THE ROLE OF VALUES IN THEORY CHOICE: DEBATING BIOLOGICAL RACE

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ABSTRACT

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Can so-called non-cognitive values be used legitimately in good science? I argue that they have a legitimate role in several areas of science, namely the context of discovery, the context of investigation, and the context of application, but that they do not have a legitimate role in the area of scientific theory assessment. In this area they ought to be avoided because their use in this area makes for bad science. I then use the debate about whether or not race is a biological concept as an application and case study to illustrate the use of the values which ought to be used for assessing theories, cognitive values. I conclude that biological race is not real and we can make this judgment without using non-cognitive values.

In the introduction I clarify various issues involved in the values debate. I then explain why I believe that the disagreement regarding whether or not race is biologically real makes for a helpful case study when considering the role of values in science. I show that the disagreement is often seen as being motivated by various non-cognitive values; that is to say, each side claims the other is being inappropriately influenced by non-cognitive values.

In Chapter 1 I lay out the various arguments for and against whether ancient civilizations held to the concept of race and show that there are problems with both sides of the issue and thus, neither side can say that the fact that the ancients believe one way or the other helps their case, as is often claimed. In Chapter 2 I critically examine some of the major theories of race and suggest that they do not work. I conclude that while we can find all kinds of differences between groups these differences do not seem to allow us to make any meaningful biological distinctions.

I then raise the question again whether or not non-cognitive values have influenced the race debate.

In Chapter 3 I work through an account of how non-cognitive values function in various areas of science and argue that in good science non-cognitive values ought to be avoided in the context of assessment. Chapter 4 lays out an account of why cognitive, what I call epistemic and non-epistemic values, are to be used to assess theory choice. I argue that other so-called non-cognitive values for instance, ideological, psychological, ethical values, etc., ought not to be used. I argue that only those values that aid in justifying or directly assessing beliefs as true (epistemic values) or those that aid in our understanding and explanation (non-epistemic cognitive values) ought to be used to judge a theory as good, and non-cognitive values do not do this. Finally, I suggest that the lack of consensus on the biological reality of race may be due to different approaches motivated by different research programs and not necessarily the inappropriate influence of non-cognitive values.

Chapter 5 attempts to apply the notion of the appropriate use of values in scientific assessment. I make a distinction between the question, "What is a biological race?" and the question, "Do biological races exist?" I maintain that these are two different questions and result in different metaphysical assumptions about the existence of race, and thus lead to different research programs. I pose two problems facing those in research programs that assume and posit the reality of biological race, a classification problem, which seems to cause the notion of race to fail what I call the non-epistemic cognitive values test, and a sorting problem, which seems to cause the notion of race to fail what I call the epistemic values test. I maintain that as a result of these problems race is not biologically real.

To Verma,
Who for me is beyond value.

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INTRODUCTION

Anthropology has been called the study of human races.[1] In 2001 and 2002, Ann Morning interviewed over 40 university professors in biology and anthropology about their definitions of the term "race." Almost 40 percent described races as groups of people who share certain innate, inherited biological traits. In contrast, over 60 percent argued that races do not correspond to patterns of human biological variations (Morning 2006). This implies that there is no scientific consensus in this important area. George Gill suggests that, "slightly over half of all biological/physical anthropologists today believe in the traditional view that human races are biologically valid and real." He believes that those that do not are biased. He states:

> Why this bias from the "race denial" faction? This bias seems to stem largely from socio-political motivation and not science at all. For the time being at least, the people in "race denial" are in "reality denial" as well. Their motivation (a positive one) is that they have come to believe that the race concept is socially dangerous. In other words, they have convinced themselves that race promotes racism. Therefore, they have pushed the politically correct agenda that human races are not biologically real, no matter what the evidence (Gill 2000).

In a 1985 survey of some 365 biological anthropologists the question of whether or not biological races existed in Homo sapiens was answered yes by 181 of them and no by 148 of them (Lieberman and Reynolds 1996). An earlier study by the same authors indicated that the position that physical anthropologists take on this issue tends to correlate with their social status and cultural background (Lieberman and Reynolds 1978). Specifically, this earlier study tested

[1] Paul Topinard is considered the first to write a general text on anthropology in 1876. His definition indicated it is, "the branch of natural history which treats of man and the races of man" (Smedley 1999).

the assumption that those with greater privilege and less social marginality, labeled "overdogs," would be more likely to accept the dominant view on the existence of races which prevails in the larger American society and those who experienced less privilege and more social marginality, labeled "underdogs," would be more likely to question the outlook which prevails in the larger society. It was found that the overdogs were more likely to hold that races do exist (the dominant view in society) and those labeled as underdogs were more likely to hold that races do not exist. The above suggests that there may be some correlation between social position, the social values that are held at various social positions, and that these things work together to effect theory choice.

This highlights at least two issues independent of the "side" that Gill takes. First, the question of whether or not human race is a biological concept does not seem to be settled by scientists and it is worth trying to understand why. Second, according to Gill and the earlier Lieberman and Reynolds study, perhaps non-cognitive, i.e., social values influence the opinion of scientists on this issue.

This raises further questions: Do these correlations between views and values indicate that something has gone wrong with respect to the science here? Should anthropologists and other scientists check their values and political views at the door when they do science, in order to remain objective? Or should they quite explicitly not do this because of the importance of the values-issues at stake?

The role that various values play in theory choice is an important and much debated issue in the philosophy of science, and thus, the issues raised above lead to a broader set of issues of more general significance: Do non-cognitive social, political, even personal values have a function in scientific theory choice? If so, what function do they have? Ought non-cognitive

values (as opposed to cognitive values, like explanatory power or predictive ability), to be used to determine theory choice?

The Question of Values

Traditional objectives of the philosophy of science include the justification of science's claim to objective knowledge and the explanation of its empirical success. Logical empiricism sought to do this by giving a special role to formal logic in the philosophy of science and an appeal to observation as the basis of objectivity in science or at least some special set of statements which have a close tie to observation, and thus free science from biases.[2] These attempts were already beginning to show all the signs of their eventual failure from their own internal problems when the ideas of Kuhnian historicism came on the scene.

Kuhn's historicism is a view that approached questions in the philosophy of science by looking at the social structure of science and the social and psychological mechanisms underlying scientific change. In regard to theory choice logical empiricism made a distinction between the "context of discovery" and the "context of justification" and had dismissed social notions as being part of the "context of discovery" and thus not logically analyzable. The logical empiricists thought that there was a reasonably clear distinction between epistemic and social values. Science, it was believed, should be guided only by cognitive values, and the logical empiricists believed that this was the case (Reichenbach 1951). However, Kuhn and others retold classic cases of great advances in science with an eye to exposing the role of social values rather than relying on the impotence of the logical empiricists' epistemic values to decide which theory was right. An attempt was made to show that social values played an important role in scientific

[2] See Ayer, A.J. (ed.) 1959. Logical Positivism. New York: The Free Press, for various articles written by the Logical Positivists.

change and theory evaluation. Observations were seen as theory-laden and theories across paradigms were seen as incommensurable. Science was seen as a form of organized behavior with a specific social structure. Kuhn's argument in this area can be summarized as something like this: if theories are underdetermined by evidence, then something else is contributing to theory choice. He suggested that perhaps cognitive values were being used and thus, this would allow us to hold on to our traditional picture of an objective science. But, he argued, these values do not seem to be sufficient to justify theory choice. These values seem to be too imprecise, or the use of them inconsistent, or theories that rely on them incommensurable. Therefore, theory choice seems to be based on subjective preferences that allow for social, political or personal values to enter into theory choice and thus perhaps bias it in some way. Kuhn argued that theory evaluation was judgmental rather than algorithmic, as the logical empiricists had thought (Kuhn 1977).

Kuhn's ideas came in for quite a bit of criticism; however, they were very influential in promoting a more social basis for science. For instance, feminist critiques of science have embraced some of Kuhn's ideas. They have argued that some theories, especially in biology, contain male bias. These theories are considered influenced by personal, social or political values and thus are androcentric. Sandra Harding 1986 points out that there are some rather different perspectives one can take here. She has proposed classifying feminist criticisms of science into three categories: feminist empiricism, standpoint epistemologies, and feminist postmodernism. Feminist empiricism regards science as essentially objective; however, when its methods and rules are not followed it results in the influence of social, political and gender biases leading to "bad science." Standpoint epistemologists maintain that contextual (i.e., non-cognitive) values are essential to science and that some are better than others. Feminist post modernism abandons

objectivity and maintains that different standpoints tell different stories about the world and none are any better than any other (Harding 1986).

The "strong program" in sociology of science also embraced Kuhn's ideas. These sociologists and other social scientists sought to study the close details of scientific work and concluded that scientific agreement was "constructed" via negotiation between people whose main interests may not be in describing the way the world works. It is argued that non-cognitive social values to a large extent determine the results of science and that scientific knowledge is not the result of the relations between scientific theories and the structure of the world but is a social fabrication (Latour and Woolgar 2003).

While most philosophers of science do not believe that the "strong program" has it right, they do believe that in various ways social and non-cognitive values play a part in theory choice. What part these values play in theory choice is an issue of much disagreement. For instance, some argue that social values influence the formation of research agendas and perspectives taken or the range of experiments deemed relevant or the use to which research results are put, but *not* the actual findings of science (Nelson 1996). Others claim that if non-cognitive values determine scientific theories then they influence which theories become contenders and thus the leading program is in part the result of these values and thus inevitably influenced by them (Okruhlik 1994).

While much of the dispute has been whether epistemic values alone can suffice to guide science, a rather recent line of thought (especially, with Longino 1990) is to argue that there really isn't any way to draw a sharp epistemic/social values distinction. The idea here is that scientific objectivity can be properly understood only in social terms.

One of the goals of this dissertation is to examine these issues and see if there is a way to develop a strategy for choosing between theories, hypotheses and models that considers the role of these various values while still achieving an objective connection to the world. Is some form of the epistemic/social value distinction even tenable? And even if social (non-cognitive) values influence the decisions that actual scientists make, do they only contribute biasing factors to theory assessment or do they aid in some type of objectivity?

James R. Brown 2001 has laid out what seems to be a helpful way to conceptualize this issue. I would suspect there are several more positions that could be taken regarding these issues, for instance, each of the areas Harding has identified but it would take us too afar field to consider all of them. Brown identifies four attitudes towards the question of epistemic and social values. He labels the distinction as that between cognitive and non-cognitive values:

1. There are no non-cognitive values at work in science.
2. All of science is impregnated with non-cognitive values; there is no objective distinction to be made between good science and bad.
3. Non-cognitive values sometimes play a role in science, but they invariably lead to bad science; good science is free of non-cognitive values.
4. There is an important distinction to be made between good science and bad science, though both are shot through with non-cognitive values.

Most philosophers of science would dismiss (1). It does seem that non-cognitive values do play some role in science as I will show below. Also, I believe that most would dismiss (2). This is the position of the "strong program" in the sociology of science and there does seem to be some objective distinction between good and bad science.

Position (3) is close to the traditional position and to what Harding labeled as feminist empiricism and (4) is close to positions such as Longino's. I prefer a position somewhere between (3) and (4) and will attempt to explain and defend it. I will argue that non-cognitive values play a legitimate role in several areas of science. However, in the assessment of hypotheses and theories they lead to bad science and scientists ought to work to rid science of non-cognitive values in this area.

I believe the debate regarding whether or not human race is a biological reality makes a helpful case study to aid in examining the role of values in theory choice. This is because it does not seem that scientists have come to a general consensus regarding the reality of biological race and it has been claimed, as was seen above, and seems plausible that the disagreement may be motivated by various non-cognitive values.

There are several questions that I want to consider regarding race. First, what do we mean by race? There are various views on what race is. For instance, many view race as only a social construct. If this is the case then the debate about whether race is a biological concept or not is over. So I will lay out the various views on race and give some reasons why it is important to consider whether or not race is a biological concept.

Second, I want to ask the question, "Has race always been recognized?" That is, did ancient civilizations hold to the view that biological races exist? Many that argue that race is only a social construct also argue that the view that race is a biological entity is a modern concept and that it came into being during the age of discovery (Montagu 1974, Smedley 1999, Zack 2002). On this view biological race was a justification of the different treatment of those who were, for instance, in slavery, where enlightenment ideas did not seem to allow for this since these ideas held to human equality. While whether the ancients held to a biological concept

of race or not will not ultimately settle the debate about the reality of biological race, it stands to reason that if the ancients did hold to a biological concept of race, one who holds that races have no biological reality and only recently came into existence because of social and political reasons would need an explanation of why it seemed to exist in ancient times, that is, why it is not recent as they claim. Further the establishing of the existence of a biological notion of race by the ancients would seem to allow the questioning of the claim by those who deny its existence that since race is recent it is a social, not a biological concept.

After considering the ancient views I examine modern theories of race. I will consider two approaches to the notion of biological race, one based on the typological concept of race and one based on the populationist concept of race. I will take a critical look at these various theories of biological race showing the strength and particularly the weaknesses of each. This will allow us to see that no theory of biological race seems to be able to do the job, that is, give a definitive answer to the question of the reality of biological race. No theory gives a satisfactory account of what a race is; all seem to fall short and have various problems. If the various theories of biological race fail then that leaves open the question, does biological race even exist. This question we will take up in Chapter 5.

The discussion about the various theories of biological race will allow us to think through the issues**.** I will argue that non-cognitive values play an important role in science. They function in various scientific contexts. There is the context of discovery, the context of investigation, the context of application, and the context of justification (which I will call the context of assessment). I will argue that non-cognitive values play a legitimate role in the context of discovery, investigation, and application but not in the context of theory assessment.

How theories ought to be assessed seems to rule out a legitimate role for non-cognitive values. However, first, I want to give a fuller account of why race is a helpful issue to consider.

The Question of Race

As I have indicated I believe race and various issues surrounding race can be a helpful case study to aid in examining the role of values in theory choice. There are several reasons for this; first, the issue of race is controversial, and second, the proponents and opponents of race as a biological reality seem to use the same data to argue to different conclusions, this may be a result of their values, and finally, arguments for the social construction of race imply that the position is justified for social and political, that is, non-cognitive reasons.

Race is Controversial

First, the issue of race is controversial and highly subject to the influences of non-cognitive values. This is the case for several reasons, they include: emotional attachment issues, the social consequences involved, opponents' perceptions of bias in those holding an alternative position and thus as being motivated by social reasons, and for historical reasons.

Emotional attachment issues: Each person seems to have some emotional attachment to what he or she perceives as his or her race. Gossett remarks that:

> There could hardly be a subject more likely to involve prejudice than that of race. Unlike religious, political, or social ideas, human differences which we have elected to call racial differences are a part of our physical endowment which we are born with and cannot change. All of us belong to one race or another or to a combination of races, and thus all of us are involved to some extent in an emotional attachment to the idea that our own

> race is at least potentially equal to others. Much debate over the merits and defects of race has taken place in a peevish and ill-tempered atmosphere, one in which the opponents frequently "get personal" and tell members of other races home truths about themselves (1963, 409 – 410).

Emotional attachment and "getting personal" can lead to failure of objectivity. While emotional attachment to a view may allow for a high degree of motivation, scientists can also miss important evidence because of attempts to hold on to a position for personal reasons when it is no longer warranted. Thus, it can be argued that emotional attachment when it leads to this type of error can be viewed as a psychological value that is not part of "good" science.

The social consequences: Also, there is a difference in social consequences between the results of holding that a race exists or not as opposed to coming to conclusions about other subjects. Unlike more impersonal subjects like particle physics for instance, race is controversial because it bears more directly on our self-image and our view of human nature (Brown 2001). Domains such as particle physics and the like, usually deal with issues remote from our lives and personal interest. What the inherent character of a subatomic particle (spin, charm) is and the inherent character of a person of a particular "race" is, arguably results in different social consequences. Thus, there is a case to be made for the position that social values ought to be brought in that is, they need to be taken seriously.

Domains like biology and in particular anthropology seem particularly vulnerable to social and political value influence. Since humans are both subjects and objects, classification of humans seems inevitably a social issue as well as a biological issue, and therefore the recognized categories have power to validate inequalities and injustices which are irrelevant to say, the study of fruit-flies (Marks 1995). In discussing eugenics Marks indicates:

> In humans, the biological differences among human groups reinforce the social divisions that may also exist. If all social groups received equal treatment-had equal rights and equal opportunities for advancement-the study of the biological differences among them would be straightforward. It is not so straightforward, however, since the differential treatment often accorded to different groups can find a validation in the biological differences that may accompany them. In other words, locating constitutional differences among human groups can provide a justification for treating those groups differently (1995, 101-102).

Thus, racial classification seems more susceptible to the influences of social and other non-cognitive values because of the social consequences that may apply to various racial designations.

Opponents' perception of bias: Race is also controversial because opponents perceive the reasons for holding an alternative position as being motivated by social reasons and thus admit bias. For instance, the issue of intelligence differences between races seems to fit this bill. In 1981 Stephen Jay Gould's highly influential and popular book (winner of the National Book Critics Circle Award) *The Mismeasure of Man* was published. In it Gould argues that in the past two hundred years the history of intelligence measuring has been characterized by two fallacies, what he calls the fallacy of "reification", which is the tendency to convert abstract concepts into entities, and the fallacy of "ranking", which is the propensity for ordering complex variation as a gradual ascending scale. He maintains that reification allowed for intelligence to become an entity located in the brain and that ranking allowed for assigning all individuals their proper status in the single series by assigning an objective number which in the twentieth century was represented by their IQ (intelligent quotient). Gould argued that scientists were influenced by

social and cultural prejudice leading them "to invalid conclusions from adequate data, or distorting the gathering of data itself" and "that determinist arguments for ranking people according to a single scale of intelligence, no matter how numerically sophisticated, have recorded little more than social prejudice" (Gould 1981). That is, social and cultural values lead to these conclusions.

Neven Sesardic is not impressed with *The Mismeasure of Man* and in his book *Making Sense of Heritability* says that others were not either. He indicates that in contrast to typically favorable and laudatory comments in the popular press, the reviews in Nature, Science and other professional journals were highly negative and severely critical. He quotes a portion of Blinkhorn's review in the Journal *Nature*:

> With a glittering prose style and as honestly held a set of prejudices as you could hope to meet in a day's crusading, S. J. Gould presents his attempt at identifying the fatal flaw in the theory and measurement of intelligence. Of course, everyone knows there must be a fatal flaw, but so far reports of its discovery have been consistently premature… [The Mismeasure of Man] is a book which exemplifies its own thesis [that science is necessarily influenced by social prejudices]. It is a masterpiece of propaganda, researched in the service of a point of view rather than written from a fund of knowledge (2005, 38).

Sesardic goes on to claim that the important point is that Gould's central argument against hereditarians is based on a misunderstanding of the position he is criticizing. However, for our purposes we see here that both Gould and his reviewer seek to indicate that social values drive the positions that they are criticizing.

Another example might make the point clearer. J. Philippe Rushton in his book *Race, Evolution, and Behavior* maintains that race is biologically real. He indicates that from his twenty years of study he has found that "in brain size, intelligence, sexual behavior, fertility, personality, maturation, life span, crime and family stability, Orientals fall at one end of the spectrum, Blacks fall at the other end, and Whites fall in between" (Rushton 2000). Yet in his review of Rushton's book Armelagos argues "after 250 years on which race has been the subject of intense study, and even with the advances in molecular biology made in the last 40 years, race as a viable biological unit of study still eludes us. Except for the study of gene flow, race is dead as a scientific method for understanding human variation" (Armelegos 1995).

One can ask how race is dead if Rushton found so much evidence for it. Rushton seems to answer this when he says, "data on race differences reviewed in this [his] book and the evolutionary models proposed to explain them conflict with what has become known as "political correctness," a mindset that subordinates knowledge and inquiry to ideological discipline about social equality. Presenting misinformation, and the deliberate withholding of evidence, have become all too characteristic of even evolutionary scientists when they write about race." (Rushton 2000)

So Armelegos claims that race is dead as a scientific method for understanding human variation, yet Rushton maintains that race is very much alive and the reason that his views are rejected is due to "political correctness," that is, non-cognitive, political and social values entering into science.

Others who hold to the biological reality of race, as Ruston does, argue in a similar vein. Thus, Sarich and Miele, while putting the idea less pointedly, make a similar claim: "the apparent intractability of America's 'race problem' (or what some would say is more accurately

described as ‘problem with racism’) have led many individuals, perhaps even a majority, in the media and the social sciences to come to the sincere belief that eliminating the word ‘race,’ or downgrading it from a biological concept to merely a social construction, is a necessary if not sufficient condition for eliminating racism as well” (2004, 2). George Gill complains, “At the beginning of the 21st century, even as a majority of biological anthropologists favor the reality of the race perspective, not one introductory textbook of physical anthropology even presents that perspective as a possibility. In a case as flagrant as this, we are not dealing with science but rather with blatant, politically motivated censorship” (Gill 2004). And Michael Levin quips, “Ironically, denial of the reality of race often prefaces a denunciation of race bias, with little explanation given of how people can respond to a trait that no one possesses and no one understands” (Levin 1997). Levin goes on to say, “often hereditarianism is rejected not on evidential but moral grounds, because it is bad, or ‘racist’.” Many of these writers see motivations other than scientific considerations, for instance social consequences, overriding what may be clearly seen by them, that biological race is real. Thus, the issue of race is controversial because of the perceived reasons for holding an alternative position by its opponents – “they” are bias.

Historical reasons: Finally, the issues surrounding race are controversial because of historical reasons. Historically, the classification of races has been associated with hierarchical ranking, eugenics, the justification of genocide, and other social ills. For instance, in the United States and other parts of the world race was used as a justification for slavery. Blacks that were brought to the United States from West Africa as slaves were viewed as a different race than whites; they had different features and characteristics that allowed for their subjugation. In the Dred Scott decision of 1857, which helped spark the America Civil War, Chief Justice Roger

Taney wrote that the treatment of Africans in America was justified because they were "beings of inferior order" (Olson 2002, 181). Montagu quotes an anonymous source who indicates:

> The Negroes were not beasts, but neither were they quite men. They added another link to the Great Chain of Being, strengthening it. They were designed to be subordinates, with faculties proper to their station. They were meant to hew and delve, and the gentleman and his lady need have no qualms as their voices floated in from the fields or up from the kitchen. The British public could go merrily ahead rattling the coins in a full pocket and relishing the taste of sugar…. Were the slaves to be released and hoisted out of their rank, they would quickly drop back again to where they belonged. They simply lacked the innate abilities of the whites (1974, 20-21).

This history has made the subject of race highly controversial. For instance, in 1964 Congress passed the Civil Rights Act, which banned all forms of racial discrimination. However, it was soon noticed that the elimination of discriminatory laws was not producing the fully integrated society that leaders of the civil rights movement had envisioned. Thus, in 1965 President Johnson issued Executive Order 11246 in which the Department of Labor was enjoined to issue government contracts with construction companies on the basis of race. He called this affirmative action toward equality (Pojman 2006). Affirmative action can be defined as an "effort to rectify past injustice as well as to produce a situation closer to the ideal of equal opportunity by special policies" (Pojman 2006, 619). Affirmation action is controversial because it can be viewed as reverse discrimination, that is, discrimination on one race in the present for discrimination toward another race in the past. This implies the existence of identifiable races both in the past and present. But, why should present individuals be punished for past acts that they did not commit? And if race is not "real" then who was discriminated against in the past

and who ought to be discriminated against in the present? Further, affirmation action is paradoxically race-conscious, since it uses race to bring about a society which is not supposed to be race-conscious (Pojman 2006). So the issue of race is controversial because of the various past social practices and remains so even when there is an attempt to address those practices.

These four reasons then, emotional attachment issues, the social consequences involved, the perceptions by opponents of the reasons for holding an alternative position, and historical issues make the study of race controversial and seems to allow for non-cognitive values to influence various positions on race. Hence, issues regarding race can be helpful in the discussion about the role of values in theory choice.

All Sides use the Same Facts

The second reason I believe that issues regarding race can be helpful in the discussion about the role of values in theory choice is because it seems that the very ideas that one would expect to be used in providing a case for the existence of biological race have been used to show that biological race does not exist. All sides to the modern debate seem to use similar ideas and evidence yet interpret them in different ways. Cartmill 1998 indicates that to a surprising extent, physical anthropologists in both camps make similar assertions, cite similar sources, and express similar fervent opposition to racist practices and beliefs. The difference between them is mainly one of emphasis. The findings that one group admits grudgingly and seeks out reasons for disregarding are spotlighted by the other group as the central facts that reveal the way things really are. For instance, in a 1995 issue of *Evolutionary Anthropology*, two physical anthropologists (Armelagos 1995; Harpending 1995) were asked to provide separate reviews of the same four books (Shipman's *Evolution of Racism*; Murray and Herrnstein's *The Bell Curve*;

Rushton's *Race, Evolution, and Behavior*; Mark's *Human Biodiversity*) dealing with issues of race and human genetic diversity. Each reviewer praised the very books that the other condemned (Cartmill 1998). Thus, one could come to the conclusion that aspects other than physical facts are driving the debate, i.e. social or political values. And then one can ask ought these values to be used to drive the debate or is it illegitimate for them to do so because it makes for "bad science". If it is illegitimate to allow these values to drive the debate then one can ask what happens when there is an attempt to eliminate them. Are they able to be eliminated? And if they are not does this mean that "bad science" is all we can have in this debate. That is, are illegitimate values so deep, so entrenched in this debate that the issues are not able to be resolved satisfactorily, i.e., scientifically?

Race as a Social Construction

The third reason I believe that issues regarding race can be helpful in the discussion about the role of values in theory choice is that many have argued that race is a social construct (Blum 1999, Graves 2002, 2004, Smedley 1999, Root 1998, Zack 2002). These theorists argue negatively, that race is not biologically real, that there is no biologically objective way to define race, it is not a biologically objective category that exist independently of human classifying activities. And they argue positively, that race is constructed socially. It is a social category. We see both the negative and positive aspects in Blum's remarks:

> I want to make it clear that I join the current consensus in regarding *race* as a scientifically invalid concept, and one that is misleading in ordinary discourse because it tends to imply the validity of some scientific notion of *race*. However, the notion of a racial group is meant to denote a group whose historical and social experience is/has been

> shared by being regarded as a "race" and treated as such. This definition confers a certain social and historical reality on the notion of *race*, but makes it clear that such a concept can be understood only as a socially constructed one, not a biologically authentic one (Blum 1999, 261).

Note that Blum indicates that race exists, however, not biologically but as a social construct. It is "real" but it has a social and historical reality. So Blum can both deny that race exists but affirm that race is real. For him there is no paradox because two different notions of race are being considered, a biological one and a social one.

Thus, social construction arguments for race seem to allow for the existence of race to be based on what we, as social agents, believe and not on nature. That is, these theorists view race as dependent on us, not independent of us like, water, trees or flowers. Thus, race exists because we believe it exists. For these theorists race is real for social reasons, and thus it would seem that at least some non-cognitive reasons are part of the justification for it.

So, since race is controversial, and the proponents and opponents of race as a biological reality seem to use the same data to argue to different conclusions, and finally, because arguments for the social construction of race imply that the position is justified for social and political reasons, it seems that a case study about race and the questions surrounding its biological reality will provide a helpful way to consider the role various values play regarding theory choice.

Values and Race

As has been indicated, there are at least four major areas in science where non-cognitive values may influence scientific theory choice. These various areas can be seen as various

scientific contexts. First, there is the context of discovery. The context of discovery can be considered the theory generation phase of science. Theories can be generated in any number of ways including through dreams, political ideologies, and even religious views. The source of a theory however, is generally not considered an indicator of it truthfulness, explanatory power, usefulness, etc. It can be argued though that if non-cognitive values influence theory generation in the context of discovery, and the theories that are generated by these influences are the only theories we have, then non-cognitive values must influence theory choice. Applying this to our discussion of biological race, one could argue that if theories of biological race are generated due to racist ideology or if the position that there are no biological races is generated to avoid racist ideology and these are the only theories we have then non-cognitive values must influence theory choice, because these are the only theories to choose from.

Next, there is the context of investigation. In this context non-cognitive values may influence how researchers conduct their work, what observations they may make, how well they appraise the evidence, why they show interest in a subject, and how diligently they pursue it. Value influence in these methodological and motivational aspects is not considered problematic. Why a person shows interest in an area or how diligently they pursue the research is not considered a way to determine why we should choose one theory over another. However, it can be argued, that if non-cognitive values so permeate science in this area then they inevitably influence theory choice. For instance, if the desire to show that biological races do or conversely do not exist influence how researchers conduct their work, what observations they may make, how well they appraise the evidence, and how diligently the subject is pursued, then it seems that these types of non-cognitive values so permeate the research that theory choice is inevitably influenced by these values.

Then there is the context of application. In this context non-cognitive values function as guides to the application of our scientific knowledge in practical decision making. Whenever the application of scientific knowledge has non-epistemic consequences, especially adverse social effects, then we are obligated to refrain from applying that knowledge. However, it can be argued that these types of adverse consequences of a theory allow a role for non-cognitive values to enter into theory choice. A theory ought to be chosen because it has the least adverse practical, social, or perhaps political consequences. So, for instance, a theory about biological race can be chosen because it has the least adverse social consequences.

Finally, there is the context of assessment. As was indicated, the logical positivist made a distinction between the context of discovery and the context of justification. The context of discovery was considered the theory generation phase of science and the context of justification (what I am calling the context of assessment) was where theories were tested, confirmed, and or corroborated. This context served as the means to determine or assess which of the variety of theories that were generated from various sources would be recognized as the one that most matched the world. One way that values may enter into the context of assessment is to recognize that no theory is ever completely confirmed or corroborated, etc., theories are underdetermined by evidence. That is to say, that facts can be used to support more than one theory; two or more theories can be empirically equivalent, and since this is the case room is open to allow non-cognitive values to enter into the assessment process as a way to select between competing theories.

Thus, we see that there seems to be many opportunities for various non-cognitive values to enter into science. And based on this, it can be argued that the various roles that non-cognitive values have in these scientific contexts allow non-cognitive values to influence the choice of

theories.

Arguments for non-cognitive value influence in theory choice include then:

- That theory generation allows for the influence of non-cognitive values in theory choice;
- That in the context of investigation non-cognitive values so pervade science that they inevitably influence theory choice;
- That the consequences of a theory allow a role for non-cognitive values to influence theory choice;
- That the underdetermination of theory by evidence allows a role for non-cognitive values to influence theory choice.

The first two arguments seem to indicate that non-cognitive values have an inevitable influence in theory choice. That is that, non-cognitive values are unavoidable because of scientific methodology. The latter two arguments do not seem to indicate that non-cognitive values are inevitable but that they in fact ought to be used. For instance, the argument regarding consequences indicates that if adverse consequences are seen as a result of accepting a theory then the adverse consequences, be they social, political, etc., ought to be used to determine if that theory should be selected. The underdetermination argument indicates that non-cognitive values are needed to arbitrate between empirical equivalent theories.

I will attempt to show that non-cognitive values do play a legitimate role in science. They are appropriate in the context of discovery, the context of investigation, and the context of application, but they invariably lead to bad science when they are used to assess the merits of various theories in the context of assessment. Good science often is and always ought to be free of non-cognitive values in the assessment of theories. I will conclude with an attempt to reach

some conclusions about the existence or nonexistence of a biological concept of race.

Approach

There are several ways to examine the issue of the biological reality of race. We could give a definition of race and then see if theories of race meet that definition. But there is a problem here. A dictionary definition for instance, can be seen as a claim about the standard meaning of words in a particular language. Thus, if we define race as the way it is used in English, we are assuming that race is real because it is used that way in the language. However, from the history of science we know that there have been many terms that were given very precise definitions that were later discovered not to refer to any entity, i.e., phlogiston, caloric, the electromagnetic ether, etc.

Another problem with definitions is that definitions often are based on assumptions that may have theories built into them and if one does not agree with the underlining theory then the definition will not be accepted. For instance, if one held that skull size and shape determined race, which was implied with the nineteenth century field of anthropometry – the physical measurement of human body form with special emphases on cranial features - then all one has to do is go and measure heads and determine which of several groups of people are different races. But if the theory is not accepted then this "scientific" enterprise does little to convince one that biological race exists.

Sometimes different definitions emphasize different features. Molnar 2001 lists several different definitions with different emphasis from more than fifty years in biology and anthropology. They include the following: Dobzhansky emphasizes incident of genes, "Races are defined as populations differing in the incident of certain genes, but actually exchanging or potentially able to exchange genes across whatever boundaries (usually geographic) separate

them." Mayr emphasizes geographic subdivision, "A subspecies is an aggregate of local populations of species, inhabiting a geographic subdivision of the range of the species, and differing taxonomically from other populations of the species. Brues emphasizes traits, "A race is: a division of a species which differs from other divisions by the frequency with which certain hereditary traits appear among its members. Among these traits are features of external appearance that make it possible to recognize members of different populations by visual inspection with greater accuracy. Members of such a division of a species share ancestry with one another to a greater degree than they share it with individuals of other races. Finally, races are usually associated with particular geographic areas." Montagu substitutes ethnic group for race in order to emphasize the cultural aspects of race, "An ethnic group represents one of a number of populations, comprising the sinble species Homo sapiens, which individually maintain their differences, physical and cultural, by means of isolating mechanisms such as geographic and social barriers. These differences will vary as the power of the geographic and social barriers acting upon the original genetic differences varies." As a result of different theoretical assumptions or the emphasis of different aspects of human variation there seems to be little agreement about a definition.

Even if we start with an acceptable definition there are still problems. For instance, Graves suggest that a biological definition of race is "a population of organisms differing from others of the same species in the frequencies of hereditary traits; a subspecies." He accepts this definition but then argues that when we apply this definition to the human species, we readily arrive at the conclusion that no biologically defined races exist in the human species because if race is defined as a population that has achieved the subspecies level of genetic differentiation, no such divergence currently exists in our species (Graves 2002). Thus, the problem here is that

even though Graves accepts the definition there are components of it that are still problematic for him. How much difference are we to consider? Which traits should be examined? Graves does not believe these types of questions can be answered in a manner that would allow for the existence of race. Definitions must come into play at some point, but often it is best to consider various theories first and then let the specific scientific definition emerge at the end of an investigation.

Another way to examine the issue of race is to catalogue the various positions and then attempt to discover which position fits the biological reality. However, for the layman, at least without some background it would be very difficult to determine which theory fits. It has been argued that when we say that someone understands a particular discipline, among other things, we are saying that she appreciates what constitutes a good reason in that area (McPeck 1981[3]). However, a minimal condition for understanding a good reason in any field is that one understands the meaning of the often specialize and technical language in which the reasons are express, because in deciding upon a question it is usually not the logical validity of an argument that we find difficult but rather the task of determining whether certain premises are in fact true. We frequently cannot determine whether evidence is good or not, because such a judgment depends upon special knowledge. One has to be a "fellow participant" in the particular domain of meaning to appreciate the proper significance of the evidence. With this warning in mind and in an attempt to gain some background and understanding of the terminology, concepts, and

[3] McPeck holds that the way to teach critical thinking is to teach students the epistemology of a subject, which for him is the analysis of good reasons for the various belief claims of the subject. And since these claims are different for different subjects the standard approach of teaching general critical thinking skills is not only wrong but also useless. While I agree that teaching the epistemology of a subject is a good way to assist in learning a subject I do not agree with McPeck that this makes teaching critical thinking in general wrong and useless. We can and do talk about and teach about critical thinking as a subject in its own right. There do seem to be some general principles of critical thinking.

ideas used in the race debate, and thus help us to become more of a "fellow participant" regarding this issue, I will examine the various theories of race. I will in Chapter 2 give a catalogue of the various positions and then attempt to explain why people on one side or the other believe that the various positions work or don't work in terms of the fit of a position to the biological reality. The watching of this dispute and debate will, I hope, give a sense of the nature of the problems and some insight into how they have been attempted to be resolved. But first, in Chapter 1 I will consider whether or not the concept of race is a recent invention or not, then in Chapter 2 I will consider the various theories of race and highlight the most salient problems with each.

Taxonomy of Racial Views

At this point it might be helpful to clarify the distinction between race as a social category and race as a biological notion and to lay out several positions regarding the reality of race. What do we mean by "race"? That is to say, what are we purporting to talk about when we are talking about race? What are we attempting to refer to with the term "race"? There are various ways to answer this question. We could say that what we are purporting to talk about when we talk about race is something we think is real but it is not, it is an illusion as was done when scientists held to the Ptolemaic view of the universe, the caloric view of heat, phlogiston, or when common folks held to belief in witches, warlocks or fairies. On this view race does not exist, there is no such thing as race and when we speak about race we are in error. We can say that on this view race is an illusory conception.

Another view of race talk has it that what we are talking about when we speak of race is a socially constructed reality, as when we speak of money or husbands or the government. On this

view race is not something studied by natural scientists but by social scientists. Thus, race exists, but it exists because we have constructed it, race is a social construct.

Yet another view of race talk has it that when we talk about race we are talking about a biological entity. On this view race is a scientific kind like water or more specifically, a biological category like tiger or mammal. This view holds that races are real biologically, they exist in nature and can be discovered by scientific investigation and are even known by common sense folk notions. This position in its historic form ignored the issue of race as a social reality or in its more contemporary form has rejected the social reality of race.

Or finally, we can combine the latter two ideas about race and understand race as both a social and a biological thing. In this view the social reality of race is built on the biological reality of race. Since race is a biological reality there are social realities that emerge from it that are based on the biological reality.

So when we ask the question does race exist we have several options. We can say no, race is not real, it is an illusion. We can call this racial skepticism. Or we can say yes, race is real and it is either a social construct or a biological concept or some combination of the two. This would be a racial realist response and the various options under this response would be various theories of race. Thus, we need to be clear about what we a proposing when we say we are talking about the reality of race. If we are racial skeptics we would say that we are not talking about anything that exists in the world. If we are racial realists we could be talking about race as a social construct or race as a biological category. Table 1-1 sets out these various positions.

Table 1-1 Views on Race		
Position	**Is Race a Biological Reality**	**Is Race a Social Reality**
Skepticism	No	No
Realism (R)	----------	----------
R1 - Social Reality	No	Yes
R2 - Biological Reality	Yes	No
R3 - Social and Biological Reality	Yes	Yes

The question I want to focus on is whether or not race is a biological category. Does race exist in the natural world? What are the biological notions of race and do they refer to something in nature? I will call someone who holds to the biological reality of race a racial realist and someone who does not a racial skeptic. Although it should be understood that even if race does not exist as a biological entity that does not rule out the existence of race, the option of race as a social entity is still open.

Before we move to considering some theories of race I want to first look at the debate about the views of the ancients. Can ancient views tell us anything about race? I want to ask the question, "Has biological race always been recognized?" That is, did ancient civilizations hold to the view that biological races exist? As mention above, many that argue that race is only a social construct also argue that the view that race is a biological entity is a modern concept, it came into being during the age of discovery. While whether the ancients held to a biological concept of race or not will not ultimately settle the debate both sides, skeptics and realist, claim that the position of the ancients strengthens their argument. Realists can argue that if the ancients did hold to a biological concept of race, then skeptics who hold that biological race is not biologically real and was just a reaction to a social situation that arose out of a conflict of Enlightenment ideals would need an explanation why it seemed to exist to the ancients. Conversely, skeptics can argue that if the ancients did not hold to the existence of biological

race, this fact adds strength to their claim that race is just a recent invention in response to Enlightenment ideals. Also, skeptics can argue that if the ancients did not hold to the existence of biological race, this shows that they were consistent with what science shows today. But realists can say that if the ancients had racial categories that match those produced by today's science, then it would be a miracle to produce such agreement if there was no underlying biological reality. Let's consider this debate.

CHAPTER 1

RACE: MODERN INVENTION OR ANCIENT REALITY

As we consider ancient civilization's views of race we are immediately confronted with controversy. Is the concept of race old or new? Did the ancients hold to some notion of different races? If so, what was it? If not, when did the idea of race come about? How did it begin? If they held to some concept of race did it mean the same to them as it does to us? As might be imagined, theorists are divided on these questions.

Many racial skeptics hold that the notion of race is a relatively new concept while racial realists hold that the concept is as old as the ancients. Thus, Gossett maintains recognition of racial differences is ancient, "what is certain is that the tendency to seize upon physical differences as the badge of innate mental and temperamental difference is not limited to modern times. The racism of ancient history, even though it had no science of biology or anthropology behind it, was real, however difficult it may be for us to judge the extent of its power" (1963, 3). Montagu notes, "A study of the cultures and literatures of mankind, both ancient and recent, shows us that the conception that there are natural or biological races of mankind which differ from one another mentally as well as physically is an idea which was not developed until the latter part of the eighteenth century" (1974, 18) and further, "within any society, in earlier times, men might be persecuted or made the object of discrimination on the grounds of differences in religion, culture, politics, or class, but never on any biological grounds such as are implied in the idea of 'racial' differences" (1974, 21). But Sarich and Miele argue that "early civilizations clearly depicted the distinctive physical features of the major races with which they were familiar. Their literature shows that they also attributed behavioral characteristics (fairly or

unfairly) to the different races and explained them according to the knowledge of their day" (2004, 29).

Each one of these authors and others who hold various positions on the biological reality of race, seem to come to different, in fact, contrary conclusions as they examine ancient societies with regard to the existence of race. Their conclusions more often than not correspond with their conclusion about the existence of race. For instance, Sarich and Miele hold to the existence of biological reality of race, while Montagu does not.

What we want to do in this section is to look at how the facts from history is used to argue for one or the other position. Both sides acknowledge that the ancients recognized physical differences in different groups of people. And both sides agree that different groups treated other groups differently. Racial skeptics argue however, that the recognized physical differences did not amount to racial differences and the different treatment of one group toward another was based largely on social and cultural differences and not biological differences. While racial realists argue that ancient societies' recognition of physical differences was based on race and the difference in treatment of one group toward another was based on biological and not just social and cultural differences. The question is, did the recognition of physical differences amount to the recognition of different races and was the different treatment of one group toward another based on racial differences or only on social and cultural differences.

We will briefly look at some of the various claims and then attempt to assess why and how claims were made and what can be determined. I will first give a summary of the arguments against the ancient belief in races then I will give a summary of the arguments for the ancient belief in races.

Race - Modern Invention

Those who argue against the view that ancient civilizations held to race differences usually hold the position that race was created during the age of discovery when Europeans came in contact with people who were very different from themselves. This position maintains that race was socially constructed at this time due to a number of reasons but foremost that of slavery. "The modern conception of 'race' owes its widespread diffusion to the white man. Wherever he has gone he has carried it with him. The rise of racism is associated with slavery and the growing opposition to it, so that it is not until the second half of the eighteenth century that one begins to encounter its development" (Montagu 1974, 15). Snowden maintains that based on his research:

> Nothing comparable to the virulent color prejudice of modern times existed in the ancient world. This is the view of most scholars who have examined the evidence and who have come to conclusions such as these: the ancients did not fall into the error of biological racism; black skin color was not a sign of inferiority; Greeks and Romans did not establish color as an obstacle to integration in society (1983, 63).

Two arguments are advanced to show that biological race is a modern concept not held by the ancients. First, it is argued that in the ancient world social position was not based on physical features, anyone could and did advance socially and politically. Second, it is maintained that ancient civilizations for the most part did not make distinctions among each other based on biological characteristics. When distinctions were made, the argument goes, it was based on cultural or other non-biological differences. We will briefly consider each claim.

Social position was not based on physical characteristics

Snowden indicates that Blacks, like other foreigners, played an important role in the Egyptian army. "Not all Kushite mercenaries in the Egyptian army were inducted by force: volunteers saw the advantages of a career in the Egyptian army-an avenue to prestige in Egypt" (1983, 39). Kushite was the name given to those with black skin that live south of the Nile delta. As the above indicates, they were able to gain stature and influence in the Egyptian army. History also shows that during the period of the Twenty -fifth Dynasty in Egypt a black skinned Kushite ruled in Egypt (Snowden 1983). Thus, there seemed to be no barrier for social positions in ancient Egypt.

In Greek civilization, we find that there was apparently no relationship between slavery and race. Blacks and whites were slaves and, blacks as captured peoples might be slaves; but there is no indication that they were either more or less suitable than others for this state and we find environmental and cultural explanations for such qualities as courage and military prowess (Gossett 1963). In Rome the various wars produced an enormous number of slaves that encompassed members of many racial groups including those from Syria, Galatia, North Africa, and Gaul (Graves 2002). Thus, it seems that in the ancient world social position was not based on physical characteristics.

The ancients made distinctions based on culture and other notions not biological features

Montagu indicates that for the Egyptians "'the people' were those who lived in Egypt, without distinction of race or color. Once a foreigner came to reside in Egypt, learn to speak the language, and adopted Egyptian dress, he might finally be accepted as one of 'the people' and was no longer the object of superior ridicule. Libyans, Asiatics, Africans, foreigners of every

kind, once they had become acculturate, could obtain Egyptian citizenship and achieve the highest positions" (1974, 16).

In India high-caste individuals had lighter skins and narrower noses, but there was no consistent correlation between caste and features maintains (Gossett 1963). The position of the ancient Hebrews can be seen in their Scripture. On the brink of entering into the promise land Moses warns the ancient Israelites that they are to drive out and destroy the nations that they find there. They are not to make treaties with them nor are they to intermarry with them. But this is not based on Israelite biological distinction but for religious reasons, "for they will turn your sons away from following me [the Lord] to serve other gods, and the Lord's anger will burn against you" (Deuteronomy 7:1-4). When individuals in the land did accept the God of Israel they were accepted as part of the nation. For instance, as Joshua leads the nation in taking Jericho, Rahab the prostitute, who had confessed belief in the God of Israel and helped the men sent out to spy out the city was accepted as part of the nation along with her family when the city fell, "She lives among the Israelites to this day" (Joshua 6:25).

The Greeks felt that all non-Greeks were barbarians. However, non-Greeks could shed their barbarian status by simply adopting Greek Culture. Hippocrates felt that the Greeks were superior to the Asiatics but his explanation was based on environment rather than race: he believed that the infertility of the soil made Greeks self-reliant (Graves 2002). Isocrates thought of Hellenism as a thing of the spirit rather than of race. He wrote, "So far has Athens distanced the rest of mankind in thought and in speech that her pupils have become the teachers of the rest of the world; and she has brought it about that the name 'Hellenes' is applied rather to those who share our culture than to those who share a common blood" (Montagu 1974, 18). Menander the Attic poet wrote:

For me none is a foreigner

If so be he is good. One nature is in all,

And it is character that makes the tie of kin.

Montagu notes, “The Greeks, as also the Romans, were singularly free of anything resembling race prejudice. A study of the culture and literatures of mankind, both ancient and recent, shows us that the conception that there are natural or biological races of mankind which differ from one another mentally as well as physically is an idea which was not developed until the latter part of the eighteenth century” (1974, 18).

Race - Ancient Reality

If the notion of race is a recent concept, as the racial skeptics argue, then one would not be expected to find references to race in the ancient world. If race is not a recent concept then the argument that race is not real and was only socially created as a result of Enlightenment liberal’s attempt to justify their treatment of slaves seems to fall apart. That is, for many racial skeptics, a premise in their argument against the biological reality of race is that it is recent and a result of the social conditions that arose during the age of discovery. But if the ancients did hold to race then this reason for maintaining that race is not real is defeated. Sarich and Miele suggest:

> Examination of the art and literature of non-European civilization shows that race was not suddenly “constructed” out of thin air by Europeans in the Age of Exploration to justify dispossessing and oppressing people of color. Contrary to the … consensus view of contemporary social science, the art of ancient civilizations of Egypt, Greece, Rome, India, and China, and the Islamic civilization from AD 700 to 1400 shows these societies classified the various peoples they encountered into broad racial groups. They sorted them based upon

> the same set of characteristics-skin color, hair form, and head shape-allegedly constructed by Europeans when they invented "race" to justify colonialism (2004, 30).

Racial realists point to three lines of evidence to show that race is not a new concept; that it existed in the ancient world. First, the ancients recognized distinct physical features of the various people around them. Second, the ancients used the same set of physical features that we recognize as races today. And lastly, ancient civilizations' notions of superiority were based on these various physical features. Racial realists argue that while the ancient recognition of races does not prove that race is a biological reality, it does make it plausible because it shows that the notion of race is not a new concept. Let us look at each of these claims in turn.

Distinct physical features were recognized by ancient civilizations

Early civilizations clearly depicted the distinctive physical features of the major races in their art, literature and oral traditions. They distinguished people by their skin color, facial features and hair texture as is done today.

In Egypt, there is indication of the recognition of race differences in the portraits on the walls of the royal tombs from as early as 1350 BC. Four colors were used for the various kinds of people represented: red for the Egyptians, yellow for their enemies to the east, the Asiatics, white, with blue eyes and fair beards, for the people from the north, and black for their southern neighbors (Gossett 1963). Sarich and Miele, suggest that the Egyptian monuments were not mere portraits, but also an attempt at classification (2004, 33).

The Great Hymn to the Aten looks at the diversity that was recognized in early Egypt. The author is unknown but has often been identified as Akhenaten (1379-1362 BC). The hymn

suggests that Akhenaten considered Aten as the only god, and creator of the universe, and the various peoples in it.

> *How manifold it is, what thou hast made!*
> *They are hidden from the face (of man).*
> *O sole god, like whom there is no other!*
> *Thou didst create the world according to thy desire,*
> *Whilst thou wert alone: All men, cattle, and wild beasts,*
> *Whatever is on earth, going upon (its) feet,*
> *And what is on high, flying with its wings.*
> *The countries of Syria and Nubia, the land of Egypt,*
> *Thou settest every man in his place,*
> *Thou suppliest their necessities:*
> *Everyone has his food, and his time of life is reckoned.*
> *Their tongues are separate in speech,*
> *And their natures as well;*
> *Their skins are distinguished,*
> *As thou distinguishest the foreign peoples.* [4]

It is argued that this hymn documents the earliest written account of race differences.

The earliest civilization on the Indian subcontinent existed along the Indus valley between 2500 and 1750 BC. There are few explicit references in ancient Indian literature to race, however, in the Rig-Veda, a sacred text, there is a description of an invasion by the Aryans, of the valley of the Indus where there lived a dark-hued people (Gossett 1963). It is noted, "The Hindi word for caste is *varna*. It means color (that is, skin color), and it is as old as Indian history itself" (Sarich and Miele 2004).

In ancient Chinese literature historians of the Han Dynasty in the third century BC speak of a yellow-haired and green-eyed barbarian people in a distance province (Gossett 1963). The ancient Han Chinese applied the term "Hu" to barbarians, like, for instance, the Yuezhi who had hairy, white ruddy skin, deep eye sockets, prominent noses and beards. These people were

[4] Taken from Wikipedia, http://en.wikipedia.org/wiki/Great_Hymn_to_the_Aten. Last accessed on October 26, 2013.

likened to monkeys. But they did not apply this term to the Qiang, another barbarian group, who had a Mongoloid appearance and among whom some of the Yuezhi lived. Both groups were denigrated as uncivilized to the Chinese, but the Qiang were deemed to belong to the same racial stock, where as the Yuezhi were viewed as being part of a very different stock (Sarich and Miele, 2004).

The ancient Israelites were forbidden to intermarry with the nations around them however some of them did anyway. In the Song of Songs, a love poem attributed to King Solomon, he pursues a lover who describes herself as "dark am I, yet lovely." She asked that others "do not stare at me because I am dark, because I am darkened by the sun" (Song of Songs 1:5 and 6). The prophet Jeremiah asked the rhetorical question, "Can the Ethiopian change his skin or the leopard its spots" (Jeremiah 13:23)? It is suggested that this rhetorical question "shows that they considered skin color to be a permanent, inherited, racial characteristic" (Sarich and Miele 2004).

For the ancient Greeks and Romans the Africans' color was regarded as their most characteristic and most unusual feature. The Greeks, followed by the Romans were the first of many peoples to apply to blacks, or their country, names emphasizing color-Ethiopians, Negroes, blacks, colored peoples (Snowden 1983).

Manilius (1st century AD.) in his poem on astrology mentioned the groups who were to be included most frequently in a familiar ancient "color scheme": Ethiopians, the blackest; Indians, less sunburned; Egyptians, mildly dark; and Mauri (Moors), whose name was derived from the color of their skin (Snowden 1983).

Xenophanes, the first European to apply to Africans a physical characteristic other than color, described Ethiopians as black and flat-nosed; Herodotus was the first to call attention to the hair of African Ethiopians, the "woolliest" of all mankind. Diodorus described Ethiopians

who lived near the Nile as black, flat-nosed, and woolly-haired; and the idea that a white man could pass for an Ethiopian by merely blackening his body was ridiculed by Petronius, because color alone does not make an Ethiopian; a complete Ethiopian disguise requires several basic modifications in the white man's makeup-in hair, lips, and facial scarification (Snowden 1983).

Thus, we see that distinctive physical features were recognized by ancient civilizations. However, racial skeptics maintain that this is not enough to show that the ancients recognized race. They suggest that accepting any historical reference to color and other physical characteristics of populations as a reference to race is defining race too broadly (Smedley 1999). That is, if shades of skin are enough to indicate race what about other physical characteristics? On the Egyptian tomb walls are whites with blue eyes, in ancient Chinese literature historians of the Han Dynasty in the third century BC speak of a yellow-haired and green-eyed barbarian people in a distance province; does the recognition of variation in eye color raise to the level of race, if no, why not? Racial skeptics see realists as reading their own racial notions into the ancient descriptions.

Ancient civilizations used the same set of features as we recognize today

Racial realists also argue that the ancients used the same set of physical features as are recognized today to distinguish between various races. We can call this the "clairvoyant argument." If ancient civilizations had racial categories that match those produced by the DNA methods that did not come into existence until recently, then ancient civilizations would have to have been clairvoyant to produce such agreement if there was no underlying biological reality to race (Sarich and Miele 2004).

It is noted by historians that the blacks of ancient artists often bear a close similarity to racial types designated in the modern world as "black," or "negro" (Snowden 1983, 17). As early as the latter part of the third millennium BC, the Egyptians depicted blacks with broad noses, thick lips, and tightly coiled or woolly hair, the same characteristics by which European anthropologists of the nineteenth century would define the Negroid race (Sarich and Miele 2004). Snowden also notes, "in addition to their observations on color, classical writers commented on the Africans' woolly or tightly coiled hair, the broad, flat nose, the thick, everted lips, and occasionally other traits. Xenophanes, who described Ethiopians as black and flat-nosed, was the first European to apply to Africans a physical characteristic other than color" (1983, 10).

When Alexander the Great's army reached India, the Greeks described the people as being blacker than all other peoples except the Ethiopians. Sarich and Miele note, "Foreshadowing nineteenth-century anthropologists' racial classifications, the Greeks recognized that black Africans' hair form differed from that of even the darkest-skinned Indians. In other words, the Greeks believed in race and did not believe it was just 'skin deep'" (2004, 37).

One of the most detailed descriptions of characteristics of black Africans from the ancient world appears in the poem *The Moretum*, attributed to Virgil (1st century AD). A female character is described as "African in race, her whole figure proof of her country-her hair tightly curled, lips thick, color dark, chest broad, breasts pendulous, belly somewhat pinched, legs thick, and feet broad and ample" (Sarich and Miele 2004). Snowden remarks, "In this succinct metrical description the author of *The Moretum* delineated several characteristics of the Negroid division of mankind in language remarkably similar to that of modern anthropologists" (1983, 10), as table 2-1 shows.

Table 1-2 A comparison of Blacks as described in *The Moretum* and by anthropologists E. A. Hooton and M. J. Herskovits adapted from Snowden, Frank M. 1983

Racial Trait	The Moretum	E. A. Hooton	M. J. Herskovits
Color	Dark color	Integument rich in color	Reddish brown to deep brownish-black
Hair	Tightly curled hair	Tiny curls	Hair wiry, tightly curled and lying close to the scalp
Lips	Puffy lips	Thick lips	Lips thick
Shoulder or pectoral area	Broad chest	Omitted	Broad shoulders
Waist	Belly somewhat pinched	Omitted	Narrow waist
Legs	Thin legs	Thick legs	Arms and legs slender and long in proportion to stature
Feet	Broad and ample feet	Flat feet	Omitted
Breasts	Pendulous breasts	Omitted	Omitted
Other	Omitted	Wide nose, narrow heads, round foreheads, protruding jaws and receding chins, integument poor in hairy growth	Broard nostrila, high cheekbones, prognathous faces, with an acute facial angle; short stocky build and heavily muscled, triangular shaped torso

Note that the clairvoyant argument indicates it would be a miracle if the agreement between the ancients and modern categories did not imply an underlying biological reality to race. However, at best it seems to show that the ancients recognized the same human variation that is in existence today. But again, noting human variation does not seem to rise to the level of

recognition of race. One could argue that clairvoyance is not needed to recognize human variation, in fact since humans have not significantly changed since ancient history began to be recorded; it would be a miracle if ancient civilizations did not recognized the similar variations that are seen today. But does that mean they recognized them as various biological races? What does it mean for them to recognize the same categories that we do? Gossett notes:

> The confusion over methods of determining race differences shows up most sharply in the widespread disagreement over the number of human races. Linnaeus had found four human races; Blumenback had five; Cuvier had three; John Hunter had seven; Burke had sixty-three; Pickering had eleven; Virey had two "species," each containing three races; Haeckel had thirty-six; Huxley had four; Topinard had nineteen under three headings; Desmoulins had sixteen "species"; Deniker had seventeen races and thirty types (1963, 82).

It seems at least in these examples, that how many races there are depends on how they are counted, that is which features are used to determine race, if this is the case it seems difficult for modern racial realists to say that the ancients use the same set of features we use today to distinguish races.

Notions of superiority for the ancients were often based on physical characteristics

Finally, it is maintained that notions of superiority and discrimination in the ancient world were often based on physical characteristics and this can be seen as recognition of race. For instance, difference and inequality was an integral part of medieval Europe. But the serf, the slave, the peasant, the artisan, the lord, the king – were all allotted their place in society by divine sanction not physical characteristics. If physical characteristics seem to determine one's status in society

then, one could argue that recognition of race was the reason. Gossett observes that in ancient Egypt color prejudice depended on which ethnic group held sway. When lighter-skinned Egyptians were dominant they referred to the darker group as "the evil race of Ish." When the darker-skinned Egyptians were in power, they retorted by calling the lighter-skinned peoples "the pale, degraded race of Arvad" (1963, 4). Montagu notes, "The ancient Egyptians considered foreigners to be rustic and uninitiated, and indeed, distinguished between themselves as 'men' and Libyans or Asiatics or Africans" (1974, 16). The ancient ruler Sesostris III (1878-1843 BC) characterized the Kushites, blacks from south of Egypt, as "craven," "poor and broken in heart," and "not a people of might" (Snowden 1983).

In India the same sacred text that tells of the invasion of the Aryans into the Indus valley indicates that the god of the Aryans, Indra, is described as blowing away with supernatural might from the earth and from the heavens the black skin which Indra hates. The dark people are called "Anasahs"- nose-less people- and the account proceeds to tell how Indra "slew the flat-nosed barbarians." Having conquered the land for the Aryans, Indra decreed that the foe was to be "flayed of his black skin" (Gossett 1963).

The claim about superiority, however, seems to be the most difficult to maintain. For instance, Snowden suggests that worthy of note is the fact that the Great Hymn to the Aten quoted above "looks objectively at mankind's diversity in skin color, speech, and character, making no claim to Egyptian superiority. Seeing all peoples as creations of the Aten" (1983, 39).

A virtual laundry list of counter-examples could be listed to show that the ancients did not consider their own physical features as superior. Blacks became rulers in ancient Egypt (Snowden 1983). The Persians had "respect for the customs and languages of others, and

Alexander the Great exhorted his soldiers to mingle and intermarry with the people they conquered (Smedley 1999). In ancient Israel Moses married a black woman described in Numbers 12:1 as a Cushite[5]. When his brother Aaron (the first High Priest of Israel) and his sister Miriam began to talk against Moses because of her they were punished by the Lord. Miriam turned leprous and Moses had to intercede to the Lord on her behalf. Seneca emphasized that there was in reality nothing unusual about the physical characteristics of different racial types: Amongst his own people the color of the Ethiopian is not notable, and amongst the Germans red hair gathered into a knot is not unseemly for a man. You are not to count nothing odd or disgraceful for an individual which is a general characteristic of his nation" (Snowden 1983).

It seems therefore that there are problems with both the claims that the ancients did not believe in races and that the ancient did believe in races. What this brief study seems to show is that if we distinguish between ethnocentrism (group superiority based on cultural differences) and racism (group superiority based on biology, physical features and genetic make-up) it seems that most ancient groups exhibit ethnocentrism without racism but racism was seen at various times and places. And so it seems neither side can say that the fact the ancients believed one way or the other helps their case. But even if the notion of race arose recently it could be that the reason it did so is because science finally was able to determine it. If this is the case then race did not have to arise recently as a result of political and social reasons. It could have arisen even recently as a result the discovery of biological races. Thus, I want to consider some of the modern biological theories of race.

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[5] In Egyptian texts and in the Old Testament the land south of Egypt where blacks lived was called Kush (Cush). It was known as Aithiopia (Ethiopia) by Greek, Roman, and early Christian authors (Snowden 1983).

CHAPTER 2

RACE: THE MODERN THEORIES – WHAT IS BIOLOGICAL RACE?

Often scientists begin their research with a problem to be explained and, broadly speaking, we can say that scientists use theories to explain the phenomena they investigate. In an attempt to explain scientists may ask *what* questions (what is water? water is H_2O, or, using examples for our purposes, what is race?), *how* questions (how did races come to be), and *why* questions (why do groups appear different in different geographical locations). This chapter seeks to examine efforts people have made to answer the question: What is biological race? We will examine several theories of race and consider the problems associated with each. We will see that no theory seems to overcome the problems that are associated with it.

The Essentialism Concept of Race

"No man who thoroughly investigates with an unbiased mind, can doubt that the Negro belongs to a distinct type… we may safely say that there is in the Negro that assemblage of evidence which would, *ipso facto*, induce the unbiased observer to make the European and the Negro distinct types of man" (Dr. James Hunt (1833-1869) founder of the Anthropological Society of London 1863).

Two major theoretical frameworks have emerged in history as a way to define races; there is the idea of race based on essentialism or typology and the idea of race based on populationism. As Sankar 2008 explains, typological race refers to the theory that humans are divided into natural, discrete groups that can be identified and distinguished by their intrinsic properties revealed in appearance and the like. These properties vary across races but are consistent among members of the same race and are thought to pass as a bundle from parents to children and thus maintain the races as separate groups (2008, 275).

Typological race, however, fell from favor along with scientific support of races as naturally existing, discrete groups with the rediscovery and development of Gregor Mendel's work in genetics. It was found that no hereditary essence passes from generation to generation and thus there was no basis for the typological traits that proponents unsuccessfully sought in support of their theory. An alternative idea of open populations emerged as the model. This theory holds that population groups exist but they do not have natural or fixed limits. They are distinguished based on differences in gene frequencies (2008, 276). Today race realists reject the idea that there are essential, unbridgeable, unchangeable differences between populations. Race realists today base the idea of race on some aspect that is founded on populationism. I will examine essentialism first and consider the problems associated with this concept and then consider notions of race based on populationism.

Aristotle is usually considered one of the first to apply an essentialist analysis to living things. Elliott Sober explains that Aristotelian essences, as the defining component of species, were constitutive: The essence of a species or natural kind was believed by Aristotle to be present in each member of the species, and it was what made it a member of that species (Sober 1994). So an attribute is essential to an object if it is a property that an object must have in order to be that kind of thing. For instance, the essence of water is H_2O. Nothing can be water and not have the quality and if something does not have the quality it is not water. Early race theorists picking up on Aristotle defined races as natural kinds in terms of an essential property possessed by all and only the members of the same race.[6] The existence of an essential property would allow for certain characteristics to be transmitted together. So for instance, there might be specific biological traits that were present in all, or at least most, people who were considered

[6] See Eze, Emmanuel Chukwudi. 1997. Race and the Entlightenment – A Reader. for various Enlightenment views on race.

black, and descriptions of those traits could be used as a biological foundation for definitions of blackness. The explanatory entity may not be known or able to be observed but if there is evidence that a cause exists, the inability to observe the entity does not take away from it being posited. Thus, the one-drop rule, which says that a person is black if he or she has at least one black ancestor anywhere in the family history, was justified during the nineteenth century by a belief that physical racial essences were passed down through the blood. The foundation for the rule was a belief in racial essences. The essence of each race was assumed to be in the blood, melanin, or cranial shape and size and it was revealed in appearance, temperament, morality, and intelligence. Thus, race was embodied in a person (Marks 2008), every representative of a race conforms to the type and is separated from the representatives of any other race by a distinct gap – there are fixed characteristics that distinguish one from the other.

On this view racial differences originated long ago and ended long ago, and man's physical character, and therefore his racial types, had ceased to undergo profound changes, except through the intermixture of existing races, and one goal of biology or anthropology is to classify people into the fundamental racial types, using the slight morphological variations discovered in the species, variations which are assumed to be unchanging (Stepan 1982). Individuals from a particular group have traits that are present in combinations peculiar to their group. Thus, physical differences are proof that races exist. So, it is not hard to distinguish a dark-skinned African from an Australian aborigine or a dark skinned Indian and all of these from a European or an Asian because these people tend to have some traits in association that others do not have. For instance, Europeans tend to have light skin, straight or wavy hair, noses of narrow to medium width. Sub-Saharan Africans tend to have dark brown or black skin, wiry hair, and the

like. East Asians tend to have pale-brown to slightly yellowish skin, straight black hair and brown eyes.

There are two major problems with the essentialists approach. First, there is so much variability in nature that one rarely finds a single property possessed by all and only members of the same group identified as a race. We know for instance, that blood types do not co-vary with other traits. That is, for most blood group systems (ABO, MN) they appear in all groups that are identified as races, and there are no uniform variations that correspond to the identified races. If there are no properties found in all and only members of a particular group, then there are no racial essences.

The second major problem with the essentialists approach is that it is based on an outdated pre-Darwinian model. As Lisa Gannett 2004 notes, with the evolutionary synthesis of the 1930's and 1940's and rise of population genetics, notions of race that prevailed in the eighteenth, nineteenth, and early- twentieth century were discredited. Once Darwinian ideas took hold combined with Mendel's genetic ideas it was recognized that populations are always changing and evolving in their genetic composition and thus, races could no longer be conceived as permanent, static entities.

Population thinking, with its emphasis on the extents to which populations are genetically heterogeneous and genetic difference, or are quantitative or relative not qualitative or absolute, replaced typological or essentialists thinking as the accepted way to conceive of species' biological diversity. If there are no genes that are present in all and only members of particular racial groups then there are no racial essences (Gannett 2004).

Populationism Concepts of Race

The population idea recognizes races, not as unchanging categories, but as the average difference between groups. Thus, Graves 2002 defines race as geographically isolated subpopulations. Collins 2010 adds to this that the subpopulation shares certain characteristics in higher frequencies than other populations of that species. So the populationist concept maintains that there are no unique common features or 'essence,' no entirely distinct groups, but only average differences in certain traits. But this is sufficient to divide the world into races. Thus, Europeans are more likely to have blue eyes than Africans. So according to the populationist view of race just because there are no pure races does not mean there are no races.

Based on populationism there are several possible empirical bases for race; phenotypic, geographical, genotypic, and genealogical. Sarich 2004 uses most of these in his definition of race. He argues that races are populations, or groups of populations, within species, that are separated geographically [geographical] from other such populations and distinguishable from them on the basis of heritable features [genealogical]; not unique, fixed genetic features but statistically, in the frequencies of particular alleles [genotypic]. I will begin with phenotypes as a basis for race.

Phenotypes as a basis for race

Phenotypes are observable traits that are the results of the genetic code working in combination with environmental factors. The argument from phenotype is rather straight forward and probably the most common; that is, it is the one that folk concepts of race seem to depend on, and seems to be the most obvious. Individuals from a particular population have traits that are present in combinations peculiar to their group. Thus, physical differences are

proof that races exist. So, it is not hard to distinguish a dark-skinned African from a dark skinned Indian and both of these from a European or an Asian because these people tend to have some traits in association that others do not have. For instance, Europeans tend to have light skin, straight hair, noses of narrow to medium width. Sub-Saharan Africans tend to have dark brown or black skin, wiry hair, and the like. East Asians tend to have pale-brown to slightly yellowish skin, straight black hair and brown eyes.

Also, there seems to be a wide agreement on ascriptions of race. Levin surmises that one hundred randomly chosen individuals sorting passers-by on an urban street would, without hesitation or collusion, almost always agree on who is black, white, or Asian. Moreover, the race others would non-collusively ascribe to an individual is almost always the race he unhesitatingly ascribes to himself. Thus, such systematic agreement must rest on some objective basis – possibly misconstrued, but present and detectable (Levin 1997).

Skin color seems to be the major common sense criterion for racial membership and identification. Skin color differences are taken for granted as evidence of racial difference, if not considered to be racial differences in themselves, and skin color is assumed to be the evolutionary result of ancestral geographical environment. Thus, dark skin is seen as increasing fitness in sunny climates, because it offers protection against ultraviolet (UV) rays that can cause skin cancer. Light skin facilitates the synthesis of vitamin D in climates with little sunlight. Absence of vitamin D can cause rickets thus light skin gave a selective advantage in these climates.

There are several ways that the notion of race based on phenotype can be criticized. First, one can attempt to show that for each individual trait, say skin color differences, does not provide a basis for race. For instance, Zack in criticizing skin color differences as a basis for race

indicates that structurally, hair and skin color are part of the same epidermal systems in mammals, and differences in epidermal coloration within other species are not normally used as a basis for subspecies or racial taxonomies (Zack 2002). The argument here is that skin color does not determine race because that is not how subspecies are determined by scientists. This trait is thus ruled out as a trait for race by general scientific method. Or we could note that for other traits such as blood groups for instance, their distributions do not match the division of the world into races.

Or one can attempt to criticize the whole approach. There are at least two ways to criticize the whole phenotypical approach; one we can call the "clines" argument and the other we can call the "no covariance" argument. Let us first consider the clines argument.

A cline is "a geographic continuum in the variation of a particular trait" (Park 1999). The change in a trait from one area to another is gradual as opposed to sudden and absolutely distinct, and it is usually correlated with a gradient in the climate, geography or ecology of the groups. The clines argument maintains that traits accepted as racial criteria do not fall into discrete and mutually exclusive categories as required by the common sense taxonomy, but instead are matters of degree, and are continuous among, between, and within different social racial groups. Thus there is no clear distinction between groups and so no phenotypical basis for race

One way to argue against the clines argument is to give the "bald counter-example." The "bald counter-example" maintains that the claim that there is no distinction between being bald and not bald, because there is a continuum between the two states, is a slippery slope fallacy. One can divide individuals into categories of bald and not-bald, everyone in the not-bald category will have more hair than everyone in the bald category. So, even though baldness is a matter of degree and the trait measures continuously among the individuals the division and the

distinction is clear. The same can be said of various racial traits and a clear distinction can be made.

Zack 2002 does not buy the bald counter-example argument when it comes to racial traits. She reasons that racial divisions based on say skin color, are not orderly in the common sense taxonomy of race as the trait of being bald is. If racial division based on skin color were orderly, then every black person would have darker skin than every white person. But, in society, some black individuals have lighter skin than some white individuals and some white individuals have darker skins shades than some black individuals. She concludes that skin color as a primary racial phenotype, is therefore not an empirical basis for common sense racial taxonomy, despite what is generally believed.

Other racial skeptics believe the issue is even worse than this, because even if there was an orderly continuum, that is, even if every black person is darker than very white person (which is not the case), the problem would still stand. The issue is not; is there a place to make the division, the issue is, why make the division in one place rather than another. That is to say, what would be the biological basis to make the cut in one place as oppose to another (Glasgow 2009). Thus, the clines argument shows that any division seems to be based on an arbitrary, non-biological decision. In other words, the clines argument grants the reality of difference sides of a line drawn but says that there is no biological reason to draw the boundaries in a particular way and call different sides of the line different racial groups.

The second way to criticize the whole phenotypical approach is the no “covariance argument.” It maintains that traits that are supposed to be associated with the various racial groups are largely independent of one another, that is, they do not cluster to form a particular racial type. The distribution of one trait rarely matches another. Thus, a racial division based on

the distribution of one trait will invariably differ from that based on another. The expression of one trait does not predict a particular expression of another (Park, 1999). For instance, Sri Lankans of the Indian subcontinent, Nigerians, and aboriginal Australians share a dark skin tone, but differ in hair type, facial features, and genetic predisposition for disease. Kenyans and Peruvians have greater lung capacities and red blood cell counts from living at high altitudes, yet they differ in skin color. Or take the genes for sickle cell anemia. It occurs in large amounts in people who live in tropical areas because if you have one sickle cell message and one normal message, you have a better chance of surviving malaria. So the sickle cell anemia message is in high frequencies in populations in western Africa, the Middle East, the Persian Gulf, the Mediterranean, and India. Thus, someone from Ghana is genetically closer to someone from Syria than to someone from Kenya, because Kenyans don't have a high frequency of sickle cell messages (Graves 2004). So, for instance, from a medical diagnosis perspective, if sickle cell is suspected, then the correct diagnostic approach is not to try to determine the race of the individual, but to ask which if any of the high frequency of malaria regions the person's ancestors are from.

Thus, there is no such thing as a discrete group that is distinguished by certain features that are unique to that group. There are no features that are possessed by one group to the exclusion of others; no 'race' traits present in all members of one group and none of another. All populations overlap when traits are considered, and in almost all populations, all traits are present. Thus, the distributions of individual traits are not concordant: they do not match up. We could carve up the world into a number of different groups depending on which traits we decide are important. But if this is the case, then the notion of race based on phenotype seems meaningless.

A final issue with this basis for race is that phenotypical variations represent evolutionary adaptations, many of which are very local. Such changing traits within populations, and the presence in every population of traits that could in principle be present in any population, undermine the division into groups, and the uniformity within groups that the traditional concept of race seems to connote (Zack 2002).

Races are geographically and genetically distinct populations

The notion that races are geographically and genetically distinct populations is one way of expressing the generally recognized fact that human genetic variation is correlated with geography. That is, one can argue that some genetic variants that produce physical or behavioral traits occur significantly more often in some areas, or in some ethnic groups, than in others. For example, most of the worlds' people who have very dark skin and woolly-textured, tightly curled hair live in Africa south of the Tropic of Cancer. Although many people who live elsewhere also meet this description, the great majority of them are descended from people who lived in sub-Saharan Africa and who immigrated into other parts of the world. For instance, in North America they have formed persistent ethnic groups with a tendency toward preferential mating within the group and thus, maintaining high frequencies of recognizably "African" facial appearance and other traits, (Cartmill 1998).

However, Cartmill 1998 criticizes this approach. He argues that studies usually draw data from individuals born in some particular geographical region, for instance, in North America, but if North American individuals are racially different, then races are not geographically distinct because all these people inhabit the same region. Further, if Negroid and Caucasoid people

occur on every continent, it makes no more sense to describe these groupings as geographical subspecies than it would to describe redheads or people with type A blood as human subspecies.

Another problem for this approach is what might be called the "within/between" argument. This argument maintains that virtually all of genetic variation is within a population, not between 'races,' and very few genes are exclusive to one part of the world (Lewontin 1976). Thus, if we were to take any two people from anywhere in the world, the basic genetic difference between these two people would typically be around 0.2 percent, even if they came from the same geographic location. But the racial characteristics that many think are major differences account for only 6 percent of this 0.2 percent variation, which amounts to a mere 0.012 percent difference generically (Cameron and Wycoff 1998). Thus, the racial differences are trivial and overall, there is more variation within any group than there is between one group and another.

There is a bigger problem for this approach. If we define a race as a division of a species which differs from other divisions by frequency of certain genes, then were we to test for enough genes we could find a statistical difference between virtually any two populations, for instance, between populations in two different cities, and if any population in the world can be defined as a race then as we saw with the phenotypical concept, the concept is meaningless (Malik, 2008).

The Genealogical basis of race – ancestors' continent of origin

As we saw above, to define race it is insufficient for two populations to be geographically separate and genetically distinct. We require an additional means of affirming that the peoples of say, Europe and Africa are distinct but those from two adjacent cities are not. The next common definition of race suggests that it is based on genealogy - that is, that ancestry might be the

answer (Malik 2008). On this account Africans are people with primary ancestry in sub-Saharan Africa and Caucasians include those with ancestry in Europe and West Asia.

According to the out of Africa theory various people migrated out of Africa and moved across the various continents to form different groups. Thus, groups that migrated from Africa some 60,000 years ago by chance would have had slightly different genetic profiles and would have picked up genetic mutations that would have passed on from generation to generation in the particular location. Different ancient migrations are acknowledged by different sets of genetic markers. So for example, as Malik 2008 notes sometime between 80,000 and 50,000 years ago, a man living in northeast Africa suffered a mutation on his Y chromosome called M130. This man or one of his sons joined the first band that left Africa and eventually ending up in Australia. The M130 mutation is virtually unknown in populations west of the Caspian Sea but as one travels east it becomes more common, and is found in up to 60 percent of Australian Aborigines. Thus, this type of effect can be an indicator of the various races.

But defining race by continent of origin is really to establish in which of the first major migrations a group's ancestors took part. About four percent of total human variation comprises differences between the major continental groups. Where this transforms contemporary descendants of the original wanderers into distinct races depends on how one wishes to interpret that difference.

So, for instance, Michael Levin's account of race focuses on the geographical location of ancestors. He believes that the definition of race that captures ordinary usage and the usage of evolutionary biologists refers to birthplace of ancestors. Thus, he asked us to assume the out of Africa hypothesis that is generally favored by anthropologists and molecular biologist is correct, according to which mankind evolved in Africa, branched off into Europe about 100,000 years

ago, and branched off from there into Asia about 70, 000 years ago. He maintains the branches have interbred in historic time. So, letting 25 years mark a single generation, a Negroid may be defined as anyone whose ancestors 40 to 4400 generations removed were born in sub-Saharan Africa. Mongoloid and Caucasoid are defined similarly, with Asia and Europe in place of Africa.

Further, Levin indicates that because comparisons of blood group frequencies in the white, African, and conventionally identified American black populations indicate a white admixture of about 25% in the blacks in the American North and 10% in blacks in the American South, an American Negroid can be defined as anyone 75% or more of whose ancestors 40 to 4400 generations removed were born in sub-Saharan Africa.

Finally, Levin maintains that familiar observable criteria for race like, skin color, hair texture and facial bone structure, do not define race. Rather these traits serve as contingent indicators of ancestry, observable correlates of geographical origin, used to identify that less observable trait (Levin 1997). Thus, the geographical origin of one's ancestors determines ones race and typical racial features help to identify it.

One question that this view raises has to do with why these particular branches were chosen. Why not choose when humans branched into Australia or when humans branched into the Americas? In fact the out of Africa consensus has prompted some writers to claim that the sole human race is the Negro or black one since everyone's geographical origin is out of Africa.[7]

Also, if any American 75% or more of whose ancestors 40 to 4400 generations removed were born in sub-Saharan Africa is American Negroid, what do we call Americans for whom 74% of their ancestors 40 to 4400 generations removed were born in sub-Saharan Africa, or 73%? And why not call those conventionally identified American black populations that indicate

[7] See Zack, 2002

a white admixture of about 25% in the American North one race, and those about 10% in blacks in the American South another race? It seems that Levin has predetermined what a race is and then come up with a way to distinguish them. In other words the criteria seem to be arbitrary.

So to say that continental group is how race is defined is to say something trivial. For it to be non-trivial two questions need to be answered. What is it about continental groups that distinguish them as races? And why should continental groups, as opposed to other groups, be defined as races? One might argue that genetic differentiation is greatest when defined on a continental basis; such differentiation is significant because many illnesses and diseases appear to be racially distributed. But while each continental group does possess a genetic profile slightly distinct from others, the consequences of early human migration, continental groups represent neither the greatest degree of genetic differentiation within humankind, nor necessarily the most useful way of dividing up human populations. As we will see in Chapter 5 the greatest genetic differentiation is not between continental groups, but between Africans and non-Africans and as was indicated above, from a medical diagnosis perspective, the best diagnostic approach is not to try to determine the race of the individual, that is the continent of ancestral origin, but to get a clue to the problem by asking which if any of the high frequency regions the person ancestor's is from, regions that could encompass multiple continents. Thus, it is an arbitrary choice defining continental groups as races.

Racial Cladism

Racial cladism is very similar to the views we discussed when we were considering geographical origins of ancestors. In fact, it would seem that racial cladism is just a special case of the geographical origins of ancestors. A major distinction is that given this concept there is no

reason to believe that races exist now or at least will exist for long. Andreasen 1998, a major proponent of this view, believes that races once existed, but due to recent historical events, they are on their way out. They are fading out of existence due to the voyages of discovery, colonization and immigration.

Cladism views races as monophyletic groups, groups who are ancestor-descendant sequences of breeding populations that share a common origin. It describes the evolutionary history of a species in terms of a phylogenetic tree whose branches represent the subdivision of ancestral breeding populations into multiple descendant breeding populations. A breeding population is a set of local populations linked to one another by reproductive ties that are reproductively isolated from other such populations. Cladistic classifications have both a conventional and an objective aspect. The way groups get assigned to a level is conventional. Thus, one can cut the branches of the tree at any particular level. Racial divisions are objective because they represent a process of evolution branching that has taken place independently of human classification activities (Andreasen 1998).

The cladistic concept of race has it critics.[8] There seem to be several problems with the concept. First, the cladistic concept of race and its treatment of breeding populations and groups of breeding populations require the maintenance of extensive reproductive isolation. Many would not agree that the isolation required has taken place. Second, the phylogenetic tree model that is used is based on computer programs used to generate trees from genetic distance data. The construction of these trees assumes the validity of the "candelabra model" of human evolution that characterizes races as branching lineages. But there are other possible models. For instance, Templeton favors the "trellis model", which assumes that gene flow has always

[8] See Zack, 2002 and Lisa Gannett, 2004.

occurred among the world's populations, and thus, preventing evolutionary branching (Templeton 1998). The cladistic concept of race seems premature given the lack of empirical data regarding which of the two models is correct (Gannett 2004).

Also, as Zack points out, cladistics as a taxonomic methodology has always been applied to taxa at the species level or higher. If cladistics were applied to the subspecies level, there would have to be independent evidence that the subspecies groups were already well-delineated according to some other biological classificatory system such as phenetics. Without independent justification for an application of cladistics to groups more specific than species there would be nothing to stop the application of cladistics to very small genealogical groups with distinct hereditary traits, such as families, and the problem with applying cladistics that far down is that it fails to preserve the taxonomic feature of cladism (Zack 2002, 77). Zack here attacks the conventional aspect associated with cladism, i.e., where to choose to draw the lines. That is to say, if the way groups get assigned to a level is conventional, then how do we determine at what level to stop?

Summary

There are several conditions that nature could have used to divide our species into biological significant varieties.[9] First, isolate a breeding population. Second, wait for some distinctive heritable characteristics to appear. Third, give their conjunction a selective advantage. Fourth, let selection operate for a very long time in the isolated population. But, human evolution did not proceed this way. According to our best evidence, human populations have not been

[9] Parks, 1998, uses a similar list. They include: 1) Some degree of population isolation with limited gene flow; 2) Environments different enough to promote adaptive selection in different directions; 3) Genetic variation among the populations; 4) Enough time. Regarding time, he believes that if the other conditions were met there would have been enough time.

geographically isolated for long enough periods, and during all of natural history there has been too much inbreeding between populations. And while we can find all kinds of differences between groups they do not seem to allow us to make any meaningful biological distinctions.

As Malik 2008 has pointed out, the problem for race realists today is the opposite of that for the 19th century racial scientists. Then, racial scientists 'knew' the significance of race but could not find a way of defining differences. Today we can clearly define differences between populations but the significance of such differences no longer seems clear.

If essences, geography, phenotypes, genotypes, and genealogy are the only known candidates for biological race and they all fail, then we can argue that either we have not found the correct theory of biological race or it would seem that there is no correct theory of biological race. If we have not found the correct theory we can either keep looking or question whether or not there are races. I question the assumption of race in chapter five. If there is no correct biological theory for race then one may suggest that since the various theories of race do not work and yet people still clearly posit biological races perhaps non-cognitive values are driving race realists. So has non-cognitive values influenced this debate? I will turn to this issue next. We will first consider the role of values in science in general and then we will consider how they work in theory choice.

CHAPTER 3

WHAT IS THE ROLE OF VALUES?

Introduction: How Values Function in Various Scientific Contexts

In considering the complex relationship between science and values it is important to distinguish the various ways values can enter into science. One way is to consider how values function in various scientific contexts.[10] First, there is the context of discovery. In this context theories and hypotheses are generated. Non-cognitive values can quite appropriately play a role in the source of theories and hypotheses that are generated. There is also the context of investigation (designing a study, collecting data, etc.). Regarding collecting data for instance, non-cognitive values can justify placing procedural constraints on scientists – for example, requiring experimental subjects to be treated humanely or human subjects be give informed consent. In the context of application, non-cognitive values can help determine what level of certainty in a scientific theory is demanded before it is accepted as a guide for action (Anderson 2004).

Reviewing the main issues in each of these contexts offers an opportunity to stress the unavoidability of values in science while allowing us to see how non-cognitive values need not be involved in theory choice even if they are part of doing science. I will argue that non-cognitive values play a legitimate role in the context of discovery, investigation, and application but not a legitimate role in theory choice which I will call the context of assessment. How theories ought to be assessed rules out a legitimate role for non-cognitive values. In Chapter 4 I lay out an account of why some types of values are to be used to assess theory choice and why others ought not to be used.

[10] This list of "contexts" while not the same, are motivated by discussions in Longino 1990, Anderson 2004 and Dorato 2004.

The Context of Discovery

The context of discovery can be considered the theory generation phase of science. Theories can be generated in any number of ways. The source of a theory though, is generally not considered an indicator of its truthfulness, explanatory power, etc. However, as I have suggested above, both sides in the race debate seem to indicate that the other side is motivated by non-cognitive values. Realists maintain that one of the sources of anti-race theories is the desire to be politically correct and that this calls into question the conclusion. Skeptics maintain that at least one of the reasons theories of race were generated was because of the need to justify how people who were perceived as different, were being treated and thus, race was "discovered" as a result of political and social motivations. So one can argue that if theories of biological race are generated due to racist ideology or if the position that there are no biological races is generated to avoid racist ideology and these are the only theories we have, then non-cognitive ideological values must influence theory choice because all the theories available have been so influenced. Thus, in general, one can say that if non-cognitive values influence theory generation in the context of discovery, and the theories that are generated by these influences are the only theories we have, then non-cognitive values must influence theory choice.

The issue I want to consider here is whether these facts about the generation process mean that theory choice is inevitably influenced by non-cognitive values. That is, if theories are "contaminated" as a result of being influenced by non-cognitive values when being generated, does that mean there is no way to avoid non-cognitive values entering into theory choice? I will argue that a) the theory generation phase (the context of discovery) does not have to be influenced by non-cognitive values and b) even if it is, theory choice does not have to be so influenced, and c) if the choice of theories is so influenced it results in bad science. Social,

political, ideological and other non-cognitive values ought not to influence theory choice because these various values do not help to determine the way the world is and in fact are very likely to hinder us from doing so.[11] A theory or hypothesis is more likely to be true if supported by epistemic and cognitively relevant reasons, but there is no good argument that a theory is more likely to be true if supported by social, political, or ideologically commitments.

The logical positivists made a distinction between the context of discovery and the context of justification.[12] The context of discovery was considered the theory generation phase of science and the context of justification was where theories were tested, confirmed, and or corroborated. Martin Curd indicates:

> On the one hand, there is the psychological question about how a scientific hypothesis first arises in the mind of the individual scientist. This often involves … nonrational

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[11] While this may not be a surprise to those on the other side of this issue, there are some who argue that there is no way to know the way the world is. There is no fact of the matter with regard to the world. For instance, Putnam 1992 suggests that internal realism denies that there is a fact of the matter as to which of the conceptual schemes that serve us so well…is really true. He asks us to imagine a world in which there are three objects x1, x2, x3. Then he asks, "How many objects are there in this world?" On a commonsense notion of "object" there are three objects in this world. But, Putnam suggests, "Suppose … like some Polish logicians, I believe that for every two particulars there is an object which is their sum… [then] I will find that the world of "three individuals"… actually contains seven (x1, x2, x3, x1 + x2, x1 + x3, x2 + x3, and x1 + x2 + x3) objects (Putnam 1992, 96). Thus, for Putnam it seems that one's conceptual scheme, to a certain extent, determines the way the world is.

I think there is a way the world is and what Putnam really shows here is that there can be various descriptions of the world and of course this is true, but it is not true that every description of the world is the way the world is, that is, not every possible description will be true. Thus, one's social, political, ideological, and other non-cognitive values do affect one's conceptual scheme, and so one's description of the world, but I am suggesting that these various descriptions need to be supported by epistemic and cognitively relevant reasons in order to believe one is getting at the way the world is.

[12] The phrases were introduced by Hans Reichenbach 1951. Reichenbach was attempting to clear up an interpretation of the hypothetico-deductive method as an "irrational guessing." He writes, "The act of discovery escapes logical analysis; there are no logical rules in terms of which a 'discovery machine' could be constructed that would take over the creative function of the genius… logic is only concerned with the context of justification" (Reichenbach, 1951).

> influences as well as a liberal amount of inspired guesswork. Anything that helps answer the psychological question about the origins of scientific hypotheses falls within the context of discovery. On the other hand, once a scientific hypothesis has been formulated, questions about what kind of evidence supports it and to what extent place us in the context of justification, where we are concerned with relations of inductive support and confirmation (Curd and Cover 1998).

Thus, the source of scientific theories could be a result of any number of things, political or economic ideology, and even various religious, ethical or aesthetical beliefs and this would not count as a reason to call something bad or illegitimate science. But the context of justification served as the means to determine which of the variety of theories that were generated from various sources would be recognized as the one that most matched the world. It was believed that while social, political, ideological, etc., values played a role in the context of discovery, as indicated above, only concerns of inductive support and confirmation were legitimately considered in the context of justification.

However, this notion has been questioned. For instance, Kathleen Okruhlik argues that the generation phase allows for the influence of various non-cognitive values even in theory choice because current models of scientific rationality hold that theories are not directly compared to nature (as the logical positivists thought) but view theory choice as "irreducibly *comparative"* (Okruhlik 1994). Thus, the distinction between the context of discovery and the context of justification may not be lost but it is at least blurred and scientific theories are far more the products of social, political, even psychological sources than the logical positivist understood. She lays out a scenario where theories are compared with their extant rivals and a decision point is reach based on the competing theories.

Okruhlik argues,

> Traditionally, philosophy of science has been quite willing to grant that social and psychological factors (including perhaps gender) play a role in science; but that role has been a strictly delimited one, contained entirely within the so-called context of discovery… all that matters is the context of justification…. You test the hypothesis in the tribunal of nature and if it holds up, then you're justified in holding on to it-whatever its origins. The idea here is that the canons of scientific theory choice supply a sort of filter which removes social, psychological, and political contaminants as a hypothesis passes from one context to the next. (Okruhlik 1994).

But, she indicates, that if social values influence theory generation in the context of discovery and the theories that are generated by these influences are the only theories we have; then social values must influence theory choice. This is because, as Okruhlik maintains, we can only compare a hypothesis to its extant rivals, to other hypotheses which have actually been articulated to account for phenomena in the same domain and developed to the point of being testable. Thus, "if our choice among rivals is irreducibly comparative… then scientific methodology cannot guarantee … that the preferred theory is true-only that it is epistemically superior to the other actually available contenders. But if all these contenders have been affected by social factors, nothing in the appraisal machinery will completely 'purify' the theory" (Okruhlik 1994). So, even if one grants that standards of theory assessment are free of bias from non-cognitive values, non-cognitive values still may permeate the very content of science. We can call this the "contamination by generation" argument; it implies that once theories have been generated by means of non-cognitive values then no set of criteria can guarantee the elimination of non-cognitive values in theory choice.

The argument can be structured as follows.

P1 - Social (non-cognitive) values inevitably influence theory generation in the context of discovery.

P2 - Theories generated in the context of discovery are the only theories we have.

P3 - Theory choice is irreducibly comparative.

P4 - Thus, theories generated in the context of discovery can only be compared with other theories that are generated in the context of discovery.

C - Therefore, theory choice is inevitably influenced by social (non-cognitive) values.

Thus, Okruhlik maintains that the content of theories is inevitably influenced by social values and only by recognizing this and taking steps through the social arrangements of science to include diverse viewpoints can science hope to improve objectivity. Diverse viewpoints can contribute rival hypotheses that can help bring biases due to non-cognitive values to light. So we see that the justification for this view seems to be that the invention of rival theories brings to light biases and can lead to a much sharper criticism of accepted theory than does the mere comparison with observation. In this view then, non-cognitive values are not only unavoidable but also are helpful to the process of scientific theory choice. The restructuring of science that allows for non-cognitive values that operate in the context of discovery to bring in other viewpoints aid in coming up with a diversity of theories that can be considered in the context of justification. Let us examine this position a little closer.

First, P2 and P4 seem to amount to the truism that we can only compare theories with other theories that have been generated, so P2 and P4 do not seem to present a problem. Next, regarding P1, clearly theories can be generated any number of ways and there can be the unavoidable influence of non-cognitive values during the generation of theories. So if P1 is

correct then values inevitably influence theory generation. But, I am not sure that theories must *inevitably* be influenced by non-cognitive values during the generation stage as P1 indicates. For instance, recently philosophers have argued that even though there may not be a logic of discovery in terms of an algorithm, there does seem to be certain methods that can be used as heuristics that can aid in discovery. For example, Lindley Darden 1980 has argued that "interfield connections" can be a source of theory development, where connections to well-developed related fields can be a source of new ideas. Aharon Kantorovich 1993, suggests that novelty in science is generated through events dominated by "serendipity and tinkering." Robert Pennock 2000, has suggested that Darwinian mechanisms can be used to make novel discoveries. I would suggest that even given these various suggestions, this does not rule out the fact that scientific discoveries can come from non-rational sources but I think that they indicate they do not have to come this way.

Also, if P1 is correct, I am not sure her solution solves her problem. Her solution is to allow for circumstances to exist that enable the generation of theories (inevitably influenced by non-cognitive values) from different perspectives in hopes to add variety to the theories that are generated. But recall that her issue with relying on the context of justification was that it could not guarantee the truthfulness of the preferred theory. She indicated, "if our choice among rivals is irreducibly comparative… then scientific methodology cannot guarantee … that the preferred theory is true-only that it is epistemically superior to the other actually available contenders. But if all these contenders have been affected by social factors, nothing in the appraisal machinery will completely 'purify' the theory" (Okruhlik 1994). However, it does not seem that adding theories that are unavoidably influenced by non-cognitive values in the generation stage can guarantee that the preferred theory is true either. It would seem that adding a variety of theories

can only help to show that one theory may be epistemically superior to the others, but this is the same result that she says is obtained during the context of justification without adding other value-laden theories to the mix. That is, we are left to wonder given Okruhlik's account, that if all theories, even those generated that help bring biases to light, are contaminated then can there ever be objectivity in science. It could be possible that we are only increasing the amount of biases that are imported into theory content. Why does the fact that many people generating various theories mean that we have gotten rid of biases in theory content if they are inevitable in the context of discovery? Neither numbers nor variety seems to "guarantee" that we gain objectivity in theory generation. It could be that we are just increasing the number of bad theories. So if all available theories are biased then bias will continue to infect the theories we have to choose from and it does not seem to make a difference how many are generated. Thus, it does not seem clear that theories generated as a result of different perspectives based on different values and interests eliminate biases nor guarantee truth. In other words, her solution does not seem to solve the problem.

This leads to another issue with P1. We might say that Okruhlik's position indicates a type of genetic fallacy. For instance, let's say that I generate a theory about some phenomena from tossing tea leaves. This seems like a very irrational way to generate a theory. However, can we say that the theory is irrational just because it was generated in an irrational way? Can we say that the theory is "contaminated" by the value of superstition? What if we test the theory against what we find in the world and find that it accurately describes the phenomena and makes accurate predictions about it, in fact that it seems to be true. Does this mean that we would still have to label the theory as contaminated just because it was generated in an irrational way? I would think not. I think we need to make a distinction between *generating* a theory as a result of

non-cognitive values and *accepting* a theory because of non-cognitive values. The former may be appropriate, the latter is not. It does not seem that non-cognitive values contaminate theories that are accepted just because they may help generate them. Non-cognitive values would seem to contaminate a theory in the justification process only if one accepts a theory because of why or how it was generated. Or put another way, the superstitiously generated theory could prove to be true in spite of how it was generated; if the theory is shown to be true it makes no difference how it was generated. The source of the generation does not matter to its truthfulness.

Thus, P1 seems to be problematic and, just by adding theories even from different sources, does not seem to eliminate bias that P1 claims exists. Also, it may not be the case that theories are inevitably influenced by non-cognitive values in the generation stage, there may be ways to generate theories without value influence, and finally, even if they are so influenced that does not mean that the contamination necessarily affects the justification stage. In fact it is bad science to accept a theory based on how it was generated.

We have also seen that P2 and P4 are just a truism regarding comparing theories. This leaves P3 - theory choice is irreducibly comparative. I want to argue that P3 should be weakened. While inference to the best explanation through theory comparison is surely a part of theory selection sometimes, I do not think that it is always the case that theory selection is irreducibly comparative. I think sometimes theories are compared directly to the world without being compared to other theories to determine whether or not they are true. That is, it seems that we can say that even if theory evaluation partakes of comparison with other theories sometimes as part of the process, the most salient feature of theory assessment seems to be how well a particular theory matches the world. Scientists are justified in accepting or rejecting a theory

only if it meets a particular standard of evidence, not just because it is the best theory available when compared to others.

Ana Smith Iltis 2008 makes this point, although indirectly, in her discussion about human subjects of research and the assessment of outcomes of treatment:

> At one time, we accepted that ulcers should be treated with rest and dietary changes rather that antibiotics, which we later learned were necessary to fight the bacteria *Heliobacter pylori* that caused ulcers … Similarly, it was accepted that premature infants should be exposed to high concentrations of oxygen, which we later learned led to blindness… These are only examples of cases in which a certain approach to treating a condition became a standard of care without the systematic assessment of outcomes. Once outcomes were measured, the error in judgment was clear. The medical community has learned that true knowledge is available only by measuring outcomes, that is, by engaging in research (Iltis, 2008).

Note that Iltis is concerned with "measuring outcomes" not with comparing one theory to another. This is because the world pushes back on our theories. Exposing infants to high concentrations of oxygen led to blindness and we did not need another theory to see this. In fact, in order to eliminate a theory there is really no need for a comparison between theories. It would seem that one could make an honest evaluation without necessarily having to compare two or more different alternatives. For instance, one could determine that a particular key does not fit a lock without having to compare it to other keys. Or one could determine that some notion has an internal contradiction and thus can be dismissed without the need to compare it with another position. A distinction can be made between a 'test' which can be interpreted as a way to determine if something does what it is suppose to do and 'comparing alternatives' which

may mean determining if something is the best way to do something. We can evaluate something in its own right by testing it without having to compare it with something else. Dembski asks us to "consider the following hypothesis: 'The moon is made of cheese." One does not need additional hypotheses (e.g., 'The moon is a great ball of nylon.') to eliminate the moon-is-made-of-cheese hypothesis." He argues that:

> There are plenty of hypotheses that we eliminate in isolation, and for which additional competing hypotheses do nothing to assist in eliminating them. Indeed, often with scientific problems we are fortunate if we can offer even a single hypothesis as a proposed solution …. What's more, a proposed solution may be so poor and unacceptable that it can rightly be eliminated without proposing an alternative (e.g., the moon-is-made-of-cheese hypothesis). It is not a requirement of logic that eliminating a hypothesis means superseding it (Dembski 2002).

Thus, while in many instances we compare theories to one another to assist in choosing one over another it is not the only way theories are chosen or eliminated.

Another way to put this is to say that sometimes we can know a theory is not a good one because of its failings even before there is another theory to compare it with. McMullin, 1987 suggests, "A theory with features that are perceived as *ad hoc*, for example, may be perfectly adequate as a means of accurate prediction, but these features will count against the theory as explanation and will prompt efforts to find an alternative." Note that in this instance even before an alternative is found the theory is not considered as a good explanation.

In chapter two we considered two approaches to the race question. One approach was the essentialism concept; the other was the populationism concept. We found that when new and better science was done essentialism was replaced by the populationism concept. In this instance,

it seems theory comparison was at work. The new research showed that populationism is a better explanation in biology. However, when we considered the various theories of race that were based on the population approach we did not compare them with one another we considered each theory in its own light and found it wanting. Thus, theory choice is not irreducibly comparative; theories can be compared directly to the world and if all the theories we have are problematic I think we would have to say that none are acceptable and we need to keep looking, but I do not believe we should say we need to accept theory x because it is the best of what we have.

What I have attempted to show then, is that regardless of what takes place in the context of discovery, theory choice does not have to be "contaminated" by non-cognitive values. First, it is not clear that theories generated in the context of discovery must be inevitably influenced by non-cognitive values. Second, while it is appropriate in many instances to compare theories to help determination the best explanation, this is not the only way to choose a theory, nor should we accept a theory just because it is the best theory we have, we can always withhold judgment until something better comes along.

The Context of Investigation

Research has been defined as gathering information to answer a question that solves a problem (Booth et. al. 2008). Non-cognitive values can enter into this context in a variety of ways. They may influence how researchers go about their work, what observations may be made, and how well evidence is appraised; why we show interest in a subject and how diligently we pursue it. They may function as selectors of different fields of investigation, what looks interesting and important to us, and the choice of problems to be investigated as well as how resources will be committed to those projects and how many resources will be committed. They determine what

the moral constraints on investigation are, and act to place restrictions on experimentation, especially but not exclusively on humans. By making value judgments explicit, scientists will be more likely to pay attention to them and thus assess them in critical ways. Thus, it seems like why we show interest in a particular question and how diligently we pursue it are legitimate roles for non-cognitive values to play in the context of investigation.

However, because a problem is important for social or political reasons or because a scientist is interested in solving it does not seem to give it a direct bearing on whether a theory regarding that problem is true, confirmed, or rationally credible. As Mauro Dorato has indicated, economic, moral, or religious points of view may illuminate some phenomenon and may function as a selector of some facts as causally relevant factors. However, once we try to explain the causal relevance of say, economic factors in the phenomenon, by using economics as a selective principle, the causal link we thereby establish is (or is not) valid, according to the evidence it has, independently of our particular economic (or religious) convictions. For instance, military and political interests may have motivated the study of the theory of trajectory of projectiles but the epistemic warrant of the theory enabling us to do the calculations does not depend on the different military and political interests (Dorato, 2004). Thus, while non-cognitive values may legitimately provide motivation for research and even motivation to believe a claim, they should not be understood as reasons that provide support for a claim. That is, while non-cognitive values can be completely acceptable at the investigation stage, we can hold that such judgments are not acceptable at the evaluation stage. They should not be used to determine the truth about what they motivated us to find.

The Context of Application

In this context non-cognitive values can have a legitimate function as guides to the application of our scientific knowledge in practical decision making. That is, they may play a role in determining at what level of certainly a scientific theory needs to be before it is used as a guide for action. Whenever the application of scientific knowledge has non-cognitive consequences, especially adverse effects, then we may be obligated to refrain from applying that knowledge. Also, when we are ignorant about the outcome of applying a theory we should consider the consequences of our decision to apply the theory, especially if the application could result in adverse consequences. Notice we are not talking about whether we should accept or reject a theory because of the perceived consequences if it is applied. If we believe a theory is true it should be accepted regardless of the outcome if it is applied. If we believe a theory is false it should be rejected regardless of the outcome if it is applied. If we are unsure whether a theory is true or false we should withhold judgment until we have more evidence, especially if the application of the theory could result in unwanted adverse consequences.

Note how this position might be used in our examination of theories of biological race; legitimately we could chose to apply or not to apply a theory about biological race because it has adverse consequences or perhaps, little adverse consequences. The argument could run something like: in the debate about biological race the application of scientific knowledge has non-epistemic consequences that could be adverse, for instance, in determining the biological status of others in society that status may be used to support social injustice or inequitable divisions, so the relevance of the consequences should be involved in determining whether to apply a theory or not because there are political and ethical, that is non-cognitive values involved in the application. Thus in this scenario, non-cognitive values ought to be involved in whether

the theory should be applied. Or in the debate about biological race the application of scientific knowledge has consequences that could be beneficial, for instance, in determining the biological status of others in society we could find that certain diseases and treatment responses cluster by race and thus, in determining race we will be aided in determining who has what diseases and how they might be treated, so the relevance of the consequences should be involved in determining whether the theory should be applied. But in the debate about biological race we do not acceptance or reject race as a biological category because doing one or the other may lead to adverse consequences. That is, we ought not to believe that races do not exist or do exist just because there could be adverse non-epistemic consequences as a result of so believing. This would be to allow, I believe illegitimately, for non-cognitive, perhaps social and political values to determine the way we believe the physical world is made up, or at least our fear about the way the world might be made up to determine the way it is, and our social and political values and fears do not seem to have a direct bearing on whether a theory about the world is true, confirmed, rationally credible, or epistemically warranted.

One way that this context has been used to argue for allowing non-cognitive values to influence theory choice has been called the argument from "inductive risk." This argument has several supporters (Rudner 1953; Kitcher 1985, 1997; and Douglas 2000, 2006, 2009). The argument maintains that the envisioned consequences of a theory if wrong should be an input in deciding whether a theory should be accepted or rejected.

While I believe that in theory application consequences should be considered, the question here is, "should consequences determine theory choice?" That is, should the envisioned results of what may happen if a theory is *applied* determine whether or not the theory should be *accepted*? I believe not. A theory or hypothesis is more likely to be true if supported

by empirical evidence, and epistemic and cognitively relevant reasons than being supported by my desire to avoid adverse consequences if the theory is false. My desires ought not to influence theory acceptance because they do not determine or track the way the world is. A key question in this regard is can the acceptance of a theory and the application of that theory be separated from one another; I believe scientists can make this distinction and I will argue this below.

I will briefly examine Rudner's and Kitcher's claims regarding this matter and argue that allowing consequences if wrong to determine theory choice is bad science, and then I will consider Douglas's argument from inductive risk and argue following Mitchell 2004, that it conflates the role of the scientist in determining the acceptance of a theory with the scientist's role in applying policy regarding a theory, that is, it conflates *action* regarding a theory with *belief* about a theory.

An early statement of the argument from inductive risk comes from Rudner 1953 who argues that scientists make value judgments in choosing between hypotheses because a scientist must decide whether or not to accept or reject a hypothesis. But no scientific hypothesis is ever completely verified. Therefore the scientist must make a decision that evidence is sufficiently strong to warrant the acceptance of the hypothesis. This decision is not based solely on the evidence but for Rudner, on the *importance,* in an ethical sense (and thus a non-cognitive value judgment), of making a mistake in accepting or rejecting the hypothesis. He indicates, "how great a risk one is willing to take of being wrong in accepting or rejecting the hypothesis will depend upon how seriously in the typically ethical sense one views the consequences of making a mistake" (1953, 3). Thus the acceptance or rejection of a hypothesis is based on non-cognitive value judgments.

Rudner's argument can be formed as follows:

P1 – Scientists either accept or reject a hypothesis

P2 - But no scientific hypothesis is ever completely verified

P3 – Thus, before accepting or rejecting a hypothesis a value judgment must be made in light of the importance of a mistake if wrong.

C – Therefore, value judgments about the consequences of being wrong are unavoidable in accepting or rejecting a scientific hypothesis.

Kitcher 1985, in arguing against sociobiology, maintains that the envisioned consequences of the acceptance or rejection of a given scientific theory should be part of deciding whether the theory is accepted or not. He is concerned that "if we are wrong about the bases of human social behavior, if we abandon the goal of a fair distribution of the benefits and burdens of society because we accept faulty hypotheses about ourselves and our environmental history, then the consequences of a scientific mistake may be grave indeed" (1985, 9). Thus, given this important concern he argues that political considerations should play a role in determining when evidence is sufficiently strong for accepting a theory:

> Everybody ought to agree that, *given sufficient evidence* for some hypothesis about humans, we should accept that hypothesis whatever its political implications. But the question of what counts as sufficient evidence is not independent of the political consequences. If the costs of being wrong are sufficiently high, then it is reasonable and responsible to ask for more evidence than is demanded in situations where mistakes are relatively innocuous (1985, 9).

Lest we not be clear, Kitcher clarifies his position in a later paper:

> …where the stakes are high we must demand more of those who claim to resolve the issue. So, it was relevant to point to the political consequences of accepting some of Wilson's claims about human nature because doing so makes us aware of the need for more rigorous arguments and greater certainty in this area (1997, 280).

I understand Rudner's use of "the *importance,* in an ethical sense" as the use of non-cognitive values and Kitcher's "political consequences" as related to non-cognitive values. Also, it needs to be pointed out that Rudner and Kitcher are not talking only about the application of theories but the acceptance or rejection of theories, that is, which theories ought to be believed or not. As Steel 2010 puts it, "what makes the argument from inductive risk controversial is its claim that nonepistemic values should influence what hypotheses and theories scientists believe, not merely what research projects they choose to pursue or hypotheses they choose to investigate" (2010, 17).

Thus, for this position acceptance is in view; the consequences of a hypothesis help determine the level of evidence that is needed to accept it. If my hypothesis has bad consequences if I am wrong, I need strong evidence for it before I accept it and if my hypothesis does not have bad consequences, if it is "relatively innocuous," as Kitcher says, then the evidence need not be as strong to accept it. So for example, if I have evidence that race is biologically real and you give me reasons why believing this has bad consequences if wrong, according to this view those consequences ought to cause me to need a greater level of evidence before I should believe race is biological real. Or, conversely, if I have less evidence to show that biological race does not exist and we agree that the nonexistence of biological race does not lead to bad consequences, then I should believe that less evidence suffices to accept the position that biological race does not exist. But this seems wrong.

There are several ways one might criticize the argument from inductive risk: One could argue that it is not the scientist's job to accept or reject a theory (Jeffrey 1956) or similarly one could argue that the scientist when faced with uncertainty just needs to be clear about the uncertainty without making a judgment regarding a theory. One could argue that concern about consequences does nothing to refute a theory, that is, show it to be wrong, and thus it should not be used to determine theory choice. Or one could argue that the argument from inductive risk fails to make a distinction between a scientist's belief and a scientist's action toward that belief and as a result unnecessarily attributes non-cognitive value judgments to theory choice. Let's consider these various objections.

Accepting or rejecting a theory: Consider Rudner's P1 - Scientists either accept or reject a hypothesis. Is it the scientists' job to accept or reject a hypothesis? Richard Jeffrey 1956 argued early on that it was not. Jeffrey felt that the activity proper to the scientist is the assignment of probabilities to theories. For Jeffrey, scientists do not accept or reject theories, this is left to others. The work of the scientist is to assign probabilities.

Jeffrey asks us to consider a sample of polio vaccine in a certain lot. It is tested and found to be free of active polio virus. This imparts a certain probability to the hypothesis that the entire lot is good. He asks, "Is this probability high enough for us to rationally accept the hypothesis?" He then contrasts this with a similar problem regarding roller skate ball bearings. A sample is tested and found satisfactory and thus gives evidence at the same level of probability as the vaccine case that the whole is good. He suggests, as he believes Rudner points out, that if the probabilities were just enough to lead us to accept the bearings, we should reject the vaccine because of the graver consequences. But, he asks, what determines these consequences? There is nothing in the hypothesis, "This vaccine is free from active polio virus," to tell us what the

vaccine is for, or what would happen if the statement were accepted when false (1956, 242). He adds that for all we know from the hypothesis it might be intended for inoculating pet monkeys and thus, one's confidence might be high enough for this action without being high enough for inoculation of children. So, for Jeffrey, there is a distinction between accepting a hypothesis (this vaccine is free from active polio virus) verses an action as a result of accepting a hypothesis (let's inoculate children with this vaccine). He adds, "in the case of law-like scientific hypotheses the distinction seems to be invariably necessary; there it is certainly meaningless to speak of the cost of mistaken acceptance or rejection, for by its nature a punitive scientific law will be relevant in a great diversity of choice situations among which the cost of a mistake will vary greatly" (243). Thus, one can take from Jeffrey's argument that there is a distinction between accepting or rejecting a hypothesis and acting upon a hypothesis, and what determines the consequences of a hypothesis is how the hypothesis is acted upon. Since the scientist cannot determine or know all the ways a hypothesis might be acted upon, the scientists cannot know or determine all the consequences of accepting or rejecting a hypothesis. Thus, the scientist's proper role is to provide the rational agents in the society which he represents with probabilities for a hypothesis in given situations; it is not the business of the scientist as such, least of all of the scientist who works with law-like hypotheses, to accept or reject hypotheses.

Rudner had attempted to already deal with this type of objection in his paper. He suggests that even with this type of account the scientist is still *accepting* the degree of probability:

> But a little reflection will show that the plausibility of this objection is apparent merely. For the determination that the degree of confirmation is say, *p*, or that the strength of the evidence is such and such, which is on this view being held to be the indispensable task

> of the scientist *qua* scientist, is clearly nothing more *than the acceptance by the scientist of the hypothesis that the degree of confirmation is p or that the strength of the evidence is such and such*… (1953, 4).

Douglas, in weighing in on this discussion, accepts Rudner's rejoinder to Jeffrey regarding this issue. She suggests that Jeffrey provided no direct response to Rudner's argument (2009, 54). But it seems to me that Jeffrey did acknowledge this objection. He replies that it is no more the business of the scientist to "accept" hypotheses about degrees of confidence than it is to accept hypotheses of any other sort (1956, 246).

Now what of Jeffrey's probability objection? It seems Jeffrey's distinction between accepting or rejecting a hypothesis verses acting upon a hypothesis gets it right and I will say more about this below, but what about his claim that scientists do not accept or reject theories but that the activity proper to the scientist is the assignment of probabilities to theories? This does not seem to be the case. Consider the following scientific theories: the Ptolemaic view of the universe, the caloric view of heat, the phlogiston theory of burning, and the existence of ether wind. Scientists have not assigned degrees of probabilities to these theories, they have rejected them. They have determined that these theories are not reliable, do not warrant belief, and are not supported by the evidence. If to reject a theory means that one does not believe the theory is correct in these ways, then it seems that scientists have rejected these theories. And since scientists do make these types of claims about theories it seems, contrary to Jeffrey, that scientists do accept or reject theories.

However, it does seem that Jeffrey gets it right with regard to his distinction between accepting a hypothesis verses an action as a result of accepting a hypothesis. If we do not know how a hypothesis is going to be applied it seems difficult to maintain that the consequences if

wrong need to be considered. Thus, while I agree with Rudner that scientists can either accept or reject hypotheses, I do not agree that the consequences if wrong need to be taken into consideration for the acceptance or rejection of a theory, as I believe Jeffrey shows, we need to know how the theory will be applied to determine how and if the consequences should be considered before we take action. This separation of belief from application and the understanding that non-cognitive values may enter in at the application stage and not the belief stage of theory development seems to show the proper role for non-cognitive values.

However, there does seem to be another stance one can take towards a theory, scientists do not have to accept or reject a particular theory; there are instances in which a scientist can remain neutral about a theory. That is, as indicated above, one can always withhold judgment until something better comes along or until there is more evidence. Thus, if the consequences if wrong appear to be too adverse then the scientist can just hold out applying the hypothesis until more is known about the hypothesis. Why accept or reject a theory that we do not have enough information about to make a good decision? Presumably, we make a decision from ignorance like this because we need to apply the theory in some way and thus we are not afforded the luxury of waiting, but as we have suggested, the need to apply a theory is different from accepting or rejecting a theory.

Finally, as noted in the previous section, a theory can be rejected by just considering the merits of the theory, that is, the evidence for or against it, even before the consequences if wrong are considered, and thus the intrusion of values seem to be eliminated in this instance.

Consequences and evidence: As we have seen, Kitcher suggests that if the costs of being wrong are sufficiently high, then it is reasonable and responsible to ask for more evidence, and Rudner says, “Obviously our decision regarding the evidence and respecting how strong is

'strong enough', is going to be a function of the importance, in the typically ethical sense, of making a mistake in accepting or rejecting the hypothesis" 1953, 2. As I understand it, the claim seems to be about the function of the importance of making a mistake plays in our decision regarding the strength of the evidence. That is, according to the proponents of the inductive risk argument, we need stronger or less strong evidence based on the importance of making a mistake in accepting a hypothesis. If the importance of making a mistake is high we need greater evidence in order to accept a theory, if the importance of making a mistake is low we need lesser evidence in order to accept a theory.

Thus, if I believe T because of E but T has the adverse consequence of A if wrong, then A must cause me to need more evidence. Thus, if I believe race is biological real because there is variation in human populations but holding that biological race exists if wrong, has the consequence of unjust social inequality, then we must say that the fear that unjust social inequality may exist if the belief that races are biological real causes me to desire more evidence. They, Rudner and Kitcher, both seem to be concerned about the high cost of being wrong and the cost seems to be undesirable consequences. Rudner fears the "seriousness of a mistake" and Kitcher fears "the costs of being wrong." The question here is should my desire to avoid the bad consequences of a theory, if wrong lead me not to accept it? It does not seem that this is the case. It seems more correct to say that I should seek more evidence if I believe my theory is true and you have reason to believe my theory is *false*, as oppose to saying I should seek more evidence if you have reasons to believe that my theory *has bad consequences if wrong*. If we are inclined to demand more or less evidence for a theory because we believe it will bring about good or bad consequences, then it seems our desire for good consequences or to avoid bad consequences and not the evidence is driving our beliefs. Sesardic 2005 suggests:

> Kitcher's recommendation that political considerations should play a role in determining when evidence is "sufficiently strong" for acceptance is a recipe for epistemic irrationally. He exhorts us to "over-believe" theories with beneficial political consequences and "under-believe" theories with harmful consequences. The result is that the fine Humean advice that "a wise man proportions his belief to the evidence" is thereby being replaced with the advice that "a wise man proportions his belief (at least in part) to the envisioned consequences of his belief" (2005, 195).

Now there is a way to allow consequences to be legitimately considered when judging a theory. But this notion of consequences has to do with what happens after a theory is applied not before a theory is accepted. If the theory predicted a set of consequences that did not come about then we would have rational grounds for questioning the theory. But this is different from seeking more evidence for a theory because the consequences we obtain if we are wrong may not be desirable. In fact this argument from inductive risk seems to have the result that for any theory if its consequences have sufficiently great adverse outcomes if mistaken then we should never accept it no matter how much evidence there is for the theory. That is, the greater the adverse consequences if wrong the greater the evidence needed to accept the theory and if the adverse consequences mean ultimate destruction then the theory should never be accepted. Let's consider a real life example.

Albert Einstein postulated the equivalence of matter and energy in his famous theory $E=mc^2$. Given this theory physicists in the 1940's knew that a fission bomb was possible. With the right amount of uranium a chain reaction of atoms could release a large amount of energy. So Einstein's ideas lead to a theory about the possibility of producing fission bombs. Chet Raymo tells of one fear in this regard:

> Physicist Edward Teller considered another possibility. The huge temperature of a fission explosion -- tens of millions of degrees -- could fuse together nuclei of light elements, such as hydrogen, a process that also releases energy (later, this insight would be the basis for hydrogen bombs). If the temperature of a detonation was high enough, nitrogen atoms in the atmosphere would fuse, releasing energy. Ignition of atmospheric nitrogen might cause hydrogen in the oceans to fuse. The Trinity experiment might inadvertently turn the entire planet into a chain-reaction fusion bomb.[13]

First, note the consequences of testing the fission bomb theory if wrong is the end of the earth, as least as we know it. If we believe those who support the inductive risk claim then we should say not only should the theory not have been tested until they had gotten more evidence, but it should have never even been *accepted* because of the adverse consequences if wrong. The theory should never have been believed. That is, no amount of evidence would warrant an acceptance of the theory because of the adverse consequences if wrong. Perhaps we would have been better off if they had not accepted the theory but of course that would not have meant that the theory was wrong and this kind of head in the sand decision may have allowed the Germans, who were also working on a fission bomb, to develop it before we did.

Rudner wondered about this issue. This is what he has to say:

> It would be interesting and instructive, for example, to know just how high a degree of probability the Manhattan Project scientists demanded for the hypothesis that no uncontrollable pervasive chain reaction would occur, before they proceeded with the first atomic bomb detonation or first activated the Chicago pile above a critical level. It would

[13] This was found at http://www.sciencemusings.com/2005/10/what-didnt-happen.html, last assessed, August 15, 2013.

> be equally interesting and instructive to know why they decided that *that* probability value (if one was decided upon) was high enough rather than one which was higher; and perhaps most interesting of all to learn whether the problem in this form was brought to consciousness at all (1953, 542).

Note what Rudner's concern is. He is not concerned with whether or not the hypothesis was accepted but with "just how high a degree of probability the Manhattan Project scientists demanded for the hypothesis that no uncontrollable pervasive chain reaction would occur, before they *proceeded with the first atomic bomb detonation,*" that is, whether or not the hypothesis warranted application. Now clearly, fission theory was accepted, but there was a concern (and rightly so) about the application of the theory, that is what would happen when it was first tried. Rudner seems to be equating acceptance with application and as has been indicated these are two different notions. Acceptance has to do with my belief; application has to do with my actions. I can accept that a theory is true but choose never to apply it because of the adverse consequences if I am wrong. For instance, I accept that James Earl Ray assassinated Martin Luther King. But if you were to ask me to bet my daughter's life on it I would not accept the bet. The consequences if wrong would be far too adverse.

Or suppose I watch for several hours as an expert acrobat high in the air rolls a wheelbarrow across a wire. He does this with an empty wheelbarrow, he does it with the wheelbarrow filled with materials, and he even does it several times with a person in the wheelbarrow. If after I watch this feat one was to ask me if I had sufficient evidence to believe and even claim that the acrobat could roll his wheelbarrow across the wire, even with someone in it I would have to say, yes. However, (and you know what is coming) if one were to ask me to get into the wheelbarrow I would probably say no. Why? Because the evidence that is sufficient

for me to believe that the acrobat can role his wheelbarrow across the wire with a person in it would not be enough for me to take the act of trust required to get into the wheelbarrow, the consequences if wrong are too high (no pun intended).

Thus, as we have seen, the Rudner/Kitcher thesis implies that for any theory, if its consequences have sufficiently great adverse outcomes if mistaken then we should never accept it no matter how much evidence there is for the theory. That is, the greater the expected adverse consequences if wrong the greater the evidence needed to accept the theory and if the adverse consequences mean ultimate destruction then the theory should never be accepted. This is not a correct approach since a theory could be accepted but not tested because of the adverse consequences if wrong.

What we can say is, the greater the adverse consequences if wrong the greater the evidence needed to *act* on a belief and claim, and if the adverse consequences mean ultimate destruction (in our wheelbarrow case my falling to my destruction) then the belief and claim should never be acted upon. Thus, we can see that a belief is different than an action regarding the belief and confusing the two leads to the incorrect suggestion that non-cognitive values are unavoidable in theory choice.

What if we took the Rudner/Kitcher proposal seriously and downgraded the plausibility of theories that were politically or ethically sensitive, that is, "according to the new rules of the game, a scientific theory is pronounced less acceptable in proportion to the perceived political [or ethical] danger of its [consequences]" Sesardic, 2005. It seems that several effects could take place:

1. We would have to wonder when scientists pronounce a theory as acceptable if it was a result of the evidence or the lack of adverse consequences if the theory were wrong.

2. If my scientific paper regarding a theory was accepted for publication or more importantly rejected would I need to wonder if it was a result of some adverse political or ethical consequences of the theory? I am reminded of what happened to Galileo. His theory was rejected in part because of the adverse religious consequences that the theory implied. In fact, for the Church no amount of evidence would be considered for the theory.

3. Or perhaps, would there be stricter standards for papers, presentations, etc. on political and ethically adverse theories.

4. Even worse, would censorship be appropriate for some works. “This theory should be condemned because of the adverse consequences if accepted!”

These ideas seem silly but if adverse consequences if wrong determine the level of evidence needed for acceptance of theories, why not for other aspects of science?[14]

The idea about accepting or rejecting of a theory as a result of the perceived adverse consequences seems to be the type of thing that Gill, who we quoted earlier, was concerned about with regard to race. He called it bias. Perhaps it will be helpful to look again at what he had to say.

> Why this bias from the "race denial" faction? This bias seems to stem largely from socio-political motivation and not science at all. For the time being at least, the people in "race denial" are in "reality denial" as well. Their motivation (a positive one) is that they have come to believe that the race concept is socially dangerous. In other words, they have convinced themselves that race promotes racism. Therefore, they have pushed the politically correct agenda that human races are not biologically real, no matter what the evidence (Gill 2000).

[14] See Sesardic 2005 where he attempts to make a case that these very things have happened.

While one may take issue with Gill's questioning the motives of those who do not agree with him, I agree that holding or rejecting a theory as a result of the belief that the theory may have "dangerous" consequences is bad science. It allows non-cognitive values to illegitimately enter into theory choice.

Claims and actions: Douglas 2009 argues that scientists should not (a normative claim) decide which empirical claims are adequately supported with no thought to the importance of these claims to society. She maintains that scientists should consider the potential social and ethical consequences of error in their work and more specifically they ought to take into account the consequences of being wrong when making empirical claims. They should weight the importance of consequences and set burdens of proof accordingly. Thus, non-cognitive ethical and social values ought to be involved when scientists are determining which claims to make. Douglas's position is similar to the Rudner/Kitcher position except that her focus is on scientific claims rather than scientific theory acceptance. Non-cognitive values enter in because scientists ought to consider the consequences of error when making claims about theories. This position seems to be based on two theses; first, Responsibility (R) - the notion that all of us have a responsibility to consider the consequences of error when deliberating over choices, and that scientists do not have any special status that allows them to avoid this general responsibility. Secondly, Authority (A) - that since scientists have a type of public authority in our society and empirical claims are public actions, scientists can be considered reckless or negligent if they improperly consider the consequences of error in their claims.

For Douglas, (R) gives scientists the moral obligation to take into account ethical and social, that is, non-cognitive values when they make choices about what to believe and claim, and (A) highlights the importance of the responsibility. I will question whether scientific belief

claims apply to society in the way that Douglas suggests, and I will argue that (A) is at best only applied to scientists when they are acting as advisors. Let us consider first the case of (R).

Douglas notes that the literature on moral responsibility in recent years has focused on three different sorts of issues: (a) competence – when is a person morally capable of making moral decisions, (b) coercion – what forces upon a person make their moral decisions not their own, and (c) causation – what conception of causality allows for a person to be responsible for their action. She does not believe that any of these issues are illuminating for her concerns. I note these issues because while I agree that some of this literature is not relevant to science per se, I believe she dismisses the issue of competence too easily and so I will return to it shortly.

Rather than deal with a, b, and c, Douglas moves on to the question of what do we mean by moral responsibility and what our responsibilities are with respect to consequences that we do not intend to cause. To start, moral responsibility is not the same as causal responsibility. One may be partially causally responsible for the actions of one's great-grandchildren, but most would say that one is not morally responsible that is, that one should not be blamed or praised for what they do. She believes the notion of giving blame or praise is a marker of the distinction between moral and casual responsibility.

> When does a causal responsibility turn into a moral responsibility? Minimally, we are morally responsible for those things we intend to bring about. In these cases, a person chooses to do something deliberately, either because they think it is inherently the right (or wrong) thing to do or because of a particular sought consequence. The deliberate choice brings in the moral responsibility. Thus, if I intend to help or harm someone and I succeed, I am morally responsible in both cases, and usually praiseworthy in the former, blameworthy in the latter (2009, 68).

So when does causal responsibility turn into moral responsibility? She believes, first, that we are morally responsible for those things we intend to bring about but additionally, that if we intend an action we are also morally responsible for side effects of that action even if these side effects are not intended. She suggests that two general categories cover unintended consequences, recklessness and negligence. She writes, "When one knowingly creates an unreasonable risk to self and others, one is reckless; when one unknowingly but faultily creates such a risk, one is negligent" (2009, 68). Thus, recklessness is being aware of creating an unreasonable risk, while being negligent is unknowingly but faultily creating a risk. By "faultily" she means that the risk the negligent person created should have been known, like starting a fire on a windy dry day that then spreads to the neighbor's house. Any "reasonable person" should have taken steps to control such a fire before it was started and thus this person was negligent. The distinction between recklessness and negligence, then, rests on whether the person reflected on the potential consequences of events going as planned or errors occurring, and on whether there was any attempt to prevent possible harms arising from the chosen action. Thus, recklessness is proceeding in the face of unreasonable risk and negligence is the failure to foresee and mitigate such risk.

Douglas believes that this type of analysis of moral responsibility applies not just to our actions but also to descriptive claims. She suggest that "making empirical claims should be considered as a kind of action, with often identifiable consequences to be considered, and as a kind of belief formation process" (2009, 70).

Her example is the unattended briefcase.

> Suppose one sees an unattended briefcase. Should one report the possible presence of a bomb? There are clear risks of error for making the descriptive claim that a bomb may be present. If one does not report it and it is a bomb, death and destruction may result. If one does report it and it is not, disruption of people's daily lives and distraction of resources away from more serious problems may result… [if] the briefcase is spotted in a busy subway station… one should report… this is the reasonable weighing of risk and uncertainty in this context…. [if the] briefcase [is] left in a college classroom, where the classroom is known to be used by a particularly absentminded colleague… while the consequences are similar, the uncertainties shift, and it is far more likely that it is the colleague's briefcase than a bomb. Checking with the colleague first is the more prudent measure. In both cases, we expect each other to reflect upon the risks of making a claim, particularly the consequences of error and the uncertainties and likelihoods involved. Thus, we can be negligent or reckless in the making of the descriptive or empirical claims (2009, 70).

Douglas maintains that this general moral responsibility holds also for scientists. Their scientific claims are a kind of "reporting", and they will have consequences so they have the same moral responsibility we all share for the intended consequences of their choices and claims, as well as for some of the unintended consequences, and they are responsible for the foreseeable consequences of their choices and claims, whether intended or not. And thus, (R) applies to scientist's empirical claims as well as their regular actions.

What of (A), the claim that since scientists have a type of public authority in our society and empirical claims are public actions, scientists can be considered reckless or negligent if they improperly consider the consequences of error in their claims? In arguing for (A) Douglas says

that scientific work is developed and discussed within a society that takes the claims made in the name of science with special authority. She maintains that there are two kinds of unintended foreseeable consequences that may be of concern for scientists. First, the consequences that will likely result as a side effect even if the knowledge produced is perfectly reliable and accurate. She set this aside because she indicates that the discussions regarding this center more on policy for science and democratic input into research agendas than on science for policy and its implications for scientific reasoning. The second is the potential unintended consequences of making inaccurate or unreliable empirical claims. She focuses on this latter aspect and comments:

> Given the public authority scientists wield, and the understanding of making empirical claims as a public action, there can be clear consequences for making a well-intended but still ultimately incorrect claim, just as with the briefcase example above. Thus, it seems that scientists can be reckless or negligent if they improperly consider the potential consequences of error based on their basic moral responsibilities" (2009, 72).

Thus, we see that scientists have the same basic moral responsibility as anyone else but because of the authority they have in our society it makes their claim especially important. Douglas's general argument can be formed as follows:

P1 - Scientists have public authority in our society

P2 - The empirical claims that scientists make are public actions

P3 - There can be social and political consequences when scientists make empirical claims in error

P4 - Thus, scientists can be morally reckless and negligent if they do not adequately consider the consequences of error when they make claims

C - So, scientists should consider (which involves social and political value judgments) the consequences of error when making empirical claims.

Thus, we see for Douglas that making value judgments about the consequences of being wrong is obligatory when scientists make scientific claims.

Douglas considers three objections to her thesis. First, perhaps someone else should have the burden of moral responsibility of the consequences of error and not the scientist; we can call this Burden (B). After all why should the scientist have to worry about this? Regarding (B) she suggests that a) the scientist is probably in the best position and thus more qualified to know the consequences of error if wrong and, b) she does not believe the constant oversight that this would require would be desired by scientists. The second objection to her thesis, we will call it Knowledge (K), is that scientific knowledge is so valuable, that is the search for truth is held in such a high esteem that all other values are irrelevant before it. Thus, scientists should be exempt from considerations of the consequences of error. She does not believe that the evidence suggest that the knowledge gained allows for this. After all, limits are placed on for instance, the type of experiments that can be administered to humans and informed consent is required. The third type of objection, we can call it Distinction (D), is that a distinction should be made between when scientists are making general empirical claims and when they are considering actions that might harm others or when they are acting as advisors. She dismisses this claim as dubious based on the authority that scientist have. Thus, the general responsibility that is commonly shared is made more important because of scientific authority and because of this authority the making of claims by scientists cannot be separated from any actions regarding theories that they

may make. In fact, as we saw above, Douglas considers scientific claims as "a kind of action." Let us take a closer look at Douglas's claims.

First, I want to go back to (R). It will be recalled that Douglas maintains that general moral responsibilities holds for scientists. They have the same moral responsibility we all share for the intended consequences of their choices and claims, as well as for some of the unintended consequences, and they are responsible for the foreseeable consequences of their choices and claims, whether intended or not. And thus, (R) applies to scientist's empirical claims as well as their actions.

Of course, there are scientific disciplines that do not seem to have socially adverse consequences if a scientific claim is in error; like for instance, astrophysics. It does not seem that a claim by a scientist that there may be life on other planets would have any adverse consequences if wrong. Or perhaps the consequences if wrong in this type of case would be so small that maybe it does not warrant responsibility if in error.

But are there any instances when scientists may not be aware of the consequences of error in making a claim but perhaps others more qualified are aware? This is a question about competence that Douglas dismissed as not illuminating for her concerns and argued that scientist are the ones who are the most aware of the work they are doing and so are the most able to consider the consequences if they are wrong. She also argued that scientists would not want to give up this responsibility in turn for constant oversight that this would require. It would seem that scientists may be able to fairly easily determine the consequences of an error when there are direct consequences to individuals involved. But we need to ask the question, "Are scientists always in the best position to know how the consequences of their errant claims might affect society"?

Let's consider a geneticist's claim that indeed race is a biological concept. Now we need to ask, how might this type of claim if wrong affect all of society? Should we expect the geneticists to know how this claim might affect society? Douglas suggests that the scientist is in the best position to know the effects of their claims because they are the most familiar with their work. But why would one expect an expert in genetics to know how her claims may affect society? Presumably, a claim about race would have both political and social consequences and so it seems we must expect the poor geneticist to know both the political effects of the claim and the social affects. Would we want the geneticists to do this evaluation in private? Consequences about a claim about race would seem to depend on many factors; it would seem that this type of analysis is far too complicated for any one person. And it would seem that it will depend on expertise that will be quite different from what the scientist making the claim will have. Should this not happen at another level? Should this evaluation be a social process especially if social consequences are involved? It would seem so. In fact, this would seem the purpose for review boards, scientific advisory panels, and the like.

One might object that Douglas is talking about more specific claims and the claim, "races exist" is too broad a claim for the type of analysis that Douglas is attempting do. The more appropriate example would be the claim that since race is real, a certain drug that seems to have an adverse effect on some population should not be used on that segment of society. Thus, the consequences of error from this type of claim (a person who may benefit from the drug would not get it) should be considered before the claim is made. But notice we have moved from a claim regarding the existence of races to a claim about how this possible fact should be applied and I have already suggested that non-cognitive values can have a legitimate function as guides to the application of our scientific knowledge in practical decision making. Whenever the

application of a scientific knowledge claim has non-cognitive consequences, especially adverse effects, then we may be obligated to consider the consequences of error before we apply them.

We need to consider another issue regarding (R). This is a concern regarding how Douglas attempted to link claims and actions. She suggested that making empirical claims should be considered as a kind of action. She did this with her briefcase example. The issue was should one report the presence of a bomb to the authorities because of the presence of a briefcase or not. Recall Douglas asked:

> Should one report the possible presence of a bomb? There are clear risks of error for making the descriptive claim that a bomb may be present. If one does not report it and it is a bomb, death and destruction may result. If one does report it and it is not, disruption of people's daily lives and distraction of resources away from more serious problems may result… (2009, 70)

She suggested that location and our background knowledge along with the consequences of error should be considered in determining how to report. First, let's agree that we have a general responsibility to report the briefcase. And let's agree that the possibility of there being a bomb in the briefcase may be more probable depending on where the briefcase is located. And finally, let's agree that we have some general responsibility in terms of how it is reported, given the consequences if we are in error. After all, one should not yell "fire" in a crowded theater unless one is fairly sure of it. Still I am not sure why we would jump to the far more serious claim of a bomb when all we can see is an unattended briefcase. Why not just report the presence of an unattended briefcase to authorities and then let someone in a better position than we are make a determination what the contents of the briefcase may be. That is, why say bomb, why not just say unattended briefcase? Even if the unattended briefcase was in the classroom of our

absentminded professor friend, why not just report the "fact" of an unattended briefcase to the proper authorities? If we do feel an obligation to our friend we can let him or her know that we saw an unattended briefcase in the classroom and reported it to campus security. After all better safe than sorry! And by the same token one can argue that scientists have a similar responsibility to give the "facts" when making empirical claims and it would seem that part of the facts could include the consequences if indeed the claim is in error. There does not seem to be any reason to go further than the evidence suggests unless one is being asked to make a decision about an action based on the claim. But at this juncture the scientist becomes an advisor and the responsibilities regarding advising seem to be different than holding a belief or making a claim. Mitchell 2004 makes a distinction between a policy question and a scientific question:

> To make reasoned choices in evaluating and accepting a hypothesis, epistemic values, broadly speaking, must play a role in determining those choices. In the context of science policy, however, the issue at hand is which actions or policies should be adopted on the basis of the beliefs that are warranted by those epistemic values. When scientists are involved in policymaking, the very same individual might invoke both sets of values…. One role is that of a scientist and as such the goals that determine the appropriate values that enter into judgment are those that secure *warranted belief*, for example, replicated experiments and large data sets. The other role, sometimes contractually acquired, is that of a government advisor in which the appropriate goals of government, often contested goals, determine the appropriate values that enter into judgment of *warranted advice* (emphasis added), 2004, 251.

Thus, she indicates that in the science aspect of judgment, the overriding goal is something like reliable information about the world. Any judgment that invokes a value known to counter the

obligation to this goal would be deemed illegitimate while in the policy area the goals regarding warranted advice are determined by for instance, governmental organizations, and may include political and social, that is, non-cognitive values.

Now we are in a position to speak to Douglas' dismissal of (D). It will be recalled that the third type of objection, (D), is a distinction should be made between when 1) scientists are making general empirical claims and when 2) they are considering actions that might harm others or when 3) they are acting as advisors was dismissed as dubious based on (A1). Based on our considerations above it may be that Douglas dismissed (D) too quickly. There certainly does seem to be a distinction between when scientists are making general empirical claims and when they are acting as advisors. As Mitchell indicates, in the policy area the goals regarding warranted advice are determined by governmental organizations, and may legitimately include political and social, that is, non-cognitive values. But the including of these values are not appropriate when scientists are considering warranted belief.

Finally, do all scientists have an authoritative position in society as Douglas seems to imply? Authority, to a first approximation, seems to be based on one's knowledge or skill and one's position. It would seem that the more salient of the conditions is one's position. One can have knowledge or skill but not be taken very seriously because one does not have the requisite position. Gregor Mendel had a tremendous amount of knowledge about the laws of the inheritance of traits but at least one of the reasons that his work was not recognized at first is because he did not have a position in the scientific community that allowed it to be. However, one can have the right position and little knowledge and still others must give way to one's authority. We can think of the President of some country who does not have any economic expertise speaking about what she plans to do economically in the next year that has a bullish or

bearish effect on the stock market. So it would seem that a scientist may have to be in a certain position in society in order to be authoritative, perhaps on some government committee or advisory board. The fact that a scientist is an expert in an area does not seem, by itself, to give that scientist an authoritative position in society.

Secondly, if a scientist does have the position to be heard, it still may not be the case that his or her claims carry prima facie authority. Even if a scientist can be heard there may be other experts with competing claims. Who would carry prima facie authority in this case? If we examine the dozens of articles that appear in scientific journals in a year we see that scientist make many claims and other scientists sometimes agree and sometime do not agree. Often unless the claim is very controversial the public never even hears about the claims. In the biological race debate, there are scientists on both sides of the issue. Who should one listen to? Even if there is scientific consensus on an issue this does not mean that the society as a hold agrees. For instance, while many anthropologists tell us that race is only a social concept, folk theories of race hold that races are based on biological traits (Glasgow, 2009).

It does not seem that scientists have the authority that Douglas believes they have. That is, it does not seem that Douglas's argument gives us a reason to believe that each scientist's claims are equivalent to actions that influence society. Since the linking of their claims with action was based on the authority that scientists have in society the link does not seem to hold. That is, scientists in general, except or unless they are on some panel or such, do not have the authoritative position in society that Douglas seems to think and so their claims do not qualify as a "type of action" as Douglas maintains and therefore, their claims can be separated from their actions. And so while I believe the consequences of error ought to be taken into account, they should be done so when one is considering applying the claim, that is, when considering the

actions that may be the result of claims. Thus, the consequences of error do not seem to require that non-cognitive values be required in scientific belief claims. We will move next to the context of assessment.

The Context of Assessment

Supporters of the position that non-cognitive values are unavoidable in science have available for them a very powerful argument when it comes to the context of assessment. It is found in the notion of underdetermation. According to this thesis, although evidence can enter into the explanation of theories, it is never enough to explain them because any evidence we might possess underdetermines the theory, that is, often more than one theory is compatible with the evidence. Thus, if evidence alone does not determine theory choice then perhaps something else is doing it, i.e., non-cognitive values. There are two major sources for the argument from inderdetermination, one historical and the other logical. The historical source is derived from Thomas Kuhn and the logical source is derived from Pierre Duhem and expanded by Willard van Orman Quine. I will examine these sources and comment on how their ideas have been suggested to allow for non-cognitive values to enter into scientific theory. We will begin with Kuhn

Kuhnan inderdetermination: Clearly the most controversial aspect in Kuhn has to do with what he has to say in *The Structure of Scientific Revolutions* regarding scientific revolutions and how paradigm change takes place. Critics and supporters alike took comments about religious conversions and political revolutions as an attack on the objectivity of science. Traditional cognitive values for theory choice, like predictive and explanatory power, seemed to be replaced with social and political ones. Notions of gestalt switches, incommensurability and the theory-

ladenness of observation seem to take objectivity and rationality out of the picture. Kuhn's account of theory change claimed that the historical record showed change to be one of revolution. Logic and data together were inadequate to require abandoning a prevailing theory or accepting a new one. The new theory's acceptance was based on non-logical judgments.

On the traditional view, theories are replaced because they are false, that is, they misrepresent reality, and the theories that replace them are accepted because they sometimes reveal this misrepresentation but more importantly they demonstrate that they represent reality more accurately. Kuhn's position in this area can be summarized as something like this: theories are replaced by revolutions which are not based on the rational standards of normal science, choice between one paradigm and another cannot be decided using only the values of normal science, something else, contributes to theory choice, it seems to be based on subjective preferences that allow for social, political or personal values to enter into it.

I want to briefly look at Kuhn's view of science from pre-paradigm to revolution. Then look at an example of how it was criticized, Kuhn's response, and give some evaluation of his response in light of our discussion regarding the influence of non-cognitive values in theory choice. In Chapter 4 we will return to some of Kuhn's ideas when we consider the types and function of values in science.

Kuhn describes movement in science from pre-paradigm science to a paradigm. In a paradigm normal science takes place until a crisis brought on by a buildup of anomalies results in a scientific revolution which leads to a new paradigm. Let us briefly look at each of these ideas.

The pre-paradigm period is a period before the development of a paradigm when science is not well organized and usually not very effective. It begins with some confusion as scientists attempt to explain new phenomena, which require identifying criteria and assumptions capable

of supporting explanations. At some point some important piece of work appears which is taken to provide insight into some part of the world and provides a model for further investigation and thus, a paradigm appears. A paradigm is a whole way of doing science in a particular field. It is a package of claims about the world, methods for gathering and analyzing data, and habits of scientific action that is the result of a specific achievement or exemplar that serves as a model that inspires and directs further work.

A paradigm's role is to organize scientific work which produces normal science. Normal science is work aimed at extending and refining the paradigm. During normal science scientists tend to agree on which problems are important, on how to approach these problems, on how to access possible solutions and also on how the world is like. Little to no debate about the fundamentals takes place, and work is done "puzzle-solving," that is, attempting to get new cases to fit smoothly into the framework provided by the paradigm. Failure is the fault of the scientists, not the theory.

However, under normal science anomalies build up. Anomalies are puzzles that resist solution. A crisis occurs when a large amount of anomalies have built up and confidence in a paradigm is loss. Revolutions are the rejection of one paradigm for another as a result of crisis. Kuhn indicates that the shift is like a conversion or a gestalt switch or a political revolution where persuasion has to take place. He indicates that:

> "As in political revolutions, so in paradigm choice – there is no standard higher than the assent of the relevant community. To discover how scientific revolutions are effected, we shall therefore have to examine not only the impact of nature and of logic, but also the techniques of persuasive argumentation effective within the quite special groups that constitute the community of scientists" (Kuhn, 1970, 94).

Thus, paradigm acceptance seems to be a political or sociological, and not a rational phenomenon. It is a matter of persuasion and not rational argument. One may ask why this is the case. Kuhn answers this by indicating that rival paradigms cannot be judged on a common scale because they are incommensurable, that is, they cannot be compared using a common standard of measurement. This means first, that people in different paradigms will not be able to fully communicate with each other, they will use key terms in different ways and thus, seem like speaking different languages. Second, this means even when it is possible to communicate, people in different paradigms will use different standards of evidence and arguments, because paradigms tend to bring with them their own standards of what counts as good evidence and arguments. Standards of evaluation vary too much across paradigms to be of decisive use. Third, if paradigms are incommensurable, then paradigm conversion is not likely to be a rational process, for reasoning itself would seem to be a paradigm-relative activity. What counts as good reasons for adopting a certain paradigm will differ in different paradigms.

Kuhn adopts a holistic conception of meaning. Because the meaning of a term or statement derives from the role it plays in a theory, changes elsewhere in the theory or paradigm can bring about significant changes in the meaning of a term or statement and thus people in different paradigms tend to talk past each other. And as we have mentioned, the two sides may appear to use the same terms when they attempt to settle differences through debate with each other, but they don't both mean the same thing by the use of those shared terms and so, the attempt fails.

The idea that competing paradigms are incommensurable is also supported by the theory-ladenness of observation. For Kuhn, observation is theory-laden. What people see depends in part on what they already believe or expect. Thus, Kuhn denies that we have access to a realm of

observational evidence that is independent of theory and could count as a source of meaning and evidence. As a result of this, paradigm-neutral observations cannot be used to judge between paradigms. That is, if it is true that all observations are contaminated by our theories or paradigms then the merits of each paradigm cannot be compared by subjecting them to testing, because different people in different paradigms will not agree about what is observed. Thus we see for Kuhn, that social and psychological values play an important role in scientific change and thus, science change is not free of non-cognitive values.

As was mentioned in the introduction, Kuhn's arguments were embraced by social scientists, historians, literary theorists, and some philosophers of science, who saw them as legitimating a critique of objectivity to which they were sympathetic. For instance, some feminist critiques of science have embraced some of Kuhn's ideas. They have argued that some theories, especially in biology, contain male bias. These theories are considered androcentric and thus influenced by personal, social or political values. Sandra Harding has proposed classifying feminist criticisms of science into three categories: feminist empiricism, standpoint epistemologies, and feminist postmodernism. Feminist empiricism regards science as essentially objective; however, when its methods and rules are not followed it results in the influence of social, political and gender biases leading to "bad science." Standpoint epistemologists maintain that contextual (social and political) values are essential to science and that some are better than others. Feminist post modernism abandons objectivity and maintains that different standpoints tell different stories about the world and none are any better than any other (Harding 1986).

The "strong program" in sociology of science also embraced Kuhn's ideas. These sociologists and other social scientists sought to study the close details of scientific work and concluded that scientific agreement was "constructed" via negotiation between people whose

main interests may not be in describing the way the world works. It is argued that non-cognitive social values to a large extent determine the results of science and that scientific change is not the result of the relations between scientific theories and the structure of the world but a social fabrication (Latour and Woolgar 2003).

Overall however, Kuhn's views came in for quite a lot of criticism. Kuhn's philosophy of science was accused of being irrational. Imre Lakatos dubbed his account of paradigm change as "mob psychology." He has been accused of being a relativist and of being a constructivist (Ladyman 2000). As an example we will consider Israel Scheffler's criticisms of Kuhn's ideas in *Science and Subjectivity*, 1982.

Scheffler offered several arguments against Kuhn's views. We will consider four of them. First, he felt that Kuhn's overall project was self-refuting. He argued that if paradigm debates are characterized by an "incompleteness of logical contact" between proponents of rival paradigms, and the transition to a new paradigm does not occur "by deliberation and interpretation," then it is self-defeating to justify this view itself by deliberation, appealing to factual evidence from the history of science. But if historians can transcend particular paradigms and evaluate them by appeal to neutral evidence, so can scientists, i.e., they can engage in rational paradigm debates which are perfectly intelligible (1982, 126).

Secondly, he argues that observational differences do not imply different objects. He maintains that it does not follow, from the fact that different paradigms organize their observations differently, that they are directed to different objects – that "after a revolution scientists are responding to a different world." From the fact that certain items are seen in varying ways under different categorizations, it cannot be inferred that they are not identical. One is reminded here of the story of the three blind men touching various parts of an elephant

and each proclaiming that the elephant is like the part that he alone is touching. Scheffler indicates that there is a contrast between seeing x and seeing x as something or other (1982, 126).

Third, Scheffler argues that incommensurability does not imply incomparability. He says that competing paradigms, for Kuhn, are addressed to different problems, embody different standards and different definitions of science; they are based on different meanings and operate in different worlds; and therefore, Kuhn argues, the proponents of competing paradigms are always at least slightly at cross-purposes and a conversion must take place before they can communicate. However, Scheffler maintains, if paradigms are indeed so different how can they be in competition? If they are indeed "rivals," they must be accessible to some shared perspective within which they can be compared (1982, 128).

Lastly, Scheffler accused Kuhn of being inconsistent in his criticism. He indicated that notions criticized by Kuhn reemerge often under new labels, in his theory. Thus, commensurability is implied by his emphasis on logical incompatibility. Falsification returns under the guise of anomaly, crisis, and loss of faith. Interpretation and deliberation are acknowledged in his emphasis on the promise of a new paradigm 'to resolve some outstanding and generally recognized problem that can be met no other way.' Scheffler noted that the critical distinction between theory-genesis and theory- justification is thus, in effect, reinstated. So Kuhn's denials taken alone form a radical and interesting departure from older views, but they seem clearly untenable. But Scheffler maintains that if we take just his affirmations of older concepts under new labels, we have a plausible but no longer novel view.

Kuhn's reply: In *Objectivity, Value Judgment, and Theory Choice*, 1977 Kuhn sought to distance himself from extreme views which give no role to rationality in the progress of science,

and which do not allow for comparison of the merits of theories within different paradigms. He offers a partial list of the characteristics of a good scientific theory which he assumes will be agreed upon by proponents of all paradigms. They are as follows:

1. Accuracy: A theory should be accurate within its domain, that is, consequences deducible from a theory should be in demonstrated agreement with the results of existing experiments and observations.
2. Consistency: A theory should be consistent, not only internally or with itself, but also with other currently accepted theories applicable to related aspects of nature.
3. Scope: It should have broad scope: in particular, a theory's consequences should extend far beyond the particular observations, laws, or sub-theories it was initially designed to explain.
4. Simplicity: It should be simple. Bringing order to phenomena that in its absence would be individually isolated and, as a set, confused.
5. Fruitfulness: A theory should be fruitful of new research findings: it should that is, disclose new phenomena or previously unnoted relationships among those already known.

These principles provide the shared basis for theory choice. But Kuhn thought that when these principles were expressed in a broad enough way to be common across all of science, they would be so vague that they would be powerless to settle hard cases. Also, these goals must be traded off against each other; emphasizing one will require downplaying another.

Kuhn maintained that when scientists must choose between competing theories, two people fully committed to the same list of criteria for choice may nevertheless reach different conclusions. Perhaps they interpret simplicity differently or have different convictions about the

range of fields within which the consistency criterion must be met. Or perhaps they agree about these matters but differ about the relative weights to be accorded to these or to other criteria when several are deployed together (Kuhn 1977).

So, in the end, since differing interpretations of these principles are inevitable and to some extent irresolvable by rational debate, the shared standards cannot provide a common ground for settling paradigm conflicts. Scientists must have recourse to personal, subjective, and psychological factors to tip the balance one way or the other.

Thus, we see that Kuhn divides science up into two distinct activities, normal science and revolutions. Looking within a period of normal science one can determine good work from bad, rational moves from irrational, and progress from lack of progress. But in a scientific revolution the rules break down, and subjective rather than objective factors seem to obtain and irrational factors seem to dominate. And it does not seem that Kuhn helped his case very much with his attempts to reply to his critics in Kuhn 1977. If the principles or values are not sufficient to determine what decisions scientists ought to make, then do they really aid his case for rationality?

Or perhaps Kuhn just over stated his case; which would be something like the claim that in theory change in science both rational and irrational elements play a part. We can characterize Kuhn's worries about the conflicts in his five principles as a worry concerning their imprecision; they are interpreted differently by different scientists or across different fields, and inconsistency; they may conflict with each other when being weighted. Since in the next chapter I will discuss the nature of cognitive values and how they work I will hold off on commenting on how principles like these might be used toward theory choice.

However, we should keep in mind that if this set of principles or something like them is so imprecise and inconsistent and they are used by scientists, why are we not faced with centuries of unresolved conflict regarding rival theories? Perhaps a set of principles like these have enabled scientists to come to consensus regarding rival theories. As one critic notes "[w]e can now see how to reply to Kuhn's point about differences in judgment concerning the application of the five ways and concerning the relative importance in cases where they point in different directions …. We can argue that the actual dialectical process which we do use in science to resolve these differences has served us well and hence we are justified in having general faith in it (Newton-Smith, 116).

The Duhem/Quine thesis: Tradition held that scientific theories could be confirmed based on observations, experience and experiment. Thus, when we base predictions on a certain theory, and those predictions turn out to be correct, this provides some evidence that the theory is correct. Popper 1959 argued that this was not the case, that scientific theories could not be confirmed by evidence but only falsified by it. So when scientists deduce a prediction from a hypothesis and then if the observation does not bear out the prediction when a relevant experiment is performed the hypothesis is falsified; thus, while positive instances could corroborate theories they could not confirm them. The job of the scientists for Popper was conjecture and refutation. Since the induction problem arises because no matter how many positive instances of a generalization are observed it is still possible that the next instance will falsify it, Popper believed that his falsificationism went a ways toward solving the induction problem. The Duhem/Quine thesis (DQT) can be used to argue against falsificationism. It maintains in its radical forms that theories can be retained "come what may." After explicating the DQT problem and explaining its relationship to Quine's strong underdetermination thesis I

will explain, following Laudan, why it is not the case that any statement can be retained "come what may" in the face of "recalcitrant experience."

There are three ideas associated with the DQT, a holism thesis, the no crucial experiments thesis, and an underdetermination thesis.[15] The holism thesis maintains that our beliefs face the "tribunal of experience" not singly but in a body. One cannot understand a particular thing without looking at its place in a larger whole. Our individual beliefs all by themselves do not carry empirical implications but only in conjunction with others, so only theoretical systems as a whole can be tested against experience. Thus, several auxiliary hypotheses as well as background assumptions are always needed to derive predictions when testing a theory. When faced with disconfirming evidence there are almost always auxiliary hypothesis and background assumptions involved that can be rejected rather than the theory being tested. For instance, some theory, T, together with auxiliary hypotheses, A, and background assumption, B, implies that some event, e, will be observed. But supposed that e is not observed. The form is like this:

T&A&B → e

Not e

Therefore, not (T&A&B)

The scientist can either deduce that theory T is false or she can blame the failure on auxiliary hypothesis A or background assumption B and maintain T.

So, given the role played by auxiliary hypothesis and background assumptions when we are testing a particular theory we are not really just testing the theory but we are also testing all

[15] For this way of laying out the issues see DeWitt, Richard 2004.

the auxiliary hypothesis and background assumptions that are associated with the theory. Thus, the holism thesis of DQT maintains that a hypothesis is typically not tested in isolation but rather what is tested is a whole group of claims, any of which could be rejected.

Another idea associated with DQT is the notion that there are no crucial experiments. The basic idea here is that when faced with two competing theories it should be possible to design a crucial experiment that would give conflicting predictions and this experiment should show at least one of the theories to be in error. However, given the holism of DQT, if tests are not just of theories but also of the various auxiliary hypothesis and background assumptions, then it seems that crucial experiments are not possible. This is because, as we have seen, when a theory is tested and disconfirming evidence is found one can make adjustments to or reject the auxiliary hypothesis or background assumptions, without rejecting the theory. Thus, the negative result obtained by an experiment could show only that an auxiliary hypothesis or a background assumption is false but not the theory being tested. Since no experiment can conclusively falsify a theory, no critical experiment is possible.

The third idea associated with DQT is the underdetermination of theories. Underdetermination arguments indicate that the data is often compatible with more than one theory. Thus, data underdetermines a theory when the data are insufficient to determine which of several theories is true. As seen above, because of the holism thesis, theories can generally be preserved in the face of disconfirming evidence, and generally it will be difficult to design a crucial experiment to decide between competing theories. As a result we can see that the available data, including the outcomes of experiments, can never fully determine that a particular theory is the correct theory, and that the competing theories are incorrect. Thus, a variety of competing theories will often be compatible with the available data. We can say then that the

theories are undetermined by the available data and this is the idea of the underdetermination thesis.

Each of the three notions of the DQT explicated above can be taken in a weak and a strong sense. Duhem is usually thought of as taking them in a weak sense and Quine has been charged with taking them in a stronger sense (DeWitt 2004). Quine's more radical stance has been criticized, as we will see below. Before we consider the criticisms of Quine's more radical views let us first understand these notions in both their weak and strong sense.

We noted above that the holism thesis maintains that when a theory is tested, the test is of a whole group of beliefs and not just an individual theory. Auxiliary hypotheses and background assumptions are always tested when a theory is tested. But, we may ask, how large a group of beliefs are we speaking about? Are we testing just a small subset of our collections of beliefs, a few auxiliary hypotheses, and some background assumptions or are we testing our whole "web of beliefs?" The conservative view holds that although a test might involve a large collection of beliefs, it is not typically our entire collection of beliefs. Duhem was of this opinion (Duhem 1954). He held that the holism thesis obtained principally in physics. Quine at times defended a more radical view. He maintained that one's whole web of beliefs face the tribunal of experience as a whole (Quine 2001). He saw all of our beliefs connected like a fabric with the stronger held beliefs in the center and the weaker held beliefs around the periphery. Thus, when information conflicts with our experience we normally are willing to change the beliefs around the periphery to make adjustments for the conflicting data but usually leave untouched the more solidly held beliefs in the center, however, they are subject to change as well. In point of fact, for Quine "the unit of empirical significance is the whole of science" (2001, 42), and if this is the

case, whenever a theory is being tested all of our knowledge is being tested. This is the strong version of holism.

With regard to the idea of no crucial experiments, there is also a weak and strong view. We can take the weak view as maintaining that when tests of competing theories give conflicting results these results can be accommodated within both of the conflicting theories. Again, this is because auxiliary hypotheses as well as background assumptions can be adjusted to accommodate that data. However, there is a stronger way to take the no crucial experiments idea of DQT. In this view the claim is that any experimental result whatsoever can be accommodated within any theory whatsoever.

Finally, let us consider the weak and strong view of the underdetermination thesis. The weak version simply holds that the available data does not uniquely point to one of two or more competing theories. Note that this is not too serious of a problem as long as there is a possibility that there is some observation or experiment the outcome of which could give reasons for choosing one theory over the other. In this view the theories may be empirically equivalent so far. Or we could make the thesis even stronger by maintaining that the competing theories may be thoroughly empirically equivalent but not evidentially equivalent. That is, there may be no observations that allow one to choose one theory over the other but there may be other evidence that allows for theory choice. For instance, both theories may have the same observational consequences but one may be simpler or have greater explanatory power than the other and thus simplicity or explanatory power is here used as evidence that one theory is to be accepted over the other (these, of course, are consider cognitive values and we will discuss this further later). On the stronger version of underdetermination, however, no evidence forces particular changes in a theory. Theory is always underdetermined by data. No beliefs are insulated from the

possibility of revision. Any statement can be maintained, no matter what experience says, if we are willing to make enough modifications to other parts of our theory, we will always be able to preserve a commitment to the truth of any theory. Quine put the matter as so:

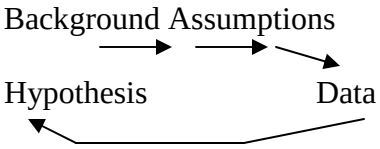
> Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system. Even a statement very close to the periphery can be held true in the face of recalcitrant experience by pleading hallucination or by amending certain statements of the kind called logical laws. Conversely, by the same token, no statement is immune to revision (2001, 43).

Thus, Quine defended a more radical form of the underdetermination thesis. For this view, there will always be more than one theory to fit the data, no matter how much evidence comes in. Theories are both empirically equivalent and evidentially equivalent.

What does this have to say about our issue with the influence of non-cognitive values in theory choice? Well, some philosophers of science use this strong sense of DQT as a way to indicate how non-cognitive values can enter into science. For instance, Longino 1990 explains that there is a logical gap between data and hypotheses. “Data, even as represented in descriptions of observations and experimental results, does not on its own however, indicate that for which it can serve as evidence. Hypotheses, on the other hand, are or consist of statements whose content always exceeds that of the statements describing the observational data” (1990, 58). For Longino this gap between evidence and hypotheses allows contextual (non-cognitive) values to influence scientific reasoning. Longino believes that hypotheses and evidence are related by background assumptions that scientists bring to inquiry. She indicates that, “a state of affairs will only be taken to be evidence that something else is the case in light of some background belief or assumption asserting a connection between the two... In the absence of any

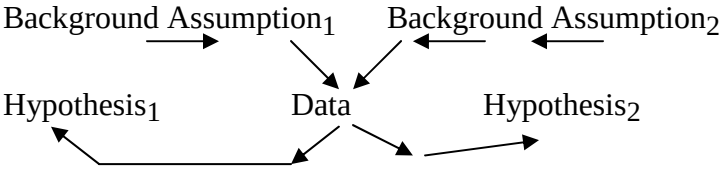
such beliefs no state of affairs will be taken as evidence of any other" (1990, 44). Thus, contextual background beliefs that include subjective biases bridge the gap between hypotheses and evidence. This can be illustrated as follows:

Figure 3- 1 Data becomes evidence for H



And by invoking background assumptions Longino is able to explain how the same data can support competing hypotheses.

Figure 3- 2 Data becomes evidence for H_1, and the same data becomes evidence for competing H_2



Proponents of different theories bring to inquiry different background beliefs and assumptions, and "in the context of their differing background beliefs and assumptions different aspects of the same state of affairs [become] evidentially significant" (1990, 47-48). The background assumptions that allow our inference from evidence to hypotheses also make room for the influence of non-cognitive values in inquiry. Because the background assumptions that mediate our reasoning about evidence are value-laden, scientists' values will shape scientific knowledge. As a result Longino understands the demand for objectivity as the demand "to block the influence of subjective preference at the level of background beliefs" (1990, 73). Longino suggest that observation and logic in their classic sense are insufficient for addressing the problem of underdetermination. Thus, we see that given Longino's analysis of the undertermination thesis non-cognitive can be brought in when choosing one theory from another.

Larry Laudan raised several questions in a very helpful critique of underdetermination and I would like to consider his ideas next.

Laudan and underdetermination: In *Demystifying Underdetermination* 1990, Laudan argues against strong underdetermination by making a distinction between what he calls deductive or Humean underdetermination (HUD) and ampliative or Quinean underdetermination (QUD). Under QUD he makes a further distinction between a nonuniqueness thesis and an egalitarian thesis. He attempts to show that HUD is true but trivial and that QUD cannot sustain the conclusions that are claimed to be derived from it.

First, how does Laudan describe HUD and why does he believe it is true but trivial? Laudan indicates that HUD amounts to one variant or other of the following claim:

> For any finite body of evidence, there are indefinitely many mutually contrary theories, each of which logically entails that evidence (1990, 269).

Laudan argues that the weakness in HUD is 1) that it is restricted to deductive logic and thus only shows that the fallacy of affirming the consequent (if theory T then evidence e, and evidence e therefore, theory T) is indeed a deductive fallacy, and 2) it provides no motivation for the claim that all theories are reconcilable with any body of evidence, that is, even if we accepted as rational any belief that logically entailed the evidence it would not allow us to hold that all rival theories are thereby equally belief-worthy or equally rational to accept. Thus, logical entailment does not equal or force rational acceptance.

Next, Laudan moves on to QUD. He makes a distinction between two theses, a nonuniqueness thesis and an egalitarian thesis. The nonuniqueness thesis is the weaker thesis. It holds that: for any theory, T, and any given body of evidence supporting T, there is at least one rival (i.e., contrary) to T that is as well supported (by that evidence) as T. The stronger thesis is

the egalitarian thesis. It insists that: every theory is as well supported by the evidence as any of its rivals (1990, 271). Laudan believes that both of these notions are ampliative, that is, they involve the notion of "empirical support" and thus include epistemic notions like simplicity and explanatory power that go beyond deductive entailment and logical consistency. Laudan thinks that Quine is committed to both doctrines, nonuniqueness explicitly, and egalitarian implicitly. Laudan links Quine to the egalitarian thesis by way of Quine's claim that - MAY: "one may hold onto any theory whatever in the face of any evidence whatever" (1990, 272). Laudan gets this from Quine's statement that we will recall from above "any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system" and maintains that in order for this not to be a truism, Quine must be claiming something like - RAT: "it is rational to hold onto any theory whatever in the face of any evidence whatever" (1990, 272).

Laudan argues that Quine nowhere provides an exhaustive examination of possible rules of rational theory choice with a view to showing them impotent to decide between all pairs of theories; he only examines the Popperian rule to reject theories that have known falsifying instances and uses this as the bases for his underdetermination. But Laudan points out that most of the strategies like changing word meanings, or abandoning the laws of logic, or pleading hallucination are "made of pretty trifling stuff" (1990. 273). He indicates that Quine gives no constraints on when it is reasonable to uses these various strategies.

The only strategy that Laudan believes is nontrivial has to do with Quine's proposal that a threatened statement or theory can always be immunized from the threat of the recalcitrant evidence by making suitable adjustments in our auxiliary theories (1990, 274). He calls this Quinean underdetermination (QUD):

> Any theory can be reconciled with any recalcitrant evidence by making suitable adjustments in our other assumptions about nature (1990, 274).

Laudan's main point about QUD is that it drops any reference to the rationality of theory choices. It is possible to take out a whole load of auxiliary theories in order to save a threatened theory. But Quine does not establish the reasonableness or rationality of doing so, as RAT demands. Saving a threatened theory by abandoning the auxiliary theories once needed to link it with recalcitrant evidence comes at a price. If we give up those beliefs without replacement, we not only abandon an ability to say anything whatever about the phenomena that produced the recalcitrant experience; we also give up the ability to explain all the other things which those now rejected auxiliaries enabled us to give an account of, with no guarantee that we can find alternatives that will match their explanatory scope.

Laudan concludes, "if it is plausible, as I believe it is, to hold that scientists are aiming (among other things) at producing theories with broad explanatory scope and impressive empirical credentials, then it has to be said that Quine has given us no arguments to suppose that any theory we like can be doctored up so as to win high marks on those scores" (1990, 276). Thus, giving up some auxiliaries does not guarantee to give us the same degree of explanatory power or empirical support as the original group, and it is likely to diminish the predictive and explanatory power of the new group as compared to the old and so Quine has not proven his underdetermination thesis.

It will be recalled that Longino claims the background assumptions that allow our inference from evidence to hypotheses also make room for the influence of non-cognitive values in inquiry. Because the background assumptions that mediate our reasoning about evidence are value-laden, scientists' values will shape scientific knowledge. It will also be recalled that Kuhn

came to a similar conclusion regarding non-cognitive value influence although for him it was a result of failure to apply and consistency interpret the various cognitive values. But we can ask why do background assumptions need to be influenced by non-cognitive values? And if they are, why can't they be judged by a list like Kuhn's that does not include non-cognitive values. If this is done then non-cognitive values do not have to enter into theory appraisal.

Also, we need to understand that underdetermination is always underdetermined. Underdetermination can never be demonstrated. Just because underdetermination may be shown with one set of rival theories does not prove it exists with all. And also along these lines, empirical equivalence can never be justified because we can never know what new future evidence may be developed. If empirical equivalence is seen as temporary, then we may choose to *pursue* one theory over the other based on non-epistemic values but this gives us no reason to *choose to hold* one over the other: from an epistemic point of view, one should conclude that we do not know. Finally, if two theories are empirical equivalent on all the epistemic values it would seem that they are the same theory – the difference being only verbal. Thus, it does not seem that the issue of underdetermination forces us to use non-cognitive values to choose between theories. So what is the difference between cognitive and non-cognitive? We will move to this issue next.

CHAPTER 4

VALUE FUNCTION IN THEORY CHOICE

In this chapter I will lay out an account of why some types of values (cognitive) are to be used to assess theory choice and why others (ideological, psychological, ethical, etc.) ought not to be used. In so doing I will respond to three objections. These objections are based on the idea that non-cognitive values are unavoidable because there is no clear distinction between cognitive and non-cognitive values. Finally, I will suggest that an important way to determine how legitimate theory assessing values ought to be used is by considering the particular research program that a scientist is involved in.

We have seen that non-cognitive values play an important role in science. In the context of discovery, theories and hypotheses are generated and non-cognitive values can provide the source of theories and hypotheses that are generated. In the context of investigation (designing a study, collecting data, etc.) non-cognitive values can direct how investigation is carried out by for instance, justifying placing procedural constraints on scientists – or, requiring experimental subjects to be treated humanely or human subjects to be given informed consent or perhaps non-cognitive values can be used to determine which topics are most important to learn about; for instance, considering what would advance science most effectively or what are the greatest needs for society. In the context of application, non-cognitive values can legitimately assist in determining what level of certainty in a scientific theory is demanded before it is accepted as a guide for action.

While supporters of the position that non-cognitive values are unavoidable in science use these various contexts to make their case, I have argued that each context offers an opportunity to stress the importance of values in science while allowing us to see how non-cognitive values

need not determine theory choice even though they are part of the scientific process. I have also maintained that non-cognitive values are not appropriate for the context of assessment. How theories ought to be assessed rules out a legitimate role for non-cognitive values. I will attempt to defend this position in this chapter.

However, one other issue regarding non-cognitive values needs to be addressed. Steel 2010 has identified a set of arguments that maintain that non-cognitive values are unavoidable because there is no clear distinction between cognitive and non-cognitive values. One line of these arguments, he points out, maintains that since there is a lack of agreement between proposed lists of values that this is grounds for there being no clear distinction (2010, 21). For instance, Rooney, 1992 argues: "That an analysis of the operation of the social within science warrants closer examination of the 'epistemic' values themselves, and that this examination is to proceed in a way that undermines the usefulness of the epistemic/non-epistemic distinction itself. The fact that there is no consensus about what exactly the epistemic values are surely provides our first clue here. We haven't seen anything resembling a clear demarcation of epistemic values because there is none to be had" (1992, 15).We can call this the disagreement objection. If there is no agreement on what the epistemic values are, then how can a demarcation be made between epistemic and non-epistemic values. Another line indicates that it is impossible to disentangle the social from the epistemic. We can call this the disentanglement objection (Steel 2010, 22). A third objection along these lines indicates that cognitive values do not have the depth to guide scientific inquiry. We can call this the dependency objection. On the dependency objection Steel writes:

> This objection is a challenge to the usefulness of the concept of epistemic values rather than to the cogency or existence of that concept. The thought behind the objection seems

> to be that defenders of epistemic values believe that, in a perfect world, epistemic values alone would guide scientific inference. Hence, the objection goes, if epistemic values can be shown incapable of this, the reason for distinguishing epistemic from nonepistemic (sic) vanishes, and the distinction is useless (2010, 23).

It should be noted that Steel takes issue with these objections as well. In order to respond to these objections in the way I would like to we need to define what is meant by the various values and how they can be distinguished.

Types of Values

To help understand the distinction between what we have been calling cognitive values from non-cognitive values I want to briefly lay out an argument for why so-called cognitive values as opposed to non-cognitive values ought to be the only ones used to assess theory choice. Then we can use this general argument to help us make some distinctions between the various values. The argument goes like the following.

1. We use values to assist in judging something as good.
2. Social values are those values we use to judge aspects of our societal relationships as good.
3. Cognitive values are those values we use to judge our theories and beliefs as good.
4. What makes various aspects of our societal relationships good is different from what makes our beliefs and theories good.
5. Therefore, different values ought to be used to judge a theory as good as opposed to judging aspects of our societal relationships as good.

Let's look a little closer at how this works. There are different standards for judging something as good depending on what is being judged. We judge a car as good in a different way, by different standards, than we judge a police officer as being a good police officer or a scientific theory as being a good theory. What determines the standard of judging something as good or bad is based on the purpose of the thing being judged. Thus, if the purpose of a police officer is to serve, protect, and keep the peace then we can judge whatever he or she does as good in terms of how that action stands in relation to that purpose. Can this analysis be applied to science? First, we need to consider the goal or goals of science. Most people's pre-reflective notions of science view it as the model of rationality, as the preeminent search for knowledge and the most successful way for finding it. The approach to science known as scientific realism explains this by maintaining that science and its theories give us a true picture of the world. Another approach, instrumentalism says that science is successful for other reasons and its goals include; obtaining theories that work, that solve problems, that explain the world and make its workings seem less puzzling. Under this notion to maintain that truth is the goal or purpose is to assume far more than is necessary to explain why scientific theories work as well as they do. Rather than attempt to defend a particular view of the goal of science I will maintain that *a* goal rather than *the* goal of science is the search for knowledge by finding significant truths. So, if a purpose of a theory is to give us knowledge by helping us find significant truths then we can judge a theory as good in terms of how it stands in relation to that purpose. Gettier problems[16] notwithstanding, if

[16] A Gettier problem consists in showing that a person can be justified in believing something as true without having knowledge. Bahnsen, 2008, 99 suggests two criticisms against Gettier arguments. First, the examples involve an equivocation and second, they fail to recognize that appropriateness is a necessary attribute for justification. He gives the example of Smith, having spoken to the company president and having counted the coins in Jones's pocket, Smith has strong evidence for the conjunctive proposition, Jones is the man who will get the job, and Jones has ten coins in his pocket. This proposition entails the further proposition; the man who will get

knowledge generally consists of justified true belief, then an important way to judge a theory is by whether or not it gives us true or approximately true beliefs that are significant and justified. Some values do not aid in directly assessing beliefs as true. Other values do aid in justifying or directly assessing beliefs as true; so only those values that aid in justifying or directly assessing belief as true ought to be used to judge a theory as good, i.e., giving us true or approximately true beliefs. It should be noted that justification comes in degrees, that is, propositions or beliefs with a lot of evidence in their favor are more probable than propositions or beliefs with only a little (Kirkham, 1995). So when I speak of approximate truth as though some beliefs are truer than others I am taking this to mean a project to justify a belief as true, that is to say, some beliefs are more highly probable or perhaps better justified than others.

What we have been calling cognitive values as oppose to non-cognitive values are the values that can be used to judge for truth. These values have been variously categorized as

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the job has ten coins in his pocket. But it turns out that Smith, who also has ten coins in his pocket, gets the job. Hence, Smith justifiable believed the entailed proposition and that the proposition was true. Yet we realize that Smith did not genuinely have knowledge. The first problem, as Bahnsen sees it, is that by its vagueness the entailed proposition from the example allows for an equivocation: "The man who will get the job" means "Whoever gets the job" at one point and "The particular man, Jones" at another. It is clear that what is justified ("Jones…") is not what is true (Whoever…"). Secondly, a person has justifying reasons for his belief only if those reasons are relevant to the truth of that belief. Strong evidence would not be considered such if it were inappropriate evidence. Irrelevant reasons are not reasons at all. In the example the entailed proposition is justified with reference to Jones but true with reference with Smith; hence his justifying reasons are irrelevant to his belief.

Even if one does not agree with Bahnsen's criticisms, though I tend to agree, Schlesinger, 1983, suggests that the Gettier problem is relatively trivial and unimportant with regard to epistemology. He maintains that everything that is important in epistemology can be adequately studied if we concentrate solely on the notion of justified true belief. He claims while it is important to know under exactly what circumstances a person is *entitled* to believe a proposition, it is not at all important to be able to decide under what circumstances a person *qualifies* as knowing a proposition. He believes Gettier examples are aimed at getting at the notion of what a person is qualified as knowing, as opposed to what one is entitled to believe. This seems correct to me, and so since in our discussion of theory choice we are considering the criteria that entitle belief I will take the position that we do not have to be concerned with Gettier type problems.

empirical values, epistemic values, and cognitive values. For purposes that will become clearer below, let us call a value that aids us in determining truth an epistemic value rather than a cognitive value. If truth[17] is a relationship between a proposition and the world, that is, if a true proposition corresponds or conforms to the world or purports to report an actual state of affairs, then epistemic values help us determine the truthfulness of the proposition. An epistemic value has value because we believe it is an aid or indicator of truth.

So, for instance, observational verification, often called empirical adequacy, is an epistemic value. If we can verify by observation that a proposition indicates an actual state of affairs or conforms to the world then we say the proposition is true. Accuracy is another epistemic value. A theory should be accurate within its domain, that is, consequences deducible from a theory should be in demonstrated agreement with the results of existing experiments and observations. What about a notion such as predictive power? If accurate predictions are true statements about the future (Steel, 2010, 18) then it would seem that predictive power is an epistemic value.

Given the definition of epistemic value as a value which is an aid to or indicator of truth then, it seems, some values can be ruled out as non-epistemic. So for instance, ideological, economic, political, or psychological values are non-epistemic values. My psychological desire for ambition, or social recognition, or the expectation for monetary gain might motivate me to pursue a theory, but these values do not seem to indicate the truthfulness of the pursued theory. We would not say that because I am able to get my theory funded, that is, because it has what we

[17] How I am defining truth here is closely related to what has traditionally been called the correspondence theory of truth (truth is what corresponds to reality) as opposed to for example, the coherence or pragmatic theories of truth. The most salient problem with the correspondence theory is the question of how does one determine when a belief or proposition "corresponds" to reality. I take it that part of the project of determining criteria for theory choice is attempting to answer this question.

might call "fundability" then it must conform to the natural world. Fundability might be considered an economic (social) value of a theory but not an epistemic value; it does not seem to tell us anything about the natural world. As Mauro Dorato has indicated, any possible inquiry into the natural world presupposes our assuming that there is a way things are, that there are facts that are independent of our wishes and preferences. Without this assumption of independence of facts from non-epistemic values, trying to find out how things are would be a meaningless enterprise (Dorato, 2004).

This distinction then, seems to allow us to overcome the disentanglement objection. We distinguish an epistemic value from a non-epistemic value in terms of the values ability to aid in assessing as true. Note here we are not attempting to determine which of the various epistemic values are the most important, we are merely attempting to show that there does indeed exist a distinction between an epistemic and a non-epistemic value.

However, there does seem to be a set of values that do not seem to be obviously epistemic values but still seem to be legitimate for theory choice. For instance, what status should be given to some of the values that Kuhn delineated? Scope: a theory's consequences should extend far beyond the particular observations, laws, or sub-theories it was initially designed to explain. Scope does not seem to be a value that allows a theory to be directly conformable to the world but it does seem to be a valuable aid in theory choice. Simplicity: a theory should be simple; bringing order to phenomena that in its absence would be individually isolated and, as a set, confused. Simplicity[18], at least in the way it is defined here, does not seem

[18] Simplicity is a notoriously difficult concept to categorize because of the various ways it can be defined. For instance, a theory could be simple in terms of having few ad hoc hypotheses or auxiliary theses. Or a theory could be simple in terms of being easy to understand. I think the notion of simplicity in any of its various definitions does not seem to apply to the idea of epistemic value because it does not seem to relate directly to a notion of a theory being true. A

to aid in determining truthfulness, but it does seem to be valuable in aiding to determine a "good" theory from a "bad" theory. Fruitfulness: a theory should be fruitful of new research findings: it should disclose new phenomena or previously unnoted relationships among those already known. Again, just because a theory is fruitful does not seem to aid in determining its truthfulness that is, we can say that just because a theory is fruitful does not mean that it is an indicator of truth, but fruitfulness does seem to be something that adds value to a theory.

Laudan, 2004 has argued that since science is not exclusively epistemic, many of the historically important principles of theory appraisal used by scientist have been, though reasonable and appropriate, utterly without epistemic rationale or foundation. He maintains that several important rules of thumb in theory appraisal speak to concerns regarding scope and generality. For example, he suggests that acceptable theories are generally expected to explain the known facts in the domain ("save the phenomena"), explain why their rivals were successful (the Sellars-Putnam rule), and capture their rivals as limiting cases (the Boyd-Putnam rule). He maintains that none of these rules can have an epistemic rationale since it is neither necessary nor sufficient for the truth of a statement that it exhibit any of these attributes. Thus, these values are not epistemic because they address questions about the breadth and scope of our theories rather than questions about their truth or probability. He suggests that these values should be called cognitive rather than epistemic and thus they are non-epistemic values.

Thus, Laudan concludes that we would like to have theories that are true. But we would also like theories that are of great generality, that focus on the things we are particularly interested in understanding, that explain as well as predict, and that consolidate existing

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simple statement about the world may not be a true statement about the world in the sense that say, a predicatively accurate statement is a true statement. I consider these notions of simplicity as cognitive rather than epistemic values. See below for an explanation of how I understand the difference between cognitive and epistemic values.

successes while moving us beyond them, and these matters for the most part are not epistemic Laudan 2004, 21.

It would seem that Laudan is correct. There do seem to be a group of values related to theory appraisal that do not focus on truth. "Cognitive" seems to capture the nature of these types of values because they seem to help us understand and explain. Using both of these notions as criteria for theory assessment, we can say then, the epistemic values can guide our search for truth, and these "non-epistemic cognitive" values going beyond the narrow epistemic ones, can guide our search for theories which are also explanatory, and the like. So how do we reconcile these various types (epistemic verses non-epistemic) of values with regard to theory appraisal, if as it seems both are legitimate in theory assessment? Well, as I have argued, what determines the standard of judging something as good or bad is based on the purpose of the thing being so judged. That is, there are different standards for judging something as valuable depending on what is being judged. So it seems what Laudan has put his finger on is the very legitimate notion that different scientists have different purposes for what they want theories to accomplish. Some primarily want theories that are true to the world. Other scientists want theories that help us to understand or explain the world. Understanding and explaining are not the same as truth seeking. So there does not seem to be one goal or purpose for a scientific theory to be considered a good one, there are various purposes and thus there are various ways to appraise theories. This then allows for other views, for instance, instrumentalists ones, of the goal of science to be considered. One holding to an instrumentalists view would consider understanding and explaining as more important for a theory than truth.

However, it should be pointed out that none of the values that Laudan has identified could be considered social, political, ethical or ideological even though they are non-epistemic. It

would seem that since social, political, ethical, etc. types of values do not assist in appraising theories with regard to truth, understanding, or explanation as the epistemic and the non-epistemic cognitive values seem to do, they should be not be considered in terms of theory evaluation and assessment. As Longino has pointed out in slightly different terminology; "the rules of inquiry… are a function of the goal of science…. While the choice of areas or aspects of the world to be illuminated by application of the rules is a function of social and cultural contextual values [like those listed above, i.e. political, ethical, ideological], the conclusions, answers, and explanations reached by means of their use and guidance are not. Even those contextual values [social, political, etc.] that do affect science remain external to the real thing, to the doing of science. When they do not, we have a case of bad science (1990, 85).

What Longino indicates is that using the various, what she calls contextual values, which would include social and cultural values, results (in some cases) in bad science.[19] From the explanation we have been using, bad science is a result of attempting to use the wrong values for the wrong purpose. So regarding the disagreement objection, we can see that scientists may disagree between proposed lists of values with regard to theory choice but this does not allow a legitimate "infiltration" of social values into the theory evaluation process. And when it does this is bad science. Nor does disagreement mean that there is no clear distinction between values. We can disagree about the number of chairs in the classroom but that does not mean that my estimation of the number of chairs is not distinct from your different estimation of the number of chairs or that there is not an objective number of chairs that is the correct number. Disagreements do not eliminate distinctions, they highlight them. Nor do disagreements imply that there is no

[19] It should be noted that Longino does not believe that this "external model" captures all the cases of science.

fact of the matter. Thus, just because scientists disagree about proposed lists does not mean that there is no clear distinction to be had.

How then does one determine which set of values (epistemic or non-epistemic) apply when assessing a theory since both are legitimate? Well, it seems that one has to look at the type of problem one is attempting to solve with any given theory. Laudan, 1977, suggests two kinds of scientific problems: empirical problems and conceptual problems.[20] It seems to me that positing a theory to give us the truth about some phenomenon can be considered an attempt to solve an empirical problem, thus an epistemic issue. Thus, an empirical problem asks a question about the make-up of the world. For instance, when Watson and Crick posited the double helix as the structure of the DNA molecule I take it that they were attempting to give a true conception of the structure of DNA. We can say that their model was meant to represent or conform to the actual state of affairs in the world. Thus, we apply epistemic values when we are evaluating theories attempting to solve empirical problems.

Conceptual problems have to do with the conceptual structure of the theory that exhibits them. Laudan 1977 identifies two kinds of conceptual problems, internal conceptual problems that arise when a theory exemplifies internal inconsistencies and external conceptual problem that arise when a theory conflicts with other well established theories or doctrines which the proponent of the posited theory believes to be rationally well founded, that is, when the theory is incompatible with other accepted theories and strongly held beliefs.

I would suggest that positing a theory to help understand or explain some phenomenon, to remove ambiguity, to reduce irregularity to uniformity, and to show that what happens in the

[20] Laudan does not believe that empirical problems relate to the truthfulness of theories. He sees empirical problems, in general, as anything about the natural world that strikes us as odd and in need of explanation. But it seems to me that explanation is a conceptual not an epistemic issue.

world is somehow intelligible and expected can be considered an attempt to solve a conceptual problem. Broadly speaking, conceptual problems in my view have to do with understanding and explaining the world. We thus, apply non-epistemic values when we are evaluating theories attempting to solve conceptual problems.

Laudan lists ambiguity and circularity as a second class of internal conceptual problem. He suggests that the increase of the conceptual clarity of a theory through careful clarification and specifications of meaning is one of the most important ways in which science progresses (1977, 50). I agree but would suggest that the issues of clarification, etc. are indicators of attempts to understand and explain and this is why they qualify as conceptual problems.

Non-epistemic values such as those that Laudan has identified and some of those that Kuhn has identified like simplicity and fruitfulness assist in assessing how well a theory is doing regarding helping solve conceptual problems. So for instance, regarding simplicity, a theory could be simple in terms of having few ad hoc hypotheses or auxiliary theses. Or a theory could be simple in terms of being easy to understand. Both of these notions of simplicity assist in our assessing the ability of a theory to help us explain and understand the world so simplicity is a non-epistemic value. A fruitful theory not only helps to explain the problem that the theory deals with but also leads to explanations in other areas and so aids us in understanding other parts of the world. Thus, fruitful theories assist in our assessing the ability of a theory to help us explain and understand the world, so fruitfulness is a non-epistemic value.

What of external conceptual problems? Laudan argues that the history of science illustrates that conceptual difficulties from philosophy, theology, logic and mathematics, political theory, and general world-view considerations provide negative evidence for the rational acceptance of scientific theories in the form of external conceptual problems. This makes sense

because if one has good reasons for holding to some proposition and if a scientific theory conflicts with that proposition without as least as good of reasons, then the proposition itself provides some evidence against the scientific theory, even if the proposition is related to a discipline outside of science. So in our efforts to understand and explain the world we want an integrated world view and if specific scientific theories go against what we have strong evidence to believe in other domains, both inside and outside of science, then conceptual problems are raised regarding the theory.

I am sure there are a number of ways to categorize various values. The following may be a helpful way to see how they relate to our discussion.

Non-cognitive Values

1. Ideological - values pertaining to political, social, economic and religious agendas

2. Psychological - values pertaining to our interests, needs, and desires

3. Ethical – values pertaining to our moral judgments concerning right and wrong

Cognitive Values

4. Epistemic – values pertaining to the truthfulness of claims

5. Non-epistemic – values pertaining to the understanding or explaining of phenomenon

I am suggesting that 1-3 do not pertain to the assessing of scientific theories. They are for other purposes. I am suggesting that 4 and 5 are values that serve to assess scientific theories. This list then distinguishes between theory assessing values (4 and 5) and non-theory assessing values (1, 2, and 3). When I use the term “non-epistemic” below I will be referring to number 5 type values.

Earlier, I criticized Okruhlik who argued that the theory generation phase allows for the influence of various non-cognitive values even in theory choice because current models of scientific rationality hold that theories are not directly compared to nature but view theory choice as "irreducibly *comparative"*. Thus, she argued, theory choice is unavoidably influenced by non-cognitive values because the theories generated as a result of non-cognitive values may be the only ones that we have. So, if the correct (true) theory is T10 and our generation process has only produced theories T1, T2, and T3, then these theories are the only ones we have, and thus, we can only consider these three theories and compare them with one another and the world to determine the better theory. However, I argued that even if theory evaluation has elements of comparison as part of the process, the most salient feature of theory assessment seems to be how well a particular theory matches the world. Scientists are justified in accepting or rejecting a theory only if it meets a particular standard of evidence, not just because it is the best theory available. We see that the world pushes back on our theories. So, even if T3 seems to be better than other options, it can still be rejected if there are clear areas where it does not fit the world.

As I indicated above, an example of this was seen when we examined the various theories of race. We did not compare the various theories based on populationism with one another, and even if we had and found that one or another seemed to be better than the others, at explaining the phenomenon say, we would still have been justified in rejecting it because of the reasons given. What we can see is that the specific way to evaluate theories, regarding their truthfulness, i.e., fitness to the world, has to do with what I have been calling the epistemic nature of theory assessment. The concern is to determine if the theory is true. But we can also see that when we want to know how well a theory helps us understand or how well a theory has explained a

phenomenon we do consider the comparative nature of theory assessment and in so doing we consider various cognitive values that are non-epistemic associated with the theory. Thus, the emphasis on the comparative nature of theory choice is more appropriate when considering a theory with regards to understanding and or explaining.

We can say that a particular theory may not be true but it is a working hypothesis to help us understand and explain. It may be argued that the Copernican theory was somewhat simpler[21] than the Ptolemaic system and so helped us to better understand the phenomenon even though it was not correct. It could be used to aid in reaching a better theory. So, in attempt to discover the truthfulness of a theory, comparing may not be the best approach, but when considering how well a theory helps us understand or explain the world it seems, the comparing nature of theory choice is more effective. However, as we have seen, this does not allow for the influence of non-cognitive values because, as I have suggested, the various non-cognitive values are for different purposes.

There are two obvious responses to all of this. The first is to ask if a theory can have the purpose of both being true and enhancing our understanding and explanation of phenomenon. That is, is it legitimate to apply both epistemic and non-epistemic values in assessing theories? The second would be to ask, if it is legitimate to apply both types of values; in what circumstances should one or the other purposes have priority? I will briefly answer the first question and it will lead us into an answer to the second.

Anti-realists like Laudan would say that the purpose of theories is to solve problems and thus the truthfulness of the theory is of lesser consequence. Scientific realists would say that the

[21] Unlike the Ptolemaic system, the Copernican theory did not require for instance, equant points (the point with respect a planet moved with uniform speed). See DeWitt 2004 for a helpful discussion of this issue.

reason a theory enhances our understanding and explains phenomenon or solves problems is because it is true or approximately true.

I suggest that answers to these questions, i.e. whether we should seek truthful theories or theories that enhance our understanding and ability to explain, show the *research program* that the proponent is working in and the research program indicates the purpose for a theory. Whether science ought to aim for truth or merely empirical adequacy, whether science ought to postulate the existence of unobservable entities to explain the directly observable phenomena or to be content to describe, predict, and control the observable phenomena is a function of the research program in which one is working.

Thus how we determine the purpose for which a theory should aim is determined by the research program that the proponent is working in. So what is a research program? Laudan has suggested that:

> "It is necessary to distinguish, within the class of what are usually called 'scientific theories,' between two different sorts of propositional networks… We often use the term 'theory' to denote a very specific set of related doctrines (commonly called 'hypotheses' or 'axioms' or 'principles') which can be utilized for making specific experimental predictions and for giving detailed explanations of natural phenomena… by contrast, the term 'theory' is also used to refer to much more general, much less testable, sets of doctrines or assumptions…. In each of these cases, we are referring not to a single theory, but to a whole spectrum of individual theories" 1977, 71.

Laudan calls this higher level propositional network a "research tradition." What recognizing this level allows us to do is to answer questions about why which types of values are being used. That is, what determines the purpose of some work in science, or perhaps the problems that arise

and thus the types of questions we should be asking and the way evaluation should take place are what I will call research programs. A particular research program is situated around a particular problem and understanding a particular research program starts with identifying the problem on which that program is focused. The problem could be as simple as a question – How many races are there? Or a complex of issues – How is variation in human species patterned? And how did it come to be this way?[22] The research program then specifies the metaphysical and methodological commitments of members of the research program; it helps to establish the appropriate entities and processes to investigate, various other appropriate questions to ask about those entities and processes, the appropriate methodologies to employ in seeking to answer those questions, and the appropriate criteria of appraisal for the various theories associated with the research program.

Let me now summarize where we are. First, I gave a general argument as to why certain kinds of values (cognitive values is what we initially called them) ought to be used to assess scientific theory choice as opposed to other types of values (non-cognitive values). I suggested that the distinction is based on the purpose of the thing to be evaluated. Scientific theories are for a purpose that makes the use of so-called non-cognitive values in their appraisal illegitimate. It seems this distinction untangles the disentanglement objection.

Next, I distinguished between two kinds of values that are used to assess theory choice. Epistemic values are for the purpose of helping us determine truthfulness of theories and non-epistemic values are for the purpose of helping us to assess theories in their aiding in our understanding and explaining of phenomena. There does not seem to be one purpose or goal that scientists want theories to accomplish. By making this distinction between the epistemic and the

[22] These questions come from Marks 1995.

non-epistemic I tried to take this into account. Some scientists want to understand why for instance, an entity acts the way it does while others are concerned with explaining how an entity acts the way it does. So, for instance, we might explain the disagreement between Einstein and Bor as a disagreement over wanting to know why subatomic particles acted the way they did (Einstein) and wanting to know how they acted (Bor). Note that the preference for one purpose over another may be driven by values (non-cognitive values) other than the values (theory assessing values – non-epistemic and epistemic values) used to best explain the entity or determine how the entity acts.

I suggested that the purpose of the theory determines the kind of values used to assess it. I think this helps meet the disagreement objection. So we can say that while scientists may lack agreement between proposed lists of values to assess theories, this disagreement does not show that there are grounds for there being no clear distinction between types of values that are legitimate for theory assessment and those that are illegitimate.

Finally, I suggested that a research program determines the purpose of the theories that are generated to solve the problems it seeks to solve, as well as how it should go about accomplishing that purpose in terms of the ontological entities that exist, the methods used to meet that purpose, and the criteria with which to evaluate the theory. By introducing the notion of research programs I am acknowledging that the dependency objection, that cognitive values do not have enough depth to guide scientific enquiry, is legitimate but that does not mean that ideological, psychological or ethical values need to be used in theory assessment. Research programs help to determine which of the various legitimate values (non-epistemic and epistemic) are to be used in theory assessment and they can help provide the depth that these legitimate theory assessing values are not able to do.

Thus, cognitive values alone are to be used in theory assessment and research programs help determine how they are to be used. I believe the inability of scientists to come to a settled consensus regarding the reality of biological race has to do with the various research programs that are being used to consider the data.

Race and Research Programs

An analogous issue in the philosophy of biology to the race issue is the nature/nurture debate, and the IQ controversy most famously waged in the 1970's between Arthur Jensen and Richard Lewontin over the best explanation of the difference in IQ scores between black and white populations is a case in point.[23] Most philosophers of science have sided with Lewontin. However, Sesardic in *Making Sense of Heritability*, 2005 criticizes Lewontin and others with regard to their position on the nature nurture debate. In commenting on the criticism that Sesardic raises between Lewonton and Jensen, Tabery, 2009 notes,

> "When faced with the ongoing existence of heritability research and defenders of such research long after Lewontin published his criticisms in the 1970s, the Lewontonians attributed the persistence to either lack of understanding or socio-political subversions. Sesardic joins this game. When faced with the anti-heritability consensus in the philosophy of science long after Jensen responded to Lewontin's critiques, Sesardic attributes the persistence of the consensus to either sheer ignorance, deliberate misrepresentation, or (you guessed it) socio-political subversions… 'The sheer level of ignorance, distortion, and flawed reasoning that characterizes the 'anti-heritability' camp is unprecedented in science and philosophy of science. Could it be that, in accordance

[23] For details see Block, N.J. and Dworkin, Gerald. Eds. 1976. The IQ Controversy. New York: Random House.

> with the above description [i.e., the weakness of the arguments against heritability], the drastic decline of standards is here due to the dominant intellectual atmosphere, in which those set to undermine heritability can hope to be praised for their political sensitivity and for opposing a dangerous theory, while at the same time they do not have to worry about being severely penalized for possible shortcomings in their logic and methodology? Could this be an explanation?' Needless to say, Sesardic thinks so", 2009, 207.

So both sides of this debate are criticizing the other side for either misunderstanding or social and political influences. Tabery, however, offers a different account of what is going on instead of the alleged socio-political subversions, he suggests that one way to explain the discordance between the opposing sides is to appeal to epistemological positions in different research traditions rather than different intellectual aptitudes or different political persuasions. He suggests that Lewontin and Jensen were operating in different research traditions—Lewontin in the developmental tradition and Jensen in the biometric tradition. While members of both research traditions have focused on the etiology of complex phenotypes and phenotypic differences, they have gone about addressing these biological phenomena in very different manners.

Tabery notes that members of the biometric tradition, such as Jensen, took their problem to be partitioning the relative contributions of nature and nurture responsible for variation in populations. Their approach to causation involved an investigation into the causes of variation, or the genetic and environmental differences, responsible for such variation. They asked how much of the variation in a particular population was due to individual differences in genes and/or individual differences in environment. And they sought to answer those questions with statistical methodologies such as the analysis of variance and its derivative heritability measure.

Meanwhile, he suggests, members of the developmental tradition, such as Lewontin, took their problem to be elucidating the etiology of individual development. Their approach to causation involved an investigation into the causal mechanisms responsible for that developmental process. They asked how do differences in genotype and differences in environment relate during individual development to generate differences in phenotype? And they sought to answer those questions with interventionist methodologies that directly manipulated genetic and environmental factors involved in the developmental process.

Thus, Tabery suggests that one way to explain the discordance between the opposing sides is to appeal to epistemological positions in different research traditions. If Tabery is correct, then perhaps what is often contributed to the influence of non-cognitive values in scientific theory choice may not be the whole story. I suggested above that the issue of race is controversial and highly subject to the influences of non-cognitive values. That this is the case for several reasons, they included: emotional attachment issues, the social consequences involved, opponents perceptions of the reasons for holding an alternative position as being motivated by social reasons, and for historical reasons. So, for example, I could argue that some notions of race are motivated by social and or political reasons and not biological ones:

1. At different times race has been sorted differently.

2. Often the way race was sorted was due to political or social pressures that were not dictated by biology alone.

3. Therefore some notions of race are motivated by social and or political and not biological reasons.

This can be illustrated by what Glasgow, 2009 points out: “the one-drop rule effectively increased the number of African-descended slaves and crudely attempted to rationalize a corrupt

notion of "white purity,' while a higher quotient of American Indian ancestry (at least a quarter) has been required for one to be officially recognized by the U.S. as an American Indian, effectively *reducing* the number of indigenous Americans," 2009, 89. Thus, in Glasgow's example we see two different kinds of sorts for race, the one drop rule and the one quarter rule. We also see that each seems to be motivated for political and or social and not biological reasons.

But just because the issue is *subjec*t to these influences does not mean that it has to be determined by them, perhaps the use of non-cognitive values influencing theory choice may not be the only culprit regarding the lack of consensus in the race debate. Perhaps various research programs, asking different questions, seeking to solve different problems can account for some of the lack of consensus. I think this is the case.

In a recent book (Longino, 2013) Helen Longino examines five approaches to the study of behavior they include; quantitative behavioral genetics, molecular behavior genetics, developmental psychology, neurophysiology and anatomy, and social/environmental methods. She attempts to highlight the underlying assumptions of these disciplines, as well as the different questions and mechanisms each addresses. In describing her finding she notes the standoff among different research approaches in bio-behavioral sciences and writes:

> Classical behavior geneticists look for and develop methods for identifying and interpreting intergenerational behavioral correlations ("concordances"), and molecular behavior geneticists look for and develop methods for identifying correlations between genetic structures and behaviors. Social environmental approaches, including family systems and developmental systems approaches, look for and develop methods for identifying environmental and social determinants of behavioral differences. Members of

each side characterize the other as politically and ideologically motivated (Longino 2004, 138).

What I want to highlight here is the idea that different approaches can lead to different conclusions and thus, facts are not self-interpreting. Bare facts alone do not tell what is going on or how to understand what is going on. That is to say, meaning is not inherent in nor does it arise naturally out of the bare facts. Facts only have meaning as they are seen within a system; they take their meaning from an interpretive model and therefore are distinct from an interpretation. An interpretation is the translation of facts into theories. If this is the case then, the real debate about race may not be whether there are any differences between populations but about the meaning of such differences. And the answer to this question may have a lot to do with the research program that is seeking to solve the problem.

For example, Moreland, 1990 points out, Linnaeus and other early taxonomists who worked on classifying organisms were guided by a [research program that was a] combination of Christian theology and Aristotelian philosophy that produced the conviction that living things had fairly fixed essences that would permit them to be organized into a hierarchy of classes.

And Jonathan Marks, 1994 makes a distinction between the two questions asked by those studying human variation; how many races are there? As opposed to the question, how is variation in the human species patterned? And how did it come to be this way, 1994, 51? The first kind of question assumes the biological reality of races and attempts to discover them. The second kind of question is seeking an answer about an etiological question about human species pattern, and appears to assume nothing about races. It would represent a different research program.

Research programs determine what questions to ask. The questions asked determine what theories will be generated. Once a theory is generated, as was argued above, scientific methodology can determine if it should be accepted, independent of both the values that generated the questions and the envisioned results of what may happen if a theory is applied. We see this very clearly in the race debate. Throughout the 18th, 19th and early 20th century the questions; Does race exist? How many are there? What they are? dominated the research. Theory after theory was proposed to answer these questions and each one was found wanting.[24] Many scientists came to believe that there were no human races, in spite of the fact that there is human variation.

And this same kind of issue seems to exist today. Edgar and Hunley writing in the American Journal of Physical Anthropology 139:1–4 (2009) indicate that they argued about how best to describe and interpret human biological variation. The disagreement focused on whether human races exist, and, even if they do, whether they should continue to use the term ‘‘race’’ in research and teaching. They eventually realized that they were re-hashing an argument that has taken place for generations and that their different training, research interests, and life experiences had led them to approach the study of human biological variation from very different perspectives. One is a genetic anthropologist interested in the global pattern of neutral genetic variation. The other is a bioarchaeologist interested in how patterns of phenotypic variation are shaped by culture in the United States. The exchange, coupled with reading of recent literature from various subfields of biological anthropology, led them to suspect that their inability to communicate is widespread within the discipline and that it hampers effective collaboration. Inadequate communication and collaboration likely lead to less than optimum progress in solving

[24] See Stephen J. Gould, 1981 for an illuminating account of this issue

important anthropological problems and send conflicting signals about human variation to students and the public, they concluded.

And as we have already noted, Cartmill 1998 indicates that to a surprising extent, physical anthropologists in different camps make similar assertions, cite similar sources, and express similar fervent opposition to racist practices and beliefs. The difference between them is mainly one of emphasis. The findings that one group admits grudgingly and seeks out reasons for disregarding are spotlighted by the other group as the central facts that reveal the way things really are.

This leads to two questions. First, does positing research programs get rid of the question of the influence of the wrong kind of values? Second, how do we solve a debate where competing research programs are coming up with different answers? The solution to both questions is to realize that the approach of various research programs have consequences. Whatever a person chooses as his or her research program will lead to theories and beliefs, which will lead to others and so on. However, some beliefs do not comport with each other, having internal contradictions and others do not map on to the world very well. If a research program leads to beliefs that do not comport well with what is found in the world this is an empirical problem that indicates it may have failed in describing phenomenon in the world and thus, it fails the empirical values test. If it leads to beliefs that do not agree with other well supported theories and it generates terms and definitions that lack clarity and are ambiguous, vague, and circular, these are conceptual problems that indicate a lack of understanding and a failure to clearly explain the phenomena being considered, thus, it fails the non-epistemic values test. I suggest that this is what is happening in the race debate with the race realist position. With this in mind, I want to move to a consideration of the existence of biological race.

CHAPTER 5

THE BIOLOGICAL REALITY OF RACE – DOES RACE EXIST?

In chapter two the question had to do with giving a theory of race. We asked, "What is biological race?" That is to say, we examined attempts people made to show what is it about an individual or group that makes that individual or members of that group the same as others in the group and different from others in another group? Note that this type of research project seemed to assume that races exist. I suggested that two major theoretical frameworks had emerged in history as a way to define races, the idea of race based on essentialism or typology, and the idea of race based on populationism. The idea of race based on essentialism answers the question, "What is race?" by indicating that a race is a group that shares a certain essence with others in the group and does not share that essence with others outside the group. I argued that this typological view of race fell from favor along with scientific support of races as naturally existing, discrete groups with the rediscovery and development of Gregor Mendel's work in genetics. It was found that no hereditary essence passes from generation to generation and thus there was no basis for the typological traits that proponents unsuccessfully sought in support of their theory.

The populationist concept maintains that there are no unique common features or 'essence,' no entirely distinct groups, but only average differences in certain traits. Race based on the population concept answered the question, "What is race?" by indicating that race is the average difference between groups. We noted that based on populationism there were several possible empirical bases for race; phenotypic, geographical, genotypic, and genealogical. After examining each of these, I concluded that human populations have not been geographically isolated for long enough periods, and during all of natural history there has been too much

inbreeding between populations for any of these notions of race to be correct. And while we can find all kinds of differences between groups they do not seem to allow us to make any meaningful biological distinctions.

In this section I will consider the question: Does biological race exist? This question, unlike the previous one questions the assumption that races exist. It does not ask, "What are the races?" but asks "Are there races?" This question implies another research program from the previous one. The previous question, what is biological race, carries with it the metaphysical assumption that there are entities in the world that correspond to some notion of biological race and implies the epistemological assumption that they can be discovered. It carries with it the methodological process of examining those entities that are considered races and pointing out just what it is that allows for the distinctions. The question, "Does biological race exist?", does not carry with it the metaphysical assumption regarding the reality of race as an entity. In fact, it questions that assumption.

I want to pose two problems facing those who posit the reality of biological race, a classification problem, which considers the notion of race with regard to the issue of non-epistemic cognitive values and a sorting problem, which considers the notion of race with regard to the issue of epistemic values. I will maintain that when so examined the notion of biological race seems to be an incoherent concept, one that does not seem to match the actual state of affairs in the world, and results in arbitrary distinctions that do not fall along biological lines. While this type of examination cannot show that races do not exist (it is difficult to prove a negative) it does show that it is reasonable to conclude that the notion of biological race is not correct.

The Classification Problem

The classification problem starts with the observation that racial classifications do not seem to possess the properties associated with a classification system. Bowker and Star 2000 define a classification system as a set of boxes (metaphorical or literal) into which things can be put to then do some kind of work. They suggest the following as ideal properties for a classification system:

1. There are consistent, unique classificatory principles in operation. So for instance, one type of principle of ordering would be to classify things by their origin and descent. A genealogical map of a family's history of marriage, birth and death would be an example. Another type of principle would be when biologists order the living world; they use the same rules to define humans (Homo sapiens) as a species as they do to define chimpanzees (Pan Troglodytes) as a species.[25]

2. The categories are mutually exclusive. Categories are clearly demarcated bins, into which any object addressed by the system will neatly and uniquely fit. So in the family genealogy, one mother and one father give birth to a child, forever and uniquely attributed to them as parents. Or a rose is a rose, not a rose sometimes and a daisy other times.

3. The system is complete. With respect to the items, actions, or areas under its consideration the system provides total coverage of the world it describes. So, for example, a botanical classifier would not simply ignore a newly discovered plant, but would find a place for it in the system.

Now while this is an ideal set of properties there are systems that come close to it. The Periodic Table of the Elements comes to mind as a system that comes close to this ideal. However, racial classification seems to fall very far from it. There does not seem to be any

[25] This second example comes from Malik, 2008, 45

objective rules for deciding what constitutes a race. As we saw above, using population as a theoretical framework does not seem to work as a basis for race. And the notion of population as applied to race does not seem to be a very conceptually clear notion. For instance, there are no generally accepted answers to the following questions: How many generations of isolation are necessary to form a racial population? How large must a racial population be? How much difference in gene frequency does there have to be between populations before we call them races (Zack 2002, 69)?

Also, there do not seem to be any objective rules for determining to what race a person belongs. People can belong to many things that we call races at the same time. One can be of the white race, the European race or Caucasian race. In the classification of the natural world, the same animal can be a chimpanzee, a mammal and a vertebrate but the species Pan troglodytes, the class Mammalia and the phylum chordate – animals with backbones- occupy different levels of the taxonomic hierarchy; each is a distinct classificatory unit. White, European and Caucasian are all considered to be the same classificatory unit, a race (Malik 2008). But White designates a color, European a geographical area, and Caucasian a race.

Also, there do not seem to be any objective rules for determining how the various terms and categories for race should be used. Note the use of terms for race that Malik 2008 found in the following article in the New England Journal of Medicine entitled, "The importance of race and ethnic background in biomedical research and clinical practice."

> The genetic determinants of the majority of these disorders are currently poorly understood, but the few examples that do exist demonstrate clinically important racial and ethnic differences in gene frequency. For example, factor V Leiden, a genetic variant that confers an increased risk of venous thromboembolic disease, is present in about 5

percent of white people. In contrast, this variant is rarely found in East Asians and Africans (prevalence, ≤1 percent). Susceptibility to Crohn's disease is associated with three polymorphic genetic variants in the *CARD15* gene in whites; none of these genetic variants were found in Japanese patients with Crohn's disease. Another important gene that affects a complex trait is *CCR5* — a receptor used by the human immunodeficiency virus (HIV) to enter cells. As many as 25 percent of white people (especially in northern Europe) are heterozygous for the *CCR5–delta32* variant, which is protective against HIV infection and progression, whereas this variant is virtually absent in other groups, thus suggesting racial and ethnic differences in protection against HIV…

Genetic variants of NAT2 result in two phenotypes, slow and rapid acetylators. Population-based studies of NAT2 and its metabolites have shown that the slow-acetylator phenotype ranges in frequency from approximately 14 percent among East Asians to 34 percent among black Americans to 54 percent among whites…

One of the best-known examples of a gene that affects a complex disease is *APOE*. A patient harboring a variant of this gene, *APOE e4*, has a substantially increased risk of Alzheimer's disease. *APOE e4* is relatively common and is seen in all racial and ethnic groups, albeit at different frequencies, ranging from 9 percent in Japanese populations to 14 percent in white populations to 19 percent in blackAmerican populations. However, a recent metaanalysis has demonstrated that the effect of *APOE e4* on the risk of Alzheimer's disease varies according to race. Homozygosity for the *e*4 allele increases risk by a factor of 33 in Japanese populations and by a factor of 15 in white populations, but only by a factor of 6 in black American populations; similarly, heterozygosity for the *e*4 allele increases the risk by a factor of 5.6 in Japanese populations, by a factor of 3.0 in

white populations, and by a factor of 1.1 in black American populations, (Burchard et al, 2003, 1170-1175).

Malik 2008 points out that what is striking about the passage is the contrast between the technical quality of the language regarding discussion of genes (V Leiden, CARD15, CCR5-delta32, NAT2, APOEe4), diseases (venous thromboembolic disease, Crohn's disease), and the explanation of the consequence of allelic variation (Genetic variants of NAT2 results in two phenotypes, slow and rapid acetylators) versus the descriptions of population differences. Population differences are entirely non-technical and often vague and confusing. Groups are described as "white", "white people (especially in northern Europe)", "East Asians", "Japanese", "Africans", and black Americans.

Also, the racial categories are unclear, speaking of groups that do not seem to belong with each other. Whites, a group defined by skin color, are compared to the Japanese, a group defined by geographic origin. In one instance, the three groups being compared are a Continental group (East Asian), an admixed group that has both African and Caucasian American ancestry but is socially defined as "black" (black Americans) and a group with a particular phenotype (whites). Yet all these different categories are treated as equivalent. Malik 2008 asks us to imagine a zoologist studying a hunting behavior and comparing dogs (a particular species) with reptiles (a class) with hairy animals (a description of physical appearance). The study would yield no useful information. The same seems to be true of comparisons of diseases between East Asian, white people and black Americans.

Further, regarding how scientists use the term, Helms et al. suggest that, "no explicit conceptual framework guides the procedures by which research participants are assigned to one category rather than another. Instead, the researchers' implicit beliefs about the meaning of race

(e.g., physical appearance, self-designation) serve as the operational definitions of racial categories and, consequently, the independent variable is also amorphous" (2005, 29).

Confusion is multiplied by the use of self-identified race as a way of assigning racial categories. Wang et al. note, "Research participants are typically asked to indicate their race (and/or ethnicity) by choosing one of a mix of options that reflect the researcher's notion of race, ethnicity, national origin, or ancestry. Because a person's self-reported identity incorporates a complex mix of biological, cultural, psychological, and behavioral factors not necessary determined by genotype or biology, racial self-referents can be highly variable and arbitrary, varying as a function of time, history, law, politics, social context, and emotions" (2005, 41).

Confusion also exists in the designation of racial ancestry. We saw above that Michael Levin's account of race focuses on the geographical location of ancestors. He believes that the definition of race that captures ordinary usage and the usage of evolutionary biologists refers to birthplace of ancestors. But consider African Americans. As was noted above they form an admixed group, that is, their ancestry derives from more than one continental group. If Levin is correct that because comparisons of blood group frequencies in the white, African, and conventionally identified American black populations indicate a white admixture of about 25% in the blacks in the American North and 10% in blacks in the American South, then as was noted above, why could we not say that African Americans should be considered a series of races depending on the percentages of the racial ancestry. As Malik notes, "The belief that genetically all African Americans should be regarded as African, whatever, their Caucasian ancestry, fits neatly into the political view of what constitutes an African America (2008, 51) and thus not a biological view. Or consider Hispanics as an example. The Hispanic population is made up of three continental groups: European, African, and Native American and thus if race has to do with

one's ancestry then "Hispanic" does not seem to be a biological category for race. Thus, the notion of ancestry in human genetics, like the definition of what generally constitutes a racial group, is ill-defined and unclear.

What I have attempted to show is that races are difficult to define and there do not seem to be objective rules for deciding what constitutes a race or to what race a person belongs. Racial classifications do not seem to possess the properties associated with a classification system and thus they fail to overcome the classification problem. It will be recalled that I made a distinction between theory assessing values (non-epistemic and epistemic values) and non-theory assessing values (ideological, psychological, ethical, etc. values). This distinction was based on the idea that values are good making qualities and how we determine what is good is different depending on the thing we are evaluating and why we are evaluating it. The classification problem deals with an issue of conceptual clarity which is a failure to meet the requirements of what we have defined as a non-epistemic cognitive value. A theory fails to meet this criterion when it does not help us to understand or explain some phenomenon, or when it does not help us to remove ambiguity, or to reduce irregularity to uniformity, or when it fails to show that what happens in the world is somehow intelligible and expected. As I have indicated, conceptual problems have to do with assisting us in understanding and explaining the world. The failure of the classification problem for "race" is really a failure to meet these various criteria – it fails the non-epistemic cognitive values test - it does not help us in explaining or understanding the world.

An objection to this line of thought comes from Armand Leroi, 2005. His comments suggest that a clear classification system is not needed to determine that races exist and what the races are.

> The physical topography of our world cannot be accurately described in words. To navigate it, you need a map with elevations, contour lines and reference grids. But it is hard to talk in numbers, and so we give the world's more prominent features—the mountain ranges and plateaus and plains—names. We do so despite the inherent ambiguity of words. The Pennines of northern England are about one-tenth as high and long as the Himalayas, yet both are intelligibly described as mountain ranges….
>
> So, too, it is with the genetic topography of our species. The billion or so of the world's people of largely European descent have a set of genetic variants in common that are collectively rare in everyone else; they are a race. At a smaller scale, three million Basques do as well; so they are a race as well. Race is merely a shorthand that enables us to speak sensibly, though with no great precision, about genetic rather than cultural or political differences.

Thus, for Leroi all that is needed to determine race is to look at our genetic make-up and name the most prominent features that we find. The precision that comes with a classification system is not needed to determine race, it is in the genes. He says, "Study enough genes in enough people and one could sort the world's population into 10, 100, perhaps 1,000 groups."

It seems what Leroi is rightly describing is the reality of human variation. Yes, humans do vary but does that variation equate to race and more specifically, does that variation allow for meaningful classification? Physical topography, as he points outs, varies as well, but is every "bump" a mountain range? As Malik notes, "if anything from the compost heap at the bottom of my garden to Mount Everest is a [mountain], then … the category [mountain] tells me little that is interesting or useful about the real world. If you have to wonder whether you need crampons and oxygen to visit the compost heap or if you believe that you can turn up on Everest in shorts

and sandals, then something is clearly wrong with your classification of 'ground elevations'" (Malik, 2008, 34-35). Now, just because there are classification problems that result in conceptual confusion, this does not mean that a thing does not exist after all many would argue that until recently there was conceptual confusion regarding the term "planet" that resulted in classification problems (and as a result Pluto was determined to be a dwarf planet) but that did not mean that planets did not exist. But if any moving body in the sky can be called a planet then the term seems to lose its meaning. And in a similar manner, if we can have 10, 100, or 1,000 groups that can all be called a race the concept of race seems to lose its meaning. Second, Leroi has raised the issue of genes and we need to ask the question what happens when we do consider the genes. That is, what happens when we move from the conceptual (is there a way to clearly classify human variations that does not result in conceptual confusion, which it does not seem to be) to the empirical (does looking at genetic variation tell us anything meaningful about race)? In other words, just because the notion of race seems to be conceptually confused, it still might be the case that we can find various patterns in the human genetic makeup that allows for us to say that races do exist empirically in the world. So we move to this empirical issue next.

The Sorting Problem

The sorting problem has to do with the amount and distribution of genetic variation. Research today considers genes in the broad context of genomes. A *genome* is defined to be one complete copy of all the genes and accompanying DNA for a species. Each person holds two copies of the genome, one copy obtained from their mother's egg and other from their father's sperm. Each copy of the human genome consists of over three billion pairs of the repetitive building blocks of DNA. These building blocks are called *nucleotides*, of which there are four

different kinds, denoted by the letters A, C, G, and T. The letter designations for nucleotides are used as a short hand for the chemical bases that give the four kinds of nucleotides their distinctive properties. Surprisingly, the genome contains far more DNA than is required to encode all of the information in our genes. In fact, only about two percent of the genome encodes genes, and about half of the genome consists of repeated nucleotide sequences with no known function (Long, 2004).

In a study in Nature by Yu et al. entitled “Larger genetic differences within Africans than between Africans and Eurasians” (2002 May; 161(1): 269–274), DNA sequences comprising a total of 25,000 nucleotides from 50 different genetic loci were obtained for each member in a sample of 30 individuals. Ten of these individuals were African, ten Asian, and ten European. The individuals on each continent were selected from local groups residing in widely spaced regions. Two key observations were made. First, the level of genetic diversity in the human species is more consistent with a small population than it is with a large population. This suggests that the human population grew extremely fast from a small group. Second, and more important for our purposes, nested subsets describe the structure of genetic diversity across geographic regions. What this means is that the genetic diversity in people outside of Sub-Saharan Africa is for the most part a subset of that found in Sub-Saharan Africans. Thus, nucleotide diversity within Europeans is nearly that within Asians while it is much higher in Africans. An interesting implication of this finding is that most common variations in the genome could be identified by studying a sample composed only of Africans, but a good deal of the common variation would be missed by studying a sample composed only of Europeans or Asians.

The pattern of genetic diversity displayed by what Yu et al. found is consistent with a model that postulates a succession of ancient founder effects that occurred with range expansion and the human occupation of new continents, the out of Africa model. Genetic differences are not just a result of natural selection but also of genetic drift and founder effect.

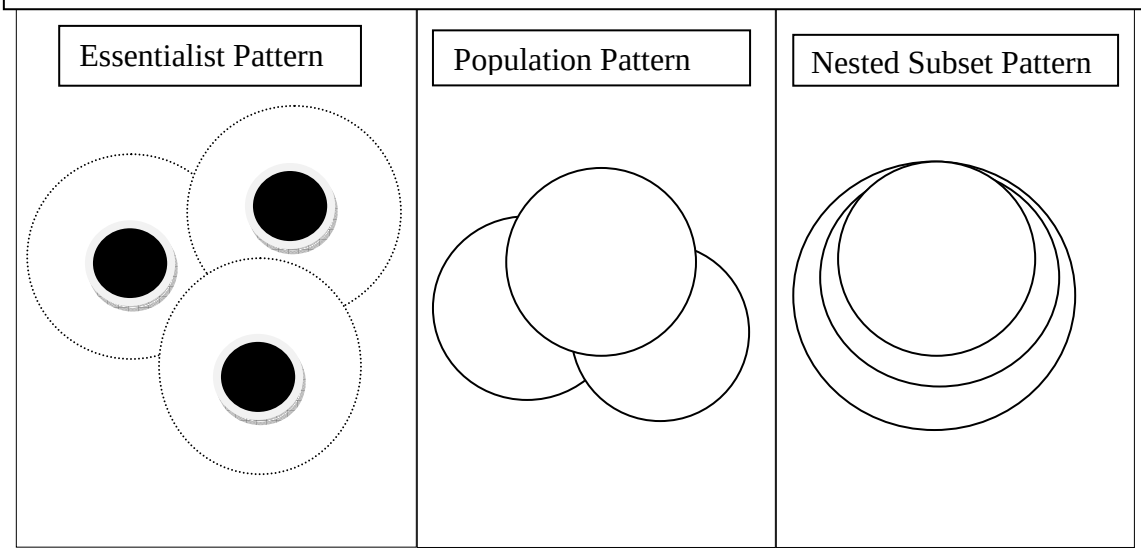
Genetic drift refers to the random (not selected for) changes in gene frequencies that can occur over time, especially in a small population. In a small population alleles for genes can appear by chance in the next generation in different frequencies than what would be expected in a large population and this drift can become permanent. Just like a few coin tosses might result in five heads in a row where as with the increase of the number of tosses the ratio of heads to tails will even out. Genetic drift is a similar phenomenon; the genetic frequencies can be unexpected and permanent.

The founder effect is the most extreme case of genetic drift. If a small number of people leave the original population and a member of the new community happens to possess a rare gene, that gene may well become common in the new group. An example is Huntington's disease in the Afrikaner population of South Africa (Long 2004). Thus, under the out of Africa model the combination of founder effect and genetic drift may have helped create genetic differences between the groups as small groups who had slightly different genetic profile migrated out of Africa and the genetic profiles of the new and old population continued to move apart as these migrants picked up mutations. In this view, the origin of the species was in Africa and the species expanded out of Africa to Asia, Europe and the Americas.

Long 2004 compares the nested finding with essentialist and population concepts of race (see figure 6-1). He notes that the essentialist and population concepts of race describe the same basic pattern of variation, but the two views differ in how they attach meaning to it. According to

essentialist views of race, variation among individuals is insignificant (represented by the dotted circles), while the ideal types are paramount (represented by the solid circle). Population race concepts recognize that variation is the central necessity of the evolutionary process and omit the notion of an ideal type. But neither of the two captures the actual pattern of DNA variation (nested subsets) and thus both theories fail with regard to empirical adequacy. Empirical adequacy is an important epistemic value the lack of which, as I have tried to argue, can spell doom for a theory.

Figure 6 - 1
A Comparison of the Essentialist, Population and Nested Subset Pattern of genetic diversity, adapted from Long, 2008



In both essentialist and population patterns of race the three races are drawn to show about the same amount of variation and roughly the same overlap between centroids. The implication is that each race attains about the same level of distinctiveness. But surprisingly, the variation in human DNA sequences does not show this pattern at all, but a nested subset pattern, Long 2008.

If Yu et al. and Long are correct then the actual pattern of DNA variation creates some unsettling problems for the definition of races. For example, it implies that non-Africans

constitute a race with respect to Africans, but Africans are not a race with respect to non-Africans. This contradicts the intuitive expectation that a race classification is symmetrical, i.e. that if A is a race with respect to B, then B is a race with respect to A. But under the nested subset pattern, non-Africans (A) may be considered a race with regard to Africans (B) because they are a subset of Africans but Africans (B), not being a subset, are not a race with regard to non-Africans (A). Yet we would expect that if non-Africans are a race with respect to Africans then Africans would be a race with respect to non-Africans. Also, Long notes these finding show for instance, that Asians and Native Americans would be a single race with respect to Europeans, but Native Americans would be a distinct race with respect to Asians (Long 2004). Thus, race concepts fit very poorly to the actual pattern of genetic variation.

The major objection to the sorting problem comes by the way of a computer program called *structure*. The *structure* program implements a model-based clustering method to infer population structure from genotype data taken from many loci and then allocates individuals into populations. It takes any set of data and attempts to find a rational way of dividing it into as many subsets as requested. Thus, it can be used to estimate the number of genetic clusters or populations present in a given data set (Bolnick 2008). Rosenberg et al. 2002 studied 377 DNA sequences from 1056 individuals spread across fifty-two populations world-wide, and using *structure* they identified six main genetic clusters, five of which correspond to major geographic regions (Rosenberg et al. 2002, 2381). Since the five major geographic regions comprise Africa, Eurasia, East Asia, Oceania, and America these results have been interpreted as showing that racial divisions based on continental ancestry are biologically significant (Bolnick 2008). Thus, it seems that the program divides the population of the world according to the continent on which they live and when the world's populations are divided into six groups five of them correlate

closely with what we call races: Africans, Caucasians, East Asians, Australasians, and Native Americans.

The study seems to suggest that however small the differences between races, the differences are sufficient enough to distinguished groups as various races (Malik 2008). If this is the case then the sorting problem has been solved. Races can be picked out from the sorting of our genes. So our issue seems to be how do we reconcile two empirical studies that seem to give different results, the Ning, et al. study that shows that the pattern for population groups is a nested subset pattern and thus implies that there are no races, and the study using *structure* that seems to show that there are five population groups and they correspond with what we call races. Which study gives the correct view? As we might expect they both do. But it depends on the question to which one is seeking an answer.

Once we understand where the number five or six that s*tructure* came up with as the number of population groups came from it is easy to see what is going on with the s*tructure* program. When running the program the *user* defines the number of populations (called K) that *structure* is asked to find and then given any value K the program searches for the most probable way to divide the sampled individuals into the pre-defined number of clusters based on their genotypes. Thus, the fact that structure identified a particular number of clusters is insignificant; it does so simply because the user told it to do so. As Bolnick noted, the program also identified 2, 5, 10, and 20 genetic clusters using the same data and the highest probability was associated with a particular replicate of K = 16 (2008, 77). Thus, it is uncertain what number of genetic clusters best fits the data set, but there is no clear evidence that K = 5 is the best estimate. So Bolnick asks why so much emphasis has been placed on the results of the analysis using K = 5. She suggests that these particular results have been emphasized simply because they fit the general

notion in our society that continental groupings are biologically significant (2008, 77). But to repeat what was indicated above, while each continental group does possess a genetic profile slightly distinct from others, the consequences of early human migration, continental groups represent neither the greatest degree of genetic differentiation within humankind, nor necessarily the most useful way of dividing up human populations. The greatest genetic differentiation is not between continental groups, but between Africans and non-Africans and as was indicated, from a medical diagnosis perspective, the correct diagnostic approach is not to try to determine the continent of ancestral origin, but to get a clue of the problem by asking which if any of the high frequency regions the person ancestor's is from, regions that could encompass multiple continents.

Conclusion

So let us take a look at where we have arrived. In the section on biological theories of race we found that two major theoretical frameworks emerged in history as a way to define races, essentialism or typology and the idea of race based on population. Typological or essentialist race referred to the theory that humans are divided into natural, discrete groups that can be identified and distinguished by their intrinsic properties revealed in appearance and the like. These properties vary across races but are consistent among members of the same race and are thought to pass as a bundle from parents to children and thus maintain the races as separate groups. Typological race fell from favor along with scientific support of races as naturally existing, discrete groups with the rediscovery and development of Mendel's work in genetics, the modern synthesis. It was found that no hereditary essence passes from generation to generation

and thus there was no basis for the typological traits that proponents unsuccessfully sought in support of their theory.

The alternative idea of open populations emerged as the model. This theory holds that population groups exist, but they do not have natural or fixed limits. They are distinguished based on differences in gene frequencies. Race realists today reject the idea that there are essential, unbridgeable, unchangeable differences between populations and base the idea of race on some aspect that is founded on populationism.

We suggested that based on populationism there were four possible empirical bases for race; phenotypic, geographical, genotypic, and genealogical. Regarding the phenotypical basis for race we found that considering traits does not provide a basis for race because differences are clinal and therefore there is no clear distinction between groups, and we saw that the distribution of traits do not match each other, that is, there is no co-variance of traits.

Regarding the genotypical basis for race we found that virtually all of genetic variation is within a population, not between 'races,' and very few genes are exclusive to one part of the world. Thus, the racial differences are trivial and overall, there is more variation within any group than there is between one group and another.

The major problem with the view that race is based on the geographical location of ancestors is that it does not spell out why geographical location of ancestors are to be taken as races. It cannot be because genetic differentiation is greatest when defined by continent because, as we have seen, the greatest amount of genetic difference is between Africans and non-Africans and not between continents. It cannot be because this is the most useful way to divide up humans because continent of origin is too broad of a category; with groups within them too

heterogeneous to lump together. Thus, it was concluded that defining continental groups as races is an arbitrary choice.

When we asked the question does race exist we found two problems, what we called the classification problem and the sorting problem. Racial classifications do not seem to possess the properties associated with a classification system. This results in a lack of conceptual clarity which is a failure to meet the requirements of what we have defined as a cognitive value. The amount and distribution of genetic variation, the sorting problem – shows a problem with both essentialist and populationist theories of race. The essentialist and population concepts of race describe the same basic pattern of variation, but the two views differ in how they attach meaning to it. The correct pattern seems to be a nesting patterning and it contradicts the intuitive expectation that a race classification is symmetrical and also, it implies that there is no good way to sort groups genetically into multiple races. Thus, the results of the classification problem and the sorting problem seem to show again, that racial distinctions are arbitrary.

As has been suggested, the scientific data seems to be the same with regard to human variation regardless of who does what study, but how the data is interpreted determines what claim about the existence of biological races is made. The interpretation of the data is based on the question that is asked, and thus, the research program one is engaged in. In other words, the epistemologically based question coming out of a research program implies a metaphysical view about the world and this view suggests various methodological approaches to get at the implied metaphysical view in order to answer the epistemologically based questions. But the study about the biological reality of race has shown that the various theories that come out of the various research programs can be assessed in the light of the values that are used by science to determine the way the world is. If a theory coming from some research program is found wanting in terms

of epistemic and/or cognitive values, then we have good reason to suspect it to be wrong. In this way we see that these various "scientific" values as opposed to social, ideological or other values are used, are sufficient, and ought to be used to answer scientific questions about the world.

Thus, our overall project has aimed to show that while there is a legitimate place for various non-cognitive values in science, i.e., in the context of discovery, investigation, and application, because of the nature of the context of assessment in determining what a good theory is, non-cognitive values are ruled out of the context of assessment. Our project also implies that when scientists disagree the disagreement does not necessarily mean that non-cognitive social and political values are the motivating factor. Disagreement can be the result of a particular approach, question or research program driving the debate.

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