However, one needs to be careful in making this claim as it might be misleading to think that individuals are basic epistemic agents for standpoint theory. Of course, an individual can (and must) self-reflect on her social location, analyze prevailing social conditions, and become aware of the hidden impacts of the dominant vision. However, as long as standpoint theory requires a political struggle to change the dominant vision, reflection is primarily a communal engagement.[14]

The claims of standpoint theory are not only about the social world but also the natural world. As Hartsock writes, "A standpoint, however, carries with it the contention that some perspectives on society from which, however well-intentioned one may be, the real relations of humans with each other and with the natural world are not visible." (Hartsock, 1987, 159) Accordingly, standpoint theory's assertion that a correct vision of the world and of prevailing social relations of dominance are available only from the position of the oppressed has important epistemic implications for scientific objectivity. For instance, it implies that a female vision, as an oppressed vision, leads to less partial and less distorted beliefs than a view that appears to come from nowhere, and hence provides more "objective" knowledge of the world. However, incorporating this "objectivity" into the idea of socially situated knowledge has proved to be a difficult one. Sandra Harding offers a promising attempt to overcome this difficulty.

[14] "Consciousness raising" activities in the 1960s is a good indication of standpoint theory's communal character.

3.3 Sandra Harding's Feminist Standpoint Theory and Strong Objectivity

In *Whose Science, Whose Knowledge* (1991) Harding argues that the so-called tension between socially situated knowledge and objective knowledge is one of the effects of mainstream philosophy of science. In mainstream philosophy of science, objectivity is typically associated with impartiality, disinterestedness, impersonality and value-neutrality.[15] These ideals mainly stem from features attributed to the subjects and objects of knowledge. In mainstream empiricist epistemology, knowledge is by definition universal. It follows that the subjects of knowledge must also be socially and historically disembodied and interchangeable. The objectivity of scientific explanation is accordingly premised on formal relations between disembodied subjects and objects that exist out there in the world untouched by human intervention. Moreover, since knowledge should be consistent and coherent, mainstream empiricists claim that subjects must be homogeneous and unitary to the extent that they are interchangeable. In contrast to the subjects of knowledge, the objects of knowledge that science describes and explains are determinate in space and time, and they are free from social impacts. (Harding, 1993) In other words, they have a mind-independent existence.[16] Mainstream epistemology contends that without these features scientific research cannot produce "proper knowledge" as there would not be any reasonable (rational) standard that could distinguish knowledge from mere opinion. Adherents to this view argue that the absence

[15] It is important to recall, however, that when we have a close look at the historical practices we see that these ideals of objectivity are often in tension with one another and cannot be endorsed simultaneously, as I discussed in the first chapter.
[16] I will have an elaborate discussion of mind-independent facts in Chapter Five.

of such standards would lead to epistemic relativism. In other words, if we give up on the possibility of a value-free point of view secured by abstract subjects and universal standards, then we foreclose all hope of rationally disagreeing with opposing worldviews. Since FST is neither impersonal nor disinterested or value-free, mainstream theorists charge that it cannot yield objectivity.

Within the framework of objectivity which is associated with the ideals of impartiality, disinterestedness, impersonality and value-neutrality, a standpoint position indeed appears to be of no help in producing "objective" knowledge.[17] Adherents to FST argue that because the subjects of knowledge are in fact embodied, research will always convey features of the subjects' social locations. Knowledge is inevitably and always socially and historically situated. Furthermore, the objects of scientific research are never "pure" in the sense of being out there in nature and untouched by human intervention. Instead they are social objects as they convey "the contemporary general cultural meaning" which is often shaped by the conventions among and within scientific communities present and past. (1993, 64) Many contributors to FST consequently argue that it is communities, instead of primarily individuals, who produce knowledge. The argument for communally-generated knowledge is founded on two grounds: First, our thoughts reflect the ethos of our time and (cultural and geographical) space. Second, our personal beliefs are transformed into knowledge when they are socially legitimated. (65) From these contentions, feminists hold that the subjects of knowledge are heterogeneous

[17] Harding identifies this understanding of objectivity tied to impersonality, impartiality, disinterestedness and value-neutrality as "objectivism".

and multiple.[18] The response of mainstream epistemologists is that this view inhibits objectivity as it allows for contradictory and incoherent accounts which undermine the universality of knowledge. For Harding, however, the choice between adopting universal standards in order to have an objective view or falling into epistemic relativism is a false dichotomy. In fact, the objectivism of mainstream philosophy of science itself is responsible for this false dichotomy. According to Harding, surrendering universal standards does not necessarily lead to a vicious epistemic relativism where there is no possibility of judging competing accounts.[19] This is a very important point for the consistency of feminist conceptions of objectivity and I shall proceed with examining it in detail.

3.3.1 Feminist Standpoint Theory and Epistemic Relativism

FST's contention that social locations structure beliefs and the norms by which these beliefs are assessed leads to the idea that different social locations generate different and sometimes contradictory beliefs and/or accounts regarding the same phenomena.[20] In order to have a sound inquiry into the relationship between FST and epistemic relativism we need to have a closer look at what this contention amounts to. At first glance this line

[18] This point also distances FST from classical Marxism. As Potter puts it, according to Marxism a proletarian standpoint yields a disinterested and objective account. Here, similar to the traditional empiricist conception of the universal knower, the proletariat is the unitary epistemic agent. (Potter 2006, 135)

[19] Interestingly, Harding notes that this false dichotomy has been embraced both by the proponents of universal standards and those who tolerate epistemic relativism as the cost of their insistence on the situatedness of knowledge. (1991, 139)

[20] Recall the Marxian explanation about diverse accounts given by the ruling class and the ruled on the social relations among them.

of thought might seemingly lead to "Truth relativism" where each (contradictory) account is admitted as equally true. However, at the heart of standpoint theory is the claim that some of these accounts (or beliefs) are less distorted than others. All claims, therefore, are not equally true. The grounds on which we decide which accounts are less distorted are, nonetheless, a challenging question for FST. This problem does not arise in a philosophical system that is committed to universal truths. According to this position, there could only be one true account of the same phenomenon. This view is an expression of a commitment to a correspondence theory of truth and the mind-independent existence of an absolute reality. Hence, within such a system evaluating whether a belief is distorted or not becomes a function of its correspondence to (absolute) reality. In other words, distorted accounts are merely false accounts. Nevertheless, those who advocate for FST do not share these commitments. They often talk of degrees of distortion and "truer" accounts. Objectivists find a tension in FST's insistence on more or less distorted accounts. Their objection runs as follows. On the one hand, FST claims that starting off research from the point of view and experiences of women provides less partial and less distorted stories about the social and natural world than research which reflects men's vision. Yet, this view seems to imply a value-neutral position against which we can decide that women's stories are less partial. On the other hand, FST criticizes claims for the existence of one true story generated from a value-neutral point of view. In fact, the consistency of FST is difficult to grasp without understanding Marxist ideas of different but interrelated levels of reality (the totality). Roughly put, there is a story of the world told by the dominant group and another that could be told by the oppressed. The dominant

group seeks to structure accounts of the world to their benefit and to have their accounts accepted as True. However, these partial accounts produced by the dominant group are revealed to be far from true, incomplete, distorting, and mistaken when looked at from the lives of the oppressed. Since the account of the oppressed group is more accurate and comprehensive than the dominant group's it is less partial and less distorted. But why not just accept that the dominant view is false and the oppressed view *true*? Because unlike orthodox Marxists, feminist standpoint theorists emphasize that social relations cut across various complex aspects of life. For FST there can be social locations other than those of the proletariat (or of women) that could produce more accurate accounts of certain phenomena. Claiming that any specific group produces *the true account* overlooks the heterogeneity and fluidity of networks of domination. For FST claims of less partial accounts are made *in relation to the dominant view*. Hence these claims are supported not by the value-neutrality of the views of the oppressed, but by being situated in a certain way within systems of domination that generate a critical viewpoint.

Another form of relativism that FST's contention could yield is "Judgmental relativism" that I discuss in the second chapter with respect to Kuhn's position. Recall that Judgmental relativism inhibits dialog and comparisons of alternative accounts. If all beliefs and/or accounts are inevitably functions of social locations as FST maintains, then there is no value-free point of view against which these beliefs could be evaluated. What this implies is that a claim could be accepted as "reasonable" or "rational" according to one point of view (of a social location) while the same claim could be deemed "unreasonable" or "irrational" according to another point of view. According to the

objectivist, in the absence of a value-free point of view there could be no "reasonable" standards for evaluating and deciding between competing claims. In other words, FST's contention reinforces claims of "anything goes" and makes rational disagreement among different points of view impossible. However, this cannot be true for FST because, as Donna Haraway argues, endorsing "anything goes" is as dangerous and problematic as defending "a view from nowhere" or a "God's eye point of view". Similar to a "God's eye point of view", "anything goes" amounts to being everywhere at once and nowhere in particular. This contradicts hardcore beliefs in the embodiment of subjects, which feminists fiercely defend. Although feminist standpoint theorists acknowledge that different social groups have different patterns of belief and different standards for judging them, FST does not lead to a form of relativism that inhibits genuine dialogue and rational disagreement among different worldviews. Nor does it deny that there is a real world out there to which theories of knowledge have to respond in some appropriate way. On the contrary, the fact that a standpoint is an *achieved* position makes dialogue and disagreement possible, and indeed necessary. Given enough effort and will, any group, regardless of its social location, can in principle come to understand (if not agree with) the opposed point of view. In order to understand (and reject) the accusation of Judgemental relativism against FST we need to examine different conceptions of rationality.

The objectivist attack on FST can be summarized as follows. Proper knowledge requires rational standards that could distinguish knowledge from mere opinion. FST's contention that social locations structure beliefs conflicts with endorsement of rational

standards. Hence, FST does not yield proper knowledge. Let's have a close examination of the two premises that this conclusion derives from.

It is true that knowledge requires standards that distinguish it from mere opinion. It is also true that these epistemic standards cannot be random if we can reliably follow them in our knowledge ascriptions. So the problem that needs to be solved is how to escape randomness of standards. Often, randomness is avoided by attributing necessity to these standards. Of course, there are different forms of necessity such as metaphysical, epistemic, moral and so forth, each of which might have different implications for my discussion. But let me be content with claiming that *in general* the necessity of epistemic standards is grounded in their universality. But in what sense these standards are universal should be clarified. Also we need to examine whether there are other ways of accounting for the necessity of epistemic standards without endorsing universalism. Does the universality of epistemic standards imply that there is a fixed set of epistemic standards that (should) apply to *all* knowledge claims regardless of their content and context? If so, then the criterion of universality is excessively demanding, as the subjects (and objects) of knowledge vary to a great extent. Finding a set of standards that applies to all knowledge claims with their diversity in nature and content seems to be implausible. Perhaps we do not need to endorse the universality of epistemic standards in order to account for the necessity of these standards. We can, for instance, argue that certain epistemic standards are necessary for certain subject matters in certain circumstances.[21] Hence, although we

[21] For instance the epistemic standards that we operate with in claiming to know, say, when the next train departs would not exactly be the same as the standards that we

cannot talk of *a priori* standards waiting to be applied in all epistemic circumstances and activities, we can talk of epistemic standards that are fixed by their contingent circumstances. In this view, deciding on which epistemic standards to adopt is not a random matter, as not all standards can be applied for any specific content and context. Now, if there is a way to reconcile the necessity of epistemic standards with the idea that contingent factors such as contextual conditions have a bearing on which standards of knowledge to adopt, we could conclude that FST does not conflict with the idea that knowledge requires "rational" standards. I understand that the relationship between rationality and universality is a controversial topic, and I do not intend to resolve long-lasting disputes regarding this matter in this limited space. However, I believe that the connection between rationality and universality is not as straightforward as is often accepted.

The dispute about rationality and universality often revolves around the controversy over assessing the rationality of competing accounts of a phenomenon.[22] The "scientific" explanation of a thunderstorm and ancient people's explanation of the same phenomenon by appealing to the anger of Zeus is one such example. It is often maintained that the former is a rational explanation, whereas the latter is an irrational explanation. In

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operate with in affirming or denying a relation between certain behaviors and specific hormone levels, as the latter is a far more complex phenomenon than the first one. Or as contextualists argue, when the stakes are high we tend to appeal to stricter epistemic standards and deny having knowledge of a certain phenomenon which otherwise we would have claimed to know.

[22] Strangely, the discussion about rationality of competing accounts often involves a comparison between drastically different worldviews. See Boghossian (2006); Wilson (1974). However, the problem of theory choice is by no means limited to cases where two theories are *drastically* different.

examining this view, let's start with some basics. Very simply put, rationality is what is present in the state of being rational and being rational refers to thinking and acting in accordance with reason and/or logic. A rational belief, for instance, is a belief that is sensible and logical, i.e. consistent with the principles of logic. According to the view that the principles of logic are universal, the rationality or irrationality of a belief does not vary across different cultures or systems of thought. However, rationality is also a normative term and an ascription of rationality implies an achievement. For instance, one's beliefs are rational if they conform to one's reasons to believe. Hence, they are rational if they are self-consistent (or internally consistent). Even with this basic information we can think of different ways of approaching the issue of rationality and universality. For example, we can claim that explaining thunderstorms as acts of a capricious god is irrational because it is not logically consistent. However, we can also reasonably claim that such an explanation is rational because it is consistent with the features of the worldview of which it is a product. In other words, it is self-consistent. Which principle should "we" appeal to in judging the rationality of an account? A problem with the first view is that there are compelling arguments about the conventional character of logical principles, as I mentioned in the previous chapter. If these arguments are correct, then appealing to logical principles in assessing rationality does not provide the universality that the objectivist seeks. If the principles of logic are indeed conventions of the western worldview, then any judgment based on them cannot go any further than being an application of the principle of self-consistency within the Western world. Some might claim that this principle of self-consistency is also a principle of logic and even if some

principles might be conventional, some others, such as this one, are universal, hence our assessments can rest on these principles. But applying the principle of self-consistency in assessing rationality would not provide us with a definite answer as to whether an account is rational by itself or not (which is what objectivists seem to be after). On the contrary, it would compel us to qualify our assessments. Rather than claiming that "the ancient explanation for a thunderstorm is irrational", we should state "according to the Western point of view the ancient explanation for a thunderstorm is irrational because it is not consistent with Western standards". This qualification, however, is not compatible with the idea that there is one and only one rational explanation for the same phenomenon.

Does this conclusion imply that we cannot "rationally" compare two competing explanations? I think accepting this conclusion would be an extension of a misleading belief that there can be *a priori* assessments about the rationality of an account, decision or action. This misleading belief often neglects the sense of rationality that refers to goal-directedness. Steven Lukes, for instance, identifies different senses of rational action.[23] In the widest sense, it is simply goal-directed-action.[24] This sense of rationality dictates that there cannot be a sensible assessment of the rationality of two competing accounts

[23] In talking about the irrationality of a belief or set of beliefs, Lukes notes that beliefs are irrational if they are illogical (inconsistent or self-contradictory), partially or wholly false, nonsensical, situationally specific or ad hoc, based on insufficient evidence and held uncritically. (Lukes 1974, 207)

[24] More elaborate formulations of rationality with respect to goal-directedness are "the sense in which an action is said to be (maximally) rational if what is in fact the most efficient means is adopted to achieve a given end"; "the sense in which the means that is believed by the agents to be the most efficient is adopted to achieve the agent's end (whatever it may be); " the sense in which an action is in fact conducive to the agent's (expressed or unexpressed) 'long-term' ends"; "the sense in which the agent's ends are the ends he ought to have." (Lukes 1974, 208)

without considering the very goal "we" want to achieve in choosing one explanation over the other. In other words, "we" make assessments of rationality against a background of beliefs and intentions even if "we" are often not aware of them. Hence, "we" deem the ancient explanation of thunderstorms irrational because it does not suit "our" western belief in an orderly universe, and our goal of achieving an account of the universe that reflects this order. Some might object to applying this "teleological" sense of rationality in deciding between two competing accounts, as this sense concerns actions and belongs to the practical domain. However, we have seen in the previous chapter that some of the widely accepted epistemic principles for "rational" decisions, such as simplicity or fruitfulness, also convey practical concerns. Moreover, whatever it is grounded in, at the end of the day decision-making is a form of action.

Returning to the question of whether "we" can "rationally" compare two competing accounts, I suggest that as long as "we" can clearly set out the goals and intentions behind each explanation as well as "our" (assessors') motivations in assessing these accounts, "we" can sensibly compare them. Of course, this view is predicated on the ideas that i) motivations are discernable and ii) adherents to different worldviews can *at some level* understand one another. In other words, it assumes that there is *some* common ground among different worldviews that allows for adequate communication.[25] Once we set out the intention(s) behind an explanation, then we can assess its rationality with respect to its success in achieving the intended goal. Accordingly, if two competing

[25] Donald Davidson's claim that there cannot be drastically different conceptual frameworks lends support to this idea.

explanations share *the same goal* that "we" intend to achieve, we can reasonably deem the one which achieves (or comes close to achieving) this goal as rational and the other—if it fails to achieve the intended goal—as irrational. For example, in assessing the rationality of the "scientific" and the ancient explanation of a thunderstorm, "we" need first to clarify the intended motive behind these explanations as well as the underlying motive that assessors ("us") want to achieve in choosing one explanation over the other. Although there is no definite answer to the question of what science is, it is fair to claim that science aims at systematic understanding, predicting and controlling the world. If the ancient explanation shares *this aim*, then "we" can claim that it is not a "rational" explanation, because although it succeeds in giving some sense to phenomena it fails to systematically predict and control the world.[26] If, on the other hand, the ancient explanation merely aims to cope with the world by attributing some sense (whatever it may be) to phenomena then it is rational as long as it achieves its end.[27]

What this discussion indicates is that we cannot reasonably assess the rationality of an account by appealing to the standards endorsed by another account for a specific goal, if the account under assessment does not share that goal. The absence of a common goal or intention, however, does not altogether prevent comparisons among different

[26] Here I do not suggest that science is the only paradigm for rational thought. What I claim is that we can deem the ancient account irrational *if* we intend to have explanations that lead to systematic understanding and controlling of phenomena.

[27] This conclusion is unlikely to be accepted by those who endorse a necessary relation between "truth" and "rationality". According to them a "false" explanation cannot be rational. But such a commitment will run into serious problems when we question the rational status of falsified theories. For instance, should we conclude that phlogiston theory is irrational because it provided a false explanation?

accounts, as we can still discuss the worth, morality and/or utility of the pursued goal and the underlying intentions of different accounts. But this would be a meta-level discussion where social, moral and political considerations come into play. In other words, the decision on which explanation to endorse cannot ultimately be grounded on *purely* epistemic grounds. Yet, it is not unreasonable to think of two cultures coming together and discussing the grounds (including the intentions) on which their beliefs on, say, thunderstorms rest. If one culture manages to convince the other about the efficiency of their standards (as well as the worth of the result which their standards aim to achieve), then they could agree on which principle(s) to adopt in assessing judgments regarding thunderstorms. However, the mutual agreement would not make the principle universal.

In sum, objectivist reactions to FST mainly stem from its identification of objectivity with value freedom. According to objectivists, the ideals of value-neutrality and disinterestedness are intrinsically related to rationality because they seemingly reinforce the unity and universality of knowledge. But the relation between rationality and universality is not beyond doubt. Many feminists, among them Harding, have argued that identifying objectivity with value freedom is misleading and dangerous. Objectivism prescribes that a value free and impartial objectivity should guide scientific research. In doing this, it indiscriminately ignores the historical and social values and interests that in practice shape the agendas, contents and results of science. Hence, Harding concludes that objectivism yields a partial and distorted explanation of the growth and success of science. Let me now elaborate on Harding's critique of the identification of objectivity with value freedom.

3.3.2 Harding's Critique of Objectivity as Value Freedom

Harding questions the efficiency and plausibility of pursuing absolute value freedom in scientific research. She specifies two problems with the objectivist conception of objectivity: first, she argues that in certain ways objectivity is conceptualized too narrowly, and second, in other ways it is conceptualized too broadly. (Harding 1991, 143-4)

According to Harding, objectivity is conceptualized too narrowly, and therefore weakly, because although it is supposed to help identify *all* of the social values embedded in scientific practice and eliminate them, what it could at best do is "identify and eliminate only those social values and interests that differ among the researchers and critics who are regarded by the scientific community as competent to make such judgments." (143) Hence, shared (biased) values of the scientific community are not detected, because when values are shared by the whole community, they become invisible. Since these values operate in various parts of scientific practice, from problem selections to data and evidence evaluations, they inevitably affect the outcome. A dangerous result, then, is that biased outcomes of research pass into "normal science", i.e. they are accepted as yielding "objective" scientific knowledge. Hence, in objectivist conceptualizations, the ideal of objectivity fails to achieve what it purports to achieve.

Objectivity is also conceptualized too broadly by objectivists because of its insistence on the elimination of *all* social values. For Harding, though, not *all* social values and interests are "bad". In fact, certain social values are often the force behind progress in science: social values help to depict problems, to decide on how to proceed

with research, and to foster or impede the acceptance of theories. In other words, social values are present both in the context of discovery and of justification.

While this last claim is controversial for objectivists, real examples in the history of scientific accomplishments support it. For instance, certain religious and philosophical commitments played a role in the acceptance of theories regarding the motions of planets. Some led to false models that hindered scientific progress while others contributed to adopting better models. Notably, the ancient Greeks' commitment to ideals of an orderly universe, wherein the planets must move in perfectly circular motions with uniform speed, influenced subsequent explanations of the planets' movements. When anomalies appeared, these were explained away (or hidden) by incorporating more and more epicycles into the model. Although earlier heliocentric models were more accurate in certain respects, dominant social forces favoured geocentric models—irrespective of their failure to explain certain phenomena—because geocentrism fit better with the other prevailing beliefs and structures of social power of the time, such as Aristotelian physics and the scriptures that kept man at the centre of the universe. Eventually, Ptolemy's geocentric model which required multiple epicycles, and which embraced circular motions for planets, triumphed over other simpler but non-circular models which also fit the observed data. Nevertheless, the Ptolemaic model violated the ideal of uniform motion. According to this ideal all motion in the heavens is constant and circular. In this model basic planetary motions were accounted for by attaching planets to epicycles. Yet, this was not enough to explain the detailed motions of planets on the celestial sphere. Hence, the model was modified (e.g. epicycles themselves were placed on other

epicycles) numerous times. Finally Copernicus's heliocentric model took over after being refined by Kepler who denied that perfect circles and uniform motion could account for the motion of planets. It is important to note that although the heliocentric model violated traditional ideals, it still conveyed a commitment, a faith in the order of the cosmos that has prevailed since the time of Pythagoras.[28] It should also be emphasized that the acceptance of Copernicus's model was not due merely to its predictive value or its compatibility with data, for the Ptolemaic model also fit the observed data, and produced remarkable mathematical accuracy. What this episode in the history of science shows is that certain social values affect the choice of a prevailing theory, with sometimes positive and sometimes negative effects. While this episode too could be explained away as belonging to a time when science was in its infant stages whereas today's mature science is immune to such "external" factors, a careful look at some of the 20th century's most important discoveries again reveals how social values still shape scientific practice.

The interplay between interests and scientific practice becomes more apparent when we consider why certain research programmes commence at certain times and in certain places. How research on yellow fever started off is one such example. Yellow fever was endemic throughout tropical Africa and Latin America. At the beginning of the twentieth century, combating this disease became "scientifically" important because the US was planning to increase its presence in Latin America. US authorities commissioned a research team composed of bacteriologists and physicians to study how yellow fever

[28] For a detailed explanation of this shift from the Ptolemaic model to the Copernican model see Haely (2008).

spread. The team concluded that a certain virus, often carried by a particular mosquito species (*Aedes egypti*) that is widespread in subtropical regions, was the cause of the disease. Once ways of eradicating the mosquito were discovered, authorities introduced it into places such as Panama where the disease was endemic. This process enabled American contractors to begin constructing the Panama Canal.[29] American interest in mitigating the effects of yellow fever are evident, yet its interest also ended up saving thousands of lives.[30]

An objectivist would likely deny the relevance to scientific methodology of interests in commencing certain projects. Such issues, after all, are confined to the context of discovery not of justification. However, such an understanding of science is not only narrow but also dangerous: dangerous because this view strips the most significant forms of responsibility—e.g. responsibility for life—from science and scientists. It generates a picture of science and scientists whose responsibility is only to the data. In the event of a catastrophe a scientist, with a clear conscience, can claim that s/he did what s/he was commissioned to do. This possibility is not unlike the American pilot's response after bombing Hiroshima—it was his patriotic duty to do the right thing. But what justifies attributing this "amoral" status to scientists? What gives scientists the ring of Gyges, enabling them to do what they will without being seen, i.e. being responsible? These questions cannot arise in an understanding of science confined to the so-called context of

[29] This example is taken from Lee (2002).
[30] Of course there are the issues of how the building of the Canal and the American presence affected the ecosystem or the lives of people in the region. Hence, discussions of the benefits of values in science need to be qualified.

justification. An objectivist might claim that these questions do not belong to "philosophy" of science, but to the "ethics" of science. However, although a question such as whether experimenting on animals is right or wrong is relatively easy to distinguish as an ethical question rather than a scientific one, certain moral considerations are so intertwined in scientific practice that they become essential parts of the methodology and the evaluation. In such cases, the distinction between *proper* "philosophy" and "ethics" becomes blurred. One of the most prominent examples of this collaboration between "philosophy" and "ethics" can be found in the works of Barbara McClintock in plant genetics.

Although McClintock's discovery of genetic transposition changed the course of studies in genetics, it was not appreciated at the time. Briefly, her discovery was that not all genes are fixed on their chromosomes. That is, there are some parts of DNA that can move around from one place to another, and that have a big impact on other genes around them. Her insistence on studying inheritance in organisms such as the maize plant, which can produce one generation each year, puzzled her contemporaries who were working on flies or microbes that reproduce frequently. Yet her work on identifying particular regions on a maize plant's chromosomes led her to be able to spot when a gene had moved. (Lee 2002, 111) According to Evelyn Fox Keller, who has done extensive work on McClintock's views of nature and science, the initial incomprehension of and dissent against her work was due to vast differences between her vision of biological organization and that of her colleagues (Keller 1996, 161). Unlike her contemporaries who sought unities in nature, McClintock's methodology rested primarily on "respecting differences

in nature". According to McClintock, nature is far more complex than our human minds can fully comprehend. Therefore, fitting everything into a set of dogmatic explanations should be resisted. This distinct methodology, which informed the questions she asked as well as the explanations she sought, led McClintock to detect phenomena that her colleagues failed to see. McClintock's insistence on respecting individual differences and her willingness to explore these differences (rather than discarding them as exceptions or aberrations) led to her observing a different pattern of pigmentation on a few kernels of a single corn plant. This observation then led to the discovery of genetic transposition. (163)

For McClintock the principle of respecting differences in nature consists in "listening to the material" and valuing the uniqueness of each organism. This principle was followed by paying due respect to the integrity of one's subject matter (in McClintock's case, the integrity of each kernel). Here McClintock's methodology intersects with ethics. As long as ethics govern our modes of conduct (including scientific conduct) the required intimacy with objects of thought doubles both as a principle of thought and a moral consideration. In other words, "listening to nature" is an epistemic principle which aims at revealing the nature of the material at hand. Yet it is also an ethical principle which guides us in determining how to approach a subject matter. This double act suggests that it is through a moral way of approaching nature that we can adequately understand its workings. However, for some this intimacy puts objectivity in question. Still, Keller notes that "McClintock can risk the suspension of boundaries between subject and object without jeopardy to science precisely because, to her, science

is not premised on that division." (164) Within philosophies that do not rest on a subject/object dichotomy the interplay between philosophy and ethics can be seen more clearly. It is in these philosophies that we can find new and fruitful ways of conceptualizing objectivity.

Once it is accepted that social values affect science and that some effects are better than others for the progress of science, the question that follows is which values are better and why. For FST, the standpoints of women and the values associated with those standpoints are better because, as a result of historically being an oppressed group, they are better situated than men to generate new and critical questions about both women's and men's lives, and the world around them.[31] One compelling problem with this position is that there might be cases where the values of the dominant group could help science progress more fully than the values of the oppressed group. Harding's discussion of how modern science has historically been led by western, bourgeois, and patriarchal values admits that these values have had both positive and negative effects on the development of science. Accordingly, we are compelled to accept that starting research from women's standpoints would not have uniform, universally positive results of the kind Harding anticipates. This recognition does not necessarily weaken FST. For Harding, FST "wants to eliminate dominant group interests and values from the results of research as well as the interests and values of successfully colonized minorities—loyalty to femininity as well as masculinity is to be eliminated through feminist research." (Harding 1993, 74)

[31] It is important to note that the oppression of women varies across class, race, age and so forth.

The main concern of FST is not merely to replace the male standpoint with an exclusively female one, but with a critical vision that reveals invisible biases in dominant thought. Except perhaps for a radically exclusionist feminist group, FST's assertion of the advantages of starting research from women's lives is not grounded on intrinsic features of womanhood. Rather, FST focuses on features of the ways in which women are situated as a result of social and historical marginalization. Because marginalization often prompts a critical stance in relation to established orthodoxies, a feminist standpoint can be more prone to detecting gender inequalities within a practice. According to FST, the decision about which values could provide better accounts, and help scientific progress requires rigorous analysis of the historical and material conditions within which a research study is undertaken, alongside a consideration of who benefits from it, and whose interests are being served or dismissed. Such analysis constitutes the heart of Harding's conceptualization of "strong objectivity".

3.3.3 Strong Objectivity

According to Harding, the existence and role of social values within scientific practice calls for a "strong objectivity" which requires "strong reflexivity", i.e. a critical examination of how nature as the object of human knowledge is constituted in social thought. Such inquiry includes a broad analysis and a systematic examination of the social values that shape scientific community and practice. Since female vision has historically been undervalued in the formation of scientific thought, and "normal" assumptions and practices of science have generally been structured by male vision, starting off from

women's lives would help shed light on discrepancies within the "normal view" that could otherwise remain invisible. This suggestion, however, is by no means definitive. There are problems with ascribing epistemic privilege to a particular group, which I will discuss in the next section. For this reason I maintain that interpretative and genealogical analyses of relations of power and social imagery should complement "strong objectivity".

It is true that "strong objectivity", which requires a rigorous study of the social sources of beliefs (including false beliefs), might be too idealistic and as unattainable as the objectivist ideal of eliminating all social values within scientific practice. Nevertheless, according to Harding, strong objectivity can be accepted as a desirable standard, adoption of which could improve science. By contrast, pursuing the mainstream ideal of eliminating all social values within science (i.e. value-free objectivity) benefits only the interests of dominating classes by reinforcing the illusion that human ideas can systematically transcend their specific historical location. This illusion discourages any attempt to analyse and show how the ideal of "disinterested" science maximizes the interests of the dominant classes. It also offers hope that scientists and scientific institutions "can produce claims that will be regarded as objectively valid without having to examine critically their own historical commitments from which, intentionally or not, they actively construct their scientific research." (Harding 1993, 71) Thus, as I discuss above, one of the most important and dangerous consequences of this misperception is that it helps scientists to ignore the origins and consequences of their research programs and practices, hence to refuse any liability for them. It can be claimed that by emphasizing

this moral aspect, FST brings ethical and political considerations into conceptualizations of objectivity, not in the sense that a scientist's duty is to be value-free, but in the sense that a genuinely objective account is a responsible account which is sensitive to possible biases it might convey as well as to the contingency of its sources. We will see a similar moralization of objectivity in Longino's account where objectivity serves as social control of biases. In the next chapter I will argue that Longino's conceptualization of objectivity should be backed up by "strong objectivity". Now let me examine some of the postmodernist criticisms of FST.

3.4 Postmodernist Criticisms Against FST

A common criticism of FST is that it assumes a unified nature for women and fails to recognize differences among women. Yet most feminist standpoint theorists reject any idea that there can be a single privileged position. Obviously, not all women's experiences are the same. Social life is stratified by interconnected layers of race, class, sex, age, education and so forth, each of which operates within and across different systems of power domination. With which women's lives, then, should we start research? Given the logic of FST, it is reasonable to think that, for example, starting off research from black women's lives would provide less partial claims than research starting from the lives of white women. Yet, this line of thought runs into a problem if we take a couple steps further and claim that starting research from the lives of lesbian disabled black women's lives would provide better accounts than the lives of lesbian black women. As we keep specifying the least privileged people in society, we could end up with a vast number of

situated singulars. This would render standpoint theory implausible and jeopardize any potential political struggle. If there is a vast number of distinct socially marginalized groups, then how are we to distinguish which group is more epistemically privileged? Bar-Ami Bar On writes,

> when one among a multiplicity of socially marginalized groups is claimed to have more epistemic privilege than the others, the usual criterion for justifying such a claim is the extent to which the group in question is peripheralized. Epistemic privilege then becomes a function of the distance from the center. Presumably the more distant one is from the center, the more advantageous is one's point of view. (Bar On 1993, 89)

She continues by pointing out that this way of thinking rests on the idea that there is a single center of power that brings about all sorts of oppression. Today, it is largely accepted that different forms of domination, such as racism, sexism, elitism and so forth, are institutionally structured in various ways in social life. Yet, it would be wrong to claim that FST ignores this multiplicity of power structures. In fact, FST rejects the orthodox Marxist idea of attributing epistemic privilege to one socially distinctive group of people. According to FST, there is no single, ideal woman's life from which thought should start. Harding emphasizes that different groups of women produce different feminisms and "each of these groups of women's lives is a good place to start in order to explain certain aspects of the social order …different feminisms inform each other; we can learn from all of them and change our patterns of belief." (Harding 1993, 60) Still, it is true that FST follows the Marxian example of utilizing commonalities among the oppressed in order to produce an effective analysis. It is fair to claim that FST has a tendency towards generalizations and that these generalizations necessarily ignore certain

differences among women. This limitation however, should not be construed as a failure. First, given the diversity of groups that utilize the logic of standpoint theory, such as African Americans or Third World people, standpoint theory also helps to emphasize their differences. And second, the tension between the necessity for generalizations and the possible problems they may generate could be turned into a fruitful resource once the multiple layers of society and the interactions among them are made visible and acknowledged. For one thing, it would compel inquirers to be extra careful.

However, both Jane Flax and Bar On find this optimistic attitude to standpoint theories problematic. Flax argues that the logic of standpoint rests on the ungrounded assumption that people act rationally in their own interests and that "the oppressed are not in fundamental ways damaged by their social experience."(Flax 1987) That is to say, since both social and natural reality are structured by the dominant groups' conceptualizations and categorizations, oppressed visions are inevitably affected and possibly corrupted by these categorizations. Yet the question of how such corruption can be assessed is a pressing issue. This criticism seems to assume that there can be a pure unsullied point from which a lack of corruption could be judged.

According to Bar On, on the other hand, grounding epistemic privilege in the practices and identities of socially marginalized groups runs into the following problem: In the process of validating the experiences of socially marginalized subjects, there is a certain degree of idealization that excludes some practices while including others. However, this process presupposes "that there are practices that in one way or another are more authentically expressive of something about the oppressed groups." (Bar On 1993,

92) For instance, nurturing and domestic practices become associated with women even though women's practices are not confined to these alone. In addition, nurturing and domestic practices are typically related to women's identity as defined by the system that oppresses them. That is why Bar On notes that attempts to reconstruct or reclaim the authentic practices of a socially marginalized group are always tainted by oppression as their original meanings change through their interactions with the practices of the oppressive system. (94) This is a very important point, especially for issues pertaining to self-identification and the internalization of stereotypes. However, it does not necessarily undermine FST. Bar On's objection could apply to earlier feminist standpoint theorists who privileged feminine traits such as nurturing, caring and so forth in acquiring knowledge.[32] In Harding's version of FST, however, the benefit of starting research from marginalized lives is that their collective concrete experiences make the marginalized more sensitive to biases. The logic of Harding's FST does not rest on traits (essential or not) common to the marginalized group. Bar On could claim that the traits tainted by oppressive systems could be collectively internalized by the oppressed to the extent that it could affect their experiences and hinder their capacity to notice biases. A woman who internalizes an identification with the maternal instinct as essential to being a woman provides such an example. In the absence of a maternal instinct, she would think that there is something wrong with her, rather than locating this feeling of inadequacy within an oppressive system. In such cases, starting research from marginalized lives does not yield

[32] One example of the sort is "Maternal Thinking" of Sara Ruddick, and another is "Object Relations Theory" of Keller which I will briefly discuss in the next chapter.

higher objectivity, as biases also pass unseen by the oppressed. That is why Harding insists that her prescription applies to everyone, not just the marginalized.[33] In other words, everyone should start research from marginalized lives.[34] But how could we mitigate the discursive damage utilized by oppressive systems? Harding's elaboration of "strong reflexivity", which focuses on the social and material conditions of biases, seems to be insufficient to erase the intricate workings of domination in social imagery. Postmodernists and poststructuralists have set out some interesting theoretical tools to reveal discursive oppressions that have concrete effects on the ways people experience and understand the world.[35] If "strong reflexivity" aims to contribute to "strong objectivity" it should encompass and make use of these theoretical tools.

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[33] It is also why she insists on the value of consciousness raising.

[34] Harding writes, "…in societies stratified by race, ethnicity, class, gender, sexuality, or some other such politics shaping the very structure of a society, the *activities* of those at the top both organize and set limits on what persons who perform such activities can understand about themselves and the world around them … In contrast, the activities of those at the bottom of such social hierarchies can provide starting points for thought –for *everyone's* research and scholarship—from which humans' relations with each other and the natural world can become visible. This is because the experience and lives of marginalized peoples, as they understand them, provide particularly significant *problems to be explained* or research agendas." (Harding 1993, 55)

[35] For instance, deconstructivist, interpretive and genealogical projects set out interesting strategies. The examination of these projects exceeds the purpose of this thesis. For a discussion of the differences between the interpretive and genealogical projects see Code (1998). According to Code, postmodern feminism has different strands: Interpretive strategies carried out by Linda Alcoff and Susan Hekman focus on the interpretations of cultural and historical assumptions underlying the processes that yield knowledge. The genealogical strand, on the other hand, "situates knowledge-production within historically changing structures of power, maintaining the radical contingency of currently hegemonic modes of understanding, legitimating, and establishing knowledge claims."

Bar On points to another critical problem about epistemic privilege. According to her, attributing epistemic privilege to oppressed groups while they do not have the power to actually exercise or benefit from it is very dangerous. Claims of epistemic privilege have been used historically to legitimize the voices of marginalized groups and to justify the authority of the marginalized to speak for themselves. Nevertheless, Bar On is suspicious of authorizing the speech of the marginalized. This is because the practice of authorization excludes and silences certain people and commands obedience to the authorized. Yet, in reality, oppressed groups do not have the power to exclude, silence or command obedience from dominant groups. She maintains that the claims for epistemic privilege

> lacking a social power on which to base them, cannot yield the same results as the self-authorizing claims of a dominant group and are, therefore, merely normative, compelling only to those who are theoretically persuaded by them, usually members of the socially marginalized group who find them empowering … by claiming an authority based in epistemic privilege the group reinscribes the values and practices used to socially marginalize it by excluding its voice and silencing it and commanding its obedience to the voice of the dominant group. (Bar On 1993, 96-97)

By having no real (perceivable) impact on the visions and on the members of dominant groups, the epistemic privilege of the oppressed would not only be self-deceptive. It also could be used as a theoretical tool to further their oppression by the dominant group. For one thing, grounding epistemic privilege on the practices of the oppressed groups confines them to speak only for themselves and only on issues that relate to their experiences. It also helps the dominant group to dismiss the problems relating to the oppressed groups on the grounds that they do not have epistemic access to oppressed groups' experiences. This

oppression is similar to Harding's discussion about the dangers of judgmental relativism. Harding claims that judgemental relativism is a conventional theoretical tool for those who acknowledge certain inequalities but are unwilling to change the underlying practices that generate them. Claiming, "it is all relative" is an effective and effortless way to dismiss real challenges. (Harding 1993, 61) Bar On writes, "[s]peech needs to be authorized only where silence is the rule. This is an oppressive rule. It need not be obeyed, and the justification of disobedience in this case is not a special kind of expertise guaranteed by epistemic privilege but rather by the demands of justice." (97) I agree with her on this point. However, demands for justice first require an awareness of injustices. According to Harding such awareness of the social and material circumstances that inquirers are in is possible through strong reflexivity. Yet, as I argue above, "strong reflexivity" should also be supported by discursive analyses. But even this is not enough. The demands for justice must also be heard and taken seriously. How this could be done is crucial. No doubt the marginalized should have an active role. They should analyze and reflect on their situations, and persist in voicing their concerns. However, since it is always a challenge for the marginalized to find a voice and a venue for speech, it is vital to regulate critical discourse diligently so that the marginalized can be heard and appropriate changes can be set in motion. Harding does not elaborate on such necessary regulations. Longino's account of objectivity offers an interesting attempt in that direction, as I will show in the following chapter.

3.5 Conclusion

In this chapter I have discussed the strengths and shortcomings of FST. I believe FST has played a very important role in opening up new epistemic terrains and showing that feminist epistemology is not only possible but also very informative. I also believe that starting thought from marginalized lives is a useful and plausible strategy to expose biases in certain areas of research. However, as Harding admits, it is necessary but not sufficient to guarantee objectivity.

FST is mostly a general critique of science (and philosophy of science) that does not elaborate on the workings of specific sciences.[36] As such, it falls short of providing an account of objectivity that is applicable to a broad range of scientific concerns. That is, although it is very informative, FST does not formulate a specific enough account to replace or to compel theorists to modify the mainstream understanding of objectivity. A compelling alternative that would bring about change in dominant conceptions of objectivity, in my opinion, is one that can communicate with the dominant view, challenge its very principles, and shake its consistency.[37] In doing so, a compelling alternative could *change* the dominant view from within. I emphasize change here, as changing the way science is conceived and the ways in which it works in favour of feminist causes is my primary principle in assessing feminist critiques of science and philosophy of science. Establishing a dialog with the dominant view requires some

[36] Elizabeth Potter's work on Boyle's law of gases might be an exception to this general outlook.

[37] The importance of maintaining a dialog with mainstream philosophers of science is also stressed by Hundleby (2012).

common ground to achieve communication.[38] This means that a compelling alternative might not be as radical (whatever that means) as one wishes. It might require using the tools of the oppressors to dismantle their power. I think this would be justified as long as it makes a positive difference in matters of epistemic justice and injustice. Moreover, a compelling alternative should not only provide criticisms but should also set out a normative account. Although Harding prescribes starting research from marginalized lives, this is too broad a prescription to constitute an alternative epistemology or philosophy of science. I believe Helen Longino's account of objectivity addresses these concerns better than FST. Having said this, I do not claim that FST has no explanatory power or that it is unimportant. In the next chapter I argue that certain FST views could strengthen Longino's conceptualization of objectivity. My point here is that a critique of the "received view" that could transform its understanding of objectivity should attack specific assumptions of empiricism. Any transformation must also compel the community that adheres to the "received view" to reorganize the questions asked and reconsider priorities within the philosophy of science. That is exactly what Longino does.

[38] This point is also emphasized by Nelson: "The point of feminist science criticism must, in the end, be to change science, and changing science requires changing the practices of scientists. Hence, scientists must be brought into the dialogue. Since scientists are empiricists, that dialogue will have to make room, at least in the beginning, for empiricists and for, at least as a topic of discussion, empiricism." (Nelson 1990, 6–7)

Chapter Four

Feminist Empiricism and Objectivity

In this chapter I focus on Helen Longino's understanding of objectivity, which stems from her contextual empiricism. I will argue that Longino's approach to objectivity constitutes a better conceptualization than the mainstream approaches to objectivity in the philosophy of science, if it is complemented by some aspects of the "strong objectivity" I have discussed in Chapter Three. To begin with I will briefly re-examine the empiricist tradition in feminism and then will turn my attention to contextual empiricism and the form of objectivity it yields. In the last section I will examine some of the criticisms brought against Longino and will defend her contextual empiricism.

4.1 Feminist Empiricisms

The main tenet of empiricism is that knowledge is grounded in experience. Yet, historically there have been various empiricist theories characterized by different empirical projects, such as British empiricism, logical positivism/empiricism and naturalized epistemology among others. Similarly, there have been various articulations of feminist empiricism, which address different concerns and problems. Yet it is somewhat misleading to talk about "feminist empiricism" as a distinctive feminist position. This is because, I believe, to the extent that feminists emphasize concrete and particular experiences they are all empiricists in one way or another.

Practicing female scientists in areas such as biology and psychology were among the first to generate feminist science criticism.[1] They specifically attacked theories about women and gender differences that legitimize and naturalize the sexist practices both in scientific practice and in society. However, their critique of the ways science was conceived and practiced was a rather weak critique because these women were trained within the same tradition as their male colleagues. Their criticisms did not extend to the main tenets and ideals of science. Briefly, the women scientists were engaged in and supported the attempts at eliminating biased practices, which were not believed to be internal features of science itself. In other words, their concern was to distinguish "bad science" from "good science". According to this distinction, biased theories are the results of misconceptions, misapplications of ideals, individual prejudices, and the recklessness of individual scientists. This situation led some scientists such as Rae Carlson (1960), Sandra Harris (1971), and Florence Denmark (1988) to advocate for greater inclusion of women in sciences both as practitioners and as objects of study. However, these solutions by themselves were weak solutions. Some more critical feminists argued that the source of androcentric bias within science was due to the intellectual structure and cognitive goals of science, which are expressions of male characteristics. Since the pedagogy that licences scientists prescribes androcentric intellectual structures and cognitive goals, unless there are some structural changes, those women scientists who are brought up

[1] Harding dubs the writings produced in the early 70s and 80s by these scientists "spontaneous feminist empiricist epistemology" because it reflects the "'spontaneous consciousness' of feminist researchers in biology and social sciences who were trying to explain what was and what wasn't different about their research process." (Harding 1993, 51)

within the same tradition as men would inevitably and inadvertently contribute to the reproduction of male biases.

Among those who advocate for structural change in scientific practice, Nancy Chodorow (1999) and Evelyn Fox Keller (1985) argue that dominant scientific norms and practices reflect the psychological and cognitive traits of white, Western, educated men in ways that serve the interests of this particular group. Against androcentrism, Keller and Chodorow defend female-defined communities in science. Central to their view is the belief that women and men in western societies have different psychological traits, which result in different cognitive styles and goals. According to them, scientific ideals such as universality, objectivity as complete detachment, autonomy, and impersonality all reflect male characteristics. In so far as science pursues these ideals it will always be vulnerable to androcentrism, and will benefit male privilege. In contrast, female characteristics buttress different values in research. For instance, male approaches to relationships (both to one another and to the object of their research) rest on competition and distancing whereas female approaches to such relationships are more collaborative and based on respect and trust. Such an approach, according to Keller, is exemplified in the works of Barbara McClintock. As I have mentioned in Chapter Three, McClintock defends having "a feeling for the organism" she works on rather than trying to distance herself from the object of her research. For Keller this feminine style would benefit science and could yield a kind of objectivity, namely "dynamic objectivity", where observers approach the observed with empathy, in order to understand the world around them. In arguing for the superiority of seeking "dynamic objectivity" Keller contrasts it with "static objectivity".

Underlying "static objectivity" is the presumption that in order to understand the world we need to conquer and dominate it. This domination is possible via distancing/placing oneself from/above the object of study. The traditional conception of objectivity as the "view from nowhere" is a typical exemplar of "static objectivity".[2] Yet, many feminists question the intelligibility of a "view from nowhere". Among the problems with this conceptualization is that it rests on a problematic ontology and methodology. This conception presumes that science aims at uncovering the nature of things as they really are independent of knowers. According to this understanding, grasping things as they really are is possible thorough complete self-extrication. Whether it is possible to attain knowledge of or even intelligible to talk of things as they really are or whether complete detachment from one's subjective conditions is possible are lingering questions in philosophy. It is important to realize, however, that at the base of these problems is a tacit belief in a sharp distinction between subjects and objects of scientific inquiry. This belief is problematic because it deems research unscientific where the objects of inquiry include or related to the inquirers themselves. Furthermore, contrary to experimental findings, this belief assumes that the subjective condition of the experimenter has no effect on the objects of the study.[3] In contrast to "static objectivity" where emotional distancing from the world/object of inquiry is required, "dynamic objectivity" prescribes connectivity with

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[2] Keller correlates the difference between adopting "dynamic" and "static" objectivity with the psychological development of two different senses of autonomy, namely "dynamic autonomy" and "static autonomy". Her differentiations and conceptualizations rest on the Object Relations Theory, the details of which I will not examine here.

[3] In Keller's psychoanalytical account ideal of "static objectivity" is distorted because it is an outcome of a distorted psychological development of boys which generates an exaggerated separateness (from the mother and the world), i.e. static autonomy.

the world/object of inquiry. Such connectivity, according to Keller, is maintained by a sense of union with the observed while the independent integrity of the world is preserved (Keller 1995, 117). Keller draws a parallel with this attitude towards the world with the practice of "empathy". She notes, empathy is "a form of knowledge of other persons that draws explicitly on the commonality of feelings and experience in order to enrich one's understanding of another in his or her own right." (Keller 1985, 117) According to those who adhere to "static objectivity," active engagement of the self with the object of study could lead to distortions in the pursuit of the things as they really are. Yet in an empathic approach, the integrity of the object of study is preserved because here what is prescribed is not that inquirers impose or project their concepts, categories or interests on to the objects of inquiry, but that they receive these objects into their self and "see and feel with the other".[4] In McClintock terms, you had to have the patience "to hear what [the corn] has to say to you" and "to let it come to you." (Keller 1983, 198)

As inspiring as it sounds, this line of thought, which seems to rest on rigid differences between male and female characteristics, faces a few problems. First, even if we accept the existence of such differences, it is questionable whether female-defined research programmes could be applicable to or benefit all sciences.[5] To be fair, I do not think Keller prescribes female-defined science as a universal rule. If she had, she would

[4] The difference between dynamic objectivity and static objectivity is closely related to the distinction between separate and connected knowings discussed in Belenky, Clinchy, Goldberger, and Tarule (1986, 100-129).

[5] Here there is also the question of how different ideals "benefit" science. Can we have a definite statement of what counts as benefiting science regardless of making clear what science is and what/who it is for?

have fallen into the same error as adherents to the androcentric convention that there is a universal method for all sciences. I have mentioned the problems with commitment to a universal scientific method with fixed standards, in the previous chapters. Many feminists, such as Longino and Harding, deny that there is a unique methodology that is applicable to all sciences. This is because which method to adopt is a function of the questions asked and the kind of knowledge that is sought.[6] Hence, although certain kinds of knowledge might require a "female-defined methodology" as Keller formulates, it may not be applicable to other kinds of knowledge. That is why general claims about the epistemic superiority of a female-defined science are problematic. Longino writes, "[h]owever much dynamic objectivity might appeal to us, there isn't a general argument to the truth of interactionism or to the epistemological superiority of dynamic objectivity." (1993, 108) The adoption of methodological (and theoretical) pluralism, however, leaves feminists with the problem of explaining how specific research programmes ought to adopt certain standards, and how subscribing to "local standards" does not undermine science. I will explore these questions when discussing Longino's defence of "local epistemologies".

Another problem that female-defined science faces, namely, emphasis on the differences between male and female characteristics, could be construed as endorsing essentialism. Those who adopt Object Relations Theory such as Chodorow and Keller typically argue that differences occur during male and female development (through childhood and adolescence), and these differences result in different gender-related styles of reasoning. If the psyche has biological origins, that is, if each sex goes through

[6] See Harding (1987); Longino (1993a).

biologically fixed psychological and radically different developments, then the charge of essentialism holds. However, Object Relations Theory is essentially about how one's relation to one's environment and other people is shaped by one's experiences in infancy, specifically those relating to primary caregivers. What follows from this theory is not that male and female modes of thought are intrinsically different, but that intellectual differences between men and women are due to differences in their upbringing which reflect and perpetuate a social division of gender roles. This implies, then, that the styles different sexes adopt are also a contingent matter. That is, men could, in principle, adopt female cognitive styles and vice versa.

For many feminists the idea that social relations affect cognitive processes and goals is key to arguing against androcentric bias in science. For instance, according to biologist Ruth Hubbard, science is part of a social process and it reflects the concerns, biases and presuppositions of the wider society. That is to say, insofar as the wider society is a patriarchal society and science is a white Western male dominated practice, the assumptions and practices of science inevitably reflect and benefit male privilege. For Hubbard, for instance, the practice of simplification (reductionism) in science is an extension of a combination of factors such as a masculine interest in controlling and managing the world, and a capitalist interest in exploiting nature. Specifically, she criticizes the way in which phenomena are defined in genetics where simplification is a standard practice. Yet, according to her, very few phenomena can actually be captured by simplification. In contrast, simplification often distorts biological evidence. For example, she argues that the samples chosen in modelling how "genes" work, e.g., fruitflies "which

have large giant chromosomes where you can actually see variations" or the bacterium E.coli "which doesn't have a nucleus but only has a single circular chromosome and where genes do seem to be continuous pieces of DNA", led to the misconception that "genes" are fixed in the DNA, and that there is a direct correspondence between traits and genes. (Aqueno 1997) And this misconception has fed misleading single factor causal explanations. In contrast, Hubbard argues that "DNA is an inert, sticky glop. It takes organisms or, at least, the enzyme systems extracted from them, along with other essential molecules, to perform the synthetic processes within which DNA specifies either the composition of its own copies or the composition of proteins." (Aqueno 1997, 5) What this means is that even a simple trait like eye color cannot be "caused" by a single gene because it involves the "participation of several proteins, the composition of each of which is specified by a different DNA sequence (or "gene")." One of the main reasons behind the misconception about genes, according to Hubbard is the reduction of DNA to a "code" that needs to be deciphered. With the announcement of the double helix model of DNA, which shows the way genes can get copied, scientists began to be engrossed in cracking the "genetic code" of life, and "the biological and chemical complexities of living organisms were reduced to abstractions about how to translate the linear "code" of DNA into the linear array of the amino acids that make up proteins."[7] This conception conveniently overlooked the essential characteristic of DNA that it is a part of living cells of organisms that have complex relationships with their environment. Avoiding complexities had important ideological and economical benefits such as finding the

[7] Hubbard (n.d.) "The Mismeasure of the Gene."

"homosexuality gene" and unwinding it, finding a "cure" for cancer, a multi-million industry, or advocating for predictive medicine so that the larger social problems with the public health sector can be swept away.

Hubbard explains how the metaphor of "code breaking" has served the interest of certain people and industries. Likewise, she argues that the way "reproductive success" is explained in terms of a metaphor where eggs and sperms are considered as investments is tainted by male bias and it contributes to reproducing an unequal social order.[8] Similarly, Emily Martin discusses how explanations of conception are tainted with androcentric language in insisting on attributing "activity" to the sperm and "passivity" to the egg in spite of the evidence for the dynamic role the egg plays at the time of conception. (Martin, 1996).

Another feminist who finds explanatory value in examining the metaphors used in prevailing scientific explanations is Donna Haraway. Her extensive work on primatology shows how social interests affect scientific practice. According to her, social needs not only determine what problems scientists tend to pursue but also the conclusions they reach. That is to say, both the context of discovery and the context of justification are prone to the effects of social needs and interests. What drives the process of science according to Haraway is the created need for certain kinds of evidence: When there is need, evidence begins to appear. This is a highly controversial claim for classical empiricists who believe that evidence provides "objective" (i.e. disinterested) support for a theory. However, Haraway's claim should not be construed as stating that social needs

[8] For more discussion of this see Hubbard and Wald (1993); Hubbard (1990, 5).

are the only factors that determine evidence. She emphasizes the intricate and complex structure of evidential relations that are situated in a social setting. I will elaborate the social character of evidential relations in the next section when discussing Longino's position. But for now, let's note that in Haraway's picture social needs are not extrinsic to the practice of evidence gathering, organizing and assessment. Here we see a departure from the early feminist empiricists' project of distinguishing good science from bad science. Haraway argues that sexist assumptions in science are not merely bad examples, but are a result of how science itself is structured. Hence, she concludes that in order to eliminate sexism we first need thoroughly to examine the characteristics of and the ways in which science usually works.

It is fair to claim that Haraway's naturalistic approach influenced both feminist standpoint theorists and later feminist empiricists.[9] However, Haraway's account also attracted many criticisms. One of the most important criticisms is Miranda Fricker's, who points out that if social needs determine evidence, then empiricism becomes implausible. (Fricker, 1994) Very broadly, if we define empiricism as the view that all non-analytic knowledge rests on the evidence acquired from experience, then accepting the impacts of social needs on evidence means that there is something beyond experience that knowledge rests on. This view undermines empiricism, because theory choice and observation assessments become contingent upon social needs. Fricker claims that without some sort

[9] Among those feminist empiricists who pursued naturalistic programmes Nelson and Longino stand out. According to Nelson, Quineian naturalized epistemology provides useful conceptual tools that are compatible with the feminist cause. For the purpose of this thesis I confine my examination of later feminist empiricists to Longino.

of empiricism there is a risk of an "anything goes" approach. Haraway, however, strongly objects to "anything goes", as it amounts to being everywhere at once and nowhere in particular, which is what the God's eye point of view (or the view from nowhere) purports to accomplish. According to her, both "anything goes" and "the view from nowhere" conflict with the situatedness of knowledge. (Haraway 1988) Yet there is still the question of how "anything goes" can be decisively refuted, and Harding does not seem to have a sufficiently elaborate explanation for this important question.

How to preserve the plausibility of empiricism while admitting the effects of social impact on evidence is a compelling problem. A promising theory that aims to prevent "anything goes" while arguing for the relevance of social values to evidence is Longino's contextual empiricism.

4.2 Helen Longino's Philosophy of Science

Helen Longino's philosophy of science rests on the idea that science and knowledge are primarily social enterprises. However, the social character of science may manifest itself in different aspects of science such as in laboratory work, divisions of intellectual labour, theory constructions, theory-laden observations, and scientific assessments. That is why we need to examine what Longino has in mind when arguing for the sociality of science In *Science as Social Knowledge* (SSK) she writes "the social character of scientific knowledge is made especially apparent by the organization of later 20th century science, in which the production of knowledge is crucially determined by the gate keeping of peer review. Peer review determines what research gets funded and what research gets

published in the journals i.e. what gets to count as knowledge." [10] (68) Here Longino identifies the scientific community as the main agent in producing scientific knowledge, and she grounds the sociality of knowledge in the collective practices of the members of the community. However, the mere fact that scientific findings are assessed and controlled by members of a scientific community working in the same field of inquiry does not by itself show that social values affect those findings. In fact, if there are universal standards that a scientific community subscribes to in evaluating scientific findings, then it does not matter whether the findings are assessed by one person or by the whole group. Recall that in the received view there was a sharp distinction between the context of discovery and the context of justification. According to this view, external factors such as social and political values are confined to the context of discovery and do not operate in the context of justification where scientific reasoning proceeds. In other words, scientific argumentation through which scientific knowledge is produced has its internal rules and is not susceptible to external factors. Accordingly, the specific practices of peer review are not relevant in the context of justification and therefore do not prove that scientific knowledge is social. However, as I have argued in Chapter Two, this approach is very narrow in its understanding of science and its workings, because it reduces science to scientific knowledge. Moreover, scientific knowledge is reduced to propositional

[10] Recall that Daston argues that changes in the organization of scientific practice in the twentieth century led to a shift in the understanding of objectivity. Objectivity came to be viewed as aperspectivity. According to Daston, as science became more and more international (and the scientific population increased) "communicability" became necessary to attain "aperspectivity". The emphasis on "communicability" is important because this notion resurfaces both in Popper and Longino, and it finds an important (yet different) place in their accounts of objectivity. I will elaborate on this issue below.

knowledge with its problematic assumptions about such matters as dislocated abstract subjects and universal standards. This narrow approach also disregards practical knowledge (i.e. know-how) which is an essential part of the scientific enterprise. Longino takes issue with this narrow understanding and emphasizes the sociality of knowledge. Underlying her insistence on social knowledge is the assumption that epistemic standards are not universally fixed. In arguing for the social character of science she attacks the distinction between the context of discovery and the context of justification, and claims that the context of justification is not immune from external factors. For Longino, the terminology of internal factors and external factors transforms into constitutive values (such as simplicity, truth, accuracy, and fruitfulness) and contextual values (personal, social, political and cultural values) respectively. She opposes the received view in its treatment of contextual values as randomizing factors, but argues that they have a bearing on "beliefs or attitudes that are *systematically* related to culture, social structure, or socioeconomic interests of the context within which an individual scientist works."[11] (64) Hence, in *SSK* and in her later works she puts considerable effort into demonstrating the relevance of contextual values in scientific argumentation. The framework of social knowledge and contextual empiricism is intrinsically related to Longino's understanding of objectivity.

[11] Italics mine. I emphasize this point because in so far as beliefs systematically relate to social structure and so forth, it is possible to track them down, thus providing explanatory power to a naturalized inquiry.

4.2.1 Contextual Empiricism

In *SSK* Longino focuses on evidential relations, as according to her the essential dimensions of science, such as inference, and assessments of the relevance or acceptability of data for hypotheses, all are based on evidence. (38) Longino argues that her contextual empiricism constitutes a positive alternative to the inadequate take on the relationship between data and hypotheses found both in logical positivists such as Carl Hempel and wholists such as Thomas Kuhn. Let me briefly explain the problems Longino finds in each school of thought.

In her discussion of the logical empiricists' approach to evidential relations Longino concentrates on the confirmation relation. Recall that the logical empiricists' goal was to find a logical formula that would disclose whether or not a confirmation relation holds between a hypothesis sentence and evidence sentence(s), just as a valid logical formula shows that an inference relation holds between premise(s) and a conclusion. In this picture the specific content of hypothesis sentences and evidence sentences has no bearing on possibilities of confirmation. That is to say "the inference to a hypothesis is not mediated by possibly value-laden assumptions." (Longino 1990, 48) Nevertheless, according to Longino, this view runs into the following problem: The inference relation requires that the same predicates be used both in the hypothesis sentence and the observation sentences. In moving from observations to a hypothesis regarding unobservable entities, however, the inference relation cannot hold since the predicates about unobservables in the hypothesis sentence are not contained in the observation sentences. Similarly for the confirmation relation to hold, observation

sentences, which confirm a hypothesis, should have the same predicates as the hypothesis sentence. Yet in cases of unobservables the confirmation relation does not hold since observation sentences referring to a class of observable entities do not entail a hypothesis sentence regarding unobservables. Thus, Logino concludes, "the sentences describing evidence for hypotheses and those expressing hypotheses are typically not related in appropriate ways." (49) This positivist approach, where theories are treated as sets of sentences, and formal relations between sentences are sought, is too restrictive and fails to account for the relationships between data and theory.

Wholism, characterized by such historically-oriented philosophers of science as Norwood Russell Hanson, Thomas Kuhn and Paul Feyerabend, on the other hand, has also failed to account adequately for the evidential relation between data and theory, in Longino's view. As I discussed in Chapter Two, wholists challenge the scientific methodology defended by logical positivists and empiricists. Their observation that there are cases in the history of science where inconsistent theories are supported by more or less same data has led wholists to deny the cumulative model of scientific progress advocated by the logical empiricists. Wholists have undermined the empiricists' fundamental assumption that observation is independent of theory. According to them, science operates within large-scale frameworks or paradigms. Observations, experiences, and the terms used in the theories are all theory-laden. (Longino 1990, 26) Consequently, observations that constitute the supporting data for confirming a theory are not independent of the framework within which the theory operates. This means that there is no neutral set of data against which to compare or evaluate theories. Since the meanings

of the terms used in a theory are also theory-laden, theories are mutually untranslatable as well as incommensurable. However, Longino thinks that the wholist account is paradoxical in claiming that two theories can be both mutually incommensurable and mutually inconsistent. If the meanings of the terms change depending on the framework, then we cannot claim that two seemingly inconsistent theories, which have common terms but operate in different frameworks, are indeed inconsistent given that the terms will refer to different things in each theory. Furthermore, the idea that both observations and hypotheses are products of the same paradigm destroys "the concept of evidence as something to which one can appeal in defending a hypothesis." (57) Here evidence becomes redundant. Contextualizing evidence to background assumptions, according to Longino, overcomes this problem. For one thing, contextualization does not make evidence theory-dependent. She agrees with the logical positivists that there is a common language with which we reason and describe phenomena. She writes, "[o]nce it is accepted that the evidential relation is always determined by background assumptions, then it is easy to see that there could be a neutral description of a given state of affairs, that is, one agreed to by both parties to a dispute, and no agreement on the hypotheses for which it is taken as evidence." (60) Accordingly, although background assumptions are usually hidden, they can be articulated through the common language that is used, and such articulations make disagreements among parties possible. Consequently, Longino argues that a contextual analysis can reveal how an inference from data to hypothesis is mediated by background assumptions without falling into the trap of incommensurability.

For Longino the problems and paradoxes faced by logical positivists and wholists

occur because each system of thought "produces an individualist logic of scientific method that fails adequately to reflect the social nature of scientific discourse." (81) She argues that a contextual analysis of evidential relations would make the social character of science apparent. Before elaborating her contextualism, let me clarify what Longino's empiricism consists in. Longino states that her position is empiricist "in treating experience as the basis of knowledge claims in the sciences." (219) This modest empiricism rejects logical positivists' verification theory as well as their project of founding knowledge on immediate sense data (foundationalism). It also rejects finding formulas for reducing all scientific statements to statements composed of terms referring to immediate sense data (reductionism), as I have discussed in Chapter Two. In fact, in arguing for social knowledge, Longino aims to disentangle empiricism from its individualism, foundationalism, universalism, and internalism. Yet, her insistence on the importance of evidential relations is a manifestation of her version of empiricism, which is essentially a theory of evidence. The contextualist aspect of her position, on the other hand, is more complicated and requires further deliberation.

"Context" for Longino refers primarily to the background assumptions against which data are assessed and evidential relevance is determined. Unlike the logical positivists, Longino is not concerned with finding a single formal criterion for evidence. She is interested in evidential relations between objects, events or states of affairs and the hypotheses they support. For Longino a hypothesis is not merely a collection of sentences, it can be expressed in different ways, for example by using maps or models. Accordingly, there can be different ways of describing the same states of affairs, and this difference is a

result of the endorsed background assumptions. For Longino, the assessment of what constitutes good evidence requires first an understanding of mechanisms behind how evidential relevance is in fact determined. (40) In other words, we need to inquire into how background assumptions operate. These two different projects—one prescriptive and the other descriptive—have sometimes been conflated by philosophers in examining science. On the one hand, those who focus merely on the prescriptive project end up with inadequate accounts, as there are no satisfactory *a priori* grounds for formulating norms for good evidence. On the other hand, those who focus merely on the descriptive account dismiss the normative task of philosophy of science. Hence, Longino argues that a good epistemology and philosophy of science should consist in naturalized inquiry about science as well as normative considerations, which are essential for distinguishing opinion from knowledge.

Longino's naturalized inquiry, i.e. explaining how data transform into evidence in actual scientific practice, rests primarily on Quine's views on the under-determination of theories by evidence. The main lesson of under-determination is that states of affairs do not carry evidential relations by themselves, and there is no unique evidential relation between a state of affairs and a hypothesis. That is, whether a state of affairs is to be taken as evidence for some hypothesis is mediated by other assumptions concerning the evidential relation between the state of affairs and the hypothesis. (41) Longino explains this idea using an example where she comes to believe that an eight-year old child has the measles because of the fact that her stomach is covered with red spots. Longino's belief about the relationship between having a red spotted stomach and having measles enables

her to take the child's red spotted stomach as evidence that the child has the measles. However, Longino could instead have come to believe that the child has the measles because a crystal ball reader had told her that if the child's stomach is red spotted, she has a disease called measles. In this case, Longino's belief that the child has the measles depends on what the crystal ball teller has told her, and on her belief that crystal ball teller is a reliable source. In both cases the observed state of affairs is the same: red spots on the child's stomach. However, why it is taken as evidence for the hypothesis that the child has measles is different in each case. (41)

An actual scientific example that Longino discusses in showing how background assumptions operate in the collection of data and evidence assessments is from evolutionary studies. Here we again see that the same state of affairs can be taken as evidence for different hypotheses. There are two rival hypotheses explaining human descent from primates. The "man the hunter" hypothesis focuses on the changing behavior of males, and the development of tool usage is explained by the evolution of hunting males. The "woman the gatherer" hypothesis, on the other hand, focuses on the changing behavior of females, and explains the development of tools used by females by appealing to the changes in nutritional needs as well as changes in the conditions of reproduction as a response to changes in the flora. In other words, it is hypothesized that the shift in environmental conditions throughout history, such as abandoning the forest for the savanna, was accompanied by changes in the modes of food acquisition. (107-8) On the "man the hunter" hypothesis, which is informed by androcentric values, objects identified as tools are accepted as evidence for the hypothesis about "men developing

stone tools and spears, and developing smaller canines, larger brains, co-operative behavior, and language." Whereas, on the "woman the gatherer" hypothesis, which is informed by gynocentric values, the same objects are accepted as evidence for "softening hard fibers or crushing seed pods, and developing larger brains, co-operative behavior, and language."[12] Here objects such as fossil bones and stones do not by themselves explain the behavior of our evolutionary ancestors. Background assumptions in each hypothesis ascribe different evidential value to them. Hence, the inference from data to hypothesis is mediated by the background assumptions.[13]

4.2.2 Longino's Conception of Objectivity

The idea that theories are underdetermined by data has been taken by some to mean that theory choice is arbitrary and irrational since there is no control over the impact of social and political values in theory assessments and preferences. That is to say, the objectivity of science is in jeopardy. In fact, if there is no unique relation between data and evidence, whether or not a specific observation supports a hypothesis becomes a contingent matter. For Longino it is true that epistemic relevance is determined by context. Yet contextualizing evidence to background assumptions makes theory choice neither arbitrary nor irrational. She writes, "[it] is rational to take some state of affairs as evidence for a hypothesis in light of background assumptions one accepts. It would be irrational to

[12] Quotations in this example are taken from Potter (2006, 100-101).
[13] Another excellent example Longino discusses extensively on this point comes from behavioral endocrinology where the hormonal causes of sex differences are considered. For a clear summary of Longino's take on in this field, see Potter (2006, 101-108).

assess evidential relations in a manner inconsistent with such background assumptions and antirational or nonrational to accept or reject hypotheses with no regard for evidence." (60) Moreover, theory choice is not arbitrary, because theories are and should be assessed and controlled by appropriate mechanisms. It is primarily at this point that Longino's epistemology becomes normative. Shortly I will discuss the specific norms that she sets out for controlling contextual values, but let me now elaborate in general terms the processes by which background assumptions are controlled, according to Longino.

First and foremost such control depends on the kind of background assumptions operating in theory construction and assessment. Background assumptions operating in scientific reasoning could be personal and/or institutional preferences and beliefs, conceptual commitments one has come to adopt as a result of their formal education, norms and methodological rules a scientific community endorses according to the aims, goals and interests of their research project; and social, cultural and political values that are predominant in society in general.[14] It is reasonable, then, to claim that background assumptions operate at three basic levels: individual, communal (local) and general (global).[15] Through these channels contextual values seep into scientific reasoning in various ways, from determining the questions asked and ignored (for instance why certain regions or species are investigated rather than others) to the descriptions, categorizations and selections of data (as in the case of evolutionary studies mentioned above). The problem with contextual values operating at the individual level is that it risks falling into

[14] Military and economic concerns could also be included in this list.
[15] It is important to keep in mind that these levels interact with and are affected by one another in complex ways.

subjectivism. In subjectivism, there is no way of distinguishing personal opinion from knowledge. If there are no determined standards to judge the merits of different sets of assumptions, then justification becomes redundant. However, this criticism rests on an individualistic understanding of justification. In this understanding, the justification of a subject's belief is assessed in terms of the support that the subject's other beliefs and assumptions yield for the belief in question. According to Longino, though, "subjectivism can be avoided by incorporating critical interaction into one's notion of justification." At the heart of her argument is the view that knowledge is primarily a social practice. She writes,

> Knowledge is the outcome not just of the cognitive agent's encounter with the world, but of cognitive agents' encounters with one another. The latter encounters bring assumptions to the surface for criticism and then endorsement, rejection, or modification. A recalcitrant agent will not defend or modify her beliefs in response to criticism. Such recalcitrance amounts to opting out of membership in an epistemic community. (Longino 1999, 342)

In short, whether a belief qualifies as knowledge or not is a communal matter, and individual idiosyncrasies are controlled by interactions among individuals. Of course, one could claim that critical interaction does not *guarantee* the elimination of damaging contextual values. There could be biases that permeate the whole community where knowledge assessments are performed, and as a result they would pass unexamined. This brings us to the second level of background assumptions.

At the second, communal level, context is composed of shared assumptions through which members of a community pursue inquiries and engage in interactions. It is important to note that background assumptions shared by society in general and by the

scientific community in particular tend to become largely invisible. If there are biases in the shared background assumptions they are also likely to become invisible and be immune to critical interaction in controlling idiosyncrasies. Because of this invisibility it is crucial to regulate the conditions under which such interactions should occur. Longino sets out four norms for making visible and controlling shared background assumptions that are also the features an ideal epistemic community should display. Insofar as these norms are required to accomplish genuine critical interactions through which contextual values are made apparent by the social production of knowledge, they also have a significant bearing on objectivity.[16]

(1) The first norm Longino sets out concerns the *venues* through which scientific inquiry is assessed. Longino writes, "[t]here must be publicly recognized forums for the criticism of evidence, of methods, and of assumptions and reasoning. This means that criticism of research ought to be articulated in the same standard and public venues in which 'original research' is presented: journals, conferences, and so on." Furthermore, this norm prescribes a positive role to negative criticism in scientific inquiry. Critical discourse should by no means be obstructed.

(2) The second norm is about the *uptake of criticism* where it is emphasized that genuine criticism must be transformative. According to this norm, "the community must not merely tolerate dissent, but its beliefs and theories must change over time in response to the critical discourse taking place within it." Here by change

[16] Longino presents these norms in various articles, but the following quotations are taken from Longino (2002, 129-134).

Longino means “the acceptance of different beliefs, the modification of beliefs, the development of new data, reasons, and arguments.”

(3) The third norm concerns *public standards.* This norm is very important for maintaining a dialog between critics and advocates of a position. Longino writes, “[t]here 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.” These standards and their applications must be transparent, and they must be modified in light of appropriate criticism. The success of the inquiry should be evaluated by appealing to the shared values and standards. She writes, “[p]articipants in a dialogue must share some referring terms, some principles of inference and some values or aims to be served by the shared activity of discursive interaction. Thus, shared elements are necessary for the identification of points of agreement, points of disagreement, and what would count as resolving the former or destabilizing the former.” Longino also stresses that these standards “are not a static set but may themselves be criticized and transformed, in reference to other standards, goals or values held temporarily constant”.

(4) The norm of *tempered equality* requires communities characterized by equality of intellectual authority, and it prescribes an inclusive scientific practice. Longino claims that an epistemically effective critical discourse requires a diversity of perspectives. This requirement ensures the broadest range of criticism for a hypothesis. According to her, “the exclusion of women and members of certain

> racial minorities from scientific education and the scientific professions constitutes not only a social injustice but a cognitive failing." Yet Longino also admits that members of a community differ in intellectual capacity. "The difficulty is that some may differ because of innate endowment...And some may differ because of schooling and other opportunities." Hence she concludes that equality must be qualified or tempered. In sum, "[w]hile the criterion imposes duties of inclusion and attention, it does not require that each individual, no matter what their past record or state of training should be granted equal authority on every matter."

The consistency and adequacy of these norms have been criticized both by feminist philosophers and by non-feminist philosophers. Specifically, the context-sensitivity of standards as well as the possibility of tempered equality have generated interesting and informative discussions. I discuss these criticisms and suggest ways to overcome some of the problems faced by Longino's account later in this chapter. But for now let me note that these norms are not presented as criteria of truth but as conditions of legitimate consensus. The aim of social interaction that Longino praises is not the correction of individual error caused by a "biasing factor." It matters not whether individuals change their beliefs as a result of criticisms but whether the community responds to the criticisms or not.[17] Hence, in keeping with her commitment to the sociality of knowledge, Longino notes that these criteria are for assessing the objectivity of communities but not of

[17] Longino emphasizes this point in her response to Miriam Solomon's misrepresentation of her position in Longino (2008).

individuals. (1990, 79) Recall that for her, the primary bearers of knowledge are communities rather than individuals.[18] Yet, this does not deny that individuals can know. Rather it means that opinions are transformed into knowledge through social interaction. In other words, individuals can be ascribed knowledge if they conform to the norms set out by the epistemic community where—ideally—genuine interaction is possible.[19]

In short then, objectivity becomes a product of intersubjective criticism. According to Longino, "[e]ffective critical interactions transform the subjective into the objective, not by canonizing one subjectivity over others, but by assuring that what is ratified as knowledge has survived criticism from multiple points of view." (2002, 129) That is to say, Longino's norms set out the conditions that distinguish subjective opinion from objective knowledge. Here objective knowledge primarily reflects the critically achieved consensus of a scientific community.[20] (1990, 79) In this account interaction requires diversity among the members of a community because some assumptions, if they are common to all members of a community, become invisible, and thereby hinder criticism. Diverse subjectivities generate criticisms that in turn reveal background

[18] It is worth noting that there is no consensus on the nature of epistemic subjects among feminists. Despite the differences in their views Nelson (1990) and Longino (1990) both argue that communities are/should be the primary subjects of knowledge. For Louise Anthony (1995) and Lorraine Code (1991), on the other hand, it is important that individuals retain some autonomy as knowers. Heidi E. Grasswick (2004) advocates for an understanding where knowing subjects are "individuals-in-communities". This understanding incorporates some aspects of individualistic epistemologies and some aspects of views that praise communities as subjects of knowledge. I will return to this point later in this chapter.

[19] Sometimes this means that knowledge ascriptions are introspective.

[20] I will discuss the ways in which objective knowledge achieved through critical exchange relates to the outside world when I examine the relationship between epistemological objectivity and ontological objectivity in the next chapter.

assumptions that may carry specific interests and biases. Longino writes "[shared background assumptions] do not become visible until individuals who do not share the community's assumptions provide alternative explanations of the phenomena without those assumptions…the greater the number of different points of view included in a given community, the more likely it is that its scientific practice will be objective." (1990, 80) As a consequence, scientific objectivity—both in its practice and what it produces—always comes in degrees.

Now it can be said that by these norms Longino aims to block the influence of (individual or group) idiosyncrasies operating in the background assumptions that lead to distorted accounts. Hence, her understanding of objectivity shares with traditional understandings the controlling of social values in scientific accounts. Yet it departs from the project of value-free science by paying due respect to certain social values through which such control is possible. In short, for Longino while certain values compromise objectivity, certain other values help to achieve objectivity. The question is which social values promote objectivity. When we look at her proposed norms, they are about openness to criticism and revisions, transparency of standards in assessing scientific practice, and the equality of inclusion and access to the practice of science—all of which secure diversity in the scientific community. These norms are typical liberal democratic ideals. Hence, a good scientific practice becomes one that is democratic. According to Longino, advancing democratic ideals lead to greater objectivity and truer accounts. An objectivist epistemology that is committed to value-neutrality cannot accommodate the democratic control of social values that Longino defends. Hence she argues for a new

epistemology and a new philosophy of science.

4.2.3 Feminist Epistemology as a Local Epistemology

Longino's objectivity requires contextual values in detecting biases. Taking different subjectivities into account in knowledge production is not a distortion but a necessity. However, the presence of different subjectivities in knowledge production demands a politically and ethically informed epistemology. What this implies is that those cognitive/epistemic values traditionally picked out by epistemic projects committed to dislocated, abstract cognitive agents to prevent so-called arbitrariness in theory choice are insufficient in certain contexts. In their efforts to reveal and prevent gender biases in scientific enterprise many feminists have offered new theoretical values, which convey democratic ideals. Longino collates and identifies these feminist values as empirical adequacy, novelty, ontological heterogeneity, mutuality of interaction, applicability to current human needs, and diffusions of power.[21] It is important to note that these values are not offered to replace the old set of epistemic values nor can they all be used in every case.[22] But they are intended for cases where scientific activity is susceptible to biases.[23] Moreover, just as a theory cannot satisfy all the traditional epistemic values, neither can

[21] Longino notes that this is not a complete set. For her detailed discussions of feminist theoretical values see Longino (1994); Longino (1995); Longino (1997).
[22] It should be noted, however, that among these values "empirical adequacy" has a speacial status because it is an essential criterion for an activity to be called scientific.
[23] Longino notes that accountability to political feminist commitments "does not demand a radical break with the science one has learned and practiced. The development of a "new" science involves a more dialectical evolution and more continuity with established science than the familiar language of scientific revolutions implies." (Longino 1996)

one theory satisfy all feminist values. Which values will be taken into account in assessing a theory depends on the content, aim and the context of the inquiry. Hence standards and values are locally endorsed. According to Longino, although values guide inquiries, "they are always subject to revision in light of information generated by their application or of other criteria or values made salient by changed circumstances." (Longino 1994) Thus, no value—whether traditional (simplicity, scope and so forth) or feminist—is universally fixed and permanent. Since there is no set of values that would apply always and in every situation, and some background assumptions might always be hidden no matter how rigorous the critical interaction is in a community, knowledge achieved through scientific consensus should always be accepted provisionally. As such, it might change as new data is generated, new values are accepted and background assumptions reassessed. (Longino 2002, 135) Hence, it can be argued that the ideal of objectivity that serves to eliminate distorting biases in a community will also be provisional.

A comparison between Longino's account and the traditional understandings of objectivity characteristics set out in the Chapter Two will help to summarize her understanding of objectivity. Longino shares the received view's contention that objectivity is an ineliminable regulative norm of science that distinguishes subjective opinion from objective knowledge. As such, practices of objectivity, i.e. methods and norms through which objectivity is sought, prevent arbitrary subjective preferences. However, since knowledge is a result of social interaction, for Longino objectivity becomes primarily a monitoring of the social norms that affect communities rather than

individuals. Moreover, her contextualism does away with the absolutist understanding of objectivity adopted by the objectivist view. Longino contends that objectivity secured by intersubjective criticism becomes a provisional achievement. Another important difference between the received view and Longino's account is her rejection of the traditional treatment of social values as causes of biased beliefs. According to Longino, although social values may lead to biases, it is also through social values that the impacts of those values are detected.

Advocating for local epistemologies where methods, norms or strategies for a given inquiry are contextualized contrasts both with the mainstream project of setting universal criteria for knowledge and earlier feminist insistence on an exclusively feminist science. The emphasis on an understanding of science as a social activity, where practices within science are underlined, paves the way to arguing for "doing science as a feminist".[24] It is worth noting that, for many feminists, Longino among them, the notion of doing science that addresses feminist concerns does not present an alternative science that replaces tradional Western science. But it suggests better ways of approaching certain areas of inquiry, where pervasive sexism is likely to affect results, than those of mainstream epistemology. Hence, although "doing science as a feminist" requires that we reconsider and revise the main tenets of mainstream empiricism, it is not as radical as arguing for a distinctively feminine science. Yet, it still faces a fair amount of criticism, both from feminists and non-feminists, some of which I will address in the next section.

[24] For more on "feminist science" versus "doing science as a feminist" see Hundleby (2012).

4.3 Criticisms of Longino's Account

So far my discussion of the impacts of background assumptions on evidential relations has been restricted to the individual and communal levels. This is because Longino's account deals mostly with the impacts of contextual values at these two levels. Individual idiosyncrasies are blocked by communal interactions, and persistent biases throughout a community are blocked by strict regulation of critical discourse. But what about globally diffused biases that are entrenched in our modes of thinking? Are Longino's norms sufficient to overcome biases at the global level? Certainly such cases require more than the regulation of critical interaction but also a very careful investigation of society in general. This investigation demands an interdisciplinary inquiry with a vast genealogical, historical and social analysis in order to reveal the biases embedded in social life. Longino's account, however, falls short of providing tools for such a general investigation. I suggest Harding's "strong reflexivity" assisted with hermeneutical analyses can compensate for this gap in Longino's account. Insofar as science reflects values and prejudices of the society in general, it is crucial to have a systematic examination of society. Moreover, for the reasons I have discussed in the previous chapter, such an examination should include marginalized views. Before elaborating on this point, I would like to explore some difficulties surrounding the idea of inclusive science, because such difficulties also apply to feminist standpoint theories which aim to democratize scientific practices.

4.3.1 Intellectual Authority and the Publicity of Science

The problem of globally endorsed prejudices threatens the project of inclusive science: if certain groups of people and their viewpoints are structurally excluded from the intellectual domain, then having a formal rule about inclusiveness will fail to accomplish the diversity sought. One feminist who questions the adequacy of Longino's norms on inclusiveness is Sharon Crasnow. In her article "Can Science Be Objective? Feminism, Relativism, and Objectivity" (2003) Crasnow points out that the intellectual authority that Longino mentions itself legitimates the exclusion of some voices. She continues by asking how in such cases the voices of the excluded can even begin to be heard. This is primarily a question about the rules that legitimize participation in scientific practice. (Crasnow 2003, 136) If the standards and rules are not universally fixed, how are they to be determined and by whom? We have seen that for Longino standards are established within a context where intellectual as well as historical, social and political norms interact. Longino defends not only the publicity of assessing hypotheses with respect to communally endorsed standards but also the publicity of the processes in which these communal standards themselves are determined. Yet Crasnow thinks that the question of *who* engages in the processes of assessing and determining standards is very important because, for her, we can assess the rules only from within our own culture. Here the pressing questions are: how can we tell that when we refuse a criticism we are not dogmatically holding on to our own standards for evaluating the evidence no matter what? And, how can disagreements about the standards of assessment be resolved? (136) These are indeed important challenges. However, a response first requires clarification of what it

means to say that we can assess the rules only from within our own culture. If this is merely a result of our situatedness "we" will inevitably fail to see the prejudices that are invisible in "our" culture. Longino could then respond by insisting that the assessment of rules and standards could not and should not be limited to "us" and the viewpoint of "our" culture. Her suggestion to include diverse viewpoints embraces different viewpoints from other cultures. The view that explains the failure of agreement or disagreement across different cultures by appealing to the idea that they endorse different norms rests on the assumption that there is no common ground for different cultures to communicate. However, contextualism does not entail this view.

In her objection I believe Crasnow is wrongly construing contextualism as leading to conceptual framework relativism, and claiming that different cultures operate within different conceptual frameworks. However, unlike working within two or more different conceptual frameworks, one can operate within multiple contexts.[25] Contexts are not conceptual frameworks: different background assumptions may constitute different contexts, yet some background assumptions are often shared across different contexts. For example, in a worldview where androcentrism prevails, analytical thinking as a "positive" value is generally attributed to men. This situation is often explained by appealing to the neurological structure of a male brain. On the other hand, in a gynocentric worldview, attentiveness as a "positive" value can also be explained by appealing to the neurological structure of a female brain. In these cases, although different background assumptions

[25]For the sake of the argument I am assuming that one cannot adopt more than one conceptual framework simultaneously. Yet it is open to question whether one can hold multiple conceptual frameworks consistently.

operate within each worldview, they also share a considerable number of assumptions. For example, they share their belief in biological determinism, and most of their knowledge of anatomy. Perhaps more importantly, according to Longino, the diverse background assumptions of different worldviews can be simultaneously articulated.[26] Hence, different cultures can assess and criticize other cultures' rules and standards (even if they do not agree with them). Moreover, in suggesting that our assessments are limited to our own culture, Crasnow appeals to a very narrow understanding of culture. She makes it sound as if culture is a homogenous bloc, and ignores its dynamic diversity and mutability. Although shared values are necessary for a culture to be viewed as a culture, there are also subgroups, minorities, and marginalized people within every culture. These groups might depart from commonly shared values, yet they nonetheless inform and sometimes transform those values in various ways. Accordingly, although there might be a dominant way of assessing "our" rules, it is never a monolithic enterprise. Hence even if "we" can assess "our" rules only within "our" culture, the content of those assessments varies. Similarly, in her criticism Crasnow seems to assume a fixed "we" which goes against the feminist contention on the situatedness of subjects. Even among the privileged groups who are included in the scientific practice there is heterogeneity *to some extent* due to their situatedness. For example, no scientist has exactly the same background as another. Even a small variation in their background could provide different conceptual tools that could spark a critical stance toward shared assumptions. Of course this line of

[26] For a similar point see Rolin (2011).

reasoning is meaningful only if we attribute *some* autonomy to particular subjects, i.e. individual scientists can help us transform "our" understanding to a certain extent. However, deeming communities the appropriate subjects of knowledge seems to overlook the moderate yet significant contributions that individuals could make in knowledge production. It is open to discussion whether Longino's account denies any autonomy to individual knowers. If it does, then Crasnow's criticism is a real challenge for Longino. Yet, it could be argued that although Longino claims that the primary bearers of knowledge are communities rather than individuals, her account allows for (even requires) individual contribution to transforming the ways knowledge is produced. I will discuss this issue further in the next section.

To be fair, for Crasnow the issue is not about who could assess epistemic standards, but whose assessments count and make a real difference. In other words, it is about who could convey intellectual authority. Hence Crasnow's challenge is mostly directed at Longino's norm of tempered equality. Recall that Longino states "while the criterion imposes duties of inclusion and attention, it does not require that each individual, no matter what their past record or state of training should be granted equal authority on every matter." The important question remains on what grounds certain viewpoints will be excluded. This indeed is an important and a difficult question, consideration of which requires an analysis of different factors that are intertwined in various ways. Hence a single-factor response, which takes into account one factor and overlooks others, cannot provide a decisive or satisfactory explanation. In mainstream philosophy of science the question of which viewpoints to exclude from science is mainly discussed within the

literature written on the demarcation problem, i.e. the problem of distinguishing science from pseudoscience. This problem was dealt with in "purely" epistemic terms. Feminists, on the other hand, reject this approach. They argue that the social location of subjects is epistemologically significant. This is because i) the way we are socially situated has a bearing on what we know and how we know it; ii) our social location highly influences the degree of epistemic credibility ascribed to us, which in turn, legitimates or delegitimates us as knowers. That is why, according to feminists, the question of which viewpoints should be included or excluded in scientific practice cannot be answered purely in epistemic terms. Social and political factors often (if not always) have a bearing on whom to include or exclude in the knowledge production. Hence, feminists ask for detailed analyses of the ways the social and the political factors come into play in scientific practice. Yet I believe a satisfactory answer to the question of the grounds on which viewpoints will be included/excluded in science should also involve a conceptual analysis of science among other things. Such analysis is especially important if we want to dismantle the myth of science as the paradigm of knowledge. I think mapping out conceptual limits for science fosters the feminist contention that there are different ways of knowing (science being one of them) better than blurring distinctions by questioning the "scientific" status of arguably non-scientific practices such as paranormal experiences.[27]

[27] In talking of conceptual limits, I do not assume that there are fixed conceptual limits for science. See note below.

Accordingly, the question of inclusion/exclusion can "in part" be seen as an extension of the demarcation problem. It is true that formulating clear-cut conditions for what constitutes science has been a compelling problem.[28] Yet there are some historically well-established features that are fundamental for an activity to be called scientific (as it is practiced today). From the way Crasnow proceeds I think she overlooks one of the most important features of science, and this failure renders Longino's norm of tempered equality redundant.

In her discussion, Crasnow cites Longino's views of how mystical experiences are excluded from science because they are not publicly shared. Crasnow's objection runs as follows: such an exclusion might be right within "our" (Western) culture where "public" is understood in a certain way. But, there may be other historical/social societies where mystical experiences can be shared publicly, and where "public" is defined in a different way than ours.[29] Hence, publicity as a way of including diverse viewpoints fails, for "our" understanding of publicity excludes some viewpoints.

I do not think Crasnow's example of mystical experiences holds, however. If Longino's suggestion were to take into consideration whatever anyone has to say regarding a certain phenomenon, then Crasnow's objection that intellectual authority

[28]It is important to note that we do not have to be committed to the belief that there are *a priori* criteria for science in order to recognize the demarcation problem as a genuine problem. Concepts and activities evolve and change as a result of historical and social occurrences. Yet we can still inquire into the discerning features of a concept or activity in its historical contingency. But because of this contingency the discerned characteristics will likely be fluid.

[29] Unfortunately Crasnow does not elaborate on the ways in which 'individualistic' mystical experiences can be 'shared'.

excludes some viewpoints might have been a real challenge. The issue at hand, however, is to include different and "relevant" viewpoints in *science*. Although we can disagree on what is unique about science and how it should be understood and practiced, what we cannot deny is that scientific knowledge is empirical knowledge. Conceptually, dependence on systematic observation, experience and experiments are necessary conditions for an activity to be called scientific. To question why it is so would be similar to questioning why this electronic device I am writing on is called a computer.[30] Regardless of the fact that science as it is understood and practiced today is historically a product of Western culture, the bottom line is that results of scientific activity, i.e. predictions, experiments, explanations, should *everything else being equal* be understood, communicated and tested by any other person at any other time. Let us call this "in-principle reproducibility" requirement.[31] In a very important sense, then, shared public knowledge is reproducible knowledge. Denying this "publicity" by claiming that it is

[30] To take the analogy further, different standards, materials or software can be used in building computers, yet each will still be called computer as long as it stores and processes data in certain ways and so forth.

[31] I admit that it is reasonable to have some reservations about the extent of this reproducibility criterion. It might be the case that some phenomena simply cannot be reproduced—or can be reproduced in a limited way. It is open to debate whether there can be a scientific explanation of these phenomena. For instance, if we want to explain women's oppression and apply the reproducibility criterion strictly, then we might have to accept that there cannot be a scientific explanation for women's oppression because it is shared, hence can be reproduced, only by a certain group of people (a further claim would be, no experience of oppression can be shared because of the particular way an oppressed is situated). This discussion invites further complex questions such as whether a private experience can be shared on the bases of commonalities between experiences, or whether one, who does not share another's experience, can come to understand it by studying the features of the context or situation that generated that particular experience. How we respond to these questions would affect the applicability of the reproducibility criterion.

defined only by "our" western standards is tantamount to denying science altogether. The reason why the mystical experiences that Crasnow mentions are excluded from science is not that they represent a different cultural point of view, but that they do not comply with this basic reproducibility criterion. This does not mean mystical experiences can never be objects of scientific inquiry. In fact, there are some research programmes that try to account for, say, paranormal occurrences such as seeing a white light in near-death experiences. Nor does it mean that they would never be reproduced as required by science. In fact, there might come a time when those experiences could be observed and reproduced with the aid of technological innovations. Yet, given today's technology, if in a culture it is believed that these mystical experiences are somehow public, although they are not observed and reproducible, then their understanding of public sharing is different from the public sharing as reproducibility that is required by scientific activity. This does not, however, suggest that mystical experiences have no worth, epistemic or otherwise. I think an instructive discussion of mystical experiences should not focus on whether they are scientific or not, but whether they can be used as resources for producing some form of knowledge.

Similarly, it might be argued that the stress on communicability as a condition for publicity conflicts with the value of midwives' knowledge, which is not "communicable", that I discussed in the first chapter. But that discussion was not about the "scientific" status of midwives practice, it was about disdain for midwives' traditional knowledge. We do not need to argue that midwifery is intrinsically scientific in order to value and legitimate the knowledge gained from midwives' experiences. (In fact, doing so would

foster the idea that a practice produces knowledge only if it is scientific.) Although the practice of midwifery does not perfectly conform to testability and communicability criteria of science, the pieces of information obtained from midwives are still epistemically valuable. Furthermore, this value is not restricted to their practical knowledge of how to deliver babies or resolve complications, but extends to their contribution to scientific knowledge as well. One of the reasons for this is that different forms of knowledge are/can be significant resources for scientists in developing explanations and/or building models.[32] In sum, I think the main problem lies not in claiming that some experiences are not appropriate objects of science, but in devaluing and/or undervaluing the forms of knowledge gained from them.

One point we need to examine here, however, is whether the understanding of publicity that rests on communicability and testability necessitates interchangeable abstract subjects. If it does, then the consistency of Longino's position is in question. After all, as I have discussed in the first chapter, according to Daston, aperspectival objectivity, which is the prevailing notion today, is tied to communicability that is secured by ideals of detachment, impersonality and disinterestedness. Daston contends that the aperspectivity ideal was developed in the moral and aesthetic philosophy of the eighteenth century as a response to problems of perspective. For instance, Adam Smith argued that self-interest is the worst and most common perspective that leads to distortions (Daston

[32] For a good example of how traditional knowledge informs scientific inquiry see Grasswick's (2004). Here, she explains how scientists are trying to incorporate the local knowledge of First Nations communities with their inquiry on environmental management strategies.

1992, 605). Shared public knowledge was pursued to erase such distortions; and ideals of disinterestedness and self-effacement by means of complete detachment were endorsed in the service of public knowledge. Daston maintains that because of the vast changes in the organization of science in the mid-nineteenth century this ideal of aperspectivity was imported into the natural sciences. (Daston 1992, 597) As the scientific community grew both in numbers and across nations, communicability became an important ideal. According to Daston, in the new democracy of scientific observers the interchangeable, impersonal observer replaced the skilled, known and trusted observer. (608-9) Individual idiosyncrasies were eliminated "through the prolonged 'averaging' of viewpoints by communication." (607) As a result, the communicability ideal came to be linked with the disinterestedness ideal to buttress aperspectival objectivity. Genuine knowledge thus became public knowledge. A significant aspect of this historical account for Longino's position is that it shows how praising the democratic ideal of public knowledge has actually led to pursuing aperspectivity in science, which is tied to disinterestedness and communicability. If this historical account is true, then we can claim that the ideal of public knowledge does not entail the involvement of situated subjects in knowledge production. However, Jennifer Tannoch-Bland has an important observation that challenges Daston's account, and which is crucial for the consistency of Longino's account. In her article "From Aperspectival Objectivity to Strong Objectivity: The Quest for Moral Objectivity" (1997) Tannoch-Bland claims that communicability has been an important issue in science since the time of Bacon. Hence the import of aperspectivity

from moral and aesthetic philosophy to natural sciences that Daston talks about needs to be reworked.

Tannoch-Bland's concern is to locate Harding's "Strong Objectivity" within Daston's historical account of objectivity. She claims that if the notion of objectivity is an evolving notion, as Daston argues, then Harding's objectivity can be seen as a part of this evolution. (Tannoch-Bland 1997, 164) This situation is also true for Longino's account. In her article Tannoch-Bland tries to find ways to escape aperspectivity while retaining the ideal of communicability, which is very important for emancipatory feminist projects. She argues that although the disinterestedness ideal, which involves enlisting impartiality, detachment and self-effacement, is associated with communicability, they are historically separate. In contrast to Daston, Tannoch-Bland maintains that what was imported from moral and aesthetic philosophy to the natural sciences was the disinterestedness ideal but not the communicability ideal. Since the communicability ideal was pursued in Bacon's time, and since there was already a notion of objectivity with no reference to disinterestedness at that time, it is an indication that aperspectivity is not the only way to achieve communicability. (1997, 165) According to Tannoch-Bland the prolonged averaging of viewpoints by communication is not the only possible response to the moral problem of perspective. Harding's notion of a cross-fertilized, enriched, responsible systematized perspective could also sustain the democratic communicability ideal. (165) Tannoch-Bland's views on this topic are significant for Longino's position too, as it paves the way to affirming the possibility of public knowledge through partial perspectives.

According to the above discussion, then, we can claim that the publicity of science does not require interchangeable abstract subjects. To support this claim let me turn to Longino and compare her position to Popper's position on intersubjectivity. As I have discussed in Chapter Two, Popper has also discussed the relationship between objectivity and public knowledge. According to Popper any scientific statement could, in principle, be tested and understood by anybody. He calls this process intersubjective testing. Here we find the same conviction as in the idea of reproducibility in defining science. In fact, I think Popper's influence on Longino is undeniable.[33] Longino's insistence on a critical community, and on the importance of a transformative critique of different viewpoints in important ways follow the legacy of Popper's falsification method, which also requires a critical discourse as a function of communal inquiry. However, as I have argued earlier, this communal exercise, according to Popper, is carried out within the set of rational (and universal) standards. In adopting the context of discovery and the context of justification dichotomy, Popper believes that social values do not have an impact on rational standards. For Longino, by contrast, not only are epistemic standards contextual but no rational/social dichotomy is tenable.[34] Although Longino's conviction about the social production of knowledge seems to be compatible with Popper's rejection of subjective experiences or feelings of conviction as unsuitable candidates for justifying statements, Popper's insistence on eliminating all that is subjective from scientific argumentation sets these two philosophers apart. As I have argued, for Longino subjective convictions, as

[33] John Stuart Mill's pluralism also has a great influence on Longino.
[34] Longino tries to unravel this rational/social dichotomy in *The Fate of Knowledge*. I will discuss her arguments in Chapter Five.

contextual values, might contribute to the transformative criticism that is required for achieving objectivity. To accept the effects of subjective convictions, however, is not to reaffirm individualism. Rather, it buttresses publicity since it serves transformative criticism, which is, or should be, a feature of (ideal) epistemic *communities*. In short, we can claim that the link between abstract subjects and intersubjective testing that yields publicity is predicated on an assumption that epistemic standards are fixed. However, in a contextualist account no such association is needed.

4.3.2 Individualism

The question of epistemic agency has been a central issue in developing a feminist epistemology and a feminist philosophy of science.[35] As I discussed earlier many feminists have found the individualism embedded in mainstream epistemology problematic. In contesting the individualistic approach to knowledge, some feminists, among them Harding, Longino and Nelson, emphasized the communal character of knowledge production. Yet, according to some other feminists, such as Louise Antony, epistemological individualism serves feminist epistemology better than the communal view. Antony focuses on methodological individualism which, contra Longino, takes individuals as the primary cognitive agents. There are three arguments Antony puts forward for methodological individualism against the socialism that Longino defends. Her first argument questions the necessity of communal interaction in achieving objectivity. She claims that although objectivity requires critical interaction, "an individual can

[35] See Hundleby (2004).

achieve some degree of objectivity by being reflective about her beliefs, assumptions, etc. i.e. the critical interaction can be internal to the individual, so the individual can to some extent meet the criterion of objectivity without social interaction." (Longino 1999, 345) This understanding is similar to the method of objectivity suggested by Nagel in *The View From Nowhere,* which I discussed in Chapter One. Here he basically claims that in order to achieve an objective understanding of the world we need to step back from our initial point of view and form a new conception within which the old one and its relation to the world are examined. This procedure could be repeated with each new conception, to acquire better objectivity. Such a method rests primarily on individual self-reflection. However, as I have argued earlier, the reliability of self-reflection for attaining objectivity is highly questionable. The success of self-reflection in detecting biases is limited to the assumptions that individuals are aware of. Moreover, achieving objectivity by self-reflection does not refute the importance of the social. As Longino rightly points out, such reflection still reflects an internal rehearsal of a social practice of criticism. Hence it depends on "those patterns of interaction through which the individual learns to reason." (Longino 1999, 345)

Antony's second argument for individualism is about epistemic agency. According to her, the social presupposes individual epistemic agency. Individuals are epistemologically basic because i) individuals cannot leave their subjective position; and ii) epistemic processes involve individualistic judgment and perception. (345) However, Longino contends that such individual agency is necessary but not sufficient for knowledge. It is true that the social is constituted by individuals in interaction and it is not

possible to eliminate the individual. Yet the interaction is as necessary as are the individuals. (345) She writes, "In the case of scientific inquiry, what is being claimed is that interactive practices (a) make personal, individual beliefs [NB: not private sensations or experiences] into knowledge and (b) transform belief in the process of challenge and response either because the content changes through that process or because the content becomes more firmly anchored in a network of experiential and doxastic states." (345) Hence, in Longino's account the individual is not completely removed from but accepted as an agent within the social production of knowledge.

Antony's last argument is about the significance of the sociality thesis. According to Antony, sociality is not a real challenge to individualism as it merely states that "human beings use each other to enhance their own individual epistemic situations". For Longino this is true if "enhancement" is understood quantitatively. In this sense, we extend each other's cognitive range as telescopes and stethoscopes do. (345-6) Accordingly, although our knowledge would be limited, social interaction in this sense is not necessary for knowledge. However, there is another qualitative sense of "enhancement" that is a real threat to individualism, as it refers to the transformation of belief into knowledge. Longino is careful to note that the sociality thesis does not target psychological individualism. So she does not claim that we could not have the beliefs we have if we did not engage in critical interactions with others. What is being questioned is whether those beliefs qualify as knowledge or not. In short, she maintains that social interaction functions for assessing the validity of knowledge ascriptions.

This dichotomous discussion of individuals versus communities as appropriate subjects for knowledge has been criticised by Heidi E. Grasswick. According to her, individualism need not entail the *atomistic view of knowers* adopted by mainstream epistemologies where epistemic subjects are characterized as generic and self-sufficient individuals. (Grasswick 2004, 87) However, she also admits the social and communal elements in knowing. Grasswick argues that beginning with a social understanding of knowers fits feminist concerns better than either an atomistic understanding of individuals or a view that identifies epistemic subjects with the whole community. Accordingly, she defends a conception of knowers as "individuals-in-communities", which accommodates both the situatedness and the interdependence of knowers on the one hand, and active and reflective agents who can transform and improve knowledge-seeking practices on the other hand. (87) Grasswick argues mainly against Nelson's model of whole communities where communities are identified "with those very same communal standards and practices." (110) This view according to Grasswick is problematic because it cannot account for how communal practices can improve and transform knowledge practices. The main reason for this shortcoming is that, a wholistic communal view fails to recognize that individuals "engage in multiple communities and communal practices." (110) In other words, individuals often have membership in multiple communities and no community is made out of individuals who subscribe to the exact same standards in their knowledge practices. Grasswick writes, "[improvement and transformation] sometimes come from either individuals or subcommunities dissenting from the current communal

practices, and challenging the norms of the practice."[36] (106) According to Grasswick, Longino's account does not face this problem because she emphasises the importance of critical interaction among individuals in knowledge production. As I discussed earlier, for Longino genuine transformative criticism requires diversity among members (Longino 2002, 148). In a sense then the differences in individuals are the force building critical communities that produce knowledge. This view can accommodate the capacity of individuals to challenge and improve knowledge systems. But as Grasswick rightly argues, "since critical interaction is so crucial to answering normative questions Longino is interested in, she spends more time detailing this interactive nature of the knowers than their situatedness." (Grasswick 2004, 111) I will discuss in the next section that this negligence leads Longino to focus on regulative standards for only a certain group of people.

Although discerning the appropriate characteristics of epistemic subjects is important for developing a consistent feminist epistemology, I think it is futile to argue which comes first (individuals or communities?). Rather than dealing with a chicken and egg sort of dilemma, feminists need to focus on elucidating appropriate characteristics for different epistemic subjects whose characteristics vary in accordance with the social locations they occupy, details of which I will discuss in the following.

[36] A good example for this situation is feminist scientists who have used "the tools and perspectives of both the scientific community and the feminist community, critically engaging with these resources" come to "recognize the presence of gender bias and androcentrism in science." (Grasswick 2004, 109)

4.3.3 The Adequacy of Longino's Norms

The transformation of belief into knowledge through social interaction is at the heart of Longino's sociality thesis. This thesis, however, puts immense pressure on objectivity. If social interaction fails to detect and erase prejudices, then, biased accounts will be accepted as "knowledge". That is why it is crucial to monitor social interaction. According to Longino, the more different points of view are included in social interaction, the better our chances of detecting biases. The norms that she sets out aim to maintain diversity in science.[37] Are they adequate, though, to accomplish this important task in real life situations?

As I have mentioned, there is a tension between Longino's norms that aim to maintain diverse viewpoints in scientific practice, and intellectual authority that excludes certain viewpoints from science. I have argued that Crasnow's example of mystical experiences does not hold, yet her concern is still noteworthy. On what grounds do we accept the intellectual authority of certain viewpoints and dismiss others? I have argued that *"*relevance*"* to *science* is one good reason. However, there might be certain groups of viewpoints that conform to the reproducibility criterion that I discuss above and yet would still be excluded from the scientific enterprise. Such cases typically occur when prejudices are diffused in our collective modes of thinking and social imagery. These prejudices

[37] These norms according to Longino are criteria for objectivity. For her, "Scientific communities will be objective to the degree that they satisfy four criteria for achieving the transformative dimension of critical discourse." (1990, 76) She also writes, "As such they constitute norms applying to the social practices and processes of cognition … Satisfaction of these norms assures that theories and hypotheses accepted in the community will not incorporate the idiosyncratic biases (heuristic or social) of an individual or subgroup." (2002, 134)

degrade the cognitive capacity and epistemic credibility of those who are notably different from the dominant group. Thus, viewpoints of the marginalized may be dismissed as *illogical*, *unreasonable*, too *sentimental*, *irrelevant*, *untrustworthy* and so forth. As I have argued, when prejudices are globally endorsed, these exclusions pass unnoticed. Specifically, when the degradations I mention are internalized by the marginalized themselves, it further complicates ways of exposing and fighting against biases. In these complex cases, having formal rules that prescribe the inclusion of different viewpoints in science seems to be insufficient to achieve genuine diversity.

Many feminists have argued for responsible knowing practices in order to prevent biases and maintain epistemic justice. However, there seems to be something missing in this literature. The talk of responsible knowing practices among feminists is typically directed to those who are already (or potentially will be, with no obstruction) in the epistemic circle. Longino's norms for an ideal epistemic community present an example of this case. Her prescription to include different viewpoints is mainly directed at scientists and philosophers of science. It is as if this group of people are the sole actors who could *allow* for different viewpoints to be heard. They are the ones who are required to pursue virtues such as open-mindedness, attentiveness and empathy in order to reinstall the epistemic credibility of the marginalized.[38] According to Longino's prescription, by

[38] For instance Harding writes, "[an inclusive science project] requires learning to listen attentively to marginalized people; it requires educating oneself about their histories, achievements, preferred social relations, and hopes for the future; it requires putting one's body on the line for "their" cause until they feel like "our" causes; it requires critical examination of the dominant institutional beliefs and practices that systematically

being attentive and open-minded, these philosophers and scientists will become aware of their biases and be willing to modify their own biased beliefs and accounts. That is, transformative criticism will be accomplished. The fact that Longino does not say anything about what the marginalized themselves could do for the ideal of inclusive science seems to support my contention that her norms are directed at the dominant group. The lack of any norms *for* the marginalized weakens the adequacy of Longino's overall prescription about inclusive science. It is reasonable to claim, then, that Longino's norms are necessary conditions for maintaining an ideal epistemic community, but sufficiency requires considering what the marginalized could and should do too. Nonetheless, I think Harding's FST points in the right direction in taking into consideration the active role the marginalized play in producing more objective accounts. Hence Longino's account should align with Harding's FST in this respect.

Harding's methodology suggests starting thought from marginalized viewpoints. Since this methodology could and should be applied by anyone (whether marginalized or not) one could question where the active role of the marginalized resides in FST. I think the active role of the marginalized is embedded not in starting thought from marginalized viewpoints, but in Harding's emphasis on "strong reflexivity" and in FST's revolutionary spirit reflected in its Marxian roots.

No doubt active engagement of the marginalized in projects of inclusive science demands self-awareness of their social (and epistemic) disadvantage. As I have

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disadvantage them; it requires critical self-examination to discover how one unwittingly participates in generating disadvantage to them... and more." (1993, 68)

mentioned, consciousness-raising activities have proved quite useful in acquiring such awareness. I think the notion of "strong reflexivity" is an important part of this consciousness-raising. Recall that "strong reflexivity" requires that "the subject of knowledge be placed on the same critical, causal plane as the objects of knowledge." (Harding 1993, 69) This means that subjects, their relations to the world (including social phenomena), their self-conceptions and assumptions, and the value systems they are operating in should be subjected to meticulous analyses, just as objects of knowledge are. It is important to emphasize again that "strong reflexivity" is not mere self-reflexivity when it is confined in an individualistic practice. It is primarily a social activity. I have discussed the limitations of self-reflexivity: it is restricted to those assumptions that one is already aware of. Longino's defence of the sociality of knowledge also applies here: Although one could in principle reflect and become aware of one's biases, the tools for such reflection are socially delivered. More importantly, "strong reflexivity" is not merely a cognitive capacity but also a social practice which generates political force.

Harding proposes that "strong reflexivity" should be embraced by everyone (both the marginalized and those who already have intellectual authority). However, its adoption and practice have a special significance for the marginalized. As the marginalized become more aware of the injustices done to them, they may start to voice their concerns more loudly, become more visible, make themselves harder to ignore, hence potentially form a political force for maintaining inclusive science. Since the marginalized do not possess the actual power to directly control and shape discursive norms, as Bar On warns us, this political force as a result of strong reflexivity becomes

even more crucial to putting pressure on those who regulate the norms. This political pressure is as important as the social and epistemic norms themselves because it will act as a constant reminder of the importance of and motivation for pursuing the social and epistemic norms set out for an inclusive science which leads to more objective accounts than a non-inclusive science.

In sum, the ideal of inclusive science is not only an ethical and political but also an epistemological ideal. The inclusion of diverse viewpoints results in more accurate and adequate accounts, as it helps to reveal and abolish the invisible biases permeating society. In order to maintain inclusion in science, social norms should be managed and monitored. This monitoring is possible by regulating the epistemic community. Here the question of who the regulators are and should be is very important. Yet, in so far as the monitoring of social norms itself is a process that is open to communal critical evaluation, the risk of regulations and regulators becoming totalitarian is minimized. Democratic regulation of the epistemic community demands norms for the dominant groups (i.e. those who are already in the epistemic circle and who have direct power over epistemic regulations) and for the marginalized (i.e. those who are excluded from the epistemic circle although they comply with the reproducibility criterion). Longino sets out criteria for the scientific community, which is likely to comprise mostly members of dominant groups, for inclusion of diverse viewpoints and achieving transformative criticism. Nevertheless, we need additional norms that could apply to the marginalized, who could and should have an active role in the maintenance of inclusive science. I argue that "strong reflexivity" is one such norm because it generates collective awareness, which

paves the way for a political struggle to be heard and make a real difference. Accordingly, there is a responsibility on the part of the marginalized too, to be included in knowledge producing activities. This process would help remove existing biases and prevent prejudices taking root. One implication of this line of thinking for philosophers is that when we talk of responsible knowing practices we need to distinguish the responsibilities of knowing subjects according to their social locations. While members of dominant groups should possess virtues such as attentive listening, open-mindedness, empathy and so forth, the marginalized need be self-aware, insistent, persistent, resistant and without fear of reprisal.

4.4 Conclusion

Longino's criticisms of the received view are important. She not only undermines the main assumptions upon which a mainstream understanding of objectivity lies, but also proposes a promising conceptualization of objectivity that could replace the former. Her account of objectivity is compelling because it preserves the core of "objectivity", i.e. control and maintenance of values, yet does not rest on an illusion of detachment and universality. By sharing this core and the assumption that there is a common language with which we can describe phenomena, she can maintain a dialogue with mainstream philosophers, practising scientists and members of the general public who are attuned to empiricist terminology. This dialogue is important for transforming some of the mistaken core beliefs that lead to biased accounts.

Longino contends that the social monitoring of values in science demands strict regulation of epistemic communities. She provides an elaborate account of the features of an idealized epistemic community. Yet her norms, while they are very important, are incomplete. I have argued that her norms are directed towards people who already have (privileged) access to the epistemic terrain. However, norms are standards of social behaviour expected from actual or potential actors (knowers). There is an undesired consequence of overlooking the responsibilities that the marginalized have (or should have) in knowledge practices: that is, the unintended undermining of the marginalized as potential knowers. I have argued that norms that are directed to the marginalized should be included in Longino's ideal epistemic community in order to reduce this risk. I think "strong reflexivity" is potentially a promising norm that the marginalized could utilize.

In this chapter I have not dealt with what is perhaps the most common criticism against situating knowledge, that is, the charge of relativism. For instance Crasnow claims that "a feminist philosophy of science should improve our understanding of traditional concepts without falling into relativism." Accordingly, she argues that since Longino's feminist account admits cultural and epistemic relativism, it cannot be a genuine alternative to a mainstream philosophy of science. I will examine this criticism in the next chapter where I explore the relationship between ontological objectivity and epistemological objectivity.

Chapter Five

Feminism and Relativism

One of the charges that most often occurs in the literature opposed to feminist philosophy of science is that it leads to relativism. In this chapter I will address two versions of this charge. The first criticism I will discuss is the general claim that feminists endorse the/a gender specificity of knowledge which leads to a form of relativism that undermines knowledge. I will examine in what ways the gender specificity of knowledge could be understood and the forms of relativism that would follow from them. I will argue that feminists need not endorse a *universal* claim about the gender specificity of knowledge. The second criticism I will discuss is Sharon Crasnow's criticism brought specifically against Longino's account of objectivity. Briefly, she claims that Longino's position does not go any further than affirming intersubjectivity, and hence does not provide a firm ground from which to constrain our beliefs. My response to this criticism involves denying the clear-cut distinction between ontological and epistemological objectivity which Crasnow presupposes. I will argue that although distinguishing things that are represented, which fall under ontological objectivity, and the processes used to represent them, which fall under epistemological objectivity, may be conceptually helpful, in practicing science it is unintelligible to talk of the things that are represented apart from the representation process itself.

5.1 Gender Specificity of Knowledge

Feminists are charged with defending the idea that knowledge is in some ways gender-specific. (Niiniluoto 1997) This claim has been interpreted and objected to in various ways. It is often seen as a relativization of knowledge to gender. However, views differ in respect to what component(s) of knowledge gender has a bearing on. Since each interpretation conveys different ontological and epistemological commitments, identification and clarification of these interpretations are important in inquiring into the relationship between feminism and relativism.

In straw arguments against feminism the relativization of knowledge to gender is presented as a universal claim. Is all knowledge gender specific? If so, in what ways is it so? One way of making sense of this universal claim is to maintain that men and women live in totally different worlds. According to this line of thought, since the objects of their knowledge belong to gender-specific worlds their knowledge is inevitably shaped by gender. Putting aside the questions about the accuracy of the claim about different worlds for now, this form of universal gender-specificity is no more interesting than claiming that knowledge is species-specific. That is, since, say, bees perceive a world drastically different from the world we human beings perceive, we could say that they live in a different world, and hence their "knowledge" of the world would be shaped by their species. The claim that knowledge is species-relative is not troublesome for many (except perhaps for those who are strongly committed to the idea that knowledge is a cognitive process, and the only cognitive beings are human beings) since the objects of knowledge for each species are different. Similarly, if men and women indeed live in different

worlds, then the claim that knowledge is gender-specific is not troublesome. However, the claim about different worlds is a controversial one. Although it may be true that women and men experience certain aspects of the world in different ways, it is implausible to jump from this claim to the idea of their living in two drastically different worlds. If it were so, it would be extremely difficult to account for the many cases (in different aspects of the world) that women and men agree upon. Moreover, it would be futile to make any judgment about the desirability of adopting the knowledge (or ways of knowing) of one sex over the other since each form of knowledge would be a function of different worlds. In what other ways can we talk about the gender-specificity of knowledge then?

5.1.1 Gender Specificity and Fact-Constructivism

A more troublesome discourse of the gender specificity of knowledge is closely tied to arguments about the social construction of knowledge. This is because, unlike the claim that differences in knowledge are grounded on natural/necessary disparities between men and women, social construction implies that contingent factors come into play in knowledge acquisition. One line of argument would ground the social construction of knowledge on the social construction of facts, i.e. objects of knowledge. A proponent of the universal gender-specificity of knowledge, then, could argue that since gender is an indispensable actor in social power relations which has a bearing on *all* facts, it inevitably plays a role in the social construction of *all* knowledge. The plausibility of such a claim obviously rests on the plausibility of the argument for the social construction of all facts. Yet, the idea of the social construction of facts is a highly controversial topic in

philosophy, and I do not intend to propose a solution for it. But I would like to question whether feminists indeed need to subscribe to such a view in adopting an emancipatory project and defending gender equality in the cognitive domain.

There are different ways of discussing the social construction of facts. I will restrict my discussion to Paul Boghossian's formulation. In rejecting fact constructivism he makes the following distinctions: For fact constructivism "it is a *necessary* truth about any fact that it obtains only because we humans have constructed it in a way that reflects our contingent needs and interests. This view stands opposed to fact-objectivism, according to which many facts about the world obtain entirely independently of human beings." (Boghossian 2006, 25) According to Boghossian, for a fact objectivist, facts about mountains, dinosaurs or electrons could obtain independently of humans because humans do not have any role in their existence. A striking point in his view is that a fact-objectivist "is not committed to *any* particular catalogue of mind-independent facts," but committed to the idea that some facts obtain independent of humans without having to state or know which facts are mind-independent. (25-26) Fact-constructivists, on the other hand, do not deny that the world contains facts about mountains, electrons, etc. but dispute the *nature* of those facts. As Boghossian puts it, for a fact-constructivist "no fact can obtain independent of societies and their contingent needs and interests." (26)

Here I would like to insert a parenthesis to point out the element of arbitrariness in the literature in labelling certain (perhaps most) philosophical positions. In Boghossian's formulation, fact constructivism is a universal claim. That is, according to fact constructivists *all* facts are necessarily socially constructed (in a way that satisfies our

needs and benefits our interests). Opponents of this position view it as relativising knowledge to our contingent needs and interests. Since needs and benefits are contingent upon circumstances, and upon whose needs are taken into account, different groups of people in different situations could construct different facts when faced with the same objects and/or relations. This situation opens up the possibility of ascribing two different truth-values to a knowledge claim about the object/relation at hand (depending on who the attributors are, and the facts they endorse). Since there seems to be no "neutral" point such as a "mind- independent world" to assess which set of facts is the "correct" one, knowledge becomes relative to the contingently constructed facts. Accordingly social constructivism construed as such is in direct contrast with the conventional coupling between objectivity and mind-independent facts which inhibit relativism. Here the assumption is that facts that obtain independently are absolute. According to this understanding since objective statements express mind-independent facts they do not change across people, cultures or communities. However, the talk of mind-independence is ambiguous and could refer to different positions which I will discuss in the second section of this chapter. Now, let me continue by pointing out that in Boghossian's view fact constructivism presents a universal claim, and fact-objectivism presents an existential claim. That is, according to a fact-objectivist although some facts are socially constructed, some other facts obtain independently of human minds. For instance, facts about money or presidential systems are socially constructed, because their existence requires human existence and classifications. However, the scope of this formulation is questionable. There could be some philosophers who identify themselves as fact-constructivists without

adopting a universal claim. For instance, a philosopher could emphasize the social construction of certain facts (including facts commonly attributed mind-independent existence, i.e. existence without any human intervention) yet refrain from making any judgment about certain other facts whose status is debatable, or adopt a sceptical attitude towards any statement regarding a mind-independent world. According to Boghossian's classification these people do not fully belong to either of the two groups. Although Boghossian's formulation is in harmony with the common-sense distinction between the natural and the social that we appeal to in "our" everyday life, it fails to encompass all possible positions that one could adopt regarding the status of facts.

More importantly, Boghossian's favouring of fact-objectivism is apparent in its formulation as an existential claim coupled with the disclaimer regarding the knowledge of the specific facts that are mind-independent. In other words, formulated as a universal claim, fact-constructivism is far more susceptible to falsification than fact-objectivism. On the other hand, when you claim that some facts are socially constructed and some facts are mind-independent, and you also claim that you do not need to know which facts are mind-independent in order to be an objectivist (or a realist), you protect yourself with a pretty strong position, falsification of which is hardly possible. This is because, you can dismiss any counter argument that presents examples in favour of social constructivism by claiming that the examples at hand belong to the class of socially constructed facts, and that they do not undermine the existence of mind-independent facts, without having to state which facts are mind-independent. However, what you actually gain by adopting such a position is debatable. In rejecting fact-constructivism Boghossian appeals to facts

about concrete objects. For instance he claims that since there were mountains before humans, many facts about mountains should have obtained before humans constructed them. Obviously, Boghossian would not want to accept antecedence to human beings as the only marker for independent existence. After all, he would want to claim that there are independent facts, say, about human beings that are not constructed. He seems to adopt some form of social kind-natural kind distinction as the examples that he puts forward for mind-independent facts fall under natural phenomena while his examples for socially constructed facts fall under social phenomena. Yet even then there are many cases the kind of which is debatable (and probably not resolvable because they might belong to both categories in certain aspects). Not all facts are as easily distinguishable as the existence of mountains (an independent fact about a natural kind) and the existence of money (a constructed fact about a social kind). How could a fact-objectivist position, with its disclaimer, guide one in dealing with complex cases such as facts about women and men or about stupidity and intelligence?[1] For example, an ultimate classification of "womanhood" in terms of the natural or the social is nearly impossible. This is because, first, even if we come up with biological conditions for being a "woman" these would not necessarily correspond to one's feeling like (or not feeling like) a woman. Such discrepancies occur because "womanhood" encompasses a complex web of physical, psychological and social elements. Furthermore, characteristics attributed to womanhood change from time to time and/or culture to culture. These changes also imply that there is

[1] Another complicated example involves the status of epistemic facts which I discuss below.

not a fixed object that 'womanhood' in its totality refers to. The problem with the objectivist position is that if we need not know which facts are constructed and which are not, then it is not easy to see the point in making the distinction. What does such a distinction serve other than conceptual comfort? I believe such comfort is not justifiable especially when social policies are based on so-called natural kinds because they are believed to express absolute facts. A likely danger of an objectivist insight is that it could easily make us blind to the elements of the social in certain established facts. Objectivism as formulated by Boghossian does not provide any theoretical means to alert us to the effects of the social in adopting a dogmatic line between constructed and mind-independent facts.

Another confusion about Boghossian's classification has to do with the extent to which knowledge-relativity is acceptable. I have mentioned that Boghossian does not deny the existence of constructed facts. I have also mentioned how the social construction of facts is seen as relativizing knowledge to our contingent needs. Hence if fact-objectivists agree that some facts are socially constructed, they should be comfortable with the relativity of some knowledge claims since, say, some other culture could in principle have constructed different facts about a given phenomenon than "our" facts about the same phenomenon. Nevertheless, in the literature, the defenders of objectivism (or realism) do not emphasize this situation enough, and they argue that no knowledge claim is/should be relative. The reason why this point is important is that the literature on realism versus relativism is presented as an all or nothing issue. This attitude dismisses those positions which embrace both views in different ways. That is, arguing for the

relativity of certain knowledge claims while not making a universal claim about all knowledge claims is not allowed.

In criticising Boghossian's formulation I do not intend to deny that there are no independent facts. However, I think adopting fact-objectivism without having a good sense of which facts are mind-independent and which are not merely provides theoretical satisfaction with no practical applicability to real life situations. I think the urgent question that needs to be answered is which of the following attitudes is "just" (in all senses of the term) for our epistemic practices, specifically those pertaining to science: i) thinking that we obtain independent unchangeable facts about the world, and base our lives and policies on this *belief* while there is a chance that we might be wrong; or ii) thinking that since we cannot "know" whether we obtain independent facts or not, we should treat all facts as if they are socially constructed (even if they are not) and always be wary of any undesirable consequences of accepted facts that benefit our interests. Feminists often suggest adopting something similar to the second attitude.

In the epistemology literature, feminists are generally presented as adopting a belief in the social construction of facts, although they mostly do not (or need not) appeal to a universal claim about the social construction of facts. After all, emphasizing the situation that certain facts about men and women are socially constructed in a way that serves male interests is not equivalent to denying there can be facts that obtain independently. For instance, Longino's commitment to the publicity of science indicates her agreement about the existence of mind independent facts. She identifies two conditions for the publicity of science: i) that we have a common language which we use

to describe our experience and within which we reason; ii) that the objects of experience which we describe and about which we reason are purported to exist independently of our seeing and thinking about them. (Longino 1990, 70) These conditions express a commitment to a minimal realism which opposes an idealistic view that restricts existence (including the existence of objects of experience) to the contents our minds. This commitment, however, does not automatically entail the view that there is one true description of the object of our experience. It is compatible with the idea that there can be multiple correct descriptions of a phenomenon. In fact, it can be argued that Longino's position is akin to the position that Boghossian dubs the social relativism of descriptions. According to this view "which scheme we adopt to describe the world will depend on which scheme we find it *useful* to adopt; and which scheme we find it useful to adopt will depend on our contingent needs and interests as social beings" (29) In Longino's view which scheme we adopt to describe the world depends on our background assumptions (which may reflect our interests and needs). As Boghossian admits, such a position does not entail fact-constructivism.

In Boghossian's terms then, feminism is not necessarily incompatible with fact-objectivism. However, many feminists would maintain that committing to the existence of mind-independent facts without a sceptical approach to claims about mind-independency or without providing any means to detect which facts are in fact mind-independent is not sufficient for achieving "just" epistemic practices. This is apparent in the importance feminists give to rigorous analysis and identification of which facts are socially constructed and how. Hence, these feminists often adopt a stronger position than fact-

objectivism. This is the intended aim of both the ideal of strong objectivity presented by Harding, and the transformative criticism made possible by the intersubjective exchange defended by Longino. The ultimate argument of feminist studies of science is that in the absence of a thorough examination of the context within which science is practised, some facts will be accepted as natural (hence fixed and unchangeable) although they are shaped by social and political interests. According to Harding and Longino, these misidentified facts are not merely a result of the failures of individual scientists but a consequence of structural vulnerabilities embedded in the scientific enterprise. Nothing in these claims requires feminists to adopt a universal claim about the social construction of facts.

Where does the gender specificity of knowledge reside in these feminist positions then? For many feminists, the gender specificity of knowledge resides primarily in methodology rather than ontology. Recall from the third chapter that for Harding knowledge is gender specific in cases where one's gender helps to reveal biases in accepted views. In the current Western androcentric worldview, for instance, women are in a more epistemologically advantageous position than men in gender related matters. Due to their oppressed positions women are more open to detect sex biases than men who do not have any interest in the exposure of such biases. Accordingly, the social (contingent) character of facts that are accepted as absolute is made visible. Here, however, the scope of knowledge that is gender specific is limited to matters about, related to and affected by gender relations. Hence it does not invoke gender specificity for

all knowledge.[2] It is important to note, however, that elaborate feminist works on various aspects of human life show that the scope of such matters is a lot broader than is conventionally thought.[3] As I discuss in detail in the previous chapter, Longino also agrees that one's gender could help reveal sexist biases prevailing in the scientific community during transformative criticism. For instance, it is more likely for a female scientist to notice and criticize the andocentric assumptions prevailing in models of reproduction where an active role is attributed to sperm and a passive role to eggs in the process of fertilizations. But for Longino, gender specificity goes further than a privilege in detecting biases. She defends the effectiveness of adopting feminist ideals in assessing certain subject matters. It is important to note that the form of relativism that this view implies is different from the one Harding's position implies. Here it is maintained that assessment of a knowledge claim (including its processes of justification) is relative to the adopted ideals which are appropriate to the goals or concerns of the inquiry at hand. In other words, while the former view relativizes achieving (some) knowledge to subjects' identities (such as gender), the latter view relativizes justification to background assumptions where power relations pertaining to gender could operate.

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[2] So it can be said that feminist philosophy of science endorses a kind of relativism pertaining to methodology. According to this view our epistemic interaction with the world, that is, how we perceive the world and form beliefs about certain aspects of it is relative to various factors such as gender, race, class, age and social position. Yet, still, it can be argued that this position does not (need not) maintain that all knowledge is relative to the above variables.

[3] See Harding and O'Barr (1987); Fonow and Cook (1991); Crowley and Himmelweit (1994); Garry and Pearsall (1996).

Admittedly, introducing feminist ideals in assessing certain knowledge claims goes against the contention about the fixity of epistemic standards. If the epistemic principles and ideals we adopt in assessing knowledge claims vary depending on, say, the subject matter and the intended goal of the inquiry and so forth, then it will be implausible to talk of the existence of absolute *epistemic facts*. One line of argument suggests that by virtue of introducing contingent elements in epistemic assessments, this view undermines the rationality of our epistemic practices and makes room for epistemic relativism. Here the underlying assumption seems to be that relativism is irrational. This assumption rests on the belief that reasoning rests on fixed principles. However, the nature of these principles, that is, whether they are *universally* fixed or *contextually* fixed is of crucial importance for the claim that relativism is irrational. If they are universally fixed, then we have to accept that relativism is in fact irrational. If, on the other hand, principles of reasoning are contextually fixed, i.e. if contextual factors have a role in determining which principles to follow in our reasoning, then we can argue that not all forms of relativism are irrational. In the next section, I will discuss Boghossian's arguments against epistemic relativism and consider how they affect the forms of relativism that I identified as adopted by Longino.

5.1.2 Epistemic Relativism: The Social Construction of Epistemic Standards

Boghossian writes "the world out there is what it is largely independent of us and our beliefs about it. There are many facts that we did not have a hand in shaping. If we want to make a true conception of the way the world is, our beliefs need to accurately reflect

those mind-independent facts. Of course, the world does not just inscribe itself onto our minds. In trying to get at the truth, what we do is to figure out what's true from the evidence available to us. We try to form the belief that it would be most *rational* to have, given the evidence." (2006, 58) He proceeds with questioning whether there is only *one* way of forming rational beliefs given the evidence. According to Boghossian epistemic relativism is the view that "there are no universal epistemic facts, that facts about what belief is justified by a given item of evidence can vary from community to community…different people may rationally arrive at opposed conclusions, even as they acknowledge all the same data." (59) He claims that epistemic relativists adopt a) *epistemic non-absolutism*: there are no absolute facts about what belief a particular item of information justifies, b) *epistemic relationism*: according to the epistemic system C, that I, S, accept, information E justifies belief B, and c) *epistemic pluralism*: there are many fundamentally different, genuinely alternative epistemic systems, but no facts by virtue of which one of these systems is more correct than any of the others. (84-85)

Boghossian opposes each of these formulations. I will not discuss the specifics of his arguments but I would like to make some general remarks about his objections. Just as he formulates fact-constructivism in a specific way, Boghossian presents epistemic relativism as the view that *no* (rational) distinctions between different epistemic systems can be made.[4] However, epistemic non-absolutism does not immediately entail that there are *no* facts that help us distinguish between epistemic systems. Before delving into this

[4] An epistemic system, according to Boghossian, "consists of a set of general normative propositions—epistemic principles—which specify under which conditions a particular type of belief is justified." (85)

issue let me note that in his discussion of epistemic facts Boghossian mostly focuses on instances of justification. In other words, he is interested in whether a piece of information E justifies the belief B or not. However, justification is not the sole component of knowledge, and therefore the scope of epistemic facts could surpass instances of justification. For instance, we can argue that instances of belief formation are closely related to matters of epistemic fact. Since the way we form beliefs is a function of complex interactions between cognitive, psychological and social processes, different people could form different beliefs about the same phenomenon.[5] Perhaps it is not controversial to claim that a belief is relative to an individual or a group of people with their specific circumstances. There is no doubt that a belief is relative to a cognitive subject by virtue of being a psychological entity. Yet it is also very important to emphasize the specificity of circumstances in which a belief is formed, because situating belief formation helps us better understand why two individuals form different beliefs about the same phenomena and why certain phenomena are missing in certain people's fields of belief formation altogether. It could be argued that this form of relativism is quite mild, and some might want to reserve the term "epistemic relativism" for cases of truth and/or rationality. However, it is often neglected that beliefs are the starting point knowledge claims. Even if we accept that beliefs are transformed into knowledge through justification, a justified belief would still be confined to the initial belief which was

[5] In claiming this I do not assert that belief forming is merely an individualistic practice. I accept that epistemic agents are not isolated and that they are dependent on one another in various ways. Perhaps the basic form of this dependency is expressed in Longino's view that individuals learn to reason through their interaction with other people. (Longino 1999, 345)

shaped by certain circumstances. For instance, if a belief about, say, a human disease is formed within a worldview where the male body constitutes the paradigm of human beings, and is "justified", it will at best produce incomplete "knowledge". Hence the relativity of beliefs to individuals in concrete circumstances has important consequences for our knowledge claims and should not be overlooked.

Nevertheless for the sake of the argument, let's grant Boghossian that epistemic relativism is primarily about justification. According to this conception of relativism, epistemic facts are constructed. However, since both an objectivist and a fact-constructivist agree that there are some facts that are constructed, we need to discuss why objectivists deny that *epistemic* facts are constructed. One of the ways to deny that epistemic facts are constructed is to endorse the view that the epistemic standards and principles we operate with are all fixed. So let's distinguish between those who hold that epistemic standards and principles are absolutely (or universally) fixed and those who hold that they are not fixed, or fixed only according to our contingent needs and goals. In the literature on epistemic contextualism the former are called invariantalists while the latter are called contextualists.[6] Although Boghossian does not use this terminology, we can call him an invariantalist as he defends the claim that "facts about what belief would be justified by a given piece of evidence are facts that must be thought of as absolute, and not as varying from social context to social context." (2006, 111) It is within this understanding that endorsing epistemic non-absolutism is seen as a barrier to a rational choice between two different epistemic systems. According to this view, rejecting

[6] Among others Keith De Rose and Jason Stanley use this terminology.

absolute epistemic facts invites the influence of our contingent needs and interests on epistemic principles, which in turn deprives us of rational (universal) criteria in deciding on epistemic systems. Nevertheless the universality of epistemic principles is not indubitable. We can, for instance, question whether *all* epistemic principles are fixed and whether there are any epistemic principles that reflect our contingent needs and goals.[7] If there are some epistemic principles that apply universally, we can question whether they can provide sufficient guidance in all epistemic circumstances or whether we can have knowledge of these principles so that we can follow them in making decisions between different epistemic systems. These questions are worth investigating because if so-called fixed principles are not sufficient to guide our decisions in all circumstances, and contextual features indeed have a bearing on all or some epistemic principles, then we are compelled to modify our epistemological investigations. Rather than clinging to search for universal principles, for instance, we should try to understand the interplay between contextual values and epistemic facts. We should question to what extent the influence of contextual values should be tolerated, and in what ways we should guard our epistemic practices against the negative effects of contextual values. It is only after providing

[7] It is worth noting that in epistemology and the philosophy science literature there isn't a determinate set of epistemic principles that is unanimously accepted. Empirical adequacy, simplicity, scope, fruitfulness, and so forth have been endorsed as rational principles in theory choice in the philosophy of science. On the other hand, some feminists have adopted ontological heterogeneity, mutuality of interaction, trustworthiness and empathy as epistemic ideals. More broadly, in epistemology the conditions for justification/knowledge such as certainty (absolute or to a certain extent), a causal link between a belief and what makes the belief true, reliability of belief formation processes, and non-existence of relative alternatives constitute some of the epistemic standards that have been put forward by various philosophers.

satisfactory answers to these questions that we can have a meaningful and informative discussion about comparing different epistemic systems. In the absence of a commitment to universally fixed epistemic principles, we must give up the search universal criteria that will guide our decisions and allow us to deem "rational" only those choices that presumably rest on such criteria. Instead we must investigat and compare in detail the contextual features of rival epistemic systems against a given (local) goal. It is through this process that we achieve a "reasonable" choice.

Turning back to the question of why we should accept the universality of epistemic facts, I propose following Boghossian's reasoning. Recall that he suggests "in trying to get at the truth, what we do is to figure out what's true from the evidence available to us. We try to form the belief that it would be most *rational* to have, given the evidence."[8] (59) So, in trying to get at "the truth" about the nature of epistemic facts, we need to figure out what it is rational to believe given the evidence. Now the question is, what evidence does an invariantalist have to have in order to claim that epistemic principles are fixed?

One way to argue for the absolute fixity of epistemic principles is to claim that they are mind-independent. The argument could proceed as follows: since epistemic standards exist outside the mind they are immune to human intervention, hence would not

[8] Boghossian's concern in this passage is to achieve an accurate representation of the way the world is. But he identifies the way the world is with mind-independent facts, as if those facts he identified as mind-dependent are not a part of the world. In other words, his "world" is a pretty "small" world. It could even be argued that because he does not apply this reasoning in his discussion of epistemic facts, he might think that these facts do not belong to the way the world is.

vary from social context to social context. However, there are a few obvious problems with this line of reasoning. Whether this immunity is sufficient to preserve absolute fixity is questionable. After all, mind-independent facts, such as facts about mountains, do change without human intervention, as a result of natural forces. Furthermore, there are also mind-independent facts that are changed by human intervention. For instance, facts about a river would change as a result of human manipulation of the riverbed. Hence mind-independence does not guarantee that there will be no human intervention. Perhaps a specific meaning is attributed to the claim that epistemic facts are mind-independent. For instance, it might refer to the non-arbitrariness of epistemic standards. Yet this meaning does not necessarily convey universality. In other words, epistemic standards can be fixed depending on specific circumstances. In this view, because epistemic standards are a function of determinate circumstances they are not arbitrary. Yet, such contextual fixity would not yield (absolute) universality.

Another attempt to preserve the universality of epistemic principles could be to ground it in reason, and claim that epistemic principles are *a priori*. However this approach contradicts the contention about the mind-independence of epistemic standards since reason is a faculty of the mind. But more importantly, it fails to provide a sufficient answer to our question regarding the *evidence* that the invariantalist possesses in order to believe in the universality of epistemic standards. In fact, does not the call for evidence require an examination of actual cases where our beliefs are deemed justified, and detecting the features that have or could have any bearing on the justification processes of these beliefs? Admittedly such an examination is a difficult task. Boghossian agrees: "it is

hard to say, even as a purely descriptive matter, precisely which epistemic principles we operate with. In their full detail, these principles are enormously complicated and even philosophers who have worked on the topic for years would be hard-pressed to formulate them in a way that is free of counterexamples." (65) Although the current absence of a consensus on a complete list of epistemic principles may not indicate an eternal failure to achieve such a list or that it does not exist, it surely undermines the evidence we seek for arriving at a rational belief about the absolute fixity of epistemic principles.

So far, my examination of the universality of epistemic facts has focused on the premise that epistemic principles, which determine whether an item of information E justifies a belief B, are absolutely fixed. However, there is another premise that the universality of epistemic facts rests on: there are no factors other than epistemic principles that have a bearing on an epistemic fact. Yet Longino convincingly argues that social values intervene within epistemic facts. In fact, she argues that epistemic principles themselves are influenced by social values. If she is right, then the belief in the universality of epistemic facts is undermined. Let's have a close look at the so-called universal epistemic principles.

Boghossian identifies observation, deduction and induction as the fundamental principles which specify a significant portion of our ordinary "post-Galilean" epistemic system. Other debated candidates for such principles that he mentions are inference to the best explanation, simplicity, degrees of belief, probability and so forth. (67) However, there still is an unanswered question: are these epistemic principles the sole actors in determining epistemic facts? Extensive studies by feminist scholars on concrete cases

show that there is always more to justification than the epistemic principles that Boghossian identifies. One possible explanation for the invariantalist contention that epistemic principles are fixed is a tacit assumption about a rigid dichotomy between the cognitive (epistemic and/or rational) and the social. Rationality is typically coupled with universality. That is, if a belief, a thought or an action is rational, then it is necessarily rational for everyone. The social, on the other hand, entails contingency. But what if it is shown that the social affects the cognitive? That is, what if what is accepted as the cognitive (epistemic) is not purely cognitive (epistemic) in the first place? Would it mean that the cognitive is not purely rational and hence is not absolutely fixed? In her book *The Fate of Knowledge* (2002) Longino has an extensive discussion of these questions which I will not examine here. Yet, it is fair to claim that her rejection of the dichotomy between the cognitive and the social reflects her commitment to the social character of science. Her reformulation of objectivity aims to accommodate the impact of the social on scientific practice and its principles. In the next section I will discuss Longino's account of objectivity in detail and defend it against Sharon Crasnow's criticisms.

In sum, charges of epistemic relativism often rest on an ungrounded assumption that epistemic standards are universally fixed. However, as I have discussed, we do not have sufficient evidence to *rationally* believe that they are so. In the absence of such evidence it is reasonable to *assume* that there are contextual elements that influence the principles we follow when we engage in epistemic practices. Acceptance of this assumption is often seen as opening room for "anything goes". Yet this presumption is mistaken. I will elaborate this point in the next section.

5.2 Crasnow's Critique of Longino's Objectivity

In her article "Can Science Be Objective?" (2003) Sharon Crasnow examines whether Longino's account of objectivity is compatible with a philosophy of science that rejects relativism. Her inquiry is guided by Ron Giere's contention that a feminist philosophy is valid only if it improves our understanding of traditional concepts without falling into relativism. (130) Crasnow's examination leads to the conclusion that Longino's account of objectivity is not compatible with a philosophy of science. This is because although it prevents a form of relativism called subjectivism, where truth is relative to individuals' beliefs, it still accommodates cultural and epistemic relativisms that undermine science. I have a few objections to Crasnow's arguments. My main criticism is that the guiding contention that she follows is problematic because it assumes, first, that it is possible to deny relativism altogether, and second that the only plausible philosophy of science is a nonrelativistic one. Hence my discussion in this chapter is not meant to oppose Crasnow's claim that Longino's account leads to epistemic relativism. I will argue that some form of epistemic relativism is inevitable, and that the form of relativism Longino's account conveys is not a vicious one. Longino's account provides a fruitful analysis that enhances our understanding of science and its concepts.

5.2.1 Crasnow's Objections

Crasnow has a few interrelated objections to Longino's account of objectivity. I have discussed some of them in the previous chapter. In this chapter, I will focus on her

concerns about epistemic relativism as it affects objectivity, and her suggestion to adopt a form of scientific realism to restore a traditional understanding of objectivity. (2003, 140)

Recall from the previous chapter that according to Longino, evidence is context dependent. That is, whether an item of information E is accepted as evidence for a hypothesis H is determined by the context, which consists of background beliefs and assumptions. Through these background assumptions contextual values enter into scientific inquiry. In order for scientific inquiry to provide knowledge "there must be some way of minimizing the influence of subjective preferences and controlling the role of background assumptions." (Longino 1990, 216). Such control, Longino argues, is maintained by the social character of scientific activity. Public scrutiny among people from different perspectives helps make visible the operating background beliefs such as idiosyncrasies and ideologies that individuals (consciously or not) adopt. According to this understanding, the primary bearers of knowledge and objectivity are communities. That is, what is agreed to be knowledge or not is ultimately a function of an epistemic community, not of individual knowers.

Longino defines relativism as the view that "there are no legitimate constraints on what counts as reasonable to believe apart from an individual's own beliefs." (Longino 1993, 113) She contends that the social understanding of scientific knowledge escapes the relativism that individualistic epistemologies face. In the social understanding of science, an epistemic community and its standards have an important role in constraining individuals' beliefs. According to Crasnow, however, avoiding this kind of relativism, which she identifies as subjectivism, is not sufficient to save the legitimacy of Longino's

feminist account. Following Larry Laudan's understanding of relativism, where "the natural world and such evidence as we have about that world do little or nothing to constrain our beliefs", Crasnow claims that Longino's account fails, because it does not show how the natural world constrains our beliefs. An important passage, which summarizes Crasnow's critique, is the following:

> Underlying recent feminist philosophy of science is the idea that we must take seriously the role that social, political, and cultural factors play in the development of science. When method is described as influenced by these contingent factors and no account is given of how some features of the independently existing world are nonetheless captured through this method, it is difficult to avoid the charge of relativism without completely reconceiving the traditional notion of objectivity. If we ignore issues of truth or the way that nature constrains our beliefs, then we are forced to find objectivity in some constant feature of good method, something that does not vary with cultural or social factors. (2003, 139-140)

This insight suggests that a legitimate constraint on what counts as reasonable to believe should involve the natural world. This claim can be interpreted in at least two ways. The stronger interpretation suggests that the natural world and only the natural world can legitimately constrain our beliefs. The weaker interpretation, on the other hand, suggests that the natural world is one of the elements among others that legitimately constrain our beliefs.[9] The stronger interpretation runs into the following problem. If the natural world is the sole actor in determining what it is rational to believe, seeking justification for our beliefs becomes futile. As Boghossian admits "the world doesn't just inscribe itself onto our minds. In trying to get at the truth, what we do is to figure out what is true from the

[9] It should be noted that these interpretations rest on different ontologies. For instance, if one is committed to the idea that "the natural world" includes everything that is, then one will deny the plausibility of the weaker interpretation altogether.

evidence available to us: we try to form the belief that it would be most *rational* to have, given the evidence. (Boghossian 2006, 58) That is to say, the natural world is often not immediately available and accessible to us. Between the natural world and our beliefs there is the whole process of evidence gathering and assessment. Claiming that the natural world is the only legitimate constraint on our beliefs is to bypass this process and wrongly assume that the natural world is directly accessible to us. Even if we grant that some phenomena are directly accessible to us, then the aforementioned claim would restrict our knowledge of the world to such phenomena, and hence would drastically limit what we can know about the world. If evidence were imposed *merely* by the natural world, the very act of seeking and assessing evidence would become redundant. An important thing that Boghossian and many other thinkers ignore is that *we* try to figure out what is true from the evidence *available to us*. The availability of evidence is often a function of physical, biological, technological as well as social and economic factors. Furthermore, the "we" who is trying to figure out what is true from the *available* evidence is never uniform. Hence, the evidence that is supposed to lead us to rational beliefs is always an outcome of a complex relationship between "us" and the natural world. In sum, it is fair to claim that the constraint of the natural world is a necessary but not a sufficient condition for the legitimacy and rationality of a belief. This claim is compatible with the weaker interpretation I mention above. It is also compatible with the context-dependency of evidence. To argue for the context-dependency of evidence is not to deny that the natural world has any constraint on our beliefs, but to emphasize that there is more to evidence than the impacts of the natural world, and that the other factors that affect evidence

assessment should be subjected to appropriate controls if we are to achieve legitimate beliefs.

Crasnow's objection to Longino's account rests on her conviction that Longino detaches objectivity from its referential roots. In other words, her account of objectivity does not refer to the natural world. Because of this separation, Crasnow argues that Longino's account fails to provide any means to judge, affirm or deny the objectivity of a viewpoint. Consequently, it falls short of adjudicating between competing viewpoints. However, the conviction that Longino's formulation of objectivity has no reference to the ways in which the world (nature) constrains our beliefs is mistaken. I think ignoring the compatibility of context-dependency with the weaker interpretation of the ways natural world constrains our beliefs leads to this misunderstanding.

5.2.2 Two Senses of Objectivity

Crasnow's criticism relates mainly to Longino's discussion about two senses of scientific objectivity. Longino claims that, conventionally, the first sense of objectivity "is bound up with questions about the truth and referential character of scientific theories, that is with issues of scientific realism." (1990, 62) Here, when we claim that science is objective we mean that the (accepted) scientific theories provide correct descriptions of the world as it is. That is, the facts about objects or the relations among them that theories depict are mind-independent. I shall call this sense the *ontological sense of objectivity* as it is tied to

the question of what there is.[10] The second sense of scientific objectivity that Longino identifies has to do with modes of inquiry. In this sense science is objective if the products of scientific practice are obtained through a method which involves nonarbitrary and nonsubjective criteria in developing and assessing scientific theories. (62) I shall call this sense, *epistemological objectivity*.

Epistemological objectivity for Longino, also has two different senses. Objectivity in scientific method can refer to the objectivity of data, as well as the objectivity of assessments of hypotheses and theories. While the first sense has to do with whether data are obtained through reliable means, that is whether experiments and calculations have been properly performed, the second sense has to do with whether assessments have been made in an unbiased and unprejudiced manner. (63)

There are many interesting questions that could be raised about the two senses of epistemological objectivity. For instance, if observation is theory-laden can we still speak of the objectivity of data? Perhaps we can think of a sense of objectivity which would allow us to talk about the objectivity of data even if observations are theory-laden. This sense has to do with what Allan Megill calls "procedural objectivity". Procedural objectivity is achieved by setting rules to limit the exercise of personal judgment. (Megill 1994, 11) This sense of objectivity goes hand in hand with standardization in sciences: "For example, rules of statistical inference and rigid interview protocols are alike designed to make knowledge as independent as possible of the people involved in making

[10] I should note that in so far as ontological objectivity has to do with "what there is", it can be formulated without adopting scientific realist commitments where "what there is" is identified with "the world as it is in itself". I will discuss this issue further below.

it." (11) As such the aim of procedural objectivity is akin to Dalston's mechanical objectivity: nonintervention by human beings. However, as I discuss in chapter one, talk of the complete absence of human impact is problematic. Megill notes that procedures are set within a discipline. They are "a matter of conventions arrived at within a particular sphere of research (as, for example, when statisticians and others talk about "statistically significant" results)." (11) Hence, it is important to underline that procedures do not guarantee the truth of findings. It is within this particular sense of objectivity that we can ascribe objectivity to data even if observation is theory-laden, if the data are collected according to the established principles and protocols. However, it would be wrong to jump to the conclusion that objective data yield bias-free accounts.

Let me clarify by an analogy. There are different ways to talk about the objectivity of a photograph.[11] There is a certain sense in which a photo is objective, and this sense has to do with the working of a camera when it *processes* and *records* what it is directed at. The working of a camera is subject to mechanical principles and procedures. These principles indicate that there is no human intervention in the *recording* of the object that the camera is directed at in the world.[12] It is not to say, however, that there is no human intervention in *what is being depicted*, because, in order to depict something, the mechanical device needs to be directed at certain objects. The choice of what objects to

[11] Recall from Chapter One that photography is a symbol of mechanical objectivity.
[12] It can be argued that even the working of a camera is not free from human intervention since it is ultimately an artifact. However, once the mechanism of how to record images is invented, the process of recording is pretty mechanical (except the cases of manipulations for artistic or political reasons). Similarly, although the rules and protocols that guide experiments are set by human beings, once they are set we can examine whether they are being objectively followed or not.

direct your camera at will affect what is being depicted. In other words, this whole process involves decisions. Hence, questioning the objectivity of a photograph goes further than considering the objectivity of what a camera does but also involves examining decisions about where to direct the camera and how to read what it says. The location of the camera reflects what the *photographer* (consciously and unconsciously) intends to do (show, represent or explain). How to read a photo, on the other hand, has to do with the *audience* and the social meaning they attribute to certain objects and relations.[13] These meanings reflect their values and prejudices.[14] Hence a comprehensive response to the question of the objectivity of a photograph should include assessments of what a photographer intends to show as well as what an actual or potential audience does with it and why. In other words, the objectivity of a photograph has also to do with how it is viewed and received/read. In Longino's discussion, I think the objectivity of what the *camera* does corresponds to the objectivity of data. Hence, even if observation is theory laden, data is objective in the sense that the collection and processing of data are subject to certain rules (just as the workings of a camera are subject to mechanical rules). Why certain things are observed rather than others has to do with the decisions or preferences of scientists, which might include unconscious inclinations. These decisions are primarily made in the course of the research. However, they are also often accompanied by social, economic and sometimes personal considerations. The examination of these elements is part of the assessment of theories. Furthermore, similar to the photography case, how the

[13] My contention is that for whatever purpose, a photograph is taken for an audience. This audience could consist of a group or a single person such as the photographer herself.
[14] For an interesting discussion on the limitations of visual evidence see Code (2014).

theories are received, what use they are put to, what other theories (or sometimes ideologies) they serve are also important aspects of theory assessments in thinking about objectivity.

Accordingly, we can claim that although objective assessments of theories require objective data, the objectivity of data does not entail fair assessments. This is because data collection and processing are one part of the scientific enterprise among others. For instance, apart from the impacts of social and political considerations, there are "internal" elements that prevent achieving absolute objectivity in theory assessments. I have discussed in the second chapter that so-called constitutive values such as simplicity, accuracy, fruitfulness and so forth do not always co-operate in assessing theories. In fact, in certain cases these ideals contradict one another. Scientists are often compelled to follow certain ideals rather than others depending on their subject matters as well as on the broader social context and the worldview of their era. In such cases, although data are objectively obtained, assessments often convey the biases that reflect the reasons for choosing to follow one ideal over others.[15] Hence objective data do not fully guarantee epistemic objectivity with respect to assessments. This is to say, in seeking epistemic objectivity we need both the objectivity of data and the objectivity of assessments. Longino's main issue is to set the conditions for achieving the latter.

Now the question that needs to be considered is whether epistemic objectivity guarantees ontological objectivity or not. Longino notes that according to common wisdom, if science is ontologically objective, it is because it is epistemologically

[15] For a discussion on this issue see Longino (1995).

objective. (1990, 63) That is, epistemic objectivity is a prerequisite for ontological objectivity. Yet, even though we can claim that epistemic objectivity is a necessary condition for achieving ontological objectivity, it would be too strong to claim that the former guarantees the latter. After all, we have claimed that epistemic objectivity is related to the issues of justification and ontological objectivity is related to issues of truth. Since justification is fallible (i.e. we can be justified in believing false propositions) we cannot claim that it guarantees ontological objectivity. What is the relationship between the two senses of objectivity then?

Longino says very little about ontological objectivity and states that her focus is on epistemic objectivity. For Crasnow, however, an exclusive focus on epistemic objectivity is problematic because for her, the question of whether relativism can be avoided by socializing scientific knowledge "is tied to the question of whether a complete account of objectivity requires examining both justification and truth." (134) However, this criticism itself rests on the assumption that epistemological objectivity and ontological objectivity can be obtained or examined separately from one another. I will argue that although the two senses of objectivity differ categorically, they are intrinsically related.[16] Longino's avoidance of an elaborate discussion of ontological objectivity might be interpreted as indicating her tacit assent to the idea that that the two senses of objectivity are distinct. But some features of Longino's account, specifically those pertaining to conditions for the publicity of scientific activity give support to my

[16] It is similar to the relation between notions such as "teacher" and "student". Although we can talk of teachers and students separately, the concept of "teacher" does not make sense without the concept of "student".

contention that the mutual dependency of two senses of objectivity is implicit in her account. If this claim of dependency is true, Crasnow's critique of Longino's account is undermined. That is why it is important to dwell on the relationship between the two senses of objectivity.

5.2.3 The Relationship between Ontological Objectivity and Epistemological Objectivity

Many thinkers such as Longino, Lorraine Daston, Allen Megill and R.W. Newell distinguish an ontological and an epistemological sense of objectivity. However the relationship between these two senses is often left obscure. This confusion I believe is the ultimate reason behind the charges of relativism levelled against feminist accounts of objectivity.

In *Rethinking Objectivity* (1994) Megill distinguishes a *philosophical* or *absolute* sense of objectivity that is concerned with discussions of 'representing things as themselves'. He argues that in this sense the role of objectivity could differ depending on whether the emphasis is on the things that are being represented or on the representation process itself. Hence, there is an ontological and a methodological dimension to absolute objectivity. While ontological objectivity focuses on the nature or the composition of things that are represented, epistemological objectivity focuses on the standards for assessing the claims to represent things as they really are. (1994, 2)

In his book *Objectivity, Empiricism and Truth* (1986) Newel, on the other hand, argues that in the philosophical sense objectivity is either assigned to 'objects', i.e.

particular bodies, entities and states of affairs that exist independent of perception and have space-time continuity, or it is attributed to beliefs, judgments, propositions about what really is the case. (16)

In general, then, ontological objectivity is identified with the correct description of the world as it is. Such a description represents the facts about objects and/or states of affairs that obtain independently. A theory is objective if the view it presents corresponds to, mirrors or represents mind-independent facts. However, there are problems with this understanding. For one thing, the notion of 'mind-independency' is vague. Does it refer to an existence independent of perception? Or does it refer to anything that is not produced/constructed by human beings (or any cognitive being for that matter)? Different senses of mind-independency have different consequences for our understandings of objectivity. For instance, as I discuss above, mind-independent facts are compared with socially constructed facts. When objectivity is defined in terms of correspondence to the mind-independent world and mind-independency is used as opposed to human products, any talk of the objectivity of social phenomena goes down the drain. For instance, we cannot talk about the objectivity of the "fact" that in patriarchal societies transgendered people face oppression or about the "fact" that today is the first day of May, as calendars just like patriarchal societies and oppression are human products. Of course human products are not limited to the social sphere. The status of many entities in the natural sciences is also debatable. For instance, what is the nature of theoretical entities? If we accept, say, that atoms are human constructs, then in this understanding there will not be any talk of the objectivity of theories about atoms. In fact we can take this view even

further and claim that we cannot objectively talk about anything. This is because, in order to control and make sense of our surroundings we categorize and conceptualize the world. Hence, our descriptions of the world always involve human categorizations, i.e. products of mind.

Perhaps a way of escaping the problems I mention above is to identify mind-independence with the ultimate structure of the world. A strict realist, such as the scientific realist that Hilary Putnam discusses in *Many Faces of Realism* (1987), would claim that the commonsense world is a mere "projection" and that "what there really is is what 'finished science' will say there is" (Putnam 1987, 4) The statements of "finished science" will typically consist of statements regarding the so-called primary qualities of objects, that is, qualities that obtain without the perception of a sentient being. Accordingly the only things that we can objectively talk of will be the underlying structure of the world. In addition to the controversy over the very distinction between secondary and primary qualities, this understanding will considerably narrow the scope of ontological objectivity.

Connected to the last point, it can be argued that ontological objectivity becomes redundant when it is identified with representations of facts about mind-independent objects. According to Newell, ontological objectivity is often used to explain "the coherence, uniformity and identity of perceptions and of the difference between what is appearance and what is not". In other words, it marks "what is really out there". However, for Newell this understanding leaves no distinctive role for objectivity "beyond the job already performed by the notion of independent particulars" (Newell 1986, 18-19). It is

circular to explain objective particulars by appealing to their mind-independent status while mind-independence is explained by a particular's possession of objective properties. (19) In Newell's terms, "to explain 'things as they really are' by calling them 'objective' accomplishes nothing without a prior grasp of the difference between 'real things' and representations, and if we already have a sense of that distinction then there is no need to invoke the notion of objectivity to provide it." (19)

Another problem that is connected to the previous point has to do with identifying objectivity with the correct description of the world *as it is* without considering the ways in which we can achieve such a description. If we were to *sincerely* consider the scope of our methods in trying to reach such a description, we would realize that they are not sufficient to yield such descriptions, and hence we would acknowledge that *we could not* achieve such a description. In other words, given the evidence that we are limited beings, and that our methods fail to fully capture the world *as it is*, it is most rational to suspend judgment about (if not deny) correct descriptions of the world *as it is*. Insisting on talking about the world *as it is* rests on the delusion that we possess what Richard Rorty calls "a God's eye view".

In bringing up the problems surrounding the identification of ontological objectivity with correspondence to a mind-independent world, I do not mean to dismiss the notion of ontological objectivity all together. On the contrary, I claim that ontological objectivity is essential for the intelligibility of epistemic objectivity. Nevertheless, I believe ontological objectivity, as referring to depicting "what there is" should be reformulated in a way which removes its association with the true description of the world

as it is in itself. Here, needless to say, I distinguish between identifying ontological objectivity with the true description of the world *as it is in itself* and identifying it with correct descriptions of the way(s) the world is. This is an important difference, because, as I have discussed above, the former view considerably narrows the scope of objectivity, and it also assumes a God's eye view. Moreover, in contrast to the latter view which could accommodate the plurality of the world, the former view presupposes a monolithic world. Acceptance of a monolithic world would diminish the range of possible objects for epistemic inquiry. This limitation in turn would result in a misapprehension of phenomena and in partial accounts that might have negative physical/psychological/moral/social effects on certain people's lives.

In talking about *the world in itself*, it is often assumed that the independent existence of facts entails that there is and can be only one true description of the world. However, this assumption is debatable. As I argue above, even facts about so-called mind-independent objects such as mountains could change as a result of natural and/or social occurrences. In such cases, true descriptions of the past become false. This situation implies at least two things: a) that reality (nature, world) is not monolithic, i.e. it changes; b) that whether a description is a correct description of the world or not is relative to the time it is assessed. Obviously, this relativity is not troublesome since what make descriptions true or false are still the changes in the world. The point is this: our confidence in believing that we avoid falling into a dangerous form of relativism in the former time-indexed cases rests on our knowledge of the changes in the world. I have been arguing, as many feminists do, that appealing to examples about concrete

(perceptual) objects, which are clearly observable, in discussions of relativism gives us false clarity and confidence about the issue.[17] Taking the independent world as the *only* means of evaluating the correctness of our descriptions runs into the following problem: in cases where we have to assess descriptions regarding parts of the world that are not immediately accessible to us, or parts of the world we have an impact on shaping, we will lack any means to arrive at a conclusion. Given that scientific explanations or theories are mostly about phenomena that are not immediately observable, it is more plausible to seek *attainable* criteria in assessing our accounts of such cases. If we cannot merely appeal to the world in assessing whether our descriptions capture the way the world is, where else other than to features of a method could we appeal for such assessments? In the absence of a direct check from the world we have to focus on method. We have to examine the meticulousness and diligence as well as the "impartiality" of the inquiry. In other words, we have to consider whether our method achieves epistemic objectivity. This task requires an inspection of all the factors that could affect our descriptions of the world. It is true that this is a highly demanding task, perhaps one that is unachievable. There is always the risk of leaving out certain factors that affect our descriptions, yet are disregarded for various reasons such as ignorance (structural or not) or lack of appropriate conceptual and/or technological tools. As a result "our" acceptance of the way the world is will be relative to the factors that "we" take into account.[18] An important thing to notice is that the

[17] For an elaborate explanation of the problems with appealing to simple perceptual objects as paradigmatic objects of knowledge see Code (1993).

[18] One might object and claim that our acceptance of the way world is different than the way world is. This distinction might be useful in accounting for our false beliefs about

acceptance of such relativity does not entail that the natural world has nothing to do with our claims. What it implies is that in certain cases appealing to the natural world by itself is not sufficient to assess the correctness of our descriptions. It also implies that because considering all factors would be burdensome (if not impossible), and would hinder the course of inquiry, we have to narrow down the context by focusing on the relevant factors that affect our descriptions. Although the issue of determining the relevant factors is complicated, these will ultimately be a function of the topic of the inquiry at hand and our intentions behind describing that subject (i.e. goal of the inquiry). Longino does not deny that the natural world has a role in this process. She maintains that one of the conditions for the publicity of science, which is a prerequisite for achieving objectivity, is that the objects of experience which we describe and about which we reason are purported to exist independently of our seeing and thinking about them. (Longino 1990, 70) This condition implies that we cannot describe the objects as we wish because they have independent existence, i.e. their features are shaped by the way the world is. Because there are various ways (different point of views) of approaching the world, the way(s) the world is accommodates diverse correct descriptions of the same phenomenon. What Longino emphasizes is that the process of deciding which phenomenon to describe, from which point of view and how to approach that phenomenon, and how to assess our descriptions of the phenomenon is carried out within an epistemic community by appealing to its

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the way world is. Yet it is important to emphasize that we can remark about our false beliefs only after "we" become aware of them. In practice, it is not intelligible to talk about the way world is apart from our acceptance of it. In other words, the distinction presupposes a God's eye view which is not attainable by us *humans*.

established standards.

For many thinkers, if we deny that the natural world imposes a single true account of the world itself, and that it is the only constraining factor for our beliefs, then anything goes. Nevertheless, a plurality of accounts does not mean that one can construe phenomena just as one wishes. This is because these accounts are usually constrained by the way world is as well as by the standards operating within an epistemic community. Thus it is not the case that anything goes. It might be argued that since epistemic communities themselves are contingent, our assessments would still not guarantee achieving correct ("true") accounts of the world. I think Crasnow's criticism that Longino's account of objectivity does not take us beyond intersubjectivity is an extension of this objection. But the objection itself rests on the problematic assumptions that the correct account of the world corresponds to *the world as it is*, and that we have direct access to it. Moreover, the contingency of epistemic communities does not mean that their worldviews are arbitrary. A worldview is (often) a coherent set of beliefs. Yet it may not be static. It is a complex web of beliefs which evolves through time in a dialectical fashion. It is a function of the interactions between a community and its surroundings—both social and natural. A discursively dangerous relativism, I think, would be the claim that *everything being equal* there can be multiple and incompatible true accounts of the same phenomenon. However, in the diversity of worldviews, things are never equal. The question then should be which account to take into consideration rather than which account is the "true" account. The answer to this question will inevitably be a function of epistemic as well as ethical, social and political negotiations. Hence, the decision as to

which description of the world to accept and work with is never arbitrary. Since the epistemic and the social values embedded in our decisions are in principle traceable (given enough effort and commitment) "we" can detect biases and idiosyncrasies that prevail in what "we" accept as the way the world is. According to Longino the more diverse points of view are included in this "we" the better chance for "us" to arrive at more "objective" accounts.

In sum, we cannot assess whether an account achieves ontological objectivity (that is, captures the way the world is) apart from considering whether it achieves epistemological objectivity. This however, is not to claim that ontological objectivity is redundant.

5.2.3.1 Ontological Objectivity is Significant

So far I have focused on how epistemological objectivity is required for ontological objectivity. But there is another important aspect to the relationship between the two: ontological objectivity is also required for epistemological objectivity. It is fair to claim that the talk of a method conceptually entails that there is a goal to be achieved. In the case of epistemological objectivity, the aim of pursuing an objective method is to achieve a "correct" description of the world. But what does a "correct" description of the world amount to? No doubt this is a very complicated question the answer of which would differ depending on the endorsed sense of truth, and the relation between correctness and impartiality which has connotations of fairness and justness. For an objectivist view the correct description of the world depicts the world as it is, where the world as it is obtains

independently of us. In this understanding, the world as it is—problematically—refers to the "objective" world. Furthermore, an objectivist epistemology rests on a rigid separation between the object of a study and its methodology. As I discuss above, the ultimate problem that leads to these mistaken assumptions is the objectivists' belief in the possibility of achieving a God's eye view. It is because of this illusion that an objectivist epistemology fails to notice the mutual dependency of epistemology and ontology, and it dismisses any possible epistemic input of discussions of the "fairness" or "justness" of our descriptions.

Nevertheless, once we let go of playing God and start seeking an epistemology with a "human face" then we can come to see that epistemology and ontology are mutually dependent. And we can reasonably maintain that an objective inquiry aims at capturing "what there is" or "the way(s) the world is" accurately without invoking the idea of a "world as it is in itself". In this understanding, while a scientific method guides us to capture correctly (to a greater or a lesser extent) "what there is" and "the way(s) the world" is, our commitments and concerns about "what there is" or "the way(s) the world is" outline our methodology. Of course what falls under "what there is" and "the way(s) the world is" is debatable. Since our topic is science (i.e. the systematic study of the structure and behavior of the *physical, natural and/or social world*) it no doubt has some reference to the world out there. Yet we can still question i) whether it consists of objects or states of affairs in the world; ii) whether the nature and/or composition of objects or states of affairs have any bearing on "what there is". These are complex ontological questions, answers to which have direct consequences for the endorsed epistemology. An

elaborate examination of these long-standing ontological questions would fall outside the scope of this dissertation. But I would like to emphasize the fluidity of "what there is" by referring back to Daston's historical analysis of atlas making.

In the first chapter we saw that Daston defines ontological objectivity as the ideal of seeking the ultimate structure of reality. What the ultimate structure of reality consists of, however, is far from clear. For instance, in the second chapter I mentioned that for scientific realists the ultimate structure of reality refers to the primary qualities of objects, and argued that identifying objectivity with accounts of primary qualities will narrow down the sense of objectivity considerably. For Daston the ideal of seeking the ultimate structure of reality goes hand in hand with the ideal of being 'true to nature'. Yet, in the course of atlas making the task of being true to nature proved to be difficult. Recall from the first chapter that in illustrating 'what truly is', atlas makers were compelled to make ontological and aesthetic judgments. This task required reducing the variety and multiplicity of nature into manageable pieces (specimens). This reduction in turn demanded decisions about the selection of which phenomena to observe and from which point of view to observe them. Daston mentions two schools of thought for accomplishing the ideal of being true to nature: The first school of thought, being committed to the existence of ideals, advocated representing *ideals* in order to be "true to nature". In turn, they sought the "best examples" of one species, and abstracted and illustrated ideal features of these examples. The second school of thought, on the other hand, advocated representing *individuals* as they are seen in order to be "true to nature." They were committed to the existence of particulars. Consequently, the illustrations (descriptions) of

the "same" species drawn by these schools of thought differed drastically. This historical case is a good example for showing how ontological commitments affect the ways in which phenomena are represented. In other words, commitments about "what there is" affect the method of representing what is true to nature.

5.3 Conclusion

Crasnow claims that Longino separates ontological objectivity and epistemic objectivity, and that by not accounting for how ontological objectivity is achieved, her position falls into a vicious relativism. I have argued that we cannot talk about epistemic objectivity and ontological objectivity apart from one another. Such talk rests on the ungrounded assumption that we can achieve a God's eye view. Longino denies this assumption. Moreover, as an empiricist she subscribes to the view that our knowledge depends on our experience of the world. Hence there is no doubt that the methodology she puts forward ultimately aims to capture how the world is, even though she rejects talk of the world as it is in itself. It is true that in a contextualist account it is more complicated to account for how the world shapes or constrains our beliefs, as there are other factors that come into play in determining the legitimacy of our beliefs. But it is also difficult for a non-contextualist account to explain how exactly the world constrains our beliefs. After all, hasn't it been the ultimate problem of empiricism since the classical empiricists that the very principle that the programme rests on cannot be grounded empirically? In truth, whatever we might say about how exactly nature constrains our beliefs would be speculative. I think it is not fair to dismiss Longino's account on the grounds that she

does not show how nature constrains our beliefs. Her account is valuable in its elaborate attempt to open up new avenues for thinking about the actual workings of science and to prescribe principles to achieve more complete and adequate as well as fair and just knowledge. Such a systematic analysis improves our understanding of science and its concepts.

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