Culture Shock and Multiculturalism

Culture Shock and Multiculturalism: Reclaiming a Useful Model from the Religious Realm

By

Edward Dutton

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In memory of my grandfather Harry Croft of Winchester (1923 – 2009)

‘On the whole my feelings toward the natives are decidedly tending to “*Exterminate the brutes.*”
—Bronislaw Malinowski, Papua, 21st January 1915, (Malinowski 1967, 69).

‘I see the life of the natives as utterly devoid of interest or importance, something as remote from me as the life of a dog.’
—Bronislaw Malinowski, Trobriand Islands, 27th December 1917, (Malinowski 1967, 264).

‘. . . our final goal is to enrich and deepen our own world’s vision, to understand our own nature and make it finer intellectually and artistically. In grasping the essential outlook of others, with the reverence and real understanding, due even to savages, we cannot help widening our own.’
—Bronislaw Malinowski, London, circa 1921, (Malinowski 1922, 517-518).

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EDWARD DUTTON
OULU, FINLAND
12th December 2011.

CHAPTER ONE

STEREOTYPES AND ANTHROPOLOGY

Introduction

It used to be widely accepted amongst anthropologists that when they did fieldwork with foreign cultures they experienced something called 'culture shock.' This book will argue that 'culture shock' is a useful model for understanding a part of human experience, whether you are a businessman abroad, an exchange student, a traveler or an anthropologist. However, in its most widely-known form, 'culture shock' has been heavily influenced by what are best termed 'implicitly religious' beliefs: logically unsustainable but fervently held beliefs, broadly of the same kind as those held by religious groups. These can be contrasted, in a spectrum, with scientific beliefs, which are held cautiously and due to the evidence proving them to be true.

Some of the proponents of these implicitly religious beliefs are using culture shock to keep the beliefs alive in the public consciousness; some, vaguely accepting these ideas, are employing it to make money and others are neglecting it for reasons also underpinned by implicit religiosity. The plight of culture shock reflects the plight of anthropology, the discipline which gave birth to it, and the dominance, in that discipline, of various forms of implicit religiosity, such as postmodernism and cultural relativism (see Kuznar 1997). This book will show how the most well-known model of culture shock is part of a broadly religious discourse. By so doing, it will aim both to better understand culture shock and to show how it can still be useful, if divorced from its implicitly religious dimensions, to broadly scientific scholars. It will also suggest how anthropology itself might be stripped of its ideological infiltration and returned to the realm of science.

The idea of 'culture shock' had been around many years when it was systematized by the Canadian anthropologist Kalervo Oberg (1901 – 1973) in a 1954 presentation (Oberg 1954) later published in *Practical*

Anthropology (Oberg 1960).[1] Oberg divided culture shock into four stages. Stage one was the 'honeymoon phase' where you found the new culture fascinating. Stage two was 'reaction.' You reacted against the culture and experienced a breakdown where you became angry and irrational and sought-out fellow expatriates and co-nationals to create negative 'stereotypes' about the host culture, and to romanticize your own. After this there was 'resignation,' where you could accept your situation and develop various coping mechanisms, and, finally, you had a realization. After sufficient immersion, you understood that the culture was 'just another way of living' and you realized that it was not worse than your own culture but simply a product of a different history. Casting aside your stereotypes, you had overcome culture shock. Oberg's model gradually made it into the popular sphere with self-help books and business seminars offering advice on how to defeat 'culture shock' and the business-damaging stereotypes it supposedly leads to.

In Defence of Stereotypes

In this first chapter, we will do three things: We will defend scientific anthropology and, in doing so, defend 'stereotypes;' we will examine the central debates in the history of anthropology and highlight the influence of implicit religiosity over the discipline and, finally, we will set out how we will show that culture shock has become infused with various forms of implicit religiosity and how, stripped of these, it can be scientifically useful.

Modern-day anthropology generally dislikes what Oberg called 'stereotypes' and attempts to dispute them (e.g. Peoples and Bailey 2009, 95). When an anthropologist reviews a new ethnography for an academic journal and does not especially like it, he may suggest that it engages in 'stereotypes.' That he uses the word 'stereotype' at all might be regarded as problematic. When Oberg presented 'Culture Shock,' 'stereotype' was a relatively new word and he had to explain what it meant. Now, I would argue that it is an emotive and insulting word with connotations of racial prejudice and can be a fallacious means of attacking a work with which you do not agree. 'Stereotype' has the potential to work as a smear. Fostering a charged atmosphere and playing on popular, fervently-

[1] Whenever individuals are discussed as historical figures, rather than simply cited, I have, where possible, provided their nationality, years of birth and, where appropriate, death. When referring to the work of individual scholars, I have, where possible, stated their discipline.

believed ideologies (see Ellis 2004) it may lead to an academic opponent's work being unjustifiably dismissed or to intellectual opponents feeling intimidated and so less likely to engage in debate.

But stereotypes can be useful as long as they are used with caution. Any taxonomy engages in stereotypes and does so because it is only through some kind of essentialism that you can gain a foothold on the mountain of knowledge and so begin to ascend it. The notion of species, and making divisions between the appearance and behaviour of different species, involves some form of stereotyping whereby individual differences are played-down, but it is useful as a way of beginning to make sense of the world. In creating a stereotype, we tend to present an example of the 'type' which embodies all the characteristics that are seen to distinguish that 'type.' Accordingly, the stereotype is an extreme or ideal version of the category in question. But breaking up reality into these stereotypes is useful to the extent that it helps us to better comprehend reality and make predictions. Thus, we might make a distinction between 'extrovert' and 'introvert.' This may seem simplistic and the division involves extreme types, in that most people fall somewhere between the two extremes. But this division is useful as long as it permits us to make predictions about the behaviour of individuals according to which stereotype they can cautiously be placed in, and if it allows us to better comprehend their behaviour. Equally, in the following analysis, we will employ Philosopher Sir Karl Popper's (1966b) division between 'civilization' or 'science' and 'religion' or 'tribe.' Again, this is a binary division. It does not seem to appreciate the complex differences between different 'religions' or different 'civilizations' or the way that most societies fall somewhere between the two extremes. But positing these two extremes (the archetypal 'civilization' and the archetypal 'tribe') is useful if it allows us to better make predictions about these societies and understand how and why they operate as they do. If, for example, we wanted to understand why a given society produces far more inventions than its neighbour, Popper's division might be a useful means of helping us to understand why. It would be a predictive, and thus a scientifically useful, model.[2]

The view that anthropologists 'don't like stereotypes' is itself a stereotype because I can think of a few that defend them (e.g. Fox 2004, Ch. 1), though it is probably broadly true. There were certainly stereotypes, when I was an undergraduate at Durham University, about the kind of student who studied 'anthropology.' In other universities, or time periods, the stereotypes would have been different and I would argue,

[2] See Chapter Five for a discussion of the origins of stereotypes.

therefore, that these undergraduate stereotypes may be useful in understanding the essential divides in anthropology and even the religious influence upon it. The Durham University anthropologist was probably female, from a professional middle class (though not upper-class) background,[3] inclined to wear ethnic-style clothes, politically socialist (and probably accepting cultural relativism) and well-travelled, perhaps on a 'Gap Year' abroad before starting university.

In some ways, this stereotype might put us in mind of American anthropologist Margaret Mead (1901 – 1978).[4] She was female, from a professional middle class background, educated at Columbia (an Ivy League university), had eccentric dress sense (she used to carry a staff) and was politically left wing.[5] However, an important difference between Mead and our undergraduate stereotype about 'anthropologists' was that, whatever their merits (see Orans 1986 or Freeman 1983), Mead produced works describing 'cultures' and attempting analysis of them. Anthropologists, according to the stereotypes when I was a student, seemed to do this less than one might think. They were encouraged (so it seemed) to reject the whole idea of 'culture' as old-fashioned (or 'static'), somehow imperialistic (because it assessed a 'culture' through a foreign category), too popular, 'reified' . . . and so describe their observations without using the concept at all. And because they were 'Westerners,' they were encouraged to write essays engaging in self-analysis which looked at their position as anthropologists, their influence on those they studied, the political dimensions to the 'exercise' or 'project' of anthropology and whether they could really make any statements about whatever it was they were studying. This is, of course, a 'stereotype' but to the extent that it was accurate (based on limited empirical observation) this evidenced postmodern influence on anthropology. Even more so than with other 'modern' degrees, such as 'sociology,' anthropology was academically light, self-absorbed, produced lots of jargon, and was heavily influenced by various fashionable but inconsistent ideologies (see Chapter Two).

This would not have been the stereotype of an anthropologist had I been beginning my degree at Durham University 1899 rather than 1999. Putting aside the fact that you could not study social anthropology at Durham as a separate discipline back then, the stereotypical anthropologist of 1899 would have been quite different. He would probably have been

[3] See Argyle (1994) for a discussion of class dynamics in Britain. For America, see Ortner (2006).
[4] For an interesting examination of Mead's life see Shankman (2009).
[5] I appreciate the debate over the usefulness of this division. For further discussion see Gabb (2007) or Bobbio (1996).

professional middle class like his modern equivalent. But he would have been male, politically conservative (favouring gradual change and practicality over idealism[6]), he would have accepted, unlike many cultural relativists, that a significant component of character was hereditary and fixed and he may even have believed that character was substantially inherited, he would have dressed fairly conventionally and may well not have been especially well-travelled, though his study might be packed with intriguing paraphernalia from tribes and foreign countries. Unlike the modern anthropologist, he would probably have studied Theology, Classics, Philosophy and possibly Law as well.[7] Indeed, social anthropology itself was grounded in science and, in particular, Evolutionary Theory.[8] By contrast, in November 2010, the American Anthropological Association voted to remove the word 'science' from its Mission Statement, leading to an acrimonious academic and media debate (see Wood 29th November 2010 or Berrett 30th November 2010).

In some ways, this stereotype might put us in mind of Sir James Frazer (1854 – 1941). Also from a professional middle class background, Frazer, who was Scottish, originally studied Classics at Glasgow University and later at Cambridge University before briefly studying Law. His understanding of religion was clearly influenced by Evolution, as he argued that societies moved through a hierarchy of stages from Magic to Religion and then Science. He did not accept cultural determinism, arguing that there was a common human nature which could not be altered. And he was not particularly well travelled. He went to Italy and Greece but otherwise relied on sources, writing copious letters to sojourners and missionaries (see Besterman 1935). His magum opus *The Golden Bough* (Frazer 1922) did not rely on fieldwork but rather on 'armchair anthropology,' rather like an historian relying on others' observations as his primary sources. When I began to study anthropology in more detail as a postgraduate, one of my tutors began the course by dismissing the work of Sir James Frazer precisely because he was an 'armchair anthropologist.' Polish anthropologist Bronislaw Malinowski (1884 – 1942) had sunk Frazer's anthropology through his enforced

[6] This is the common lexical definition of 'conservatism,' such as in the Oxford English Dictionary.

[7] Perhaps the nearest person to this stereotype at Durham University at the time was F. B. Jevons (1858 – 1936), a polymath who lectured Classics and Philosophy there from 1882 to 1930, but also researched Comparative Religion and Anthropology. See Davies (1991) for a biography.

[8] For histories of anthropology see Eriksen (2001) or Stocking (1998).

extended fieldwork in the Trobriand Islands during World War I.[9] He persuaded anthropologists that the way to make sense of a culture was through lengthy participant observation; through actually going to the tribe and living as it lived until you were competent in its language and even perceived the world through the native's eyes (Malinowski 1922, 21 - 25), thus understanding it through your own observation.

Frazer's own anthropology had involved a problematic lack of context. Frazer drew upon, for example, other people's descriptions of a particular tribal practice and used them to justify a particular model of the development of society but he was not able to probe further to see how this practice fitted into the societal whole and what its actual historical origins were. In trying to understand, for example, the origins of the succession practices of the priesthood of Nemi, he would speculate about its origins by drawing upon out-of-context descriptions of historical tribal practices in other parts of the world (see Gellner 1998, 116). Frazer himself was extremely impressed by Malinowski's use of lengthy participant observation and immersion and he made this clear in the preface he wrote to Malinowski's *Argonaut's of the West Pacific* (Frazer 1922, vii – viii). However, for Malinowski's school, Frazer's anthropology, especially its emphasis on reconstructing human prehistory from current tribal practices, was unacceptably speculative because the required evidence was lacking. The anthropologist should engage in purely 'functionalist' theories. This involved concentrating more on the present, treating a society as an organism, attempting to understand how the separate parts functioned as part of the whole and ultimately how they related to basic human instincts. However, this relationship with biology declined amongst Malinowski's successors (see Gellner 1998, 121).

Malinowski also stressed that tribal organizations and Western societies, though superficially very different from each other, were actually broadly extremely similar and that there was no clear intellectual difference between Westerners and those whom he studied. These views also became highly influential in anthropology (see Sandall 2001). However, though Malinowski said this in public, this is not what he recorded in his posthumously published diaries. In them he writes, for example, 'I see the life of the natives as utterly devoid of interest or importance, something as remote from me as the life of a dog' (Malinowski 1967, 167). He also talks about his 'strongly hating the

[9] As an Austro-Hungarian subject, the Australian authorities gave Malinowski the choice either of being interned as an enemy alien or living on the islands. He took the latter option (Stocking 1983, 71).

niggers,' (264).[10] Clearly, he is extremely emotional. That he later sees things very differently, when he is writing his ethnography back in London, would be congruous with Oberg's model of culture shock.

Science and Anthropology

Apart from the lack of actual fieldwork, it is my contention that Sir James Frazer's form of anthropology is, overall, a better one than either that influenced by Margaret Mead or the postmodernists, whose ideas we will examine. Frazer should not simply be dismissed, as Gellner (1998, 113) dismisses him, as a kind of 'King Harold' figure from the anthropological Dark Ages who is slain by William the Conqueror (Malinowski), heralding the start of the anthropology's real history.[11] Frazer's anthropology is genuinely in the tradition of science. It draws upon the available empirical evidence, in the form of sources, to answer discrete research questions and speculate intelligently based on this available evidence and this is what science does (see Popper 1963 and below). It describes and taxonomises but it does so in order to better understand how the world (or a little part of the world) works, so that further scientific enquiry can be built upon its findings, and it shows the relationship between these findings and broader scientific knowledge. Malinowski's innovation, of which Frazer approved, provided a better way of obtaining the required evidence. But with its grounding in biological science and Evolutionary Theory, Frazer's anthropology is 'consilient.' It can be reduced, in theory, to the next level of science, biology, which in turn can be reduced to chemistry, physics and, ultimately, mathematics.

Biologist Edward O. Wilson (1998) argues that we can achieve a more in-depth understanding of the world through unifying the sciences, social sciences and humanities. In summary, he maintains that knowledge is

[10] Writing in Polish, he uses the word *nigrami*, which is not a Polish word. According to Stocking (1983, 102, footnote 2), 'What Malinowski seems to have done is render the English epithet phonetically ("nigr") and add the Polish ending ("ami") . . .' Stocking demonstrates that the word was an epithet at the time, though not as strong as it is now, and that Malinowski only uses it after several years socialising with colonial administrators, prior to which he always writes 'natives.' But what is more important is that Malinowski feels hatred for these natives.

[11] Brunner (2003, x) observes that Gellner employed 'dramatic metaphors,' though I don't think the comparison between Frazer and Malinowski and King Harold and William the Conqueror works. There was no battle between the two anthropologists. Frazer approved of Malinowski's innovation.

reached both by fragmentation (in the sense of reductionism in order to gain purchase on an object of study) but also, crucially, by reconstruction. We are witnessing an 'ongoing fragmentation of knowledge' (8) as we divide into many sub-disciplines and 'consilence' would consequently be positive for scholarship. Consilence is metaphysical but the 'success' of science provides a strong case for its veracity. Wilson notes that ethics, social policy, environmental policy and social science are clearly closely related domains yet they stand apart with separate practitioners, modes of analysis, language and standards. The result is confusion with regard to the areas of overlap, yet it is here 'where most real world problems exist' (10). Wilson therefore argues that these specialists must, and can, reach an agreement on standards of abstract principles and evidentiary proof. He then proceeds to prove how humanity and social science explanations are ultimately question-begging (and, in some cases, simply ideological) and fully make sense only with 'consilience' into biology and psychology. Wilson's idea has been criticised, with critics citing a belief that a 'rational society' is not the same as a 'scientific society' but it has been countered that these critics then use 'science' as their ultimate model for a rational society. Wilson has also been criticised for an idiosyncratic view of 'the Enlightenment quest' but this does not undermine the logic of consilience (Segerstråle 2000, 360-361). The academic insult 'reductionist' has also been levelled. Wilson answers that a finding is only scientific if it can be reduced to the terms of the science underpinning it and it is the nature of science to look for the simplest explanation based on the evidence. To find this problematic, as implied by using the term 'reductionist,' is to find science and ultimately logic problematic.

Consilience characterizes scientific enquiry. It must be possible to reduce research in a particular discipline down to the discipline which ultimately underpins it. Secondly, 'science' must involve certain agreed characteristics. Listing the characteristics which a word connotes is widely regarded as a crucial aspect of any definition (e.g. Hurley 2008). With regard to 'science,' anthropologist Lawrence Kuznar (1997, 22) argues that these are the following:

> (1) It must be solely empirical. If a discipline is based on unprovable or inconsistent dogmas it is not scientific.
> (2) It must be systematic and exploratory.
> (3) It must be logical. This means, in particular, that fallacious arguments, such as appeal *ad hominem*, appeal to motive or any other form of rhetoric must be avoided. It also means that the research and arguments must be consistent.

(4) It must be theoretical. It must attempt to explain, to answer questions and, where possible, predict.
(5) It must be self-critical, prepared to abandon long-held models as new information arises.
(6) Its propositions must be open to testing and falsification.
(7) As it wishes its theories to be falsified and as anybody can, in theory, falsify them, science should be a public activity.
(8) It should assume that reality is actually real and can be understood. It should be epistemologically optimistic.

I think that Kuznar's defence of 'science' is highly persuasive. He posits the archetypal 'science' which can be contrasted with the archetypal 'religion' or 'tribe.' Apart from both being systems of thought and practice, 'religion' would be the opposite and would therefore involve:

(1) Belief in some kind of force or hidden hand behind the empirical universe.
(2) Inconsistent beliefs.
(3) Being unsystematic in reaching knowledge, by doing so through intuition, for example.
(3) A lack of logic, exemplified in fallacious argumentation and even violence.
(4) Predictions which are vague or inaccurate.
(5) A lack of self-criticism and thus fervent attachment to beliefs (even if they are proven to be incorrect) and ritual practices.
(6) Assenting to beliefs which by their very nature cannot be falsified.
(7) Secrets and knowledge open only to initiates and/or strong group borders, whether in terms of belief or practice, rendering some people members of a fervently rejected out-group.
(8) Epistemological pessimism and an acceptance of mysteries.

We will examine in more detail how best to define religion in Chapter Three. However, there are four small areas where I would disagree with Kuznar or wish to nuance what Kuznar has to say.

Firstly, he seems to imply that a discipline is not 'scientific' if it takes as a starting assumption a view which is controversial amongst scientists (103). But I do not think that this is unscientific if the scholar first argues that it should not be controversial and demonstrates that the view is in fact scientific or, at least, it is inconsistent to argue that it is not scientific because something else, very similar to it, is accepted as scientific.

Secondly, he implies that if a scholar states a specific motivation that is not purely the pursuit of truth (such as improving humanity in some way) this renders his research unscientific. There are many theories regarding how we should define the word 'truth' (see Dowden and Schwartz 2004). In common usage, however, something is 'true' if it is 'in accordance with the facts or actuality' (e.g. Oxford English Dictionary). This is known as 'correspondence theory,' it implies that there is an actuality to be known and, as we need, as much as possible, to have shared definitions of terms to engage in discussion (see Hurley 2008), it is how I will define 'truth' in this discussion. It might be argued that a motivation other than just the pursuit of truth is ultimately scientific because it reflects a desire to create a more scientific world in which, for example, people are more innately intelligent or the intelligent have a better opportunity to become scientists. I think these kinds of motivations are only problematic if they are placed in the way of the pursuit of truth. Otherwise, and Kuznar accepts this (217), scientists are likely to have emotional reasons for studying what they do. Their own motives (as long as they do not interfere with science) are irrelevant.

Thirdly, we should remember that there is a degree to which it is difficult for anthropology to be fully 'logical' in a very particular sense. Assuming we accept that there are benefits to qualitative analysis (perhaps that it can reveal information that might not otherwise be revealed) we must accept that certain assertions made by anthropologists about their fieldwork may be useful and possibly accurate though not scientifically proven with a probability of more than fifty percent. Thus, from the viewpoint of Logic, many anthropological assertions are at best cautiously useful and, at worst, unsound inductive arguments. But this does not mean that such evidence cannot be useful, if treated with caution. As such, I would emphasise that anthropology should strive towards a scientific standard to the greatest extent possible and, in so doing, will experience many of the successes of that towards which it strives.

Fourthly, regarding being 'self-critical,' it might be argued that a sense of humility is an important part of science. In order to be 'self-critical' there is a degree to which you must avoid being too sure of yourself and this sits well with a community of scientific practice in which new research is only deemed acceptable if deemed so by accepted scientific authorities. But, at the same time, there is a confident dimension to science. If science involves challenging received knowledge, then there comes a point where humility must be abandoned. Accordingly, the idea that science must be 'humble' is problematic if it is deployed as a way of dismissing new thinking as not being in the spirit of science because it is

(arrogantly and precociously) challenging conventional thinking. Science must be self-critical but not so much so that its humility leads to scientists avoiding challenging conventional thinking. Accordingly, a balance must be struck between humility and 'iconoclasm' (see Andreski 1974, 249) just as, as I will argue below, a society might strike a balance between tribalism and civilization or the scientific community might strike a balance between authority and the space for challenging, new thinking. This is never easy to do and it may be a continuous source of debate.

There are anecdotes about 'great scholars' being 'humble.' However, this is not borne out by the facts, if we assume that a 'great scholar' must make a significant contribution to his field. 'Geniuses,' amongst whom 'great scholars' are generally found, tend towards having psychotic personalities and are thus more prone to self-confidence, even arrogance, than humility (e.g. Simonton 1984). A prominent example, in anthropology, is New Zealander Derek Freeman (1916 – 2001). In refuting Margaret Mead's research on Samoa (Freeman 1983), he is widely seen to have discredited the then anthropological dogma of cultural determinism. It has been argued that Freeman was heavily motivated to discredit Mead by 'narcissistic personality disorder' (see Caton 2005).

Consilient anthropology, Wilson (1975, 547) argues, involves being like a 'zoologist from another planet.' Just as a zoologist examines a species as an outsider, you must attempt to achieve the same thing with the human species and its various sub-groups. Though empathy with your subjects may help in some ways, to assess them there is a degree to which you must remain detached. If you do not, then we must question whether your pursuit is scientific and wonder if it reflects some latter-day form of tribalism or religiosity (see below). But before we examine in more detail the assertion that non-consilient anthropology is effectively religious in character, it would be helpful to highlight in more detail the essential divides between anthropologists such as Sir James Frazer and those of the more naturalistic school because the naturalists would argue that their school was, indeed, congruous with science (e.g. Rees 2010b).

Positivism versus Naturalism

There are two main understandings of the aim of ethnography and to a certain degree they relate to the essentialism/ nominalism debate which we can take back beyond Plato and which we have already touched upon. We can divide between 'positivism' and 'naturalism.'

In stating this dichotomy, I must emphasise that I am aware that there are many shades in between and there is, of course, no absolute divide in

anthropology. Dean Siatta writes (comment on Berrett 30th November 2010), 'The cartoonish "science vs. anti-science" framing of this latest attempt to analyze tensions within the discipline is both dismaying and regrettable when epistemologies are available that can effectively bridge or integrate the values of science and humanism.' Siatta does not give an example of one of these 'espistemologies' but, that problem aside, the summary of the divide not only involves appeal to insult (implicitly terming an opponent's views as child-like) but this is a strawman summary of how reasonable scholars employ essentialism.

According to Popper (1966b), on the one hand, *essentialists* – following the Platonic view – argue that every concept is an imperfect reflection of the ideal of that concept (which, according to Plato, can be found in the world of forms accessible through the intellect). These forms are unchanging and it is the task of science to describe the true nature of things and thus focus on the definitions of terms. Philosopher Daniel Dennett (1995, 95) observes that scientists should 'of course' define their terms but 'only up to a point.' He provides a modern version of the so called *nominalist* critique. *Nominalists* are more interested in understanding how something behaves in different circumstances and they make use of a concept if it is helpful (see Oderberg 2007). There will always be different ways of defining a set term and different definitions will be useful in different situations. But to insist, in an essentialist fashion, that terms must always be perfectly defined before being employed leads us to a situation where we can do very little. As Dennett (1995, 39) observes, there are manifold difficulties defining a word such as 'island' but, aware of the intellectual difficulties, we can still use the concept as a tool to further our understanding. Some form of essentialism is necessary in order to taxonomise the world and further understand it but focussing so intensely on the precise meaning of words leads us to a situation where we cannot really go any further.

The first suggested aim of ethnography is usually summarised as 'positivism.' According to archetypal positivists, the aim of science is to answer causal questions. Science is based around the empirical method, scientists accrue their observations and answer their questions within certain theoretical frameworks and ultimately within the framework of Logic. We can apply this method to understanding societies. The problems are that the societies change more quickly than the physical universe, the observer may influence the society by observing it and the observer is personally and psychologically fallible. Nevertheless, positivists argue that 'science' is the only legitimate model for social science even if they

engage in the immersion fieldwork associated with the 'naturalist' tradition.

By contrast, many 'naturalists' argue that social science involves a different form of knowing. They appeal to science for their model but in a different way. For naturalists, the social world should be studied in its natural setting, which includes immersing yourself in the culture. The primary aim should be to describe what happens and how the people involved understand what happens. For many naturalists, a key element of this is having an attitude of 'respect' for the 'natural world' and the worldview of the people being studied; something sometimes summarised as the empathetic perspective of the anthropologist. Naturalism generally makes a number of other assumptions. Many naturalists are sceptical of the view that people's behaviour can be scientifically predicted, claiming that human life is too complex for this. There is a movement amongst naturalists which is sceptical of science more broadly and many suggest that you should have a commitment to the group being studied above any commitment to scientific principles (see Kuznar 1997). In this latter sense, naturalism turns anthropology into a kind of political project; something more than just science.

Regarding the immersion idea, it strikes me that, ultimately, this is what any thinker can do. You immerse yourself in something relatively new, as an outsider, and you can thus reach new insights about it as you become an insider.[12] But the common naturalist position with regard to theory appears to be problematic. If you describe what you see you are inherently engaging in some kind of theorisation because in order to describe it you must use language and language involves categories which in turn reflect a worldview and therefore some kind of theory, no matter how implicit (see Gentner 1982). Accordingly, once you start describing a society, you are engaging in some kind of theory and you are therefore not that different from the positivist.

However, anthropologist Tobias Rees (2010a) has provided a very interesting defence of the opposite position in the *Journal of the Royal Anthropological Institute*. He argues (162) that 'anthropology is informed by the insight that general theories hinder rather than further ethnographic inquiry . . . what is needed now is not general theories but rather the

[12] Berger (1963, 23) thus argues that sociological discovery involves 'culture shock' but without the geographical displacement. Nader (1974) argues that 'culture shock' can occur in any research setting. I would agree and add that this evidences the accuracy of applying the idea of 'shock' to cultural immersion.

insight that theories are not the remedy. The way forward is inquiry.'[13] I am not sure that this implied distinction between 'general theory' and 'inquiry' is particularly easy to sustain. Wilson (1998, 42) argues that the hard sciences are akin to gold prospecting. Scientists find an area where there are still things we do not quite understand and they start digging. They are interested in discrete questions, just as Rees suggests is true of anthropology (Rees, 162): 'How does this work? Why does this do this?' However, one cannot divorce their 'inquiries' from 'theory.' One cannot 'inquire' without some kind of theory because an inquiry involves answering a question and the possible answers are inherently 'theories.' To make the 'native point of view' (162) comprehensible to the reader there will be some kind of theoretical foundation, no matter how implicit. A 'general theory' is an extension of discrete theories: it is an attempt to answer a relatively broad – 'general' – question which unites various discrete inquiries. If this is a hindrance to ethnography, it is hindrance to any branch of science. It risks the same obvious formal fallacies but this does not mean it is of no use – if drawn upon carefully – in making sense of an object of inquiry and furthering understanding. A general theory of canine behaviour is useful, even if there are philosophical pitfalls to be navigated, and the same is true, though even more careful navigation may be required, of the general theories of human behaviour suggested by scholars such as Wilson (1975). It is not 'general theory' that is problematic in ethnography but 'dogmatic general theory,' and this point is equally true with any other branch of science. As Dennett (1995, 39) notes, 'essentialism' may be a problem but 'cautious essentialism' is necessary to pursue enquiry.

Rees (2010b) has presented a thought-provoking response to these concerns. In an article entitled 'On the challenge – and beauty – of (contemporary) anthropology,' he states: 'One can certainly think through the conceptual presuppositions one brings to research without articulating them in the form of a falsifiable theory' (896). This statement seems to imply that you should act on intuition, rather than work out and present your theories and if this is what Rees means it is not scientific. Nevertheless, this is stated as a crucial criticism and yet there is no attempt to back it up at all. Rees (2010b, 897) argues that fieldwork involves disorientation and a shattering of preconceptions, as if there is a moment of vacuum-like nothingness from which insights appear *ex nihilo*: 'But what happens when one enters the field? Is it not always the case that

[13] It might be argued that the need to 'enquire' implicitly accepts that there is an objective reality which can be better known.

fieldwork . . . profoundly shatters the preconceived? . . . this derailment invalidates the preconceived, robs the ethnographer of the conceptual framework with which she has arrived in the field.' These are interesting observations but expressing ideas – through language – involves some kind of category system and, accordingly, no matter how implicit, some form of theory about how things operate, because a world-view, and thus theory, underpins language. If what Rees describes is his own experience then this appears to be akin to a religious experience and we will explore this in Chapters Eight and Nine. But it is appeal to intuition to ignore a reasoned argument in favour of your own emotional experiences. These experiences should be examined scientifically to see how common they are, in what circumstances they occur and, within a scientific framework, why they occur.

Secondly, returning to my critique of anthropological naturalism, I do not agree that human life is so different from life studied by zoologists. The zoologist influences that which he studies and he is fallible, the zoologist may deal with quite complex organisations but the zoologist would not suggest that you cannot predict animal behaviour. You can do this and you can do this with humans and, indeed, different groups of humans and this is convincingly proven, even if you must be careful not to be too sure of yourself and even if the predictions might be broad and comparative. For example, Wilson (1998, 184) observes that understanding human 'mating strategy,' influenced by the fact that females have more at stake in sexual activity than males, has allowed biologists to correctly predict, cross-culturally, human patterns of mate-choice, courtship, sexual permissiveness, paternity anxiety and gender-based behaviour differences. 'Mating theory' predicts that, on average, men will be concerned with their partner's fidelity while women will be more concerned with material resources and security. Men would be prone to promiscuity, women more coy and selective. This is all borne out by the datum. Human societies are more complex than chimpanzee societies (because they have developed more complex communication for example) but they are not fundamentally different and, therefore, they do not require a fundamentally different method; they require a more nuanced and more cautious version of the same method. This, again, raises the issue of essentialism.

Historically, we have been used to drawing a clear line between 'humans' and 'animals.' Darwin, and research built upon his theory, demonstrates that there is no such clear line (see Darwin 1859). Humans are a modern form of ape and this is reflected in much of their physical appearance and their behaviour (see Wilson 1975). Though individual humans differ, just as individual chimpanzees do differ, we can, carefully,

posit an average 'human nature;' a human behavioural stereotype. Behaviour reflects an animal's brain and that brain, like any other part of a biological organism, will have survived in the form in which it has (giving humans the inclinations which it does) because this form led to the greatest reproductive success in a particular environment. To cut humans off from animals, as somehow fundamentally different, is not in the spirit of either Evolution or cautious essentialism and it risks missing an important means of making sense of human behaviour. Moreover, as we will see later, the *tabula rasa* model of human nature, which was once dominant in social science, is no longer tenable. Margaret Mead's (1928) seminal research in Samoa purported to prove cultural determinism, and therefore that humans were a blank slate, through its findings that Samoa was non-violent, sexually liberated and was a negative instance in terms of teenage angst and rebelliousness. This was the positive proof of cultural determinism and it was discredited by Freeman (1983) who found that Samoa was extremely violent and Mead's research was flawed, not least because mischievous teenagers deliberately lied to her (see Freeman 1999). Moreover, Mead only spent a few months conducting her fieldwork and did not live as a Samoan, choosing instead to live at the American Naval Base. Even more importantly, scientists have found, and continue to find, specific genes which account for specific and significant personality differences, such as differences in anxiety levels (see Chapter Ten).

The idea of having a commitment to the group (or 'empathy' for the group) above your commitment to science appears to me to be an unjustifiable idea in that it renders groups of humans special and above the scientific method, and this, I would argue, involves a *de facto* failure to accept the full consequences of Evolutionary Theory, which positions Man as evolved from the zoological sphere. Regarding the issue of respecting the group, I would submit that we should approach the group with an open mind. But that does not mean you should simply present the group's view and avoid any analysis with which they might disagree. This would be putting a dogma or principle above the search for truth; a sign of religiosity, not science. It may be useful to have an 'empathetic perspective,' just as it would be when studying any pack animals (see Lyons and Fitzgerald 2005, 239), but if this is placed above the search for truth then this leads us towards tribalism, where people's feelings, about dogmas and rituals, are put above the pursuit of truth (see Popper 1963).

Moreover, all the points which can be made about the supposedly subjective anthropologist could be made, perhaps less stridently, about the 'hard science' zoologist, further demonstrating that a different method is not required. The zoologist influences the animals he studies to some