Testing Times

Confirmation in the Historical Sciences

A thesis submitted for the degree of Doctor of Philosophy of The Australian National University

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Statement of Originality

I confirm that the material contained in this thesis is my own original work, and that, to the best of my knowledge, it contains no material previously published or written by another person, except where due reference is made in the text.

Ben Jeffares

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Abstract

In this thesis, I argue that a good historical science will have the following characteristics: Firstly, it will seek to construct causal histories of the past. Secondly, the construction of these causal histories will utilise well-tested regularities of science. Additionally, well-tested regularities will secure the link between observations of physical traces and the causal events of interest. However, the historical sciences cannot use these regularities in a straightforward manner. The regularities must accommodate the idiosyncrasies of the past, and the degradation of evidence over time. Through an examination of how the historical sciences work in practice, I show how they can confirm these unique causal histories, and the limits to their confirmatory strategies.

1. The Problem

Approximately 2.5 million years ago, creatures with bodies very much like ours, but with significantly smaller brains, appeared throughout the old world. From its first appearance in Africa, through the Levant, Europe and the Indian sub continent, and eastwards as far as Java, this proto-human creature spread remarkably far and fast. Seemingly, it did so without language, without clothing, and without much beyond a set of enigmatic tools.

Such an account of the past, even this brief one, has much in common with stories. In fact, Misia Landau has outlined how many accounts of human evolution have a great deal in common with hero myths. A human ancestor confronts a series of challenges, and these challenges transform a pre-human ancestor into a modern human (Landau 1984; Landau 1987; Landau 1991). Yet, while many early versions of human evolution may have had parallels with myths, the stories that told by paleoanthropologists are not fictions. They are the products of research and investigation. They are the products of science. As such, we are entitled to ask if such a story is true or false. We want to know how much credence to give to such a story. In short, we want to know why we should believe such a story.

But story telling makes for a strange sort of science. After all, the stories that paleoanthropologists tell are not generalisations about the past, and we typically think of the sciences as confirming generalisations. When we think of the sciences, particularly the experimental sciences of physics and chemistry, we think of regularities: laws, models and generalisations. And we also think of the testing and confirmation of these regularities, and the role they play in explanations. The aim of paleoanthropology however is not to come up with regularities. Paleoanthropology is interested in telling the story of the evolution of human ancestors at various levels of detail.

This example is from the study of human evolution. However, a number of other historical sciences tell similar stories. A geologist might claim that a particular geological deposit is the result of a sequence of events, providing a temporal ordering for a number of quite specific processes that have structured the deposit over time. Paleobiologists may well provide an ordering for an evolutionary lineage, linking a series of fossils into an evolutionary history. An archaeologist might construct a history for a site, connecting disparate layers of archaeological evidence into a narrative sequence. The construction of narratives, of stories of the past, is a project that all the historical sciences share.

At first blush, these narratives do not neatly fit into our common conception of science. The "product" of historical research, these narratives and accounts of particular things, seem rather different than the usual product of the sciences. The legacy of positivism within philosophy of science has included an assumption about what the sciences are trying to do. Good scientific products —the outcomes of research— are law like statements, regularities or models with a wide applicability over a number of cases. These hypothesised regularities should be applicable across time and space.

This idea of good scientific practice has structured much of the debate within philosophy of science: for instance the debates about covering laws and the role of laws in explanations. How these products of research, these regularities, get selected, confirmed and deployed as explanations is very much the essence of the debate within the philosophy of sciences. In contrast, the stories the historical scientists want to tell will typically be of a very particular sequence of events and a very particular subject. This does not fit neatly into a law-based model of science.

On top of this, these stories are the *final* product of the historical sciences. One does not typically use these stories, no matter how scientific and reliable they are, to generate further claims about generalities. These stories are not just datum for further claims; they are ends in themselves. A complete geology of the earth would not just be a catalogue of the laws of geology and the ways that these laws reduced to physics. A complete geology of the earth would be a collection of historical accounts at various levels of description. At its most detailed, it would be a collection of histories that accounted for the history of particular geological formations. This in turn would be compatible with a larger scale history that accounted for a region, which would be coherent with a general history of the earth. So too for biology. To account for the evolutionary history of an organism is to elucidate its adaptive history and its evolutionary trajectory. This in turn might be subsumed into a larger history of a bio-geographic region, or a phylogenetic group.

The rare philosophical analysis of how the historical sciences confirm their accounts of the past have typically attempted to subsume the histories of historical scientists into a law-based view of science. Where philosophers of science have looked at the historical sciences, there has been a presumption that historical claims should be seen within a law-like framework. Philosophers have talked about history, and indeed, there is a thriving philosophy of history, but lessons learnt within the philosophy of history have rarely been transferred to an examination of the historical sciences per se.

What seems to have happened in philosophy of science is that physics and chemistry have been taken as the exemplars of good scientific practice. Chemistry and physics generate as their "product" regularities and law-like statements that are universal, and philosophical debates have centred on these sciences: their methodologies and the products of their research. When philosophers have paid attention to other sciences, they have done so by either trying to "shoehorn" these sciences into the physics/chemistry model —seeing other sciences as derivative of physics and chemistry— or they have gone their separate way, and discussed the particular problems of a special science. Now, while this is obviously a caricature, I take it that this is a sketch of the philosophy of science that people would recognise.

I suggest that philosophical work in some of the historical sciences is now in a position to help provide a more complete picture of the sciences as a whole. The historical sciences have a different character from the experimental sciences, and that can potentially shed light on scientific progress, and scientific methodology in general, through a process of determining their distinctive features, and by determining what they have in common with the experimental sciences.

The first section of this chapter will briefly outline the source of challenges to the traditional positivist conception of the sciences. In particular, the challenges come from philosophical work done on history, biology, and archaeology, with a small amount of work done on geology.

This thesis is a synthesis of ideas developed in response to the problems of various historical sciences. In part, it tries to find a commonality across the historical sciences as a whole. It also attempts to find commonalities with the experimental sciences. In so doing, I isolate particularly 'tricky' parts of the project of understanding the past. Part of what we are interested in is whether there are any distinctive constraints on the historical sciences.

This thesis is driven by how the sciences actually work. It is not going to come up with some rules about what makes a 'good' science. Rather, I am interested in detecting commonalities in confirmatory strategies across the historical sciences. The questions I am interested in are: Given a claim about the past, what would it take for a historical scientist to abandon that claim, and accept a new one? What sort argument is required? Is an archaeological argument different from a geological one or are there underlying commonalities? Is there something distinct about the sciences of the past that separates them from the "experimental" sciences?

In short: How do historical scientists actually confirm their claims about the past?

1.1 The Cast of Characters

In this section, I am going to provide a brief overview of some of the historical sciences that are going to play a role in this thesis as suppliers of ideas, of challenges to a traditional conception of sciences, and as targets for analysis. They challenge a traditional conception, and supply ideas, because philosophical work on the special sciences has put pressure on our conception of science a practice. Consequently, we can bring to this project work done in the philosophy of history, the philosophy of biology, and the philosophy of archaeology. Ideas developed in these fields can contribute to an overview of the historical sciences. Ideally, we should be able to see commonalities across these disciplines.

Given the unique character of the historical sciences it is surprising that geology has received little philosophical attention. A quick check of the index of philosophy of science texts will undoubtedly show lots of references to physics and chemistry. Texts of the last few decades will show increasing references to biology. Geology however, is strangely absent. Where geology is mentioned at all, it is frequently as a case study in theory change, with continental drift exemplifying (See for instance Giere 1988 Chapter 8).

This lack of philosophical attention to geology is reflected in the references to geology in journals. A search of the contents of the journal Philosophy of Science from 1988 to 2008 reveals 248 articles containing the word 'physics,' 67 containing the word 'chemistry', 206 containing 'biology,' and a mere 14 that mention 'geology.' Of that 14, only two, Carol Cleland's 2002 article (Cleland 2002), which we will discuss in detail in chapter 3, and Derek Turner's reply which we will cover in chapter 5, (Turner 2005), are the only articles that appear to address geology's problems as a historical science. Archaeology, another explicitly historical science, has not fared much better, receiving only 7 mentions in Philosophy of Science over the last ten years.

Geology is then a good target for an analysis of the historical sciences. If an account of the commonalities of the historical sciences is right, it better include geology as the science of earth's history. Nevertheless, as a philosophical sub-discipline, it is not an active contributor of ideas to this thesis, apart from the work of Carol Cleland. Theoretical work done by geologists themselves will play a role in this thesis. But geology's main role in this thesis is as a target for analysis.

1.1.1 Philosophy of History

The emphasis on laws and generalities has come under pressure from three distinct areas of philosophical research: history, archaeology, and biology. For historians, the notion that their historical narratives can be effectively subsumed under broad covering laws is contrary to the intuition many historical scientists have; narratives work as explanations without recourse to laws. This is particularly the case in human history, due to an apparent lack of anything resembling covering laws or generalities. Without well worked out "laws" of human history or of human social or economic activity, the notion of covering laws seems problematic to say the least. We will discuss this in more detail in the penultimate chapter on cognitive archaeology. This intuition about the utility of narratives, and the concern about the lack of covering laws, raises questions about a view of the historical sciences based upon regularities, and as David Hull notes:

> …contemporary philosophers of science have been no more anxious to overthrow their entire analysis of science just to accommodate the intuitions of historians than sixteenth-century scientists were to abandon all of Aristotelian science just to eliminate a few epicycles. Thus, philosophers have been forced to argue either that historical narratives do not concern unique sequences of events or else that they are not explanatory. (Hull 1975 p253)

Positivist philosophers have typically tried to argue that, either a) contrary to historians intuitions background generalities *do* in fact play a role in history, or, b) the product of historical research, historical narratives, are not really explanatory.

This thesis argues that narratives are explanatory. It also argues that the historical sciences do use regularities. However, this thesis also argues that those regularities are rarely law-like in the sense that positivists would recognise: the regularities utilised by the historical sciences are much more restricted in scope. And it also argues that the utilisation of regularities is in a fashion distinct from the experimental sciences.

1.1.2 Philosophy of Biology

The second source of pressure on the traditional story comes from another area of philosophical enquiry: philosophical work looking at biology and in particular Darwinian evolution. Unlike physics and chemistry, biology does not regularly cite laws. And, crucially for us, biology can in many cases be historical as well. The theory of Darwinian evolution is a historical science. The paleoanthropological example of the evolution of human ancestors draws attention to the historical and Darwinian nature of the narrative of human evolution.

Biology is a science we take seriously, and as noted earlier, a science that has had a great deal of philosophical attention. Equally, biology has in very important ways rejected a positivist account. Biology doesn't fit in with the positivist conception of the historical sciences any more that history does. It also provides narratives and histories of the evolution of organisms, lineages and life on earth.

1.1.3 Philosophy of Archaeology

The philosophy of archaeology is perhaps the oddest contributor to a philosophy of historical science. Its relationship with the philosophy of science is something of a historical quirk to do with personalities within the discipline. Although much theoretical work in archaeology had gone on prior to the 1950s, (Trigger 1990) as archaeology became more professional, it became more self reflective about its empirical endeavour and its status as a science. Building on the earlier work of Julian Steward and Leslie White, in the1960s through to the 1980s the American archaeologist Lewis Binford articulated a view of archaeology that became known as the "New Archaeology," and later processual archaeology.

Archaeology on Binford's view needed to be scientific, and Binford took Hempel's hypothetico-deductive model of science as the template for a scientific archaeology. This adoption of the Hempelian model was clearly problematic. Like history, archaeology possessed nothing that looked even remotely like the covering laws that are needed for the hypothetico-deductive method to work. Despite this, Binford's adoption of a philosophical position prompted archaeologists to ask meta-theoretical questions about their discipline. These questions centred upon the aims of being a science, how this was to be achieved, and whether this was good aim to have. Archaeological self-reflection gained the attention of a small number of philosophers. The resulting mix between archaeology and philosophy became a "meta-Archaeology." On the philosophical side, the contributors included Merilee Salmon, Peter Kosso, Patty Jo Watson and Alison Wylie (Walker 1981; Salmon 1982; Kosso 2001; Wylie 2002). These philosophers, many working in a post-positivist tradition in philosophy, abandoned a law-like view of archaeological science. Consequently, they came up with alternative views on what makes archaeology a science.

What we have here then, is three disciplines that have received philosophical attention: history, biology and archaeology, and one that appears to be ignored, geology. All are overtly historical, and none fit easily into the positivist conception of science.

This thesis is in part a synthesis of ideas that emerged in the philosophy of these historical sciences. In particular, this thesis is a synthesis of ideas from the philosophies of biology and archaeology. This synthesis is directed at answering two questions we can ask of the historical sciences in general. The first question is: What kind of science produces narratives as the product of its investigations? We will address this question in chapter 2, utilising the analysis historical narratives provided by David Hull. The rest of the thesis will address the second key question for the historical sciences: How do we confirm these historical narratives?

Having confidence in the veracity of historical narratives is important After all, if we are looking at human pre-history, some of the stories that paleoanthropologists tell make claims about us, and our view of ourselves. If we have reasons to doubt some of what they say, then we might have reason to qualify the claims they make. A claim that men have an evolved tendency to behave aggressively is a claim we must take seriously if we are to understand our social world.

Moreover, sometimes such historical stories are supposed to be explanations of contemporary phenomena. We use history as a means of explaining the present. We explain the form of biological traits in reference to a selection history. We explain geological features with reference to a history of erosion processes, or the long-term movement of tectonic plates. And we explain some facts about humans, both biological facts and cultural facts, with reference to history and historical processes.

In some cases, we even use history to predict the future, and to make decisions about possible courses of action. Claims about our environmental future, how differing sea levels, temperatures, atmospheric composition and so forth will alter our world, are informed by our understanding of changing environments in the past.

So, our understanding of the past is important. It tells us about the world, tells us about ourselves, and tells us about our possible futures. Consequently, understanding the confirmatory strategies of the historical sciences, and how reliable they are, is also important.

1.2 Confirming History

The problem of confirmation in the historical sciences is one that has a number of facets. The most obvious of these, and the one that most concerns the popular observer, is the fact that the historical sciences appear to have a problem unlike that of the experimental sciences when it comes to observational access. The past cannot be witnessed, so there is no way of knowing "for sure" what happened. This is true, and there is no easy way to answer the die-hard sceptic. If nothing else, the history of metaphysics and epistemology demonstrate that there is no knockdown answer to pig-headed scepticism. But the question for the less obtuse is one of probabilities. How reliable, how much credence can we assign to the claims of historical sciences? Surely, less than that we can assign to the experimental sciences for their claims? After all, the historical sciences seem systematically different from the experimental sciences.

This intuition has three aspects. Firstly, there is the problem of observation. Historical scientists cannot witness the past, so what evidence do they have for their claims about the past? Secondly, the historical scientist is not interested in testing generalities, they are interested in testing narratives about particular things in the world. This leads to the question about how a narrative, and a causal history of a particular and single object can be tested. In this regard, the historical sciences suffer in comparison with the experimental sciences, which are interested in generalities.

The third problem is that not only are processes in the past unobservable and unique, they are frequently on a scale that can be both temporally and physically large. This is particularly true of geology. So how do historical sciences deal with the issue of physical scale, particularly when combined with the first two difficulties they face. We will look at the problems one at a time, but it should be borne in mind that these problems overlap and reinforce one another.

1.2.1 The Historical Inference

The first of the three problems that confronts the historical sciences is that of the problem of evidence. The logic of historical enquiry is frequently shaped by the fact that the historical scientist is engaging in an explanatory narrative for something that exists in the present, something that *is* observable. Contemporary observable phenomena are the initial target for the historical scientists' enquiry. The archaeologist Lewis Binford defines the task of the archaeologist in the following way:

> The Archaeologist investigates phenomena that he has reason to believe remain from the past. These investigations are conducted in the present, resulting in all the observational statements generated being contemporary facts. How does an archaeologist convert these contemporary observations or facts into meaningful statements about the past? (Binford 1981 p22)

Contemporary observations initially define the historical scientists explanatory project. The point is to explain the current state of things. As Binford notes above, they focus their investigations on phenomena that they "believe remain from the past," and then attempt to utilise these observation statements in making claims about unobservable past events. This basic inference, from observable physical evidence to some past cause or process, is what I will refer to as the "historical inference." From observable contemporary phenomena, a historical scientist infers something about the past. A good part of this thesis will be concerned with this issue, and the various forms the problem takes in different areas of science.

What counts as contemporary observable phenomena differs from discipline to discipline. For the historian, written texts of historical agents are observable phenomena; for the archaeologist, material remains; for the paleobiologist, fossils; for the geologist, contemporary features of the landscape. The historical inference, from observations of contemporary phenomena to a claim about the past is bedevilled with the problem of securing the link between observation and claim. There is frequently, as we shall see, debate about whether something is connected to the past in an informative way.

Nevertheless, this focus upon observable contemporary phenomena may well be what distinguishes the pseudo-historical from the historical sciences. Claims about a lost city of Atlantis are popular speculation, perhaps of interest to researchers of popular culture. Such speculation is unanchored by observations of physical remains; they emerge from myths and stories. This would change if archaeologists discovered a previously undetected sunken city, with some of the characteristics of the popular accounts. Speculation on Atlantis would be on a much firmer footing. Without such a contemporary observation, Atlantis and other such claims about the past remain ungrounded and outside the realm of legitimate speculation.[1] On this analysis, the problem is less history's un-observable nature: the problem is how to test and confirm these inferences from contemporary observations to past causes. We will deal with this problem in detail in chapter 3.

This problem is not one that is unique to archaeology: geology, the "Paleo" sciences like Paleobiology and Paleoanthropology, and the forensic sciences, are all engaged in making claims about the past, and all

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[1] Despite what many would take as similarly mythical in texts such as the Bible, Biblical Archaeology and the Archaeology of other religions retain creditability because of ruins, remains and cultural leftovers that are observable contemporary phenomena. Various parts of religious texts can be effectively "anchored" by alternative sources of evidence that are harder to question. Sacred texts play a role in these investigations, but what distinguish them from empirically idle myths are alternative sources of contemporary evidence such as ruins. Consequently, no one doubts that ancient Hebrew cities such as Jericho, and Kings existed, even if one doubts the embroidered stories of the Bible and the precise nature of the events related in such texts. It is one thing to make up a story of a mythical past. Its quite another to find the ruins of a long lost city that conforms to that myth. As we shall see, the fact that events in the past leave multiple lines of evidence is crucial in providing credence to historical scientists.

of these claims are initially dependent upon contemporary observable physical evidence. An evolutionary history for a species starts out with observations of a contemporary, observable organism, or a contemporary observable fossil. Geology works backwards from contemporary geological features, archaeology from ruins, remains, and on occasion, written texts.

Consequently, the way that pre-historians work shapes some of their story telling. The first task is to determine what the story is to be of in particular. The logic of investigation used by pre-historians proceeds from things known in the present to claims about the history of those things. We cannot see the past. However, we can claim that the reason that things are the way they are now is because of past events. Therefore, the logic of pre-historical investigation is such that the identification of contemporary phenomena comes first.

So while part of what we want is narrative about the past, the logic of the discovery process is one of identifying contemporary phenomena that are traces of past events. A surprising variety of things are the results of the past. The shape of human skulls is a contemporary phenomenon shaped by history. A shard of pottery and the particle residues on a stone tool have a causal history. Landscapes and geological features are shaped by the past, but can be observed in the present. The bio-geographical distribution of species is a contemporary phenomenon that along with the adaptations and traits those species possess are the results of natural selection and a changes in the distribution of populations through time. DNA sequences and their variants within and across species are also contemporary phenomenon. In chapter 12, we shall see that the behaviours and cultural practices of modern human groups are also contemporary phenomena with important links to the past. However, in order to use these things as evidence of past events, we need to be able to reliably claim that these things *are* the result of past processes. This will be the topic of chapter 4.

What prevents pre-historians and paleoanthropologists from being about nothing and staying anchored, is the fact that they are telling stories about contemporary observable objects. One end of a lineage of changes is fixed, and that constrains the shape of that lineage. The more we know about what changes are possible, and what are not, between adjacent steps in a lineage, the tighter the constraints become. This potentially limits their capacity to make some claims about the past; we might ultimately want those causal histories to tell us about past people and behaviours. Nevertheless, initially pre-historians deal with observable phenomena, be they species, fossils, cultures, ruins, mountains or whatever, as their first order of business. The historical scientist's epistemic problem is how to go from the observable present, to the unobservable past.

The fact that we begin with the traces need not limit us to narratives about physical traces. Take for instance an archaeological find of a piece of pottery. One might be inclined to think that this logic of proceeding from observation to claims about the past limited one to hypotheses directly concerned with the physical facts of objects. There is a real issue here that has plagued archaeology for some time. Making claims about the composition of the pottery, its decoration and how this was achieved, all these seem eminently doable. Making further claims about the cultural significance of the pottery doesn't seem quite so bound by the observable facts of the pottery shard. It is difficult to confirm this kind of hypothesis. This issue is one that will be of crucial importance in chapter 11 when we look at archaeology, where much of the history we want to provide is not of technology and artefacts, but of behaviours and cultures, social practices and institutions. But the first order of business will, typically, be an observation about contemporary facts, and it is from these we shift to the unobservable. This historical inference is crucial and much of this thesis will be concerned about the move from the observable present, to the unobservable past, and how this process is made reliable.

1.2.2 The science of the local and the unique

I have been discussing three problems of the historical sciences. The first problem, outlined above, was the problem of making claims about an unobservable past based solely on contemporary evidence. The second problem is the difficulty of making claims about particular events based on particular pieces of available evidence. They are clearly entwined problems. The historical inference is frequently about particulars; how that thing there, got to be where it is now; how this thing here, was shaped by history.

In this section, we will examine the second of the two problems, that of producing a particular type of explanation: a narrative that accounts for a particular feature of the world. An archaeologist might want to provide a history of a particular archaeological site, or in some cases a particular archaeological find. A forensic scientist wants to provide a history to account for the death of a particular person. A geologist might want an account of the formation of a particular geological feature.

This focus on the singular and the localised makes the historical sciences quite different in intent from the experimental sciences. The experimental sciences, because they deal with indefinitely repeatable events and processes, initially appear to have a major advantage. The experimental sciences are interested in generalities. They want to know the regularities in the world. In contrast, the historical sciences are frequently interested in explaining particular things in the world. So, a physicist wants to account for the behaviour of matter generally. But an archaeologist might be interested in the subsistence patterns of the first human inhabitants of a particular island in the South Pacific. And a geologist might want to account for the formation of a particular geological feature. This apparent difference in interests, generalities versus particulars, leads on to the subsequent advantages that the experimental sciences appear to have.

Because the experimental sciences are interested in generalities, there are frequently more instantiations of a generalisation available for them to observe. A physicist can observe the behaviour of an object under the effects of gravity, and they can do so *repeatedly*. The chemist can repeatedly observe the solubility of sugar in warm tea without having to reconstitute her sugar cube; another sugar cube will do the same job. Hypotheses about generalities can be tested through repetition of observations. The experimental sciences can repeatedly observe situations, generalise from them, and successful hypotheses will make predictions about future observations. The experimental sciences' search for generalisations allows for the possibility of repetition of observations to confirm or falsify their hypotheses about the world in general. These observations can happen in the present, with observations of the starting conditions, intermediate phases, and so forth. The experimenter can witness the entire causal chain of events and effects from start to finish, repeatedly. The experimental scientist can even artificially induce situations to test hypotheses, perturbing variables in such a way as to get insight into what matters to a process, and what doesn't. The upshot is that the experimental scientist can manipulate situations, repeat them with variations, and come to conclusions about what matters by varying starting conditions and contingent factors.

The experimental sciences can confirm hypotheses about generalisation through repeated observations, and through experimentation on processes to which they have full temporal access. This is what gives us confidence in the generalisations of the experimental sciences. The hypothesis of the experimental scientist has a different relation to evidence than the hypothesis of the historical scientist. For the experimental scientist, the hypothesis that deals with generalities is a one to many relation; one hypothesis has many instantiations in the world, all of which might provide evidence for their claim. For the ideographic scientist such as a historical scientist a hypothesis is about one event, and consequently, one piece, or more usually, one set of evidence, with each member of a set being unique.[2]

The historical scientists' focus on a particular event, or a particular history, seems to work against repeated observations. There is only one archaeological site that the archaeologist is interested in accounting for, and it may well be unique, often startlingly so.[3] The same goes for the geologist explaining the unique properties of a geological feature. The historical scientist is frequently interested in confirming a hypothesis about a particular feature, in effect a hypothesis with a single instantiation of evidence, rather than a hypothesis about a generality with potentially multiple instantiations. It is a hypothesis about a particular feature of the world, and consequently even with a time machine, the hypothesis cannot be tested by repeated observation.

1.2.3 The Issue of Scale

Even if the historical sciences could overcome the prior difficulties of singularity and lack of observational access to early parts of a causal chain, in many cases the historical sciences also lack the ability to intervene in events in the same way as the experimental sciences for a distinct reason: that of scale. Repeating processes that take hundreds of years, or duplicating processes like uplift of tectonic plates, simply cannot

[2] As we shall see in chapter 3, the notion of a set of evidence, rather than a single piece of evidence, is actually crucial. However, this requires that a case can be made that various individual pieces can work together as a 'set.'

[3] Its also worth noting that in the case of archaeology, and on some occasions Paleontology, the investigation of a site actually destroys it, making the configuration of a site unavailable for future investigations. While individual pieces of material can be reinvestigated, the relation of finds one to another must be accurately documented, as this is destroyed in the recovery process.

be done. While some historical events are not at such a scale, many are, particularly in geology where both physical and temporal scale matters.

The historical sciences then have two epistemic difficulties to overcome, and a pragmatic one. They can't confirm their hypotheses about the past with observations due to the lack of access to the past, so they must engage in the historical inference. They can't confirm their hypotheses with comparisons of tokens of the same type because they are hypotheses about particular times or places. And even if they could do either of these, there is on occasion the pragmatic difficulty that they cannot confirm their hypotheses about the past through interventions, because of the temporal or physical scale of their enquiries.

The result is a problem of confirmation. With no ability to observe their objects of enquiry, or to intervene on processes, there is seemingly no way to confirm hypotheses.

1.3 Summarising the Problem

Let's try to get in order the problems faced by the historical sciences, first, by showing the advantage that the experimental sciences have. Take a single observer in the experimental sciences. The individual may have a hypothesised process *X*, and she can observe under controlled conditions repeated instantiations of this process, as she has full temporal access to the event. She can then confidently generalise from this process. The result is that she can have confidence in her claims. (See Figure 1-1)

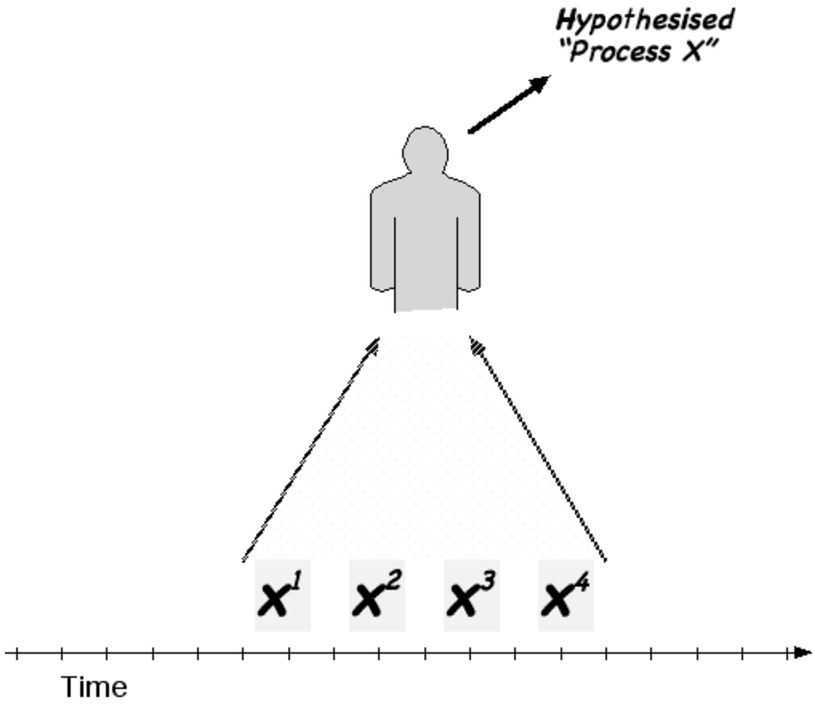

Figure 1-1: A single observer. A single observer can witness multiple instantiations of a process "x," and hypothesise about that process. They have full temporal access to that process, and can engage in multiple observations. They may even be able to observe the process in action, and not just its results.

This temporal access need not be for a single individual. An epistemic community that shares certain measurement standards and reporting criteria may be able to have temporal access over much longer time frames. In fact, there is no particular reason why this community need be physically or temporally contiguous. So long as the previous observers communicate reliable data, then an individual observer may gain access to temporally inaccessible events.[4] Environmental history, and in particular the history of changing sea levels and temperatures, relies at

[4] Clearly, what counts as reliable data may be problematic. Just how reliable testimony is, is of course a constant worry for the historian. For the scientist, reliable testimony also includes problems of differing epistemic standards, and differing forms of measurement and so forth. Most information generated by previous eras of researchers utilised by historical scientists consequently comes from the 19th century onwards, with the advent of standard scales of measurement. The reconstruction of comet cycles using ancient accounts of sightings a case where even differing epistemic standards can be overcome. Archaeologists however, can utilise a much broader range of sources including ancient texts (See Kosso 1993; Kosso 2001).

least in part for historical measurements to make claims about changes over time.

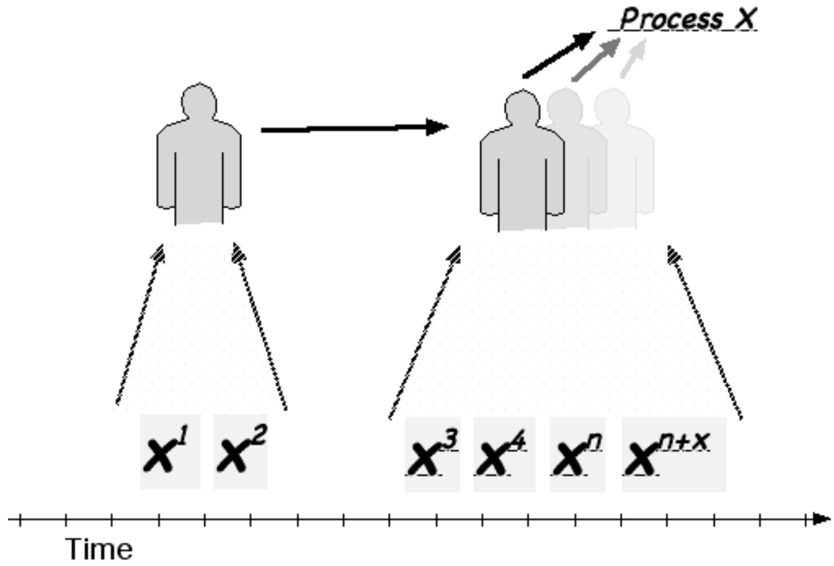

Figure 1-2: An epistemic community may in fact be responsible for various individual observations, and this could dramatically affect the temporal access to events of interest. Historically placed agents can enhance this access to events in the past through texts and historical documents that document their eyewitness accounts. However, this is somewhat reliant on shared reporting standards and techniques.

The problem faced by the historical scientist however, is the lack of any observer in the past, coupled with the fact that they are not engaged in repetitive observations. Rather, their hypotheses concern a single event in the past. However, the historical scientist does have the advantage that an observation may well be available in the present, with objects they "believe remain from the past." (See Figure 1-3)

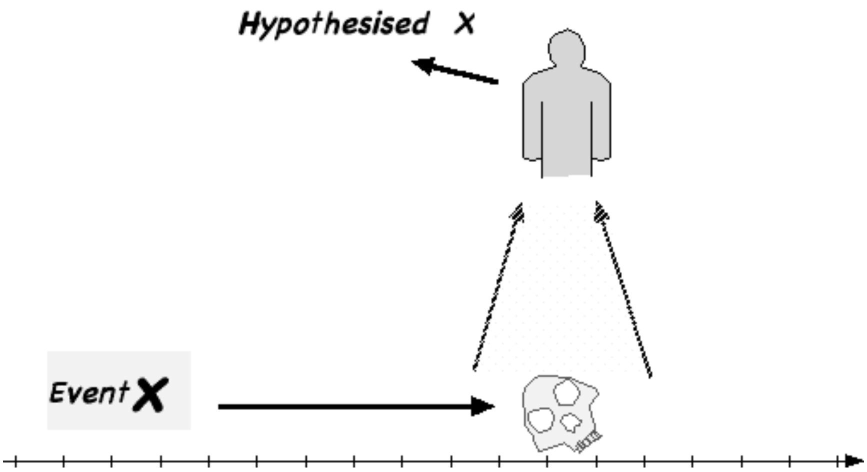

Figure 1-3 The historical inference: A historical scientist wishes to make claims about an unobservable past event *X*, utilising observations in the present, in this case, of a fossilised skull. The logic of enquiry is to start with the contemporary observation. Although much can be said about the particular object— it is a skull, its mineralized, and so forth — the hypotheses will be about the past causes of the object, the unobservable event *X*.

The historical scientist then looks significantly disadvantaged. The lack of repeatable observations, the lack of observational access to the event or process of interest, all raise difficulties for the historical scientist in how to confirm their claims about the past. The standard techniques of the experimental sciences —experiment, repetitive observations and generalisations from multiple instantiations— all look to be out of reach for the historical scientist.

What I want to show in this thesis is that the historical sciences are not quite so challenged as they appear. They have means to test hypotheses about the past, and they consequently do have answers to the charges of lower epistemic standards than the experimental sciences. They can overcome the difficulties outlined above. But in overcoming these problems, the distinction between the historical sciences and the experimental sciences becomes blurred. The historical sciences are not only interested in particular events, nor in making claims about the history of particular things. They too seek regularities in the world, and have to in order to secure their claims about the past.

1.4 The Road to Come

The next six chapters of this thesis deal with the basic confirmatory strategy of the historical sciences. However, before we can do that, we must look at just what it is we need to confirm. Consequently, chapter two will look at the structure of historical accounts: what they do, and the structure of narratives. This chapter will lean heavily on Hull's (1975) analysis of historical accounts, as I think it is the most straightforward exposition on offer.

Subsequent chapters of this first section aim to show how one confirms a basic claim about the past. Carol Cleland in particular provides us with some machinery to achieve this goal that I outline in chapter 3. Her insight is that it is a collection of evidence that allow us to choose between hypotheses. Cleland sees this as a methodology distinct to the historical sciences. However, I show in chapter 4 that this confirmatory machinery is itself parasitic on understanding causal regularities. The fact that causal regularities of various types play a role opens the possibility of utilising the tools of the experimental sciences. Chapter 5 extends this view, and puts forward a different picture of the sciences as a whole, one that takes the temporal location of the sciences seriously. It also looks at potential challenges to this view of the sciences. In particular, we raise the problem of evidence dispersing beyond recovery; a problem that much of the rest of the thesis attempts to address.

Chapter 6 extends the notion of using contemporary observations by examining the uniformitarian assumption in geology. This chapter deals with the pragmatic issue of scale in the historical sciences. Chapter 7 is a case study, showing how changing theories and models applied in contemporary settings change our views of the past.

Section 2, chapters 8 to 10 inclusive, looks at the more complex task of moving from claims about events and single processes, to the formation of chronicles and then the construction of narratives. We start by looking at collections of evidence, and the pragmatic difficulties of building a chronicle. We also introduce an important aspect of historical science practice, that of 'tacking' between hypothesis and evidence.[5] This process turns nomothetic, general models of processes into ideographic and unique accounts of particular evidence.

Chapter 9 concentrates on the task of constructing a narrative, and the requirement that temporal and local context be taken into account. Again, the tacking analogy is used, but I suggest that the tacking

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[5] The tacking analogy is borrowed from Alison Wylie (Wylie 1989) and will be introduced in more detail in the chapter.

procedure is a three-place movement between evidence, hypothesis, and historical context.

Chapter 10 is another case study, showing how a general model or regularity, has to take into account localised information for it to become a good narrative. The debate I look at, the megafaunal extinction debate, has on my view been characterised by an overly simplistic application of a model, and has failed to take into account historical and local context.

In chapter 11, I examine a potentially difficult task for the historical sciences, that of complex human behaviour. This chapter looks at human prehistory, and in particular the problems we face when looking to reconstruct the belief systems of past cultural groups.

Chapter 12 summarises the thesis, isolating the scope and limits of the historical sciences; what they can say and cannot say reliably, and sums the commonalities across the historical sciences.

2. The structure of historical Accounts

This chapter looks in more detail at the stories of the historical sciences, the narratives of the past. The analysis provided here is not exhaustive, and its basic idea owes much to David Hull's (1975) paper looking at explanation in history. However, this chapter is not just about explanation per se. While explanation is important, we must also see these explanatory narratives in the light of confirmation. With this issue in mind, this chapter analyse the structure of the narratives that the historical sciences provide. The aim is to show how they work as hypotheses about past events.

In first section, I will show how narratives work as explanations. Part of the explanatory target for narratives is current states of affairs, and as such, narratives are explicitly tied to evidence. This link to current states of affairs provides narratives with the ability to be tested by observations, and much of the rest of the thesis will expand on the how this is achieved. I also show why historical scientists prefer narratives as explanations, rather than cite single regularities as explanations.

In the second section, I will introduce David Hull's description of narratives as the documentation of transformations of a central subject. Hull's analysis breaks down narratives into its constituent parts, and sets up the confirmatory problems for the historical sciences. It also shows how narratives can utilise regularities.

In the final section, I summarise the Hull's analysis in such a way that the confirmatory target of the historical sciences is clear.

2.1 The Explanatory Target

What is the purpose of historical narratives? This section will argue that narratives are explanations of states of affairs. They explain features of the world. Because of this, the narratives of the historical sciences are tied to observations of states of affairs; a point that will be crucial for the rest of the thesis. I will then briefly explain why narratives are particularly useful for the historical sciences. Explaining contemporary states of affairs requires accounting for the contingencies of history, and narratives can accomplish this task in a way that citing regularities cannot.

One of the chief roles of narratives is to explain contemporary states of affairs. For example, a narrative of the evolution and spread of *Homo erectus* acts as an explanation for a set of fossils we think are similar, and various tools associated with these fossils. The evolutionary narrative is the explanans, the statement that does the explaining. The fossils, the archaeological finds, and to some extent modern *Homo sapiens,* are the explanandum, the things that the narrative explains. So all the statements describing the fossils, archaeological evidence and so forth are the explanandum, the explanatory target. (See Figure 2-1)

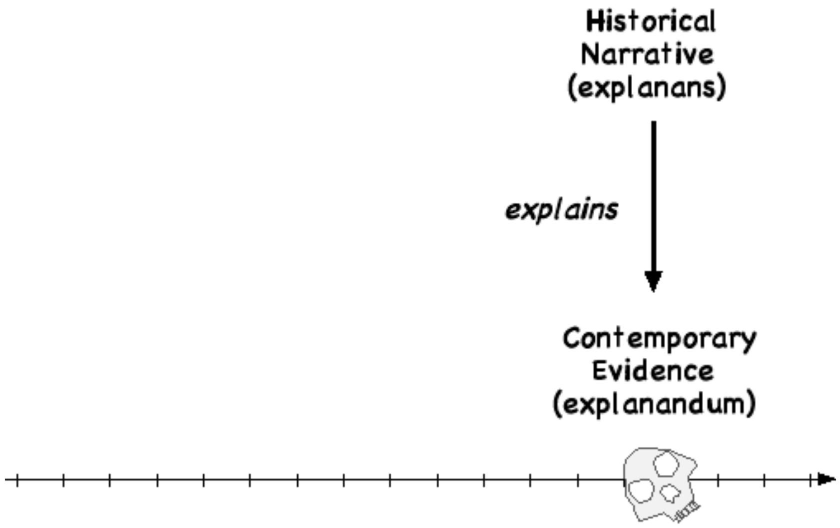

Figure 2-1 Narratives can act as explanations for contemporary evidence. So while a narrative is about the past, it acts as an explanans, it does the explaining, for an explanandum, a particular and local piece of contemporary evidence.

There is much more to be said here. For a start, a historical narrative rarely explains a single piece of evidence. After all, a full narrative of a *Homo erectus* skull —what it is, how it came to be where it is, and its causal history— will not only explain that skull, it will also partially explain a variety of other observable pieces of evidence: stone tools, a disparate collection of fossil finds, and perhaps even the origin of *Homo sapiens* as a species, along with other assorted observable "facts." The explanatory narrative will inevitably cite a range of evidence in support of the narrative; a point that will become crucial in chapter 3.

A narrative does not just explain observable states of affairs. For instance, a narrative of human evolution, with the *Homo sapien* evolutionary lineage as its central subject, not only explains observable contemporary fossils, and observable modern humans; it also explains the role of extinct species in shaping the world as we see it now. The species themselves are unobservable; only their traces remain. Nevertheless, a complete narrative of human evolution explains these unobservable ancestors in the course of explaining observable evidence. In the human evolution example, our narrative of human evolution could stop at the extinct human ancestor *Homo heidelbergensis*. The narrative of human evolution up to the appearance of *Homo heidelbergensis* explains *Homo heidelbergensis* just as the complete narrative explains contemporary *Homo sapiens*.

However, narratives are connected to contemporary observations, for accounts of the past explain features of the observable present. Narratives cite physical evidence in support of their explanation and in so doing explain that evidence. Narratives on this account are not quite the suspicious characters that they appear to be at first glance. They explain contemporary states of affairs, including various pieces of physical evidence, and they explain historical states of affairs.

2.1.1 The preference for narratives

Narratives are not quite the normal kind of scientific explanation, and particularly not the sort that is provided by the experimental sciences. So why might historical scientists prefer narratives to other kinds of explanation? To explore this question, we will use a distinction drawn by Kim Sterelny between two kinds of explanation.

Sterelny has suggested there is a distinction between actual sequence explanations of history, where one tracks the minutiae of causes and effects, with robust process explanations, where one utilises higher levels of processes that one can compare across cases (Sterelny 2001).

To explain how these two explanations differ, we will use both kinds of explanation to explain the appearance of weeds in a potted plant on a windowsill. The first explanation for the weeds is a robust process account.

A robust explanation for the presence of weeds in a indoor potted plant might be as follows: People tend to have their windows open at a time of the year when some species of plants disperse wind blown seeds. In this explanation, we appeal to general processes and general regularities to explain the particular token event, the presence of weeds in a particular pot. Note here that such regularities can explain not only the appearance of weeds in my potted plant, but also in yours. Further, it might account for why the presence of weeds in indoor potted plants is a frequent occurrence in my house, but rare in other peoples. I tend to have my window open more often than other people.

Thus, these robust process accounts can provide us with important comparative information that allows us to explain the commonalities across tokens —why we both have weeds in our plant pots— but also contrastive information; why my pots have more weeds than other peoples. In such cases, we are relying on regularities we hold to operate across tokens: windborne seeds, people's habits of having windows open in warmer months and so forth.

Robust process accounts are then very amenable to comparison across cases, and as a consequence depend upon the very sort of regularities deployed by the experimental sciences. However, such a robust account might not accommodate *some* instances of weeds in pot plants. A pot plant in an air-conditioned office without opening windows is not covered by the robust account just provided. Such a unique case might require what Sterelny calls an actual sequence explanation, a unique explanation that cites the particular variables and unique contingencies of history. What's more, an actual sequence account only explains a single token. It is a unique explanation that only explains one thing.

For the case in question, we first have to nominate a particular potted plant and its weed: My potted plant, and not another individuals, and a particular weed, not just weeds in general. An actual sequence account of how a particular weed got into my plant pot, would force the describer to detail the events that allowed a windborne seed to get into the soil of the particular pot in question. On such and such a day, the house owner left the window open. It was windy that day, and a particular weed was dispersing seed, one of which drifted in through a window. Because of the dynamics of the air currents, a seed was deposited in the fertile soil of my plant pot. Such an explanation might well be very difficult, although not impossible to confirm in detail, but it would account for the facts of this particular instance. This actual sequence account would in effect be a narrative that accounted for a state of affairs: the presence of a weed in a particular pot plant.

On occasions these actual sequence explanations, are necessary. The past is complex, and when explaining the past we can rarely use a *single* robust process as an explanation. That's not to say that on occasion we don't cite a single robust process that has occurred in the past to explain something in the present. At times we do. But in order to explain a state of affairs, the historical sciences must frequently construct an explanation that details a series of events. An explanation of a landscape cannot cite a single robust process. It is the result of a series of processes occurring one after another, frequently of quite different types. The rise and fall of a past culture cannot cite a single process; there is an interaction of a number of processes that lead to its downfall. To show this, and to show how narratives work, we turn to the analysis of narratives provided by David Hull.

2.2 Narratives

Like historians, paleoanthropologists and other historical scientists are interested in providing a narrative. A paleoanthropologist might be interested in a narrative of human evolution. An archaeologist might want to provide a history for the settlement of a particular piece of the world. A geologist might want to provide a history for a particular mountain range.

When looking at narratives as explanations, David Hull suggests that the first consideration is that they have a unified subject.

> If historical narratives are viewed as descriptions of historical entities as they persist through time, then the currently accepted analysis of science need not be modified to account for the unity evident in historical narratives. (Hull 1975 p254)

The idea here is relatively straightforward. Take a standard piece of historical narrative, such as the changing political regimes of the city-state of Rome; the shift from a republic to a dictatorship and then an emperor (Scullard 1986; Holland 2005). Although much of a text of such a story may well refer to different individuals, different events and so forth, the unity to the text is provided by a central subject: the city-state of Rome and its governance. Although many forces might be at play in such a narrative —from ambitious politicians to economic factors— a single subject unifies the narrative. At times, the factors cited may be a little obtuse; the role of individuals, economic realities, social pressures, and other processes cited in the narrative may impact on the central subject in complex ways. Nevertheless, we can see a central subject for a narrative as a unifying thread that embraces a disparate range of events, processes, and causes.

Of course, the experimental sciences may also have a central subject for their accounts of the world. A physicist will have an account of the transformation of a particle under certain conditions. A chemist will document the changes in a molecule. The statements generated by scientists frequently document a change in a central subject. However, a historical narrative is distinct in that it documents a series of changes through time for a particular object. Although the various species and their morphology may change, the bio-geographical range may expand and contract with various ice ages, and the physical evidence may be varied and open to interpretation, a narrative of human evolution is unified by a central subject, the ancestral lineage to *Homo sapiens,* and this central subject undergoes not one, but *many* transformations. Just as in the case of the fall of the Roman Republic, there might be various factors involved, with the central subject undergoing a series of transformations due to a variety of factors. In geology, a geographical location might provide a central subject. For instance, the narrative in such a case may account for the geological history of a particular region, citing various processes that overlay, distort, and change the landscape through time. In fact, in many cases, it is a particular feature of the contemporary world that provides a central subject for a narrative. We want an explanation for the evolution of a particular species, an account of the formation of a particular mountain range, the narrative for a particular cultural group.

The central subject for a narrative need not currently exist. We can have as our central subject an extinct genus such as the mammoth, or a long forgotten culture that once centred on a ruined city such as Babylon (Adkins 2003).

What counts as a central subject may of course be problematic. The robust australopithecines are now considered by paleoanthropologists to be a side branch on the hominin family tree, and not part of our human ancestry. Nevertheless, they are more often than not included in a narrative of human evolution. In its later days, the "Roman" empire was governed from Constantinople and western European townships such as Ravenna, and not Rome. Despite this, a narrative might still have as its central subject the fall of the Roman Empire. We could also choose what could appear to be a trivial subject for a narrative; for instance the changing hirsuteness of Roman emperors. As one can see, should we wish to raise difficulties for this notion of a central subject, this is not too hard to do. However, for the purposes of this thesis, I will simply note this potential problem and move on. For what follows, I will adopt a pragmatic position that the interests of the science, and the interest of the historical scientist, define a central subject for a narrative. A narrative of the evolution of human dentition is just as reasonable as a narrative of the evolution of the precision grip or a narrative of the hominin lineage as a whole. The choice of the investigator, and the investigative goals of the science dictate the central subject of a narrative.

However, as we shall see in the subsequent sections of this chapter, the choice of a central subject is partly determined by the evidence available. Scientists do not have a free hand in the matter. We shall also see in the case study of chapter 7 that our understanding of the past can actually change just what we take the central subject of the narrative to be. The choice of a central subject is not always arbitrary. At this stage however, we can rest with the pragmatic definition that the investigator defines the central subject of a narrative.

2.2.1 From chronology to narrative

The first step to breaking down our narrative into manageable pieces is thus to conceive of the narrative as being an account of a single subject. The next step is to see that the central subject undergoes a series of transformations. (See Figure 2-2)

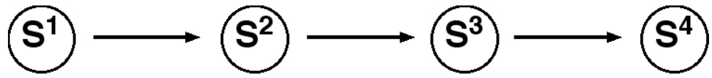

Figure 2-2. A historical subject 'S' undergoes a series of changes or transformations. A chronicle would relate the sequence of changes from one state to another, typically in a temporal order.

At this point, without further information about the nature of the transformations, we have something we can describe as a chronicle: a temporal sequence that documents the changes in a central subject. That temporal sequence may itself require confirmation via various dating techniques, so one of the important tasks is getting the sequence in the right order. This in itself can be a demanding task. Prior to the advent of carbon dating and other atomic dating techniques, some archaeological chronologies of Europe and Mediterranean pre-history were actually wrong. Colin Renfrew recounts the revolution in European archaeology brought about by new chronologies constructed with the aid of carbon dating (Renfrew 1973). On top of the difficulties of dating, there is the problem alluded to above: that of identifying the central subject of a narrative through time. The difficulties of constructing a chronicle will be dealt with in more detail in chapter 8.

Suffice to say that at this point, a chronicle, even a well-confirmed one, is not an explanation. It contains no causal information, and is purely a documentation of states of central subject through time

The next step is to look at a chronicle and to try and account for those transformations in states. These transformations are the linkages between the various states of the central subject. It is at this point that a further body of scientific knowledge starts to play a role. The role of background sciences, including experimental science with its generalisations, is to specify the potential transformations. As Hull puts it: "… scientific theories tell us what can happen" (Hull 1975 p266). Consequently, our historical narrative is not just a statement of a sequence of historical states for a central subject, and it is certainly not just any story. It also details the causes or processes that transform the central subject over time. This detailing of the processes involved in the transformation of a central subject marks the difference between a chronicle and a narrative.

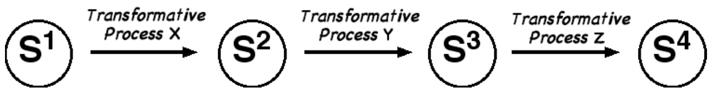

Figure 2-3 Ideally our narratives would not just document the various states of a central subject through time; it would also include the various processes that transform the central subject at successive stages or nodes. These need not be the same sort of processes, and in some cases, the transformative processes may be very different.

The various transformative processes that link the historical states need not be of the same character. The processes involved in the fall of the Roman Republic include a number of very different causes that link successive stages: The actions of ambitious politicians, economic growth outstripping administrative capacity, and external political threats all play a role at various times. Even weather may play a role, with a series of poor harvests transforming a self-sufficient state to a net importer of foodstuffs. Thus, our processes include the psychology of individuals, economics, and environmental effects. In the historical sciences, we may well expect a similar variety of transformative processes. Natural selection, climate change, predators and new co-evolutionary opportunities can transform populations of organisms at different points in their history. Landscapes can be shaped by ice ages, erosion, animal browsing, forestation, deforestation by human agriculturalists, and damming for power generation plants.

Nevertheless, these transformative processes should be 'within the realm of the possible.' According to Hull, they should be processes well within the domain of the sciences. So on Hull's analysis, historical narratives cite causal processes that are within the domains of the sciences.

In some cases, the transformations between states might be dependent upon previous processes setting up conditions for later processes. Tectonic uplift of a mountain range needs to occur before erosion and the formation of an alluvial plain for instance. Nor need there be a strict, one thing after another, temporal sequence. Often, we might think that transformations are the result of multiple processes operating simultaneously. For instance, we might speculate that in our Roman case, the combination of economic pressure operating continuously, coupled with the actions of key individuals, played a role in transforming the governance of Rome.

In fact, in many historical situations it is highly likely that more than one process involved. The formation of river systems may be a combination of deposition and erosion but at a rate modified by changing forestation patterns and glacial sequences. The erosion and deposition is continuous, but glacial-interglacial cycles speed up or retard the rate of erosion and even changes its character. Rather than seeing a strict sequence of processes, we may frequently need to see interactions of processes.

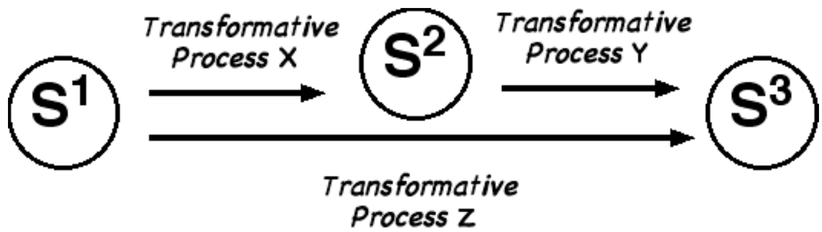

Figure 2-4 In this instance, the transformation of the central subject from state 1 through to state 3 is via multiple processes. Transformative process X is only operative from the state change from 1 to 2, and Y, from 2 to 3. The transformative process Z however, is operative over a number of states. We might think of Z in this instance as an underlying trend, or a continuous process. It may or may not be effected by the two shorter, more punctuated processes.

Narratives thus have the following character. They document the changes in states in a central historical subject. Those changes in states will be the result of the operation of particular processes. We may well speculate that processes are operating in tandem rather than sequentially. These processes might work at quite different levels and invoke causes from quite different domains. Volcanism and erosion are two rather different processes, and in a narrative recounting the formation of a volcanic island, we may well invoke both processes. Each process may well be linked to the final state of the island in its own way.

With Hull's analysis in mind, we can move on to the final task for this chapter, which is identifying the confirmatory task of the historical scientist.

2.3 The epistemic task

At this point, we can begin to see how we can isolate our epistemic tasks, for tasks they are. In effect, we can break down our narrative into a string of processes or transformations. Each process can be viewed independently for the purposes of confirmation. (See Figure 2-5)

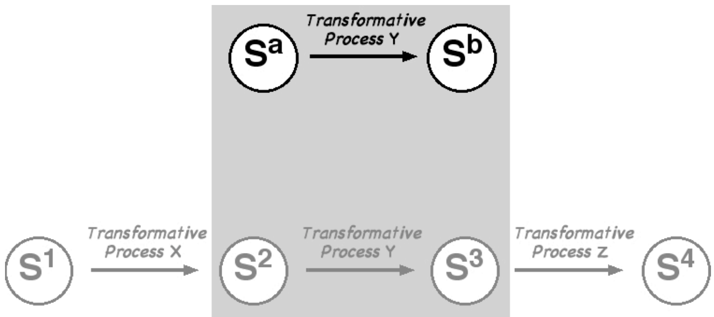

Figure 2-5 We should, at least in principle, be able to isolate a section of a narrative, and view it as a process, or causal relation, in its own right. In the sequence S^1 to S^4 we can Isolate S^2 and S^3, and treat them as a process in their own right (S^a and S^b) On this analysis, a narrative becomes a sequential string of processes, which are related in our final account by a central subject. Each individual transformation may be a quite different process.

Under Hull's analysis, it seems that a narrative is a compound exposition of various causal relations, all of which operate on a single central subject. A narrative is made up of numerous sub explanations, all dealing with single transformations from one state to another.

The issue is then how we can confirm this narrative. Remember that a narrative is supposed to explain current states of affairs, particularly evidence from the past. So, it must account for the particularities of current configurations of observable evidence. However, that evidence must also do confirmatory duty as well. It is the available physical evidence available that helps us choose between rival hypotheses. Physical evidence constrains our hypotheses and prevents them being ad hoc stories.

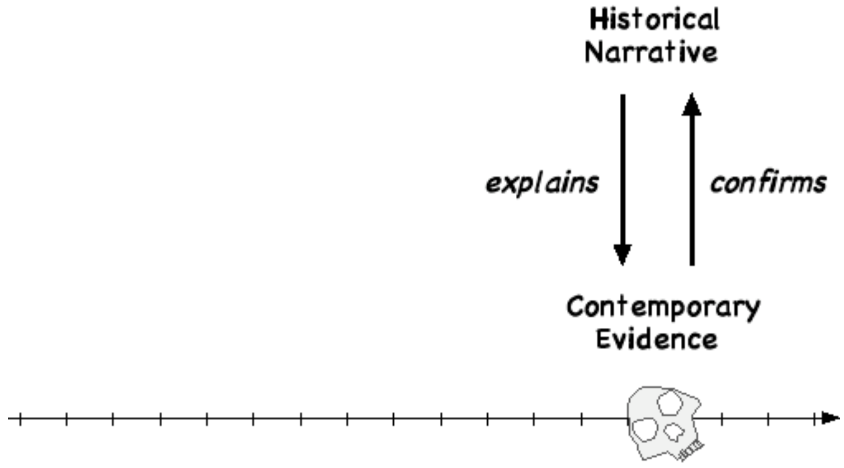

Figure 2-6 A historical narrative explains contemporary evidence by providing a unique causal history that accounts for the existing state of affairs. However, that physical evidence, or collection of physical evidence, also plays a role in confirming that historical narrative and any claims we make about the past.

We need not try to confirm the whole narrative at once. The benefits of viewing a narrative in Hull's way, as a series of transformations, are that we can instead try to confirm individual nodes, the individual transitions, within a narrative. As we shall see as we go on, the process is one of confirming a chronicle, a sequence of state changes in a central subject. This requires identifying a subject through time, and the temporal location of the various stages. This is not always as straightforward as one might think. So, the construction of a chronicle requires some measure of support. It too requires evidence, and it too requires confirmation.

For an explanatory narrative, we need to cite various causal processes that account for the changes in the central subject of the narrative. It is then a matter of identifying, and confirming, the transformative processes that change the state of the central subject of the narrative.

By viewing narratives in this way, it makes it clear what it is we are looking to confirm. Rather than a complete narrative, we are in a position to work with transformations of the central subject. We can confirm the transformative processes as individual processes.

2.4 Summary

This thesis is not primarily concerned with the problems of explanation in the historical sciences. Rather, it is concerned with the processes of confirmation. How can we determine which hypothesised account of the past is the more likely, given a range of choices?

At its most broad, this is a matter of choosing between alternative narratives. However, this is not entirely straightforward, as we have also seen that such narratives can be effectively broken down into a series of steps or sub-explanations, each of which may be confirmed or discussed in its own right.

Detecting a series or lineage in the broad sense for a central narrative subject is then a matter of detecting a node in a causal chain, and accounting for the particular transformation from one state to another. Nevertheless, we can see that the various states, the transformations, and the trajectory between various transformations should be importantly constrained by the plausible, and by our best sciences of the day. Equally, such nodes are constrained by the broader narrative. Temporal ordering may play a role, as does developmental or other trajectory constraints. Erosion occurs after formation in geology. Adaptive radiation occurs after adaptive innovation in evolutionary lineages. There may well be other constraints as well, and these need to be understood, and confirmed in their own right.

Investigations into the individual nodes within a narrative may of course change our views on the trajectory of the narrative. This too is a problem that we shall look at. In a later chapter, we shall see how a large-scale narrative, the extinction of the North American megafauna, has been modified significantly to account for empirical information generated by investigations into various individual processes in the large-scale narrative.

The first task, however, is to investigate how we can make sense of individual nodes in our explanatory chain which makes up a historical narrative. In the case of human evolution, our historical narrative should not only detail the various ancestral states, the possible ancestral species for *Homo sapiens*, it should also detail the various processes that underlie the transformations between various states. Ideally, these processes should not be unique, nor should they look like special pleading. They should be part of a general body of acceptable theory.

The epistemic task for the historical sciences then is twofold. The first is to identify the existence and temporal location of various states of a central subject for a narrative, the construction of a chronicle of events. The second is to determine the processes that link states, transforming one state into another. These two things clearly may reinforce one another. And while it may well be that typically, a chronicle of states is provided first, with the transformative processes provided second, this needn't be the case. On some occasions, general processes that act as potential transformations might be elucidated, and from this, various states predicted. The search for "missing links," is precisely this ordering. Natural selection predicts change that is gradual, and gaps in a chronicle are expected to have states predicted by the posited transformational process.

Nevertheless, the point to grasp from this chapter is this: While the final product of the historical sciences may well be the provision of a narrative that explains features of the present, the epistemic task breaks down into a number of challenges. Broadly, these are: The identification of individual events, or states, in the past; the construction of a chronicle of the central subject; and an account of the various transformations in the central subject.

3. Reasoning from Consequences

What we saw in the last chapter was a way of viewing narratives as a sequence of state changes in a central subject. The central subject unified the narrative, and acted as an explanation of contemporary observable facts. For example, the central subject of the human ancestral lineage united a narrative of human evolution. This central subject is transformed over time by a variety of processes. The full narrative would document the processes that caused these state changes.

We also saw how these historical narratives explain current states of affairs, observable phenomena. The narrative of human evolution explains contemporary humans, contemporary fossils, and other observable phenomena.

In this chapter, we begin the process of looking at how these observable phenomena are used to reconstruct the past. Contemporary phenomena are not only the explanatory targets for the historical sciences, they are also the evidence for historical claims. Observable evidence is used to confirm narratives. It is certainly true, as we shall see over the course of this thesis, that different historical sciences will use different methodologies and different tools to confirm elements of their claims about the past. The state changes in a historical narrative can be the result of different kinds of processes, and in some historical sciences, the science of these processes might be very disparate indeed. For instance, the history of Earth's atmospheric temperatures is the result of biological processes (changes in carbon dioxide levels due to differing amounts of anaerobic and aerobic organisms), celestial mechanisms (Milankovitch cycles and similar), interactions between sea currents and changing land masses, (Stanley 1992; Stanley 1996) and erosion patterns. All these processes are the 'domain of competence' of quite different areas of study. However, in this chapter I argue that there is a common logical structure to the confirmation methods used in the historical sciences: a common structure to the relationship between evidence and hypothesised past causes of that evidence.

To show this, I will utilise the insight of Carol Cleland, who demonstrates that while the practical sciences of historical enquiry are very different, they have a similar underlying logic of confirmation when it comes to the process of identifying particular events or transformations in the central subject of the narrative. The plan for this chapter is to outline Cleland's work, and, in particular, the means by which we can discriminate between alternative hypotheses about the past. The second part of this chapter will extend Cleland's idea, deal with some criticisms, and prescribe some potential limits to her methodology. We will explore these limits to Cleland's methodology, and how to extend her ideas, in the subsequent chapter, chapter 4.

3.1 Cleland

Carol Cleland argues that the historical sciences can make reliable claims about the past. However, Cleland is also aware that the historical sciences have the distinct epistemological problem outlined in previous chapters: that of inferring unobservable past causes from observable phenomena. Cleland provides an account of a methodology shared by the historical sciences. This methodology enables historical scientists to choose between rival narratives, or rival hypotheses. The methodology provides a test for historical claims.

We will discuss this methodology initially only looking at stages within a narrative: events or individual nodes within a narrative sequence. There is an important relationship between a narrative in its entirety, and stages or nodes within a narrative, and we will come to this in detail in subsequent chapters, particularly chapter 9. However, at this point, we are only interested in the how we can confirm a particular node or process. The example that Cleland uses is an event within narrative that explains the extinction of the dinosaurs. Cleland's case study is how one can confirm that a meteor played a role in the extinction. She is just interested in this particular process —an event— that causes a change of state for the central subject. So, in what follows, we are primarily interested in events, a stage within a narrative: a particular process that transforms a central subject in a narrative.

This is a different kind of confirmation than that in the experimental sciences. As outlined in previous chapters, the experimental sciences have been typically concerned with processes that are potentially repeatable, and inducible under laboratory settings. The historical sciences are often concerned unique combinations of elements that are unobservable. Cleland's machinery is concerned with identifying these singular, unique nodes in historical sequences. These historically contingent, temporally located events are quite different than the generalities of the experimental sciences, which are typically universal and a-temporal.

3.1.1 Lewis

The starting point for Cleland is the work of David Lewis, and his paper "Counterfactual Dependence and Times Arrow"(Lewis 1979). Lewis' ideas are framed within a deterministic universe. If we assume such determinism, then for any observable fact, there is at least one determinant in the past, and one in the future. There is "…a minimal set of conditions jointly sufficient, given the laws of nature, for the fact in question" (Lewis 1979). For any currently observable state of affairs, there is some causal process that is responsible for that state of affairs coming about. We can view this in light of the transformation of states outlined in the previous chapter. For any state of a central subject of a narrative, there will be a prior state and a process, for the subsequent state. In Lewis' language, for any state or node in a causal chain, there is at least one prior *determinant* for that state.

Lewis' particular interest is in an asymmetry that emerges in counterfactual reasoning about such causal chains. To demonstrate, we will work with a simple example, the transformation of a retail outlet from an ordered shop of fragile wares to a disordered one, with the causal process being the presence of a bull. In such a situation, the counterfactual "if the bull had not entered the china shop, then there would not be shards of pottery all over the floor" is true. This is a straightforward counterfactual, and given the circumstances, we can all agree upon its truth. There is an observable fact, the pottery shards, and a prior determinant; the entry of the bull into the china shop. This is something very like the position a historical scientist finds herself in. The historical scientist wants to reason about a prior historical determinant for a current state of affairs.

For Cleland and our purposes, the important component in Lewis' reasoning is an asymmetry. In his article, Lewis points out that there is an asymmetry in the number of prior determinants and subsequent facts available for counterfactuals about the case in question. The counterfactual "if the bull had not entered the china shop, then there would not be shards of pottery all over the floor" and the counterfactual "if the bull had not entered the china shop, then there would not be a overturned display table" are both potentially true in the given situation, but, they both share the same antecedent: a bull's entry into a china shop. A single prior determinant can in fact have multiple consequent facts with which we can reason counterfactually. To convince someone that there was in fact a bull in the china shop, we potentially have more things to reason *from* than we require: shards of pottery, overturned display tables, upset shop owner, and so forth.

What's more, Lewis argues that this will typically be the case. Events have multiple effects, and each of these is a potential epistemic road back to the event.

> "Whatever goes on leaves widespread and varied traces at future times."(Lewis 1979 p474)

An overabundance of consequences of actions means that there will in principle always be multiple counterfactual statements available to us to reason about the past.[6]

Cleland interprets the resulting asymmetry in the following way. The asymmetry emerges because using contemporary states of affairs to reason about the future is more problematic than using contemporary

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[6] Lewis' primary concern is to show that there is an asymmetry in counterfactual reasoning. He does this by considering the ways in which we need to change the actual world where the bull has entered the china shop, to a possible world where it didn't. To alter the actual world and render the set of counterfactuals false we need only change one antecedent fact: the initial presence of the bull in the china shop. However, to alter the world, and render the set of counterfactuals false by changing subsequent facts, we must change numerous things about the world; the overturned display table, the shards of pottery, the upset shop owner, and so forth. Lewis concludes that the possible world where the antecedent event did not occur —the world where the bull didn't enter the china shop— is thus much closer than a world where the bull *did* enter the china shop but there are no subsequent consequences. For our purposes, the full details of Lewis' argument are not necessary. All that Cleland requires is the notion that events disperse their consequences, and that there are consequently more subsequent facts available to reason from than prior determinants. Lewis does not rule out the convergence of causes, and hence overdetermination of events by the past, such as the death by many bullets of a firing squad's target. However, he does suggest that such causal overdetermination —an event with multiple prior determinants— is so infrequent that it is almost miraculous (Lewis 1979). Consequently, the asymmetry lies in this dispersion of consequences, so that typically, there will be more later facts to reason from than prior facts. As we shall see, even this need not necessarily be the case. In fact, all that is required is that events in the past leave traces.

states of affairs to reason about the past:

> Localized events tend to be causally connected in time in an asymmetric manner. As an example, the eruption of a volcano has many different effects (e.g., ash, pumice, masses of basalt, clouds of gases), but only a small fraction of this material is required in order to infer that it occurred; put dramatically, one doesn't need every minute particle of ash. Indeed, any one of an enormous number of remarkably small sub-collections of these effects will do. Running things in the other direction of time, however, produces strikingly different results. Predicting the occurrence of an eruption is much more difficult than inferring that one has already occurred. There are too many possibly relevant conditions (known and unknown), in the absence of which an eruption won't occur. (Cleland 2001 p989)

In our bull in a china shop case, we can infer the prior event of the bull from a range of evidence. We can point to the remains of a single broken pot, or to the remains of many, and reason about the wisdom of letting bulls into ones premises. The important point to note here is that there is a multitude of downstream effects that act as a signature of the past event. This configuration of evidence is an important lever that we will exploit for what follows in this chapter, and for much of the rest of the thesis. Events have multiple consequences, and different events will have different *sets* of consequences.

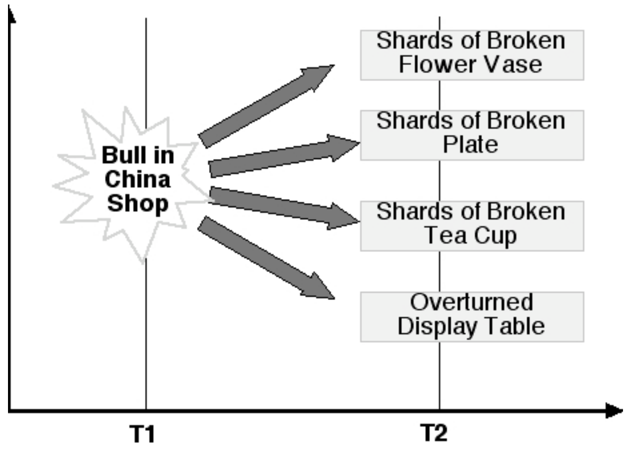

Figure 3-1: At time T1 a single event occurs, the entry of a bull into a China shop. Observing at time T2, we have multiple consequences of this action. We can utilise any one of these later consequences for reasoning counterfactually. If the bull did not enter the china shop, there would not be shards of a broken vase over the floor, If the bull did not enter the china shop, there would not be shards of a broken plate over the floor, and so forth. All these later "states" share as a common prior determinant the entry of the bull into the china shop. However, reasoning in the opposite direction is not so obvious. If we are observing at time T1, making a prediction about broken pottery is less obviously secure. Perhaps the bull won't bump into the teapot display table, perhaps it will.

There is a crucial caveat to make at this point. All that Cleland takes from Lewis is the multiple consequences of prior events, and consequently, the availability of multiple causal relationships from which to reason. All Cleland requires for her purposes is the possibility that historical scientists exploit this overabundance of consequences of prior events. Because the downstream effects of any event disperse into multiple traces, we can work backwards from these traces. As an example, the eruption of a volcano has many different effects (e.g., ash, pumice, masses of basalt, clouds of gasses), but only a small fraction of this material is required in order to infer that it occurred; put dramatically, one doesn't need every minute particle of ash (Cleland 2001 p989). Because of this dispersal of the downstream effects of events into multiple traces, this allows us to infer an event, the eruption of a volcano, from multiple traces, any one of which is sufficient to generate a suitable counterfactual. Going through all the evidence potentially provides counterfactual redundancy.

Once again, in the language of Lewis and the determinants of counterfactual statements: This "… abundance of future traces makes for the like abundance of future determinants. We may reasonably expect overdetermination [of counterfactuals] toward the past on an altogether different scale from the occasional case of mild overdetermination toward the future" (Lewis 1979 p474). Consequently, in theory at least, we have a great many determinants of precedent facts available to us for the purposes of reasoning. [7]

3.2 Cleland's Machinery

Cleland makes use of Lewis' analysis in the following manner. There may be two hypotheses about the cause of an observable current fact, in this instance, the sudden change in the fossil record, which shows the extinction of the dinosaurs. We can frame two counterfactual hypotheses, that explain this fact: 1) If the meteor did not hit, then the fossil record would not show rapid faunal change and the extinction of the dinosaurs. 2) If there were not a rapid cooling in the earth's climate due to increased volcanic activity, then the fossil record would not show rapid faunal change and the extinction of the dinosaurs. These counterfactuals are both plausible; they are consistent with general mechanisms that connect the biology and the physical environment. But they offer competing explanations of the fossil record of this world.

This is another way of making the point that the historical sciences are interested in particulars over generalities. The historical sciences are not interested in the history of possible worlds; they are interested in the history of this world. And that is an important point of departure for

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[7] In early presentations of these ideas, a thought provoking consequence of utilising overdetermination to reason about the past was pointed out. The view implies a universe where there is a constant dispersion: Things are constantly breaking up into multiple consequences. (There have been attempts to make sense of counterfactuals utilising thermodynamics and the tendency of the universe towards entropy (See for instance Kutach 2002), attempts that even the authors admit are not entirely satisfactory.) Where things get a little strange however is that if this style of reasoning is dependent upon the physics of entropy and a slowly dispersing universe, then if the universe at some point stops dispersing from the big bang, and starts heading for a "big crunch," it might be the case that we can expect more and more cases where multiple determinants would have fewer later facts. So, it seems entirely possible that should the universe start heading for a "big crunch," we can expect this kind of reasoning from multiple determinants to be increasingly useful for predicting the future: From multiple observations now, we should be able to make predictions about single events in the future. Thanks to Peter Godfrey-Smith and the ANU Philosophy Seminar audience of 8/12/05 for this discussion and other points raised.

Cleland from Lewis' work. In our example of the bull in the china shop, we were discussing a world in which an enraged bovine had entered a retailer's premises. In the historical sciences, we are unsure of which world we are in: a world where there was a meteor strike, or a world where increased volcanism caused the extinction. Our explanatory target, the contemporary observations of the change in the fossil record indicating the extinction of the dinosaurs, has two equally viable counterfactuals. The contemporary evidence of a change in the fossil record cannot distinguish between the two hypotheses.

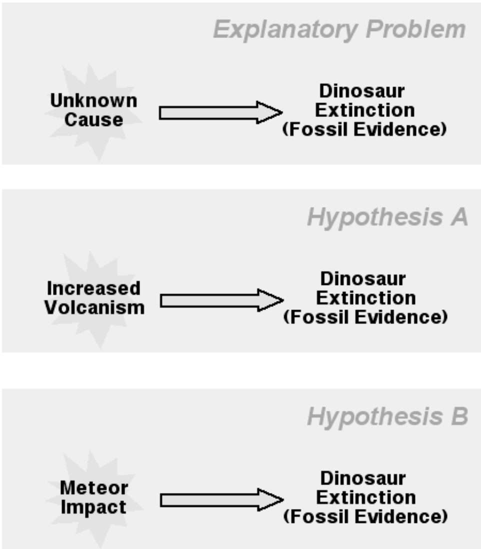

Figure 3-2: An explanatory problem may have two alternative hypotheses. In this case, evidence of a faunal change in the fossil record, the extinction of the dinosaurs, has two competing hypotheses; A and B. Because they both explain the fossil evidence, they are effectively empirically equivalent without further evidence.

It is at this point that the dispersal of consequences comes in. Cleland points out that due to past facts having multiple consequences, we can potentially use additional sources of evidence to distinguish between hypotheses. We can isolate which possible world —a world where a meteor hit or a world where there was increased volcanic activity— is the actual world. We can do this by framing additional counterfactual hypotheses, or a single counterfactual with conjunctions in its antecedent. E.g., If the meteor did not hit then (the fossil record would not show rapid faunal change and the extinction of the dinosaurs, there would not be a layer of shocked quartz in the geological record, there would not be an iridium layer in the geological record, there would not be a big hole in the ground.) Determining the truth of these additional counterfactuals by observing their consequents in the world helps isolate which possible world we are in. We can determine which of the possible worlds is this world, by looking for additional consequences.

3.2.1 Testing Alternative Hypotheses

Cleland's argument then, is that because we can expect past events to have multiple downstream effects, we can use these to discriminate between hypotheses. If we have two distinct hypotheses about the relation between a past cause and a particular piece of observable evidence, then we can use other downstream effects to discriminate between the two. Put abstractly, should we wish to discriminate between hypotheses H1 and H2 for an explanatory target e1, we can use other pieces of evidence to discriminate between them. Because downstream effects disperse, there should be other pieces of evidence that allow us to make a choice between the alternatives. The downstream consequences of hypothesis H1 may be e1, e2 and e3. H2 may share the consequence e1, but might have different consequences: e4 and e5.

Put less abstractly, our two hypotheses for the dinosaur extinction, increased volcanism or a meteorite strike, share one piece of evidence; the change in the fossil record. However, additional lines of evidence, compatible with the meteor strike hypothesis, indicate that this was the more likely candidate event. (see Figure 3-3)

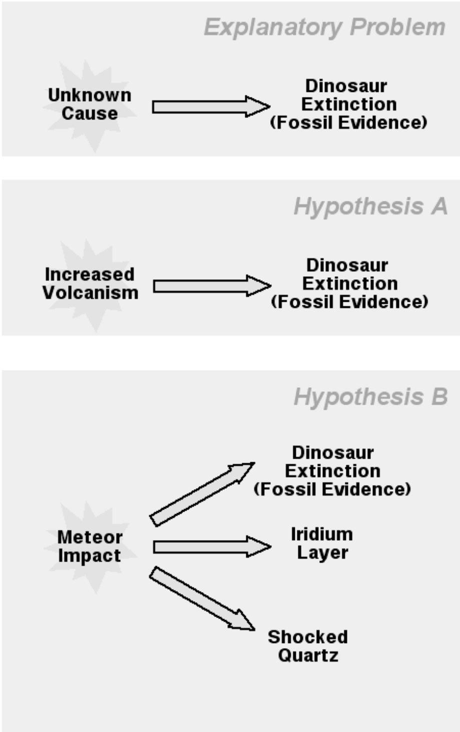

Figure 3-3: Hypothesis A and B, while utilising only the fossil evidence, are empirically equivalent and cannot distinguish between this world and a possible world. However, the presence of shocked quartz and an iridium layer in the K-T extinction boundary provide additional evidence for the meteor impact hypothesis, and allow us to determine what actually happened in our worlds past.

Cleland's analysis of the historical sciences, and their use of this asymmetry, matches the thinking of some historical scientists themselves. The geologist George Seddon wrote a lecture he entitled "Thinking Like a Geologist" that mirrors some of Cleland's thinking (Seddon 1996). Although not as explicit as Cleland, Seddon highlights cases that he regards as good geological practice, and analyses their reasoning. Seddon uses the idea of multiple working hypotheses and a notion similar to Cleland's of corroborative evidence. Hypothesised past causes should have "testable corollaries" (Seddon 1996 p491) in the form of specific signatures of downstream effects.

3.2.2 The Search for the Smoking Gun

Cleland points out that we needn't require all the consequences of a prior event in order to choose between competing hypotheses. Finding all the consequences of an event is a tall order, and in some cases may well be impossible. Rather than find all the consequences of a past event, Cleland argues, and I think she is quite right about this, that what is required is a line of evidence that can act as a "smoking gun."

Take the event of a crime. A gun is fired, a person is killed, and the unknown assailant flees. Like other events in the past, such an occurrence leaves multiple traces. The single "event" has multiple ramifications in the physical world, which can be observed. Some effects might have subsequent modifications to be sure, but there are lots of effects. To remove entirely the effects of the event post its occurrence, multiple physical traces have to be removed: Hairs from the assailant, bullets, bodies, fibres from clothing, and various other ramifications. So to make it appear that the event never happened, much of the world has to be altered. To alter the world prior to the event, we only need change one thing about the world: the gun. (Or perhaps the assailant or the victim or the motivation etc.)

The point is that the causal chain diversifies into multiple traces. However, the historical scientist does not require all of the traces. They only require those traces that can be used to unambiguously point to one hypothesis or another. Take multiple observations of evidence: [oa, ob, and oc]. Now take two hypotheses, H1 and H2. If H1 accounts for [oa + ob] but is logically incompatible with [oc], and H2 accounts for all three observations [oa + ob + oc], then [oc] is the "smoking gun" that discriminates between two hypotheses about a historical event. This one downstream effect not only supports one hypothesis, is works against the alternative hypothesis.

Cleland sees this as a process where historical scientists utilise positive evidence for one hypothesis over another. The example she provides is that of the extinction of the dinosaurs. Various hypotheses had been put forward over the years, but the discovery of an iridium layer at the K-T boundary, coupled with further evidence of shocked quartz, combine to act as a "smoking gun;" positive evidence for the meteorite impact hypothesis (Cleland 2001). Not only was the iridium layer positive evidence of the impact hypothesis, it was incompatible with the volcanism hypothesis, or indeed any alternative. Iron absorbs iridium and other platinum-group elements, so the bulk of the earth's iridium is bound up in the earth's iron core. Consequently, the presence of iridium in the Earth's crust and sediments is extraordinarily low, and barely detectable. The iridium that is in sediments is being introduced from small meteorites. This accounts the extremely low background level of iridium in the earth's surface. Only a large meteorite can deposit an iridium layer in quantities that are readily detectable (Alvarez 1998). On the other hand, volcanic deposits are lower in iridium than even the standard background level, as volcanic deposits are from the earth's mantel where iridium has a chance to bind the iron in the earth's core. Increased volcanisms should actually reduce iridium quantities. Thus, high levels of iridium is not only positive evidence for a meteor strike, it is negative evidence for increased volcanism.[8]

Historical scientists can consequently test historical hypotheses by making observations of currently existing physical traces. The test is which hypothesis best accounts for multiple pieces of physical evidence. The research strategy is to find a "smoking gun," or guns, that unambiguously points to one hypothesis over another. Ideally, one particular piece of evidence will not only point unambiguously to a hypothesis, it will rule out an alternative hypothesis. The ideal smoking gun not only clearly supports one hypothesis; it also actively undermines confidence in alternatives, if not outright eliminating them.

This practice is robust and fairly common. In fact, it is probably one of the dominant strategies in the forensic sciences. The hypothesis that best accounts for the physical evidence gathered eliminates one potential causal agent over another. It is almost classic Sherlock Holmes, eliminating hypotheses that are unsupported by evidence. So up to a point, I agree with Cleland. This is the strategy that geologists and the historical scientists use for certain types of claims. Seddon comes to a similar conclusion. When confronted by an observation, rather than look for all possible consequences of a hypothesised cause, geologists need only look for evidence that discriminates between alternative hypotheses. They eliminate one hypothesis in favour of another by looking beyond the immediate piece of evidence, the explanatory target, to other downstream consequences. They effectively generate further counterfactuals that can be subjected to testing by observations. Rather than search for *all* possible ramifications of a prior event, they can search for key pieces of evidence that act as smoking guns.

3.3 Causal Chains

We have said nothing in this chapter about the how reliable these

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[8] In fact, both the iridium layer *and* shocked quartz rule out increased volcanism.

downstream consequences are, nor anything about the possibility that consequences might disperse beyond recovery. These practical aspects of the historical sciences will begin to make their appearance felt in subsequent chapters.

All that I have outlined here is the basic machinery that allows us to isolate a particular node on a causal chain. Historical scientists can utilise the downstream dispersal of consequences of an event to look for a unique signature of effects. In some cases, evidence may well be incompatible with a hypothesis, and be a negative test. However, in many cases, evidence can take the form of a "smoking gun," a positive piece of evidence that increases our confidence in one hypothesis over another.

There are, however, lingering questions here. For a start, there is an issue as to how recoverable such traces are. Dispersal is not discrete, it is continuous, and, in some cases, the dispersal of effects may be such that they are effectively unrecoverable. Crucially, Cleland's machinery assumes that we understand the effects of an event. We have to be confident that meteors really do cause layers of shocked quartz, and that iridium in geological deposits is a good signal of a meteor impact. We understand the mechanisms that link causes to their effects. To identify a smoking gun is to identify a relationship between a past event and a current observation. To identify such a relationship, surely we must already have a successful historical science. So, in a very real sense, we remain where we started: making claims about unobservable past causes on the basis of contemporary observations. It is to this problem that we turn to in the next chapter. How do we secure this link between an observation and it prior cause?

4. The requirement for background theories

In the previous chapter, we looked at a method of historical reasoning outlined by Carol Cleland. Cleland provides us with insight into how historical scientists can choose between hypotheses. Events have unique signatures of effects, which allow historical scientists to test hypotheses. However, this left us with a question: How do we secure this link between an event and its ramifications? This chapter will answer that question.

In the example that Cleland uses of competing hypotheses about the extinction of the dinosaurs, there is the lurking assumption that a meteor strike comes with certain consequences. There is an assumption that the extinction of the dinosaurs, the shocked quartz, and iridium layers in the geological record are direct consequences of a meteor strike. The downstream consequences are related by a common event. But what allows us to say that a meteor will have certain results? What makes us think that a meteor strike will have a distinctive set of downstream consequences?

Here we can begin to see that Cleland's machinery does not in fact do all the confirmatory work. While it does enable us to choose between competing hypotheses, it does so only given that identify the probable effects of various possible causes. But how can we narrow down the class of candidate causes of dinosaur extinctions; how do we relate each of these to their pattern of effects? What's more, even once we have a plausible hypothesis, we need to confirm the relationships between a prior event and downstream consequences. While Cleland has shown how a pattern of observations can be utilised to choose between hypotheses in the historical sciences, each individual observation and the purported link to the prior event are instances of the successful practice of the historical sciences; the problem we had to begin with.

This chapter begins to make explicit something that has been implicit in the discussion thus far: the role of experimental science in the historical sciences. The task of this chapter is to argue in more detail for the role in confirmation that background theories and a general understanding of causes and effects play. In the process, we will blur the distinction between the historical sciences and the experimental sciences.

As we shall see, the historical sciences are dependent upon theories about how the world works in general, and, consequently, see past processes as tokens of well understood processes types. While some of these processes may well be unique to the historical sciences for reasons of temporal or physical scale, or perhaps due to the unique subject matter of the particular historical science, many of the links between cause and effect are within the domain of the experimental sciences. While Cleland's machinery does provide a means to choose between hypotheses, it is reliant upon well-tested and well-understood regularities. These regularities play important roles in testing claims about the past.

Because regularities do play a role in the historical science, this allows us access to the confirmatory apparatus of the experimental sciences. For if, as I argue here, the historical sciences need to understand processes that are general and repeatable, they can experiment. Consequently, one of the supposed distinctions of the historical sciences outlined earlier, that they cannot test their claims about the past utilising the tools of the experimental sciences, starts to become less obvious.

To start with, we will isolate the problems clearly. The first of these is the initial plausibility of hypotheses. The problem here is how we can narrow the space of potential past events. In the second section, we will look at the second problem, the need to secure the individual lines of evidence.

Following on from this, we will look at archaeology's solution to the second problem. Because of a weird quirk of archaeology's history, archaeologists and some interested philosophers are acutely aware of the problems of evidence for historical hypotheses. As they worked through the problems, they developed some ideas about the limits of their project, and ways in which claims about the past could be secured.

In the final two sections, we will draw some general conclusions, and discuss just what kind of regularities the historical sciences use. Many of the regularities the historical sciences use are specifically concerned with how events in the past leave evidence; they are regularities that represent the relationship between past causes and observations. Representations of regularities in the form of models must also be capable of being modified to account for the specifics of historical situations, an issue that will become important in chapters 8 and 9, where we will look at the construction of narratives.

4.1 Isolating the Problems

There are two problems for the historical sciences: We need to define the space of possible hypotheses with unique signatures of consequences, and we need good reasons for thinking that a particular state of affairs is caused by the hypothesised event. While we will deal with these two problems individually, they are intimately linked. The evidence shapes our space of possible hypotheses, but in turn, we recognise potential lines of evidence because of our theories. Any particular case of investigation into the past may start with one or the other. In fact, historical sciences frequently move between evidence and hypotheses repeatedly, a point that we will discuss in detail in chapter 8. Nevertheless, at this point in proceedings, clarity is best served by treating these issues separately in two sections.

For a start, there is the issue of getting hypotheses on the table in the first place; before we can engage in eliminating hypotheses using the tools that Cleland provides, we should ideally have some plausible candidate hypotheses. In the case that Cleland uses, the extinction of the dinosaurs, why think that a meteor might even be a possible cause? This is where our background theories first come into play. As the Hull quote from the previous chapter noted; "… scientific theories tell us what can happen" (Hull 1975 p266), and such theories are going to tell us what are possible causes, and eliminate impossible ones well before we come to the process of identifying consequences.

To take a stark example of how reliant this aspect of hypothesis choice is for Cleland style reasoning, one need only look at pre-Darwinian explanations of the changes in the fossil record. Early geologists were well aware that they could arrange geological strata according to the presence or absence of certain fossil types, so they knew that there were sometimes quite sudden transitions in fauna, and extinction events across a range of taxa, and across large regions (Rudwick 1972). The explanatory target, the extinction events, were then already cause for speculation. Some of these early geologists were at pains to reconcile their geological observations with religious texts[9]. In such an intellectual climate, the

[9] One might even argue that in effect, they too were working with multiple downstream

background theories for pre-Darwinian geologists included the notion of catastrophes such as divinely inspired floods (Rudwick 1972). The theories of the day, informed by religious dogma, said that global floods and other catastrophes are possible, and thus, the evidence of the fossil record was interpreted partly with such generally acceptable assumptions in mind.

Background theories or ideas about what is possible not only get hypotheses on the table as potential causes, they can suggest limits on hypotheses, or at the very least, raise problems for conceptions of the past. The geological gradualism of Charles Lyell, and the evolutionary gradualism of Charles Darwin, both had difficulties when William Thomson, later Lord Kelvin, calculated an age of the Earth that was far shorter than their gradualist theories required (Rudwick 1972; Gribbin 2002). The reconciliation came about with Einstein, and the awareness in of the role of radioactivity for maintaining the earth's temperature. This theory change gave the earth a longer history, and fitted the gradualism of Lyell and Darwin. But before Einsteinian physics, the threat to the gradualist theories of the earth's past were real. The background theories of the day —Thomson's theories about thermodynamics— placed limits on acceptable time frames for the earth's age, and consequently how much time was available for gradualist processes.

While Cleland's machinery can choose between hypotheses with some precision, background theories about what is possible play a role in determining the hypotheses available. Our best science of the day suggests potential causes, and eliminates others. This means of course, that as our understanding of the world changes, our possible hypotheses change as well. Insights into what is possible now, will change our views on what is possible in the past. As our science improves, the space of possible hypotheses may expand as we understand new causal relationships, or contract as we set limits on causal relationships and understand the dispersal of consequences.

This then is the first time I will use a phrase that will be repeated in various forms throughout this thesis: Our understanding of the past is only ever as good as our understanding of the world in general. As our

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consequences of prior events, as religious texts were seen as legitimate sources of evidence about the past.

science improves, our understanding of the past will improve with it. But these background theories about the world are generalities. They are generalities that can be tested independently of any historical claims. Hypotheses about the past are worth investigating only if we think they are plausible, and that plausibility comes from being part of a well-understood body of scientific ideas.

4.2 Lines of Evidence

The second problem that we need to deal with is the issue of a unique signature of effects that we use to choose between hypotheses. Cleland's picture assumes that there is a secure relationship between various kinds of observable evidence and the particular past cause of that configuration of evidence. There is an assumption that shocked quartz and iridium layers in the geological record are good evidence for a meteor strike. But what allows us to say that a meteor will have a certain set of consequences? How do we know all this? In fact, we need to be able to reliably say that such and such an event in the past with have the consequences thus and so. We need to secure the observations as evidence, before they can play a role in our justification procedure. Peter Kosso refers to the background theories of that secure observations as evidence as "accounting claims:"

> …the relevant information to give an observation credibility and meaning. It is, in other words, what it takes to make an observation into evidence, evidence of something of historical or archaeological or scientific interest. (Kosso 2001 p57)

What's required is accounting claims that validate the individual lines of evidence *as* evidence. Every single piece of broken crockery, and every other effect of the bull's presence, has its own casual relationship to a bull in the china shop event. One piece of crockery might have a direct causal chain back to the bull, as the bull knocked it over. A second piece of crockery had has a less direct link, as it broke as a result of an overturned display table. The relationship from the bull to the distraught shop owner is a complex causal chain that must account for the psychological dispositions of the shop owner.

It appears then that we still have the problem of relating observations to evidence. We still have the basic dilemma of inferring a past cause from a current observation or set of observations. While, all things being equal, Cleland's machinery gives us good reasons to choose one hypothesis over another, all things may not be equal when it comes to the accounting claims for the ramifications of any past event.

The problem is that individual lines of evidence may require theories about subsequent transformations of evidence. In practice, the traces of past events that may be of interest are going to be transformed through time. We may have to construct narratives that account for the transformation of traces. Evidence can have its own history. The mineralisation process of fossils is a simple example. Bones of dead organisms will transform over time, becoming distorted and mineralised to a point where a great deal of knowledge is required to reconstruct the past organism and its environments. Part of the conviction that fossils are in fact the remains of dead organisms comes from understanding how fossilisation occurs.

The dispersal of consequences that Cleland's machinery utilises, points to the problem: This dispersal is ongoing. Many potentially informative consequences of past events are transformed through time by further dispersal. To utilise the traces of past events, we must understand the ways in which they have been transformed through time. Thus, our clean picture of the dispersal of downstream consequences becomes much more complex. The effects of earlier events can become transformed by later events. Some effects degrade further, with some degrading beyond recovery. (See Figure 4-1)

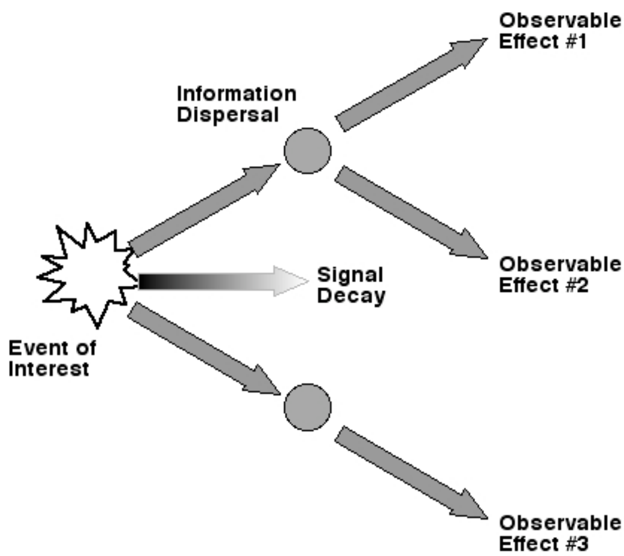

Figure 4-1: Because of signal decay and various other transformative effects, historical scientists need to understand a great deal how events of interest leave traces, and how reliably. In the above instance, an event of interest has left multiple effects, but Effect #1 and #2 are the result of further dispersal following on from the initial event of interest. One consequence of the event of interest has decayed, and is no longer recoverable by contemporary observers. Effect #3 has transformed, but is still recoverable assuming one knows the transformative process. To all intensive purposes, each line of evidence requires a narrative, to link it back to the past cause of interest.

While Cleland identifies a means to isolate particular events in the past, this implicitly relies upon an understanding of how past causes leave identifiable signatures. These signatures need to be unambiguous clues to the past. And yet, subsequent events will potentially degrade individual signals within this signature set, or in some cases transform them.

We need theories about how a causal event will have a relationship to a trace that allows us to consider it as evidence. This is particularly problematic in that some individual signals will disperse or transform, potentially making a particular consequence difficult to detect. This in turn makes the ramification set or configuration of consequences less obvious.

Historical scientists of various stripes are aware of these problems, as are some philosophers who have looked at particular historical sciences. To demonstrate how a historical sciences have dealt with this problem, it worthwhile turning to a science which has explicitly considered the challenges is poses. In particular, we will look the relationship of evidence to past causes. From this example, we can draw out some general lessons.

4.3 The Archaeologist's Solution

Much of Cleland's discussion focuses on geology and paleobiology, but at this point, it is worth bringing into the discussion a different historical discipline: archaeology. The relationship between observable evidence and a past cause may on some occasions be very messy indeed. The problem has not gone unnoticed by archaeologists. Because of some quirks in the history of archaeological thought, archaeologists from the late 1950s through to the 1980s became somewhat obsessed with becoming a science. In so doing, they engaged in a certain amount self reflection about their discipline, aided and perhaps on occasion abetted, by a number of philosophers.[10] In particular, archaeology attempted to come to grips with the relationships between observable data and inferences about the past. Rather than rely on ad hoc reasoning about particular cases, Binford advocated researching regularities between observations and past causes. This research became known as Middle Range Theory (Binford 1981; Raab and Goodyear 1984).

Middle Range Theory (MRT) is research attempting to find regularities in the formation of archaeological sites, and to find regularities between observable remains and the behaviours of past peoples. "The tracking of the flow of information from interesting past to observable present is done by the middle range theories" (Kosso 2001 p63). The aim is to understand how past events leave reliable signals, and how to sort out the signal of interest from the "noise" in the past.

4.3.1 Archaeology as Anthropology

Archaeology's task is broader than just dealing with material objects. An archaeologist may be interested in the subsistence and economics of a particular region for instance, rather than any particular object or set of objects. In effect, archaeologists are social scientists. But as with other historical scientists, the archaeologist's account is still based initially on the physical evidence she has access to. Should an archaeologist be interested in the past economics and trade of an area, her claims about trade, manufacture, and subsistence strategies rely on physical correlates in the form of broken pots, long dead hearths, and traces of tillage or crop management, and so forth. Ideas about social structures, economic interactions, or cultural practices still depend upon physical traces. The low level statements about objects are the subsequent observation statements and data for the higher-level theories.

Because archaeologists are interested in these human activities, they need to understand the consequences of these activities' configurations of

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[10] A full reading list of the theoretical work involved in the self-reflection of archaeologists and the contributions of philosophers would be extensive. The debates were kicked off most vigorously by Lewis Binford (1962; 1972; 1981), but his ideas were taken up by numerous others. Key philosophical contributors were Merrilee Salmon (1982), Peter Kosso (1993; 1995; 2001) and Alison Wylie (Wylie 2002). For a brief overview of the interaction between philosophy and archaeology, developments, and future prospects, see Jeffares (2008).

evidence. Much of this work is covered by Lewis Binford's dictum "Archaeology is Anthropology" (Binford 1962), and much work has gone into understanding how different human behaviours leave distinctive sets of evidence. Most modern archaeologists have a good deal of training in anthropology, providing a set of background theories about the range of likely human behaviour and how human behaviours, social systems and so forth shape human material culture.

Background theories about human capabilities also address the problem of the earlier section: the problem of what hypotheses are plausible. It narrows the space of possible hypotheses considerably. So anthropological training —the study of contemporary people and their behaviours— provides hypotheses. These hypotheses gain credence just because they are the result of contemporary observations. The limits to this application of anthropological knowledge to human pre-history we will deal with in chapter 11. Nevertheless, background theories and knowledge of human capabilities is a crucial area of training for archaeologists.

The key area of the Middle Range Theory that we are interested in at this point however, is its investigation into the relationship between evidence and past causes.

4.3.2 Motivating MRT

Inferring past causes from observations is a difficult task, and there are cases where this inference has gone wrong. Early anthropologists on New Caledonia encountered small mounds regularly dotted across the landscape. The claim that these were human constructions leant itself to speculation that there was an extinct culture of mound builders on these islands. Contemporary features were feeding in to higher-level claims about the past. In fact, these mounds appear to be the result of the nest building activities of an extinct species of megapode (Green 1988). During the early days of study of human evolution and its associated stone tools, in the late 19th to early 20th century, there was some controversy over the status of "eoliths" that were regarded as very early examples of worked stone. On further investigation, it turned out that the "Eoliths" were in fact the result of various geological processes, and not the handiwork of early hominins (Trigger 1990 p96). Bad interpretations of the initial physical traces such as these examples can undermine claims about the past.

The problem that faces archaeology at the basic evidence gathering level is initially that of securing signal from noise. "Eoliths" were just naturally occurring rocks, noise being mistaken for signals of intentional activity. Because archaeological evidence comes from a variety of sources, from the fragments of material goods, the cast off bones and shells from subsistence practices, the remains of dwellings etc, the first task is determining which pieces of physical evidence relevant; identifying the traces as traces, and not being fooled by pseudo traces.

We have observations of physical traces, high-level theories about past behaviours, and "middle" range theory that connects the two levels together. Middle Range Theory is then the bridge between the high level theories of human behaviour, and the observations of evidence. Research in MRT is a process of building an understanding of how observations in archaeology can be linked to past people and their behaviours. Human activities are the historical points of interest, but the downstream consequences of human behaviours need to be disentangled from other historical processes.

For instance, a behaviour that is of interest to archaeologists is whether hunter-gatherers re-use locations such as rock shelters. At various archaeological sites, a cross section of the sediment can reveal a lens shaped deposit of compressed ash and charcoal. The middle range theory that links the behaviour and the observations of these lens shaped deposits of charcoal is an understanding of how regular reuse of fire pits will build up ash over time.

4.3.3 MRT and Regularities

The reason MRT is particularly interesting it that it utilises regularities from a number of sciences. While some of these are other historical sciences such as paleoecology, paleobiology and geology, it also utilises chemistry, physics, and biology. What is more, where other sciences do not provide the necessary linkages, MRT takes on the form of active research, with archaeologists engaging in experimentation. Archaeologists observe contemporary processes, and try to find regularities in the world. This kind of research is sometime referred to as actualistic research, and geologists and other historical scientists also engage in similar investigative work.

The work Kathy Schick and Nicholas Toth exemplifies this kind of experimentation (Schick and Toth 1993). Schick and Toth experimented to clarify the relationships between various causal mechanisms and what they observed. In part, they are securing their claims about the past from false positives. For instance, if an archaeologist wants to claim that the marks left on bones is that of a Hominin, and not a dog, one way to protect against false positives is to conduct experiments to see what kind of marks dogs and human tool users leave on bones, and to compare them (Schick and Toth 1993). Carol Cleland argues that this is a key strategy of the experimental sciences (Cleland 2002), but it should be obvious that the historical sciences need to do this as well. They need to have a good understanding of general causal relationships that are going to form, and inform, their analyses of evidence.

But note what has happened here. We have moved from the confirmation of particular hypothesis about a particular feature of the world, to regularities. Repeated observation assumes there are underlying regularities in the way past facts determine later observable facts. Our evidential reasoning has started to include talk of processes, and to include repeatable experiments. The gap between the experimental sciences and the historical sciences is shrinking. Archaeologists are using regularities to determine the relationships between observations and past causes. This Middle Range research includes regularities that are quite broad in scope.

> Any theory could be used in the role of being middle-range. Being middle-range is not a feature of the content of the theory but of its use in a particular instance. The relevant middle in this sense in not meant to be of mid-generality or of mid-empirical content. (Kosso 2001 p62)

Middle range research deals with *potential* sources of evidence that would indicate a particular behaviour. The study of modern societies and groups provides the insights into this behaviour and its possible ramifications. Middle range research tries to understand how human behaviours leaves traces.

However, as we noted above, we may also have to understand how traces caused by an event of interest are subsequently transformed over time. Lines of evidence have their own causal history. Middle Range research also investigates the causal history of traces of the past. Over the course of the research Schick and Toth investigated both. They manufactured their own stone tools and used them to butcher carcasses to understand how this behaviour would leave wear patterns and other physical traces on the stone tools. So they investigated how an event of interest would have ramifications in the physical world. However, they also left stone tools in various locations in the landscape and recovered them at later periods, in order to understand how the subsequent history of a tool would transform these traces.

This research into the transformations of physical traces over time is also done by other historical sciences such as paleobiology (Gifford 1981) and paleoecology (Behrensmeyer and Hook 1992). They too are interested in detecting, and securing regularities between past causes and observations. They are interested in something that all sciences are interested in, discovering the causal relationships between an event and its subsequent consequences. In order to understand the physical traces that we observe, we must understand its causal history.

Reflection upon their scientific practice has suggested to archaeologists that they must carefully scrutinise the relationship between their observations and their claims about the past. Middle Range research shows how historical scientists can secure this relationship. They can investigate potential regularities in how events leave traces, and how these traces can subsequently transform through time. Archaeology can exploit regularities, and engage in research in regularities. Now we can turn to the task of drawing some general lessons from archaeology's example.

4.4 Processes Types and Process Tokens

A good example of a historical science with an interest in regularities is evolutionary biology. One way to read Phillip Kitcher's analysis of Darwinian biology is that Darwin provided a unifying framework —a standardised schema— for framing what are effectively historical enquires. The introduction of the new schemata sets new questions for biology, in that, after Darwin, naturalists are given the tasks of i) finding the instantiations of the Darwinian schemata, ii) finding ways of testing the hypotheses that are put forward in instantiating Darwinian schemata, and iii), developing a theoretical account of the processes that are presupposed in Darwinian histories (Kitcher 1995 p33).

The adaptations of individual organisms are instantiations of a general processes. Our understanding of the process guides us in what to look for as confounding factors, and alerts us to potential false positives and false negatives. It is our background understanding of evolutionary processes that gives us confidence in any statement about the past we wish to make.

Research into evolution by natural selection is research with direct bearing on our understanding of the past. The more we know about how natural selection can transform organisms, and the more we know about the limits of this process, the more we can make sense of the fossils and other traces from historical cases of natural selection. The Darwinian schema provides a guide to this research.

However, this focus of this research looks rather different that the focus of historical research that Cleland outlines. For Cleland and Seddon, the focus of historical research is events: the extinction of the dinosaurs, the history behind a particular feature of the landscape, and so on. The evidential reasoning they outline is only concerned with events and talking about unique causal chains. Cleland takes the historical sciences to have this distinctive feature. She has almost defined the historical sciences this way. Thus, the hypotheses of prototypical historical science differ from those of classical experimental science insofar as they are concerned with event-tokens instead of regularities among event-types (Cleland 2002 p480). The Darwinian schema suggests that research into Darwinian natural selection looks focused on regularities. How do we reconcile these seemingly different foci, with unique histories on the one hand, and regularities on the other?

As we have seen, historical scientists must secure the relationship between observations and causes. Archaeologists want to understand the relationship between observed features such as deposits of animal bones, and the agents —both hominin and canine— responsible for modifications to those bones. Geologists want to understand the process of tectonic plate activity generally. As I have argued above, understanding these relationships can be achieved with the tools of the experimental sciences to investigate common causal processes. We need to supplement Cleland's reasoning with this need to research into regularities. Cleland convinces us about reasoning from evidence to secure claims about specific contingent historical *events*, but we also need to accommodate the historical sciences' search for accounts of common historical *processes*.

Consider the eruption of a particular volcano. A particular event like this can be seen as unique, something singular. However, it shares features with other volcanic activity. Any particular volcanic eruption is a token of a common type. We can then understand an event by comparison with volcanic activity. In so doing, we acknowledge the fact that volcanic activity is widespread throughout time, and that we might be in a position to investigate volcanic activity as a general process. Once some understanding of a process is in place, we can appeal to it in our account. We do not just account for a volcanic eruption by appeals to its particular history: We also account for it by appeals to the general process of volcanic activity. This is part of our framework for accounting for its presence.

The appeal to general accounts of processes in the historical sciences is widespread, and takes different forms. For some historical sciences, the appeal is to the experimental sciences. The use of carbon dating in archaeology is an appeal to the process of carbon14 decay over time. Claims about the age of a particular sample are based on a physical process that is well understood within experimental physics. For other historical sciences, more mechanistic accounts work. The relevant parts are specified, and a causal relationship between the parts is specified. Geology works with this kind of background mechanistic assumption on occasions. An account of the history of a volcano specifies common volcanic "parts" such as magma, crust, tectonic plates etc, and specifies interactions between these parts to account for volcanic activity. In this instance, geology is not using a generality from an experimental science, it is actively constructing its own by investigating regularities across volcanoes.

A quintessential historical science, evolutionary biology, is clearly relies on a common process. Natural selection is appealed to constantly in the history of species. We frequently see justifications of the adaptive significance of a feature of an organism by reference to similar adaptations in other organisms that share similar habitats. We can check our hypotheses about an adaptation by reference to a common process that we think is operative. This reference to the operation of common processes in evolutionary biology is just as important to confirmation as the elimination of alternatives hypotheses through Cleland's mechanism. Our understanding of the paleontological record is not built de novo from the observations of fossils, but constructed on the foundations of our knowledge of the contemporary natural world.

Further, understanding general processes provides frameworks for the understanding of tokens of a process type. As we saw in chapter 2, robust process explanations can provide important comparative information;

showing how to tokens of a process are similar. However, robust process explanations can also provide us with important contrastive information, showing how a particular token differs from others and is unique. The use of theories about common processes thus frames questions that we must answer to understand the particular token event. We will go into more detail about how this process works in practice in chapters 8 and 9.

Background theories about common processes solve many of the problems historical scientists face. They limit the space of possibilities. They suggest hypotheses. They are well understood and confirmed through experiment. And they provide the crucial link between observations and past causes: justifying our use of a physical trace as a piece of evidence.

To finish this chapter, we will briefly sum up how theories play a role in the example that Cleland uses: That of the extinction of the dinosaurs. We will then briefly discuss what kind of theories these common processes are, in preparation for subsequent chapters.

4.5 Regularities, Models, and Reasoning about Processes

One of the showcases of smoking gun reasoning is the extinction of the dinosaurs. The meteor hypothesis carries weight as a past cause because of additional downstream consequences of a meteor impact. But part the security of the claims about a meteor is that non-geologists have good reasons to think that any individual meteor is actually a token event of a particular process type.

A meteor impact event is not improbable in itself; it is within the space of plausible hypotheses of cosmology and the time frames it works with. Even if one of the subsequent effects of a particular meteor impact token, the extinction of the dinosaurs, is distinctive, meteors impact are rather mundane: they are part of the process of accretion of matter in local gravity wells. The surface of the moon tells us that these meteor events are not unusual, and provide important ideas about meteor impacts. Part of the confidence we have in meteor impact events as potential causes comes from this understanding of processes. Background theories about how the world works play the role of suggesting that meteor events are potential causes.

To see this, take an alternative world, where the orbits of all celestial bodies were stable, and collisions and accretion in gravity wells were outside the realm of the natural order of things. In such a world, positing a meteor strike would be odd to say the least. In a world of stable orbits, a meteor strike would be something resembling a miracle, and any account of the past that included meteor events would need exceptional evidence, and an account of how, in a world of stable orbits, a meteor impact is possible.

Such robust accounts of processes serve the historical sciences well. They can refer to well-understood regularities and acceptable models of the world.

However, we need more understanding of impacts to confirm the hypothesis. We need to deal with the closely related problems of single lines of evidence, and a distinct signature of evidence. For example, before iridium layer can play a role in choosing between hypotheses, there has to be reasons to think iridium is scarce at the earth's surface unless it has been deposited by fallout from a meteor impact. *Then* the iridium layer can play a role in the argument, acting as positive evidence for the impact hypothesis, and potentially negative evidence of an alternative hypothesis. This link between the observation of the iridium layer and a meteor impact requires background theories and understanding how the world works.

Using Cleland's machinery requires regularities, but these need not be laws. Frequently the sciences represent processes with models, and the historical sciences are no exception to this. Models and their status is a subject of some debate (Giere 1988; Giere 1999; Godfrey-Smith 2006; Frigg and Hartmann 2008; Odenbaugh Forthcoming). But the notion that the historical sciences use models should not come as any surprise. Put at its most intuitive, the models used "…are idealized structures that we use to represent the world, via resemblance relations between the model and real-world target systems" (Godfrey-Smith 2006 p725-726). As is the case with other sciences, historical sciences frequently use complex representations, in the form of abstract theorems, or as idealised analogues, as ways of representing the world. This is particularly true when the processes involved are complex.

In application to specific historical targets, models can also include more information and attempt to represent quite specific target systems. A. Brad Murray notes that in geology, models can be both general "exploratory" models designed to understand general processes, and quite target specific simulation models that are

> …designed to reproduce a natural system as completely as possible; to simulate as wide a range of behaviors, in as much detail, and with as much quantitative accuracy as can be achieved. (Murray 2003 p1)

Models, regularities, and background theories consequently start off very general, but are modified to account for the specific target system in question. We will have much more to say about models, and how general models of processes come to be used to account for specific historical instances in chapters 8 and 9. Suffice to say at this point that models represent causal regularities that apply to many instances and gain credibility through deployment in contemporary contexts. Like any other regularity, they can be tested well before historical scientists deploy them.

Clearly there is more to be said here. Some models are capable of rigorous testing; some are not. The historical science of cosmology can appeal to the law-like statements of the experimental sciences. Geology and biology use models of common processes. Some are very specific: the tooth marks of dogs versus the marks of stone tools. All that I wish to argue here is that well understood regularities play a role in the historical sciences. Our representations of the regularities are testable, observable, and secure the relations between observations and past causes. They are a means for securing claims about the past.

4.6 Summary

Once the historical sciences start use generalities, in some instances rather high-level generalities, as tools for understanding the past, the confirmatory apparatus of the experimental sciences becomes available.

Regularities and their representation play a role in the historical sciences. It is, as yet, an incomplete picture. There are still issues to do with the temporal and physical scale of some phenomena, particularly in geology. There are also issues to do with how high level generalities work with quite localised claims, and how contingencies are dealt with.

Nevertheless, a basic model of historical confirmation should be clear. Historical Scientists are interested in events in the past. These events act as explanations of contemporary observable phenomena. Downstream ramifications of past events can be used to choose between competing hypotheses. While multiple downstream consequences provide unique signatures of past events, these, in turn, are only understood to the extent that we have an understanding of the relationships between observable phenomena and the putative past cause.

Given this role for theories, and this need to experiment and understand regularities, we need to reconfigure how we view the historical sciences in relation to the experimental sciences. This is the topic of the next chapter.

5. Temporality in the Sciences

In the last chapter, I outlined how the historical sciences, and archaeology in particular, are dependent upon regularities and a background set of knowledge to make sense of the past. In particular, they have to identify relationships between observable contemporary phenomena and the past processes responsible for the production of such phenomena. This involves experimentation, for the historical sciences can see processes as types, rather than just individual, one of a kind, tokens. These generalisations need to be modified to account for localised contingent instances of processes, and we will spend much of the rest of the thesis working through the ways general processes become embedded in historical narratives. However, in this chapter, I want to put the use of generalisations and regularities into a broader context to reinforce the idea that the historical sciences are actively engaging in experimentation, and utilise many of the confirmatory strategies of the sciences generally.

To achieve this end, I am going to introduce a picture of the sciences that takes full account of their temporality. By this, I mean that all sciences are temporally located, and all sciences are interested in processes, which, by their very nature, happen in time. Once we see this temporality in the sciences, the historical sciences become much more integrated into general scientific practice. It does, in some ways, mean that the historical sciences are users of science, in that they deal with apply the products of the experimental sciences to problems *in* the world. In this, they are an important counterpart to future directed sciences, such as engineering, and branches of ecology. Such future directed sciences also utilise a general body of knowledge; not infrequently the same tools, models and regularities utilised by the historical sciences.

Once this temporal view of the historical sciences is in place, in the second section, we will look at some limitations of this view. The limitations are primarily dictated by information preservation. If the causal chain from an event in the past disperses too much, it may well disperse beyond recovery. While much of the rest of the thesis will explore the limits of particular historical sciences, these concerns are more general in scope.

Once we have summed up these arguments, we will sketch the direction of the subsequent chapters of the thesis.

5.1 The historicity of the experimental sciences

Classically, we think of the experimental sciences as engaging in experiments and observations as a means to verify their claims about how the world works. But a closer look at experimentation with a full consideration of its temporal aspects reveals something else.

An experiment is a process that has been set up with known starting conditions, under tightly controlled circumstances. In such an experiment, the variables are, hopefully, all known, and can be adjusted over repeated experiments, to see which one matters. While 'observation' can take place during all phases of an experiment, frequently, the crucial observation is the final one, where the results of the experiment are assessed. In a very real sense, an experimenter observes the downstream consequences of a process in much the same way as the historical scientists does. The crucial difference is that the experimental scientist: a) controls the conditions under which the experiment happens so can control for contingencies, and b) the experimenter has access to the starting conditions and in some cases the intermediate phases of a process. Effectively, the experimental scientist induces a process that becomes a piece of the past with known variables.

In a sense, it is an induced causal chain, and the experimenter wants to construct a narrative that accounts for that causal chain. Typically, such causal chains are a single transformation, rather than a sequence of them. This is a view of experimental practice that recognises it as a historical process.

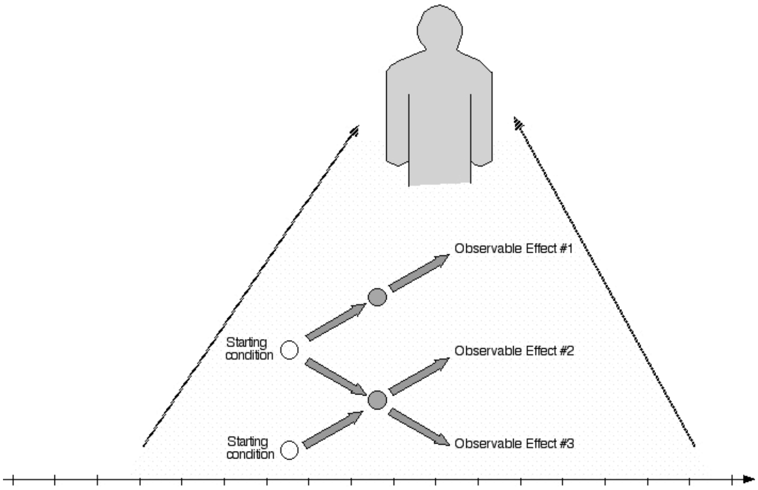

Figure 5-1 Experimental Access given a temporal dimension. The axis along the bottom is time. The experimenter has temporal access to the starting conditions of the experiment, and the subsequent consequences, in the form of observable effects. They also have control over the variables in the experiment.

A simple hypothetical case demonstrates just how historical an experiment can be. Imagine a 19th century geologist with an interest in the rates of erosion of various rock types. He sets up an apparatus in a mine, where a number of large tanks drip water onto thin slabs of various stone types. When the slab is worn through, the dripping water dissolves a lever holding open the tap that is dripping onto the stone, with the result that the drip will stop, and there will be a quantity of water left in the tank. He documents everything he has done, leaves un-weathered samples of the rock types, and a full tank to compare any evaporation and so forth. He then seals the mine, and deposits instructions with a lawyer for the mine to be opened in the early 21st century, where, according to his hypothesis, half the samples would have been eroded.

Duly, in the early 21st century, the mine is unsealed, the apparatus appears to have worked, the documentation is assessed and the results quantified. The hypothesis of erosion rates proved correct.

On the picture of the experimental sciences I have set up, it appears obvious that this was an experiment, but just a long one. It was a joint effort on the part of the initiator of the experiment in the 19th century, and those who tabulated the results in the 21st. It is also important to note that even if the 19th century hypothesis proved incorrect, it is still an experiment that could generate reliable observations, and come to well found conclusions. Despite its extreme duration, and peculiar historical structure, it is a robust piece of science.

There are actual cases of scientific work with a similar structure to this. The viscosity of some materials is such that their flow is extraordinarily slow, and observations need to take place over decades, rather than the life of a single researcher. Glass in old buildings is a good example, with the glass flowing under the effects of gravity over time such that the lower part of the windows are thicker than the upper part. Pitch also flows slowly, and the first Professor of Physics at the University of Queensland, Professor Thomas Parnell, began an experiment in 1927 that has provided data on the viscosity of this material, subsequently written up by later members of the department.[11]

The utility of observations of water and atmospheric temperature over generations for climate scientists is a case where data is transmitted between generations. Contemporary scientists do not make the observations of temperature as such, but utilise historical information for their models of climate change. The use of detailed observations and field notes from previous generations of researchers provide important information about changes in environments, wildlife and other details of interest to a wide variety of researchers.

The point behind these examples is to highlight the temporal nature of scientific observation. Results come about after a process, and it is the results that are observed, and the subject of analysis. On many occasions, although not all, the results of an experiment are used in an inference about the process. There are cases where individuals observe intermediate phases of a process. Part of the strength of the experimental method is the ability to intervene at various points in a process, as well as being able to repeat experiments and change variables. Nevertheless, the idea that later generations can, and do, generate reliable scientific knowledge courtesy of the work of previous generations should be obvious. Processes take time, and the consequences of processes are frequently used to infer the underlying mechanisms of processes.

It is also true that nature can on occasions set up situations where

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[11] Thanks to Jochen Brock for reminding me of the example of glass, and Marcel Cardillo for the example of pitch. The Queensland University "Pitch Drop" experiment, which has been running for over 70 years, can be found at http://www.physics.uq.edu.au/pitchdrop/pitchdrop.shtml

scientists can compare outcomes to duplicate repeatability, controls and other features of experiments. Many scientists would feel comfortable with the notion of a "natural experiment" and many have exploited these successfully. Natural experiments take a variety of forms, and duplicate different features of the experimental process. Biology frequently exploits natural experiments that involve repetition. The comparative method of testing adaptationist hypotheses is the prime example (Sterelny and Griffiths 1999 p241-252). In making a claim about the adaptations of an organism, a hypothesis can be tested by looking at a range of species. In some cases, this might be within a lineage. The adaptive advantages of schooling in fish can be 'tested' by looking at related species that do not school, and the adaptive advantage tied to differences in their environments.

Convergence in designs in lineages not closely related can also identify the adaptive force behind an adaptation. Optical convergence is a common engineering solution to the problem of depth perception. Such natural experiments offered by the comparative method utilise the fact that there are multiple results of natural selection. Just as the experimental scientist can compare multiple experiments, and identify the key variables that determine variation in outcomes, so too can the comparative biologist.

Nature's experiments can be messy, but not always. On occasion, the number of variables involved is low enough that an inference about the process is reasonably straightforward. This is particularly true of chemical and physical processes encountered in geology. For instance, the red hue to various soil and rock deposits can be accounted for by the oxidation of iron ores. The process is simple enough that the variables can be isolated easily.

An interesting third strand of "natural experiments" comes from analysing failures in man-made structures. The development of a body of knowledge associated with engineering has long benefited from observations of failure. These are not intentional experiments, although testing to destruction aircraft and other artefacts has been an important feature of engineering practice (Gordon 1978; Gordon 1984). Henry Petroski has argued that the analysis of failure in designs has been a potent force in the ongoing development of engineering and design (Petroski 1992; Petroski 1994; Petroski 1994). Error can be used to isolate key overlooked variables through the analysis of the resultant wreckage.

The advantage in these cases is that the initial starting conditions are well known. The engineer in a forensic role, investigating a ship that has broken its back, or a bridge that has failed[12], has the benefits of the original design, and the material involved. They lack control over all the variables, and this is why the design has failed in many cases, but reconstructing the causes of a failure is nevertheless a forensic activity, working from available evidence to reconstruct the processes. In the engineering case, one of the sources of evidence for the reconstruction just happens to be the plan of the original construction.

The key point is simply that the sciences in general make use of observations that are the results of prior processes. Experiments are done under controlled conditions, that allow experimenters to know the starting conditions and to isolate variables. In other respects, the tools of observation are remarkably similar. From observable contemporary data, inferences are made about the past. While the experimental sciences do have advantages, they too share a forensic aspect, reconstructing a prior event from its consequences. But as noted above, the historical sciences get some experimental advantages as well. Events can be simple enough to the relevant variables to be isolated. Nature can produce enough variants so that one can benefit from repetition, particularly in biology when utilising the comparative method.

Starting conditions too, may well be isolated. The methodology outlined by Carol Cleland will on occasion allow us to choose between two hypothesised starting conditions for a subsequent process of interest. There are however some exceptions to this, and I will deal with these is later sections of this chapter. At this point, I will sum up this temporal view of the sciences.

5.1.1 Temporality in the Sciences

To understand the past, and to understand causal relationships, the sciences experiment on and observe the actual world. They set up a causal chain with known starting conditions, observe the reactions and effects, they intervene in causal chains to detect its salient features, or

[12] A famous example was The Tacoma Narrows Bridge, which was built in the 1940s. Its collapse due to aerodynamic instability was famously captured on film, and the lessons learnt improved future bridge design (Petroski 2006). The film can be found at various locations on the web, notably Wikipedia.

they go out into the world and observe actual occurrences of processes. They then take the causal relations they find and generalise to other cases. They can even engage in predictions, using these known causal relations to make statements about unobservable future consequences. They use these generalisations to make hypotheses about temporally inaccessible locations.

Predictions are another example of science deploying a general body of knowledge to an inaccessible temporal location. However, in the case of prediction the deployment is forwards in time. Prediction has its own set of problems, but the chief one is the inability to predict which processes will come to bear on a subject of interest. Engineering provides examples here, with engineering failure frequently being the result of not overlooking variables that matter. The Tacoma narrows bridge case is a good example. No one realised that wind would be an issue. However, while sometimes this prediction failure is the result of an incomplete knowledge set, this can also be due to genuinely contingent factors. It is a large and complex world, and surprising and unforeseen variables can impact upon things in strange ways. Predicting the future is, in theory at least, more difficult than making claims about the past. Take a series of claims about ten fair die rolled in the past and in the future. Making a statement about the number of sixes just rolled by an experimenter relies on the ability of the observer to count the number of dice with a six face up, but by it very nature, we can only make statistical inferences about ten die that are about to be rolled.

Both past-directed sciences and future-directed sciences can make use of the very same body of scientific knowledge. Ecology is a good example of a science that does both forward directed prediction similar to engineering, and past directed statements using the same set of general tools. We want our ecologists to be able to make reliable claims about the future, to help us make decisions about land use. We would also like ecologists to deploy their tools in understanding the past, and to make retro-dictions to help understand the history that has shaped current environments and eco-systems. The past directed investigations and forward directed investigations reinforce the same underlying body of principles. In fact, successful deployment of the same set of tools adds plausibility to the tools.

Realistically, it is just that each science starts from a different end of the causal chain to understand the world. Predictive scientists work from starting conditions, while historical scientists work with end results. Both are using the underlying causal regularities of the world to piece together the causal chain.

The historical sciences are distinct from the experimental sciences in their explanatory project. As we saw in the first chapter, the historical sciences are interested in building explanatory narratives of particular episodes; the experimental sciences want to understand general mechanisms. However, what we have seen in this section is that they are much less distinct when it comes to their confirmatory project. They both use regularities, and use observations of the past to confirm their claims.

Nevertheless, the fact that the historical scientists are interested in narratives of particular episodes imposes practical problems on the historical sciences that are very different than the problems of the experimental sciences. In the next section, we will isolate these problems, and the potential limits they impose on the historical sciences.

5.2 Explanatory Targets and Localised Regularities

We have been discussing the historical sciences in terms of regularities that apply across different cases. However, as noted in earlier chapters, the historical sciences are frequently interested in particulars: localised phenomena with discrete causal histories. Much of the rest of this thesis will be directed towards understanding how representations of these general processes can be made to work with specific historical narratives. We will be concerned with specific challenges posed to particular historical sciences.

However, before we get in to the detail from particular sciences, we need to ask whether there are any in principle difficulties with the reasoning methods I am advocating. Are there any theoretical limitations? The second question is whether there are any practical limitations that are general enough to circumscribe the use of this style of reasoning. We will deal with both these issues separately.

5.2.1 Strange attractors and Stable States

Isolating a past event seems relies upon its dispersion to provide a unique signature of an event. However, some processes may well not disperse, or disperse in ways that disguise their causal origins. There may be an overdetermination of consequences, and such cases potentially threaten our ability to isolate a particular node in a causal chain or historical narrative.[13] In fact, our background theories, and our understanding of the way the world works might in fact tell us that there are states that are unrecoverable.

This is not an idle theoretical challenge. Some evolutionary systems do seem to converge on evolutionary stable states. The classic example is that of a population of individuals engaged in different strategies in potential conflicts. Evolutionary Stable Strategies, such as Hawks and Doves, typically results in a stable mixed population regardless of the starting point. Dependent upon the payoffs, passive Dove strategies and aggressive Hawk strategies tend to stabilise at a certain proportion of the population.

On encountering such a population, at first pass there appears to be no way of reconstructing the starting population. Was it all Hawks, and Doves invaded? Or was it all Doves, and Hawks invaded? The observable population would have similar proportion either way, as the population would over time move to the stable state. Other historical systems have similar properties of convergence on stable states regardless of starting conditions.

Derek Turner utilises an example of Elliot Sober to argue that such cases of local underdetermination is in principle always possible.

> Elliott Sober uses the following example to illustrate this concept of an information-destroying process. Suppose a person releases a ball from the rim of a giant bowl. A later observer happens along and finds the ball resting at the bottom of the bowl. It will be impossible for the observer to infer from which point along the rim the ball was released. No one hypothesis about the point of release is any more probable than another. In this case, rival hypotheses about the point of release are underdetermined by the observable evidence, because all of them are empirically equivalent in the strong sense. (Turner 2005 p223)

The case cited here from Sober (Sober 1991) is actually a similar case

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[13] This challenge was sharpened by the response to early presentation of material in this chapter. Thanks to Aidan Lyon and the ANU PhilSoc audience of 27th March 2007 for clarifying the problem.

of convergence. All our theories explain why the ball will rest at the base of a bowl.

The problem that confronts a historical scientist is that they may wish to include this process as part of their narrative. A biologist may well want to determine the history of strategies in a population of organisms. They can only observe the final distribution of the strategies, which will converge regardless of the initial distribution.

The answer to this problem is that we may well be able to use additional evidence to infer the starting conditions. We can utilise Cleland's machinery so long as the starting conditions have additional downstream consequences other than the stable state. The real problem with stable states is that we are only looking at one piece of contemporary evidence, the resultant state. The lesson that Cleland provides is that we should look for additional lines of evidence. In the Hawks and Doves case, if all closely related species tend to have a Dove strategy, we may well be able to infer that the ancestral state is one of Doves, with an invasion of the Hawk strategy.

The slip here is to argue for local underdetermination from a theoretical or global example. In the example from Sober, there may well be additional factors that allow us to narrow down the starting position of the ball. An actual bowl may force the experimenter to release the ball from a limited portion of the rim, due to lack of reach, or other factors related to the actual circumstances. Simple, closed systems really are difficult, but events in the world are messy, complex, and leave surprising quantities of diagnostic detritus.

All that is required is that there are additional consequences of the past *other* than the particular feature we want to explain. So long as events tend to have multiple consequences, the historical sciences have options.[14]

Take our toy example of the bull entering the china shop. We might want to provide a richer narrative than "a bull enters a china shop." We may in fact want our narrative to reach further back in time, and tell the story of which of two possible neighbourhood bulls was the cause of the

[14] Very simple physical systems at the atomic level, and possibly cosmological systems, are probably the most difficult in convergence cases.

retail carnage. At this point, our evidence from the shop may not be able to discriminate between the two bulls. However, further back in the causal chain, there may be additional evidence. If we find a broken gate for one farmer's bull, but not the other, we again have a suitable "smoking gun" that allows us to discriminate between hypotheses.

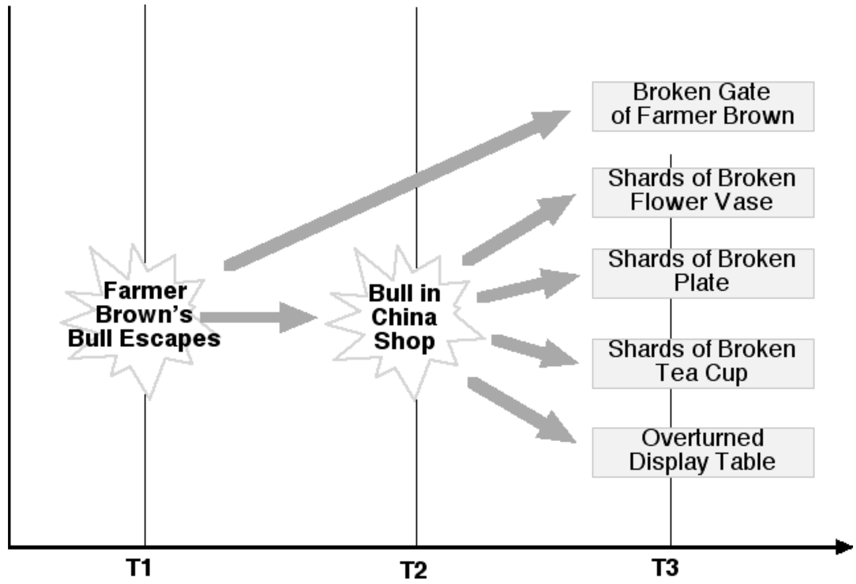

Figure 5-2 In this case, we wish to discriminate between the bull of farmer Abel, and the bull of Farmer Brown. The additional line of evidence, the broken gate of Farmer brown, an event from further back in the narrative, allows us to determine which bull caused the damage in the china shop.

In fact, this strategy, of using additional lines of evidence from more than one event is crucial, in that it provides us with additional precision. Often in the historical sciences, we are interested in causal histories that encompass more than one event. We want to provide a causal history for a central subject, and not just account for one event.

5.2.2 Information preservation

A second challenge posed by Turner, and one that is more difficult, is evidence. The historical sciences are going to be severely restricted because some causal processes leave no permanent physical traces. The first thing to point out is that this is true in some instances in the experimental sciences as well. Because of the impermanent nature of the effects of a causal sequence, scientists frequently have to resort to photography or some other recording system. Effectively, before the signal decays, a scientist records it using some kind of recording device. Recording devices require their own auxiliary hypotheses.

However, signal decay does pose an epistemic threat to the historical sciences. As Turner notes, (2005) information destroying processes can mean that the distinct "signatures" of downstream effects relied upon to eliminate one hypothesis or another may well be destroyed. Evidential traces are subject to evidence destroying processes. Some stuff simply does not survive the rigours of time. Information that could well be crucial to distinguishing between hypotheses may be subject to decay. Turner argues that in many cases, signals for discriminating between hypotheses will be unlikely to preserve. It is also worthwhile pointing out that in forensic cases, a causal agent may well be actively hostile to information preservation: Achieving the perfect crime is a matter of destroying information, dispersing signals beyond reliable reconstruction, and removing the smoking gun. Turner describes this as a problem in local underdetermination, and he suggests that the problem is pervasive in the historical sciences.

Turner provides some examples where there is no distinctive "smoking gun" that is likely to bear the test of time, and consequently hypotheses are empirically equivalent. Turner points out an obvious case, where we are not in a position to discriminate between two hypotheses about dinosaur colouration (2005 p217). Even though, metaphysically, causal pathways ramify, in many cases, the surviving traces will not be enough to determine which hypothesis is correct. Turner provides examples where epistemic underdetermination hinders choosing between alternative hypotheses about the past. On Turner's view, the historical sciences problem remains substantially intact.

> There is one very general reason for thinking that local underdetermination problems are more pervasive in historical than in experimental science. Background theories of geology, and especially taphonomy, tell us that many historical processes —the fossilization process, the processes of weathering and erosion, continental drift, subduction, glaciation, and so on— are information-destroying processes, rather like housecleaning and document shredding. (Turner 2005 p217)

In short, Turner thinks that the problem of signal decay is so widespread and pervasive that the historical sciences are radically disadvantaged. If correct, Turner's argument poses severe limits on the historical sciences. No matter how much we understand general mechanisms, no amount of science is going to understand the past in the kind of detail we would wish. Pervasive and widespread dispersal of consequences is such that we will never have enough evidence to confirm our hypotheses.

If the historical sciences were only interested in explaining contemporary traces, the problem would disappear: no traces, no explanatory problem. But, there are things about the past that we are interested in, and historical investigations are not always shaped by contemporary observations. Nevertheless, historical scientists do recognise limits to what they can achieve in the same way that experimental scientists do. What's more, scientists frequently have principled reasons for thinking that some questions are not worth investigating. These principled reasons are usually the results of background theories about how the world works, including background theories about information destroying, and information preserving, processes. So, research into dinosaur colouration will always be speculative.

There are two responses to Turner's challenge. One is whether his challenge matters, and the other looks to the history of science, and suggests that this is an empirical problem that may well be solved on a case by case basis.

5.2.3 Answering Turner's Challenge

Events ramify: they have consequences in the world. Turner argues that some of the consequences may well disperse beyond recovery. However, the fact that consequences may disperse beyond recovery implies that they may not have had consequences that mattered for a narrative.

Take Caesar's crossing of the Rubicon. In a narrative of Roman history, this is an event that clearly played a role: it had consequences that structured history. However, whether Caesar shaved the morning he crossed the Rubicon doesn't structure the subsequent narrative in any significant way. It might be a historical curiosity, but it does not impact upon subsequent events.

While a trivial example, the point to grasp is this: if an event structures history in important ways, then it would have many consequences. Dinosaur colouration may well be a similar example. While a curiosity, dinosaur colouration played no significant role in structuring history. If it did, then it would have consequences.

Another answer to Turner's concerns is that some processes and past events leave obscure and rather opaque traces. The contents of old pottery vessels was once a subject of speculation, but recent advances have allowed tools of molecular biology to be deployed to determine the typical contents of such vessels. The long sticky lipids of various foodstuffs are identifiable genetically, so we can now determine whether a container was used to store milk or wheat (Jones 2002). Although in its infancy, recent work on starch in sediments is suggesting that they may be distinctive enough to inform us about organic material long since decayed. Paleo-molecular biology, where traces of sediments are analysed for traces of DNA, again, while controversial, may also be informative. The very sediments of archaeological sites add information to our picture of the past.

Information may not be irretrievably lost after all. Part of this is clearly how science develops into the future. Should scientists work out that the colouration of modern organisms is shaped by pigments of a certain type that do not readily decay, or decay in predictable and distinctive ways, there may yet be sources of information on dinosaur colouration. This is not that far fetched. Biogeochemistry, the study of distinct biological markers in sediments, is providing good information about the history of early life. This work utilises the molecular fossils: lipids, pigments and other molecules of organic origins. The claim that these molecules have organic origins is given credibility by our understanding of modern biochemistry. Again, this is deployment of a contemporary knowledge. We have confidence in these claims about the past due to our understanding of contemporary biochemistry.

So, the second reply to Turner's scepticism is simply to point at the history of science, and the increasing development of scientific knowledge. As our understanding of the world increases, so does our access to pieces of the past that were thought inaccessible. Dinosaur colouration and other "imponderables" may yet be on the list. The preservation of organic molecules, coupled with additional lines of evidence from the dinosaur's avian descendents may yet reveal clues about dinosaur pigmentation. Aviezer Tucker notes:

> The extent to which certain properties of events tend to preserve more information than others is not a philosophical, but an empirical question. Some processes tend to preserve in their end states information from their initial state more than others. (Tucker 2004 p106)

While Turner can provide examples of irretrievable information, this is a problem that needs to be assessed on a case-by-case basis. Turner in effect admits this. In discussing the empirical equivalence between rival hypotheses about dinosaur colouration, Turner says:

> We have good reason to think this because we know that information about coloration is destroyed by the fossilization process. Our background theories of taphonomy tell us that we will never find any historical traces that render either of these hypotheses more probable than the other (Turner 2005 p217)

I have simply pointed out here that our background theories of taphonomy and other processes of signal decay, may well change. The shift here is from emphasising the epistemic problems of particular cases in the past, and instead, emphasising the epistemic prospects of science as a whole.

I think local underdetermination problems are not as pervasive as Turner argues, and do not undermine the utility of Cleland's machinery, nor do they render the historical sciences challenged in importantly different ways than other sciences. There are cases where the forensic problems of the experimental sciences are likely to be equally intractable. The temporal location of the experimental sciences emphasises that much progress in science has been made through the development of better analytic tools that allow for ever more detailed observations of results. Increases in the sophistication and resolution of observational and recording equipment are a response to local underdetermination even in circumstances where the experimenter is in control.

However, the problem of local underdetermination is a real one. There just might be cases where we have no real way of knowing which of two hypotheses is correct. The problem is simply that while the logic of historical investigation is rational, and can yield reliable information about the past, it is an ongoing practical problem to identify the relevant evidence. And on occasions, the evidence we would like is destroyed. It's the evidence and its link to the past process of interest that's the problem, not any particular method of the historical sciences. I also think that this problem is shared by the experimental sciences and future directed sciences.

We will return to the problems that Turner raises in chapter 11, where we look at changes in human belief systems. If Turner's concern about the lack of evidence of prior processes crops up anywhere, it is seemingly with these processes of minds and beliefs. Nevertheless, there is no in-principle concern: underdetermination is a pragmatic issue to be addressed on a case-by-case basis.

5.3 Summary

What I have argued for in this chapter is for a more unified picture of confirmation in the sciences, where we take their temporal location seriously. The sciences as a whole experiment, observe, and extrapolate from known data and this observation and experimentation feeds into a general understanding of the world and its causal structure. This understanding of causal relationships, and the background theories the sciences in general use, is deployed to make statements about temporally inaccessible parts of the world, and localised phenomena.

Consequently, the epistemic challenges faced by the historical sciences outlined at the beginning of this thesis are less threatening. The historical sciences research regularities, so they can test regularities through repetition of observations and experimentation. They have tools available to cope with the problem of temporal and physical scale. The problem of the unobservable past remains a difficulty, but it is one that is shared by the engineering sciences, who want to make claims about the unobservable future.

The best way to understand the historical sciences is to see them deploying well-understood regularities, causal types, to understand particular tokens. This is why introductions to the historical sciences frequently use models of change and processes that are general, and broadly applicable across multiple instances. The tools required to make claims about the past are the same as the tools required to make predictions about the future. We need to understand how one fact relates to another fact, regardless of what point in a causal chain we are examining, its end or its beginning. And this is the project of all the sciences.

5.4 Where to from here?

While I think that in theory the historical sciences can access the tools of the experimental sciences, and this means that the epistemic difficulties of the historical sciences are the epistemic difficulties of the sciences as a whole, in practice the empirical problems faced by the historical sciences may well differ from case to case. In dealing with Turner's criticisms, it became clear the problems of evidence decaying depends on the science, and the historical target. The question then becomes: Are there problems within particular domains of the historical sciences that come with particular epistemic difficulties?

The rest of this thesis seeks to work through particularly difficult cases in the historical sciences. In so doing it will achieve a number of things. It will, through the example of various aspects of scientific practice, provide the skeleton view of the historical sciences I have discussed thus far with the sinews and flesh of actual practice. We will be in a much better position to see how various branches of the historical sciences utilise the body of scientific knowledge for historical purposes in looking closer at how they work, and where the controversies remain.

These analyses will also show the regularities the historical sciences use, and how these regularities are refined to account for particular histories. Historical scientists are not passive users of scientific knowledge. Historical scientists are active contributors, whose work tests, refines, and expands our understanding of the world, its history and the way it works. Part of what the second part of this thesis examines then is just how this process of contribution works, and how the historical sciences utilise that background information.

Take the problem of temporal and physical scale faced by geology. Some historical processes take longer than the span of the existence of humans, let alone the lifetime of an epistemic community. Such processes seem to be beyond the scope of any form of observation, repetition or other aspects of the experimental method. Is it possible, nonetheless, to fit the problem of scale into the conception of the sciences that I have set out? Are there means by which we can generate suitable theories, regularities and models, test these in such a way that they can be readily incorporated into our body of scientific knowledge, and then scale them up in suitable ways for application in historical cases? This problem will be dealt with in the next chapter, chapter 6.

Chapter 7 will be a case study in how changing theories change our view of the past. We will look at the emergence of the primates, and how contemporary understanding of modern organisms shape our understanding of their evolutionary history.

Chapters 8 and 9 build on what we have developed, and show how we can go from an understanding of individual processes, to constructing a causal narrative. Chapter 10 will examine a case where the narrative process has gone wrong.

Another important problem for the historical sciences is past human agency. Our theories of psychology are incomplete at best. This incompleteness, coupled with the fact that behaviours do not preserve, makes claims about the beliefs of humans of the past, who leave no texts, but only enigmatic objects particularly fraught As long ago as the 1930s, Christopher Hawkes suggested that particular types of behaviour, particularly those guided by religious beliefs, were more difficult to assess than others (Hawkes 1954). Might Turner's concerns about local underdetermination be particularly salient in these cases? We will deal with this question in chapter 11.

6. Uniformitarianism as methodology

In the previous two chapters, we have examined the role of scientific knowledge in the confirmatory project of this historical sciences. We confirmed representations of processes are needed in a number of ways. Our background knowledge limits the space of hypotheses. Our understanding of how processes ramify suggests possible traces that can act as evidence for or against hypotheses. And "accounting claims" allow us to understand traces as evidence of past processes.

We have confidence in the representations of processes we use in the historical sciences because they are part of a body of well-confirmed scientific knowledge. However, there is a potential threat to that confidence, because the representations of processes we use are only confirmed in contemporary settings. What if the past is irreconcilably different that the present in important ways? The past could be different in two ways. Firstly, there is the problem of scale mentioned in the introductory chapter. There may be processes in the past that are quantitatively unlike those in the present. So, our models have to be able to accommodate temporal and physical scale of a sort that we may not be in a position to test. This is a methodological challenge to our use of models derived from contemporary observations.

The second way the past could be different is that there may be processes in the past that are qualitatively unlike those of the present. This is a more substantive challenge.

The issues of scale, and the thought that there may have been processes in the past unlike those in the present, have been particularly relevant to geology. Geologists deal with large formations and long time scales. For instance, the movement of continents is a process of millennia, and on a massive scale. So, the methodological challenge is important to geologists who want to understand the geological past.

Historically, the issue of events unlike those of the present was also important. The geological record seems to show sudden changes, and catastrophic events quite unlike the gradual changes we see around us now. During the early history of geology, geologists frequently posited catastrophes to explain these dislocations in the geological record.

This chapter is consequently going to focus on geology, and geology's reaction to these problems. In the first section of this chapter, we will provide a brief outline of the approach of the 19th century geologist Charles Lyell. Lyell advocated that observations in the present could provide insights into processes in the past: an approach that became known within geology as the uniformitarianism assumption. The section on Lyell's uniformitarianism brings into focus an important question: Can we understand a potentially very different past, only using theories derived from observations in the present?

The second section of this chapter will focus on issues of scale, and show the role of methodological uniformitarianism in constructing hypothesises of processes with scales and energies beyond that of what we can observe. We will examine methodological uniformitarianism by working through the example of evidence for ice ages, and how a model of processes built upon observations in the present can be accommodate issues of scale.

The third section will argue that contemporary observations can accommodate some forms of substantive differences between the past and the present. Not all: If the past really is different than the present, then we cannot utilise our understanding of contemporary processes and regularities. However, this is the standard problem of induction. Grue problems in the past are just as difficult as Grue problems anywhere else in the sciences. However, I argue that there are strategies available to the historical sciences: strategies that again rely upon contemporary observations.

The primary aim of this chapter is then to show how we can leverage our observations of processes in the present to understand the past, and to examine the assumptions that underlie such leverage.

6.1 Geology and Uniformitarianism

Uniformitarianism as a term was somewhat derogatorily applied to the work of Charles Lyell by his critic, William Whewell (Rudwick 1972 p188). The subtitle to Lyell’s three volume "Principles of Geology" sums up his methodology and what Whewell found so offensive; Lyell's work is an attempt…

> …to explain the former changes of the earth's surface by reference to causes now in operation. (Lyell quoted in Simpson 1975 p262)

In this regard, Lyell was a pioneer of marshalling careful arguments about the past, based on detailed observations of the present, and he is probably rightly regarded as the father of modern geology for this reason. He is also engaging in precisely the methodology I argued for in chapters 4 and 5: Lyell uses his understanding of regularities he observed to make sense of the past.

Lyell's uniformitarianism however can be broken down into two distinct claims. On the one hand, it is *methodological*, in that observations in the present are utilised to understand the past. This is the epistemic part of uniformitarianism. On the other hand, Lyell also argued for a substantive uniformitarianism: processes in the past operated with the same rate, and with the same energies, as they do now. So the past not only had processes in common with the present, those processes operated at the same rate as in the present (Gould 1965).

6.1.1 Lyell's uniformitarianism

According to Lyell, precisely the same geological processes, with the same rates and energies, were operative in the past. There was no room for miracles, catastrophes, or any other processes other than the gradual changes we see around us now. For Lyell, the substantive claim and the methodological claim were intertwined. If the history of the world contains nothing different than what is observable now, and it changes at the same rate and with the same energies as it has changed in the past, then the present really is a good guide to the past. The constancy of the world and its processes of change allow us to understand the past based on current observations. The core of Lyell's argumentative strategy according to Martin Rudwick is that:

> … the geological "causes" or processes observable in the present day are, he asserts, accurately representative of those that have acted in the past, not only in *kind* but also in *degree*. (Rudwick 1970 p7)(Emphasis added)

Thus, the claim of historical uniformity by Lyell —that the past has exactly the same forces and processes operative as we see around us now— comes with some substantive commitments. We do not, and indeed should not, posit historical catastrophes such as divinely inspired floods, or even naturally occurring historical catastrophes, as explanations for what we observe around us. The substantive uniformitarianism of Lyell is in effect a "no miracles, no surprises" claim about the past.

Lyell's coupling of methodological uniformitarianism and substantive uniformitarianism raises an important point, and one that we must deal with if we are to argue for the role of observations of contemporary processes being reliable analogues of past processes. The concern is simply this: does one have to be a substantive uniformitarianism to be a methodological uniformitarian? After all, if the past really is unlike the present for some reason, with different rates of change, and different energies available for changes, might not that raise concerns about methodological uniformitarianism? Might not these different energies, and different rates produce different results? Might not the present be a misleading guide to the past?

6.1.2 Geology's Explanatory Problem

In the nineteenth century, some certainly thought that substantive uniformitarianism, and potentially methodological uniformitarianism, was a suspect doctrine. Georges Cuvier and others saw the marked changes in the geological record, with distinct changes in fossil flora and fauna, as crucial explanatory targets for geological science. Cuvier and other geologists of the time also saw other geological features such as the boulder clays we now associate with ice ages, as the results of processes unlike those occurring now. The substantive uniformitarianism of Lyell suggested that the past should show a continuous, gradual change at rates comparable to the processes we see in operation around us now. One could literally measure the rate of erosion now, and use this as a guide to rates of change in the past. Lyell's uniformitarian assumption in a sense predicted that the geological record would show the results of gradual processes operating over long timeframes.

Cuvier, however, saw that the "thread of nature" had been broken at certain points…

> When [Cuvier] spoke of the thread of nature's operations being broken, and when he asserted that the processes of erosion, deposition, etc. that we see operating today were incapable of accounting for what we actually see around us (such as the boulder-clay), he was making a substantive claim — that things in the past were not the same as they are today, and that past geological processes were also different— certainly in degree and perhaps even in kind. There was a lack of uniformity in nature's operations. This was …a substantive claim. But it had methodological implications. If nature was radically different in the past from the present, how could the geologist use his knowledge of present day processes to understand the earth's past? (Oldroyd 1996 p133-134)

Cuvier and other catastrophists could in effect point to the geological record, and argue for sudden changes. What they saw were the results of processes unlike those now in operation.

Catastrophists such as Cuvier even had some independent theoretical grounds for believing this might well be the case. Physicists argued that the earth was cooling, and in the past was substantially hotter than it is today, providing more energy for geological activity (Rudwick 1972; Gribbin 2002). In effect, observable rates of change, and observable processes could not simply be extended back in time in an orderly manner, and account for the past. The evidence suggested that there were disruptions to an orderly unfolding of events, and background theory suggested there might be different amounts of energy available to drive physical processes in the past.

One can see Lyell's problem. His substantive uniformitarianism committed him to a gradualist view, with slow rates of change. Lyell was forced to argue that the geological record was incomplete, and contained gaps, and so that the appearance of sudden changes were in fact artefacts of an incomplete geological record.

Substantive uniformitarianism seems to come with a commitment to rates of change, and processes of change, comparable to the changes we see around us now. This commitment is potentially problematic, as it seems to imply an orderly gradualism. This is not a trivial issue for the historical sciences in general. Evolutionary biologists who have taken gradualism seriously have been forced to argue in a similar manner as Lyell about changes in the fossil record. The apparent 'jumps' in biological forms evident in the record are, so its been argued, artefacts of an incomplete record, and not the result of special processes.

The idea that processes in the past might not be the same as, or may have different energies from, the present, is still a live view in some circles (Hooykaas 1975). Modern theories of the early physics of the Big Bang for instance posit different physics for the early universe. The energy available for processes matters, and in some cases this energy has changed over time.

The key problem for methodological uniformitarianism is making claims about situations in the past when the energies are vastly different, or are accumulated over long periods of time such as the case of ice ages. Can we be non-uniformitarian about the past —i.e., accept different rates of change and different energies for processes— and yet still be methodological uniformitarians? To see how geologists deal with the methodological issue, we will work through an example that confronted the geologists of the 19th century: that of the evidence for Ice Ages. The substantive problem will be dealt with in the subsequent section.

6.2 The problem of Ice Ages

A key problem, for substantive uniformitarians, and thus a key example for us, was evidence we now know to be associated with ice ages. Large boulders and other material were observed well away from their source substrate.

> These 'erratic blocks' of rock could be traced from their source areas across tens or hundreds of miles, and were often perched on hill-tops or otherwise unrelated to the present drainage system. Some were the size of small houses, and could not have been moved by "causes now in operation" around them, no matter how lengthy the time allowed. (Rudwick 1969 p139)

For some catastrophists, this had been interpreted as the result of historic floods. The evidence for something different about the past went even further. "Diluvial" clays —the boulder clays mentioned in the quote from Oldroyd above— and gravels un-associated with current river systems, were also associated with these boulders. What's more, in these gravels and clays were the remains of animals long extinct (Rudwick 1969).

So, we have some contemporary observations of the results of some unknown process: large displaced boulders, 'diluvial' gravels and extinct organisms. Seemingly, no current processes seem to have effects on a comparable scale. Given these erratics, these diluvial gravels and clays, and what appear to be rather different energies available for transporting boulders, how then can we be methodological uniformitarians? The past *does* seem to have been different.

In fact, most modern geologists accept that a strong version of substantive uniformitarianism is a flawed doctrine for this very reason, even while the majority accept the utility of methodological uniformitarianism. So how do geologists reconcile the two positions? How does one approach accounting for a complex past, one potentially very different than the present, using only what one knows of the present, given that past mechanisms may not resemble extant ones?

To explore how this problem is dealt with, we will look at ice ages, and how a model of those ages is constructed based upon contemporary observations.

6.2.1 Understanding Ice Ages

The maintenance of glacier in the face of sources of heat we can see. We can measure snowfalls, calibrate ice deposition, and how a contemporary glacier persists despite the summer months. We can also see that they might have been bigger should it be cooler with less summer melting, or smaller given higher temperatures and more melt. We have a potential mechanism for changes in glacier size. Where the accumulation of snow is more rapid than the melting, we can comfortably project that a glacier will increase in size. In fact, we can even, through precise measurement, observe the changes in glacier size over summer and winter months, and even over longer periods of warmer and cooler years.

The crucial point of all this is that our observations are not static. We have access to diachronic information: a short timescale history of changes in a glacier. This short timescale assists us in securing our underlying causal mechanism. Our inference is not based on a single observation, but a series, correlated with a variable; in this case, temperature changes. This is a crucial point for what follows, and is worth restating: Contemporary observations can record a process over time, and can be correlated with a causal variable. We can 'plug into' our model of glaciers the values of variables from a temporal sequence of measurements.

Beyond this crucial observation, we have to have other sources of evidence, for the earth is as we observe it currently is *not* growing glaciers of the scale required (and may be contracting). The balance of temperatures over seasons and decades is such that glaciers are comparatively stable in size. We need further evidence that glaciers in the past were substantially bigger than they are now. We require additional lines of evidence.

From observable glaciers, we can also see the glacial moraines, the geological debris of rocks and sediments, associated with their "growth" in the landscape, and can note that this geological material is some way from its current location. We may even be in a position to determine the location of the source material of the moraines, by comparing the moraines composition with substrate and geological material further up a glacial valley. Again, we have a mechanism; a process that is observable that produces certain geological by-products. These by-products are not on the same scale as the diluvial clays and erratic boulders that ice ages have produced, but they are by-products of the seasonal melts and snow deposition.

Understanding these mechanisms through contemporary observations gives us a model of glacier activity. Such a model makes additional predictions over short time scales. A particularly cold year would predict that the glacier would increase in size. A particularly warm year that the glacier would decrease in size, and leave evidence in the form of eroded boulders, "erratics" and silt from the grinding process of glacial growth.

With all this observable evidence at hand, we can then construct a mechanistic model of glacial processes *and* its by-products, and we can have confidence in it as a contemporary process.

With this model in mind, should we then find similar moraines, erratics and other glacial detritus in the landscape some distance from a glacier, we can infer that a similar *process* has been operative to build the moraine, even if the *energy* involved has been different. Because our model of glacial activity includes a variable, temperature, that would allow the glacier to become substantially larger, we might have to presume a temperature change. Nevertheless, our model of glacial activity can accommodate this variable. Ice ages are not just big glaciers. Extensive glaciation of the sort necessary to move erratic boulders of house size, and to produce the alluvial clays, is rather different than seasonal contractions and expansions. But the very fact that they are so extensive makes additional predictions. At this point Cleland's machinery comes into play.

A hypothesis of glacial changes of the magnitude required for extensive glaciations makes additional predictions about life forms, sea levels and consequent changes in bio-geographic regions due to land bridges and glacial barriers, relative Oxygen isotopes in sea organisms and so forth. We have potential additional lines of evidence for the extent of the glaciation. Again, some of these predictions can be tested under laboratory conditions, or checked by contemporary observations. Should these predictions match up to the evidence, then moraines and landscapes formed by glacial action long past get an explanation; the actions of a historical glacier formed during an ice age. Despite not witnessing an ice age, nor being likely to, we can infer an event we cannot see, from evidence we can.

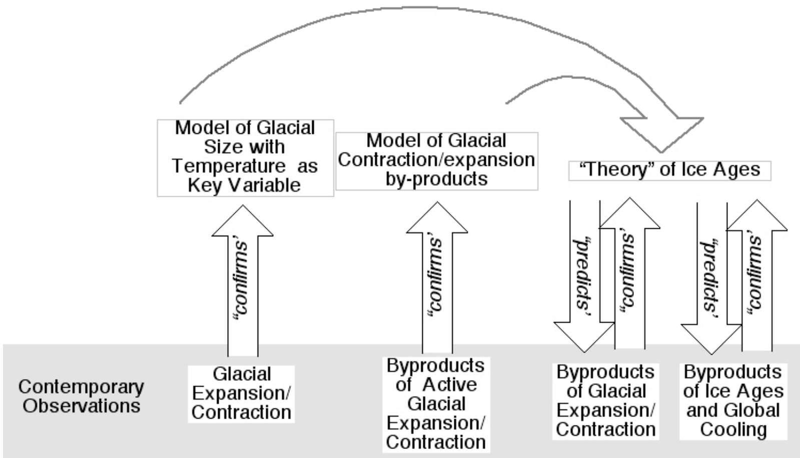

Figure 6-1 Along the bottom of the graphic is the range of contemporary observations available. This includes the synchronic information, such as the various by-products of glaciation, and short-term diachronic information, such as changes to contemporary glaciers due to temperature changes. This contemporary information can then be used to make claims about past glaciation events on a new scale, explaining further by-products un-associated with current glaciers. It also makes further predictions, including predictions about flora and fauna, sea-level changes and other phenomena.

We might not, at least initially, have an explanation for the changes in energy required to get the glaciers of the requisite size. We have, in some sense, pushed the explanatory burden back, away from the immediate problem of a landscape form, to a new problem of ice ages and why they occur. This problem is still under investigation, although changes in earth's orbit and tilt are probably the main causes. Nevertheless, no one doubts the *events* of ice ages occurred, even if the mechanics of the temperature changes required for ice ages are still open to debate.

The glacial theory, and the requisite ice ages, while dramatically different than anything we witness now, can still be inferred from evidence and processes we have access to now. We have what we might think of as "static" evidence of associations of moraines, with current glaciers, and evidence that look like moraines un-associated with contemporary glaciers. These traces are our explanatory target. Then we have the dynamic diachronic evidence of changes in glacier size over human scales, reacting to different temperatures. We scale the dynamic evidence over time to account for the static explanatory target.

6.2.2 Solving the problem of scale

Despite the fact that modern science has never seen an ice age, we can reliably infer that they have occurred. We can do this because we can observe their constituent processes, and provide some support for a model of ice ages that accounts for extreme changes.

To see this, lets run through the evidence, the hypotheses, and then the further predictions of the glacial theory.

Explanatory Targets (Explanandum):

The initial explanatory targets are transported boulders, alluvial gravels, boulder clays, and extinct species found in deposits associated with these deposits. Note here that the "evidence" requires the investigator to be able to say confidently that a particular boulder is some distance away from its source substrate, and that alluvial deposits and clays are the results of erosion activities of a certain type. This leads us inexorably to observations of processes, not just static evidence.

Methodological uniformitarianism (Actualistic research)

Expansion and contraction of glaciers over various periods as the result of changing temperature patterns provide a model with a key variable: temperature. By understanding these contemporary processes in detail, and understanding how changes in temperature play a role in the observable expansion and contraction of glaciers, we can get an informal model or analogue for past processes. We get access to an underlying mechanism that could act as the key variable for changes in glacial scale.

Additionally, close attention to glacial actions reveals that current glacial expansion and contractions have observable downstream consequences: the small-scale moraines and other geological features associated with contemporary glaciers. This provides us with consequences of the process to look for.

Hypothesis

Utilising contemporary observations, we generate a model of glacial contraction and expansion, with key variables being isolated. In this case, the key variable in our model is temperature. We can then use this model as a hypothesis about the previous extent of ice ages. We in effect scale up the contemporary processes via this variable. This hypothesis, scaled up to account for the initial explanatory target of erratics and geological features un-associated with current glaciers, also makes predictions about additional evidence.

Predictions

Cleland's machinery comes into play at this point. The hypothesised event in the past, coupled with our background understanding of the way the world works, means that we should see *additional* physical evidence over and above that of the initial explanatory target. In the case of ice ages, this additional evidence should provide confirmation of the scale of the event, something we cannot witness in the contemporary situation. For ice ages, this is amongst other things; global cooling with changes in fossilised fauna and flora distribution, changes in sea levels with consequent land bridges and faunal interchanges, and so forth. Consequently, contemporary observations unrelated to the initial target become evidence for the hypothesis. This provides a guarantee against ad hoc hypotheses, as this additional evidence is a *prediction* of the hypothesis, rather than the target that the hypothesis is conceived to explain.

The important lesson from this is that we can be methodological uniformitarians. We can use observations in the present to construct models of processes, and then apply these models to past situations. The key is often to understand the variables that matter. In the ice age case, the variable is obviously temperature. Changes in this variable allow us to scale the glaciation model in the right kind of ways.

The confirmation for our hypothesis comes in two forms. The model itself gets confirmation from observations in the present. These observations make predictions of glacial changes due to temperature changes that we can observe over short time scales. We can also observe the results of these changes in the form of by-products of the process. The scale of the ice ages gets confirmation through the additional predictions it makes. We can utilise Cleland's machinery, and isolate different lines of evidence. In effect, uniformitarian methodology confirms the model; Cleland's machinery confirms the historical scale.

In the ice ages case, it seems reasonably plausible that we can scale current processes. All that is required is that we can isolate the right variable too scale, and that that a scaled version of the model makes novel predictions. This process of scaling is used constantly in the historical sciences.

However, what if the past really is different? What if there really are substantively different processes operative in the past? It is to this problem that we now turn.

6.3 A Different Past

Not all problems of sudden and drastic changes in the past are so straightforwardly scaled versions of contemporary processes. There really are catastrophes. Take something like a comet strike, with global consequences such as the extinction of the dinosaurs. This is a classic case where we have sudden changes, and events of a sort we are not in a position to observe now. The scale of the effects on the history of life is such that the energy and its impact on matter are not something we have witnessed. Human beings at least have not witnessed a catastrophic extinction of this sort, nor its subsequent effects. There is no doubt that within biology such an event represents anything but gradualism, and it is not simply a matter of scaling up gradualist processes.

Within the domain of cosmology, comets really are a standard event. We have even seen comets, and even witnessed them (notably Shoemaker-Levy 9 impacting Jupiter in 1994) (Alvarez 1998 p145-146). So one way to view historical events such as a comet strike is by changing the scale at which we view such an event. While a comet strike is a catastrophe for an evolutionary biologist attempting to account for the history of life, it is a standard process at the time scale of cosmology. For a cosmologist, the process of accumulation of matter in gravity wells over millennia can even be considered a gradual event.

In such cases, the issue of scale is a problem of picking the right models to work with. Unlike the glacier case, we do not scale a model; we look for a catastrophe from a different science.

6.3.1 Witnessing History

A key piece of the uniformitarian methodology is the link between a series of observations and a causal variable. The uniformitarian methodology is not a process where we try to infer the starting positions of balls on a billiard table from their final resting positions. We do not have a static picture of the end positions once movement is completed; our observations are diachronic, and consequently we can reconstruct the trajectories of the subjects of interest. With a series of observations of the ball's movements through time, we can determine the trajectories of the billiard balls.

Its this very process of reconstructing past trajectories that lead physicists to think that the formation of the universe was the result of a big bang. However, this reconstruction of trajectories also implied different physics for the universe in its early stages. There were the inevitable consequences of a convergent past. Utilising their current understandings of physics, and predicting the state of the early universe, physicists come to conclusions about a substantively different physical past.

This cosmological example is in some ways a toy example in a billiard ball universe, and it clearly glosses over a great number of complications. Clearly, there are two constraints on how good such a prediction of a substantively different history can be: the accuracy of current observations, and the physical theory that allows one to derive the necessary trajectories. However, these two constraints —accuracy of observations and adequacy of theories— are general problems in the sciences, and are not unique to the historical sciences. The point to get from this example is simply that despite the historical aspect of the enquiry, it shares the same problems of the sciences generally.

It also suggests that despite there being the very real prospect of a substantively different past, we can still use the present. Should our observations of the present and our theories about the present be good enough, they will work for the past. They do this by making predictions about a substantively different past. The cosmological example does just that. Theories that work in the present, coupled with diachronic observations that provide a trajectory into the past predict that substantive differences. The test for these theories can remain the present. Methodological uniformitarianism, utilising observations in the present, coupled with good theories about how the world works, will allow us to unravel history, no matter how odd.

It seems plausible that we could know nothing about a genuinely metaphysically different past, for we would have no way of linking our observations in the present with the past. Grue problems in the past are just as problematic as grue problems in the future, for changes in the processes and properties of matter outside our temporal access, unless predicted by our theories such as the big bang case, have no links to our observations, and no good models to account for them. There is then an underlying assumption about the uniformity of nature in its fundamentals. There may well be unique configurations of processes in the past. But those processes themselves are accessible through observations in the present.

6.4 Summary

This chapter has discussed uniformitarianism in its various guises. It has been a simplistic discussion in some ways. Some authors think that uniformitarianism is much more complex than substantive and methodological (See for instance Hooykaas 1975). I have also kept the label methodological uniformitarianism throughout this discussion, and some would label active research such as that outlined here as actualist research; the research of current processes in order to gain insight into past processes.

However, I have shown that despite potential difficulties physical and temporal scale pose, and the possibility of novel processes in the past, there are means to understand, and make sense of these processes. The key is observations of contemporary processes that are temporal, and to understand the relevant variable that causes the change. In the Ice age case, the relevant variable was temperature. Monitoring of small temperature changes in contemporary settings provided the key variable that could be scaled to predict large changes.

We may also be able to use observations in the present to predict a very different past, so long as our theories are good enough, and if we have good observations of present trajectories that we can extend back in time. We can derive an understanding of past processes by a combination of contemporary observations of changes, and good theories about the consequences of those changes.

However, all this assumes a substantive uniformitarianism. It is not the substantive uniformitarianism of Lyell, where we assume that the energies available for processes are similar. The uniformitarian assumption in its modern form assumes that the regularities we see now are constant, even if the energies and scale of those regularities can be very different in the past. But this assumption points to a potential limit to the historical sciences. If the universe changes the rules from one moment to the next, this assumption is wrong and we will misunderstand the past. However, the assumption that the universe is uniform, and nature plays by constant rules, is an assumption that all the sciences share, and not one that is unique to the historical sciences.

7. A case study in changing background theory

In the previous chapters, I have argued that background theories, well-understood regularities and models, play a crucial role in determining how we interpret and understand the past. This set of theories about the way the world works comes from a pool of generally accepted ideas within the sciences broadly conceived. Some of these regularities gain acceptance through being testable independently of the historical sciences. Some gain acceptance by virtue of being part of a consensus about the way the world works.

This pool of ideas also secures the historical inference from any particular piece of evidence to its role in some larger account of the past. We understand evidence *as* evidence of the past because we understand how processes have ramifications. Theories about the world act as accounting claims, allowing us to use traces as evidence.

In order to demonstrate how this process works in practice, this chapter is going to work through an example: the evolution of primates and the 'primate adaptive suite.' The explanatory problem in this instance, the explanandum for this historical process, is contemporary evidence of two sorts; fossil evidence of presumed primate ancestors on the one hand, and modern primates on the other. Essentially, we are looking for a historical explanation for the common traits of a particular family of organisms: the primates.

This chapter will proceed through a roughly chronological account of changes in theories about primate origins and the emergence of the primate adaptive suite. Along the way, we will see how changes in interpretations are informed by our contemporary understanding of the world. Observations of contemporary processes and systems, in this case biological ones in the form of living primates and other organisms, inform our understanding of past systems and primate origins. The changes in interpretation are the result of changes in our understanding of the world around us. Sometimes, changes in theory actually change the explanatory target. An important change in our understanding of the primates was a shift in our understanding of primate phylogeny. Theory and new lines of evidence can change the explanatory target, just as much as the explanation itself changes.

Changes in the general pool of scientific knowledge have shifted the debate about the emergence of primates and in ways not wholly dependent upon the physical evidence. In fact, in the case I am going to outline here, the changes in interpretation could just as well be driven by theory alone, without any new physical evidence appearing whatsoever. Changes in our understanding of the contemporary world drive our understanding of the past as much as, if not more than, new physical evidence.

In the first section, we will look at views on the origins of primates prior to the 1950s. The early anatomists we will discuss had simple a simple version of the evolution of primates, based largely on anatomy, and a simple view of the evolution of the primate lineage. Complexity only started emerging post 1950s, with the advent of Cladistics, and increasing information from primatologists working in the field. This sparked new debates about primate origins that we will discuss in the subsequent section.

In the third section, I will look at how changes in theory have changed the lines of evidence available to primatologists. In the final section, I draw some general lessons from this case study.

7.1 The evolution of the Primate adaptive suite

Primates are particularly interesting to anthropologists and those looking at human evolution as they represent the common evolutionary heritage we share with chimpanzees and other primates. In building an evolutionary history for humans, we want to be in a position to say what humans evolved from, as this constrains possible trajectories for evolution. The evolution of modern humans has been a process of natural selection and other factors modifying and building upon primate traits, so part of the task of explaining the evolution of *Homo sapiens* includes specifying the ancestors of the primates.[15]

Thus, many accounts of the evolution of *Homo sapiens* start from

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[15] In some cases, particularly in the late 19th early 20th century, the primate adaptive suite was seen as something of a pre-adaptation for various traits of *Homo sapiens*.

assumptions about a last common ancestor for hominids. Such assumptions provide us with the starting point for an evolutionary trajectory: the beginnings of a narrative for the evolution of humans.

This case study looks at debates and claims about the emergence of primates and the adaptations that we think of as the basal primate adaptations. What we are interested in here is an understanding of the common ancestor of the primates. At this stage, we are not interested in the complete narrative of primate evolution; rather, we are going to focus on how theories play a role in our understanding of the basal primate adaptive suite. Understanding the environment and early adaptations of the early primates may well provide some clues about potential trajectories for evolution within the primate lineage.

The questions of interest here are: What were the adaptive challenges that the early primates faced? How might those adaptations have shaped a basic primate cognitive and adaptive package? And finally, how has our view of the evolution of the primate lineage been shaped by changes in our theories? The target or subject of this account is the primate adaptive suite.

7.1.1 Changing views on the evolution of primates

The traditional view of the evolution of primates emerged post-Darwin in the late 19th and early 20th century, and was partly shaped by a view of primates as human ancestors. For those looking to understand the evolution of humans, there was a tendency to see the evolution of primates as a linear progression from a tree dwelling primate ancestor, to a ground dwelling larger primate, and thence to bi-pedal hominins. The aim of the explanatory game was to fill in the gaps between humans and the rest of the biological world, and to understand how the ancestral traits of the primates had been modified over time to become the traits of modern humans.

Wilfred Le Gros Clark took this idea of a single lineage, and worked up a "grand synthesis" of primate and human evolution that dominated textbooks until the 1960s (Cartmill 1992 p105). For much of the first half of the 20th century, the explanatory target for primatologists was not the fossil material per se, nor even the primates themselves; it was human ancestry. Consequently, this target shaped how the evidence available was to be interpreted.

Early 20th century work on human evolution was directed at the construction of a lineage and a chronicle for human evolution based on comparative anatomy.[16] The key line of evidence the comparative anatomists had to go on was the shared anatomical traits of primates and other organisms. Working from limited fossil material, and the skeletal material of extant primates, they built an evolutionary lineage for the primates, and understood the key features of primate anatomy with a stereotyped picture of primate behaviour in mind.

The assumption was that the suite of shared primate traits —forward facing eyes with optic convergence, enhanced vision, grasping extremities, claw loss, and brain enlargement— were arboreal adaptations. The enhanced visual processing, optic convergence, and so forth were requirements for climbing and the ability to engage in rapid locomotion through the trees (Cartmill 1992). On their view, the shared traits of the primates showed that the primates had evolved for to fill an arboreal niche. This arboreal niche shaped features that were to be crucial to later human evolution; grasping extremities, ocular convergence, and increased brain size.

At this point, we have a relatively simplistic view the origins of the primate adaptive suite and the associated cognitive traits, one built upon guesswork as to the requirements for arboreal locomotion, observations of the morphology of contemporary primates, and their presumed evolutionary relationships. The early anatomists were really making guesses, sometimes very sophisticated guesses, about the function of the shared morphological traits of the primates. The lines of evidence were the extant primates and their anatomy, and a general theory of relatedness among the primates.

The early comparative anatomists such as Le Gros Clark were also probably guilty of viewing primates as human ancestors, rather than seeing them as related species with a common ancestry. There are hints in some early work on human evolution of directionality: an inevitable evolutionary trajectory culminating in *Homo sapiens*. Early workers presumed there were crucial steps taken within the primate lineage towards the intelligence and sophisticated cognition of later hominins,

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[16] Along with Le Gros Clark, some of the key figures in this work were G. Elliot Smith and F. Wood Jones. An academic legacy of this period is the fact that primatology is still frequently taught as part of anthropology programs in universities.

and so they looked to a particular feature of primates, tree dwelling, as a reason for the emergence of traits that would be useful later in the lineage. They were in effect 'reading in' to the fossils and morphological features of modern primates, the sophisticated behaviours their theories about human evolution led them to expect.

Thus, for the comparative anatomists of the early 20th century, the theory that drove their interpretations was to some extent a linear view of human evolution, with a limited amount of fossil material and a crude view of primate behaviours and primate systematics as further evidence.

7.1.2 Systematics and the Primate Lineage

New data and changes in theory began to complicate the simple linear view of the primate lineage and the primate adaptive suite. The theoretical changes that forced a re-interpretation came from two areas of research: systematics and increasing amounts of data about modern species. By the mid 20th century, the evolutionary relationships between the extant primates that Le Gros Clark and others posited were increasingly out of phase with detailed analyses of phylogenetic relationships.

Le Gros Clark's evolutionary picture of the primate lineage viewed shared traits such as forward facing eyes as indicative of an evolutionary relationship. Modern systematics view shared traits as uninformative of detailed evolutionary relationships. What matters for modern systematics are shared *derived* traits, or synapomorphies. Modern analyses suggest that many of the extant species that served as the data for the early anatomists lineage building, were in fact distant side branches to the primate lineage that lead to humans. Some of the species put into a position as human ancestors, were not ancestors at all.

One way to view what happened during this period is that the explanatory target changed. From a simplistic linear view of evolution, increasingly sophisticated systematics and cladistic analyses of existing primates in the mid twentieth century showed a more complex set of evolutionary relationships. People became much more aware of the idea that the primates shared an ancestor, and that the extant primates themselves could not be neatly arranged into a lineage.

With the central subject of a primate narrative changed by theories about how existing primates are related, the historical analysis was forced to change in response. Changes in theories about evolutionary relationships shifted the explanatory target, redefining the primates as a group, and consequently changing observations that were relevant. It was no longer possible to see all the extant primates as part of a lineage that led to the Hominids, Hominins, nor *Homo Sapiens*. Reading adaptive precursors to human traits into organisms that were not ancestors of humans seemed implausible, and gave primate evolution as a whole a direction that it did not in fact have.

What happened during this period in primatology was that the background theories and evolutionary lineages changed. This had a number of flow-on effects. It overturned the idea that a number of species showed important adaptive precursors for later human evolution, as some of the supposed ancestors were no longer ancestors. It also changed how we could reconcile the shared traits of the primates. In a nice orderly lineage, one could presume an adaptive package for tree dwelling, which then gets modified for life on the ground, which then led to hominins, and finally, humans. Each stage in this lineage had an extant species that could provide clues to understand the behaviour, and act as a guide to interpreting the morphology of extant and fossil species. So, chimpanzees get used as a guide to understand the transition from an arboreal life way to a ground dwelling niche. This nice neat picture gets distinctly complex when we learn that chimpanzees are not our ancestors. They might share an ancestor with us, but we are not their descendents.

The upshot of all this, was that it forced primatologists and paleobiologists to look for the evolution of the primate adaptive suite as an evolutionary event in its own right, rather than as an adaptive precursor to human evolution. The subject for the historical narrative had shifted, and the justification for events in the narrative could no longer be its final conclusion, the emergence of the higher primates, and thence to humans. Each phase in the narrative needed its own justification.

Thus, by the middle of the 20th century, the explanatory target had changed. Not, as one might think, because of new physical evidence, but because of changes in background theories about how we should understand the world, and classify the relevant subjects of study. The shift to cladistic analysis in the middle of the 20th century provided new ways of interpreting the existing evidence, that of the contemporary primates, and saw the relationships between the primates differently. Theories about the world shifted, and the explanatory project shifted accordingly.

7.1.3 Changes in functional views of primate anatomy

The other pressure to re-interpret the primate lineage came from studies of contemporary organisms. The basis for the early 20th century view of primates was the inferred function of a shared morphology among presumed near relatives. This inferred function of various traits was sophisticated guesswork, rather than knowledge of how adaptations shaped anatomy of organisms. Primates typically lived in trees, and thus the traits that the primates shared were interpreted as adaptations for an arboreal life.

This started to change as increased amounts of fieldwork and comparative anatomy started to make its presence felt. For instance, the results of decades of fieldwork by those studying animal behaviour began to challenge the simplistic view that one of the key traits of primates — forward facing eyes— are an adaptation for tree climbing. The active study of related and unrelated contemporary taxa began to suggest to workers that forward facing eyes in primates were not solely adaptations for tree climbing per se. After all, various marsupials do well in trees with little or no optical convergence. Perhaps optic convergence in primates has more to do with either particular methods of locomotion or particular foraging strategies. Comparative studies across taxa were showing that there were alternative solutions to life in the trees than the primate adaptive suite.

That the primates shared a number of features wasn't in doubt, but the reason for why they shared the features was increasingly unclear. The shared traits of the primates could not easily be interpreted as just an arboreal adaptation. Lots arboreal species do not share the unique traits that primates have. Primatologists needed a new explanation for the unique traits of primates.

7.2 New Views on Primates

In this section, we are going to examine how changing new observations were generating new models of the initial adaptation of the primates. Increasing amounts of fieldwork were changing how we understand primates. This in turn provided new alternatives for interpreting the physical evidence. These alternatives can be seen as differing views on what are the correct accounting claims that justify an interpretation of physical traces.

By the middle of the 20th century, the central subject for primate evolution had shifted: it was no longer primates as human ancestors, but the common ancestor of the primates. The target for the primatologist's research was now the emergence of the primate lineage and an explanation of the common traits of the primates.

With increasing amounts of observations of contemporary species available, alternative views to that of the comparative anatomists quickly emerged. Initially we will discuss two of these: the theories of Frederick Szalay and Matt Cartmill. We will then look at a response to these views from Robert Sussman.

Szalay suggested that the grasping-leaping locomotion of early tree dwelling proto-primates drove the primate adaptive suite (Cartmill 1992). The locomotion pattern required that early primates could build a good three-dimensional picture of their world to enable them to gauge distances accurately for an acrobatic form of locomotion: this helped explain primate optical convergence, and why other arboreal species do not have forward facing eyes. Various marsupials for instance tend not to leap from branch to branch, but manoeuvre along branches to terminal feeding points. Szalay's model of the primate adaptive suite implies a visual suite and associated cognition adapted for a specific locomotor style, with implications about self-location, distance assessment and related cognitive requirements. The grasping hands and other features that identify the primates as unique among arboreal species were also a result of a particular locomotive style.

Szalay bases his view in part upon a basal primate ancestor bequeathing a trait to its descendents. For Szalay, homologies are important; the inheritance from a common ancestor that binds the group together.

Cartmill came to a different conclusion about the primate adaptive suite, but with different contemporary organisms as his model.

> Noting that marked optic convergence is also a characteristic of cats and many other predators that rely on vision in tracking and nabbing their prey, Cartmill sought the adaptive significance of this trait in the predatory habits of small prosimian primates like Microcebus, Loris, and Tarsius, which track insect prey by sight and seize them in their hands. (Cartmill 1992 p107)

Cartmill suggests that primates are descendents of an insectivorous ancestor, and he bases his assessment on the feeding habits of prosimians, the close relatives of the primates. On this account, we can suppose selection for a slightly different set of cognitive requirements, one more closely tied to tracking moving objects, and hand eye coordination, and we see a different adaptive role for grasping hands and optic convergence.

Regardless of who is right in these debates, what is interesting here is how the different theories not only change the interpretations of evidence; but also change what *counts* as evidence. The debate is in part over differing justifications for the link between the past and the observations.

Systematics, and the common environment of the basal primates inform Szalay's view. Modern primates inherit primitive traits from a common ancestor as "evolutionary baggage." For Szalay, evolutionary systematics is the chief background theory that he relies upon. This makes the *shared* traits of primates an important line of evidence for the reconstruction of the common ancestor of the primates.

Cartmill however, claims that by themselves, shared traits are uninformative. What are required are functional explanations of traits that see them as adaptive. Cartmill is relies upon similar adaptive problems having similar adaptive solutions across lineages.

> Adaptive explanations must be general enough to predict similar adaptations in other cases and they must be rejected if they are not borne out. (Cartmill 1992 p107-108)

So, for Cartmill, traits that have independently arisen in different lineages —homoplasies— are important. Cartmill relies on natural selection optimising traits, so his primary background theory is evolution by natural selection. This makes traits shared by *non*-primates an important line of evidence, allowing functional comparisons across lineages.

7.2.1 Prediction Failure

Cleland argues that we can choose between hypotheses about past processes and events by utilising additional downstream consequences other than the explanatory target. If we posit a process to explain evidence from the past, it should make predictions of additional ramifications that we can use as evidence to choose between hypotheses.

Such predictions of additional downstream consequences can of course come about with increased understanding of the world. As we learn more about the world, we may come to realise that certain processes have ramifications of which we were previously unaware.

We can see this utilisation of additional lines of evidence with one of the challenges to Cartmill's hypothesis. Cartmill's hypothesis makes predictions about what we should see among the primates. If the common ancestor of the primates was an arboreal insectivore, it predicts the primates should share certain features as primitive traits. In particular, among the existing primates, those closest to the ancestral state —those primates with the least number of novel traits— should not only have anatomical traits consistent with arboreal insectivory; they should also have *behavioural* traits consistent with the hypothesis.

However, this prediction doesn't quite match up with the evidence. Again, increasing research of extant primates and their near relatives show that their dietary patterns and their related adaptations do not match up with what we would expect to see with a insectivorous ancestor.

> It is true that many small-bodied mammals, including primates, eat insects. However, …most small nocturnal primates feed mainly on crawling insects, many of which are captured on the ground. (Sussman 1991 p212-213)

Not only are many of the primates not engaging in arboreal insectivory, they are primarily omnivorous, with a large proportion of leaves and fruits in their diet; a fact reflected in their digestive tracts (Sussman 1991 p213). And even the primates that do seem to rely on insectivory don't quite match up to the predictions of Cartmill's hypothesis.

> ….according to Cartmill the trend towards orbital convergence and approximation in primates culminate in the slow-moving Lorisinae. Eighty-five to ninety-five percent of the prey captured by the lorises are slow-moving and conspicuous, and are detected by a highly developed sense of smell.

So even the insectivorous primates do not rely on vision to capture prey, undermining the visual predation model of Cartmill.

Sussman also introduces a new strand of evidence: comparative neuro-anatomy. The hypothesis of an insectivorous visual predator suggests that there will be similarities in the neurological structures of predators across lineages.[17] However…

> The visual anatomy of cats and primates are not very similar, and auditory stimuli are generally more compelling to carnivores than are visual stimuli. (Sussman 1991 p213)

Cartmill's hypothesis predicts that the primates have anatomical and behavioural legacies that mark them out as descendents of a visual insectivorous predator ancestor. However, those predictions, those downstream consequences, are not found. While his hypothesis accounts for *some* features of the primates —the convergent eyes, the grasping extremities— it doesn't match up with expected consequences of such an ancestor of the primates. Using Cleland's machinery, we appear to have reason to think that Cartmill's hypothesis is at the very least, problematic, if not incorrect. Expected consequences of prior events are not present. We might also have reasons for thinking that his accounting claims that justified seeing certain traits as evidence are also suspect.

7.2.2 Ecology and Adaptive Niches

Sussman, while pointing out the difficulties with Cartmill's hypothesis, proposes an alternative. We can view this new contender as a novel hypothesis, with new ramifications, and hence, new lines of potential evidence. Assuming that new clades emerge in response to new adaptive opportunities, Sussman asks whether there was a new adaptive opportunity during the emergence of the primate clade in the early Eocene. He argues that there was just such an adaptive opportunity: The primate adaptive suite was an evolutionary response to the emergence of flowering plants, the angiosperms, in the early Eocene (Sussman 1991). Effectively, Sussman suggests that this change in the flora of the early Eocene opens up a new adaptive niche for primates, and other organisms to exploit.

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[17] We shall see in later chapters on the evolution of cognition that neurological evidence may well be suspect.

> The Paleocene-Eocene boundary was a period of rapid change that involved coincidental adaptive shifts in a number of plant and animal groups, including primates. It is in the context of the interrelationships between these groups that we might find an alternative hypothesis for the origin of primates. (Sussman 1991 p214)

This alternative hypothesis, one generated by background theories about how lineages co-evolve with other organisms, and in response to changing environments and changing opportunities, generates a different set of predictions about primates and their adaptive legacies.

> I believe that the uniqueness of the earliest primates of modern aspect involved a combination of the features described by both Cartmill and Szalay. I suggest that these early primates were omnivores, feeding on small-sized objects found in the terminal branches of trees. Thus the novel adaptive shift involved two aspects: 1) becoming well adapted to feed in the small branch milieu, and 2) including a high proportion of plant material in the diet. (Sussman 1991 p214)

Essentially, Sussman is arguing for a combination of the hypotheses of Szalay and Cartmill. One that makes sense of the evidence that counters Cartmill's hypothesis, but nevertheless accepts some of his lines of evidence, along with that of Szalay. Sussman's synthesis thus accommodates both the systematics of Szalay, and the adaptationism of Cartmill.

At this point, we need to draw some general lessons from this case study. We need to show how background theory has driven changes in the interpretation of the primate adaptive suite.

7.3 Lines of evidence

I have argued in previous chapters that we utilise contemporary observations of processes to give credence to our theories about how the world works. These theories are then deployed to understand the past. What we are seeing in these shifts in the debate over primate origins is two aspects of the role of contemporary observations and background theories, and how they relate to physical evidence. Firstly, changes in theory change how we interpret evidence. This is particularly true of the interpretations of the physiological evidence of both modern primates, and the fossils of presumed ancestors.

To understand the origin of primates, we need to make sense of the behaviours and adaptations that drive the morphological characters of a related group. However, good behavioural reconstructions do not arise de novo from skeletal material. The inference from morphological features to behaviour requires that we can either infer the behaviour from engineering principles —so something like the mechanics of a bipedal limb are distinctly different from the mechanics of a quadruped limb— or alternatively, we can use comparative anatomy from contemporary organisms with known behaviours. We require models of the relationships between behaviours and morphology. The more we understand the behaviours of organisms, the better those models will be. As our understanding of modern primates and non-primates has changed, our models have got better, and our interpretation of the morphological evidence of both fossils and contemporary species has become richer.

Szalay used models of locomotion behaviour among primates, and comparative evidence from non-primates, as a way of interpreting the morphological evidence. Cartmill looks to models of traits shared across primates and other organisms. Optical convergence, according to Cartmill, is a common solution across various lineages to tracking prey.

Like archaeologist's use of Middle Range theories documented in chapter 4, primatologists require theories that link their observations to a past behaviour. Background theories play a role in the history of the primate debate, as changes in theories change how we interpret the evidence, or indeed, what counts as evidence.

The second role for background theories, again derived from contemporary observations, is the recognition of new lines of potential evidence. As our understanding of the world develops, we recognise potential processes that may have been operative in the past, and new consequences.

Sussman's alternative hypothesis, of a terminal-branch feeding niche, is a contribution of this kind. Changes in the way we understand behavioural ecology, and the potential co-evolution of plants and their pollinators, suggest a new hypothesis and a new line of evidence: Primates emerged as the result of the co-evolution of proto-primates and other pollinators with flowering plants, with the result that paleoecological evidence becomes an important contributor to debates over primate origins.

> As a result of long-term *diffuse co-evolutionary* interactions with flowering plants, modern primates, bats, and plant-feeding birds all first arose around the Paleocene-Eocene boundary and became the major seed dispersers of modern tropical flora during the Eocene. Thus, it is suggested here that the multitude of resources available on the terminal branches of the newly evolved angiosperm, rain forest trees led to the morphological adaptations of primates of modern aspect. (Sussman 1991 p209)

The upshot is a series of shifts in how we interpret the emergence of the primate lineage. Initially, they are interpreted retrospectively as human ancestors. With limited knowledge of primate habitats and behaviours, the common traits of the primates were associated with an arboreal habitat. Increased phylogenetic analysis however changes the explanatory target for the historical account, and re-emphasises an explanation of the emergence of primates as an adaptive radiation in its own right. Evidence from cross species comparisons, both within the primates and from non-primate species, suggests that common primate morphology is an adaptation for a unique locomotor style (Szalay) or an adaptation for arboreal insectivory (Cartmill).

The second role for background theories is in shaping what is a reasonable hypothesis, and shaping a testable hypothesis with downstream consequences.

It is worth noting here that none of these shifts in the debate have been in direct response to fossil evidence. Rather, the increased understanding of contemporary organisms —their behaviour, relatedness, and ecological niches— has driven new interpretations. Evidence from the past in the form of fossils has been remarkably absent from the debate as I have presented it here. While increasing quantities of fossil evidence has undoubtedly played a role, it could very well be absent.

Phylogenetic evidence and morphology of modern primates is a form of historical evidence. Organisms and their relatedness to other organisms are shaped by the past, and are ramifications of past events and processes. But our theories about why this evidence from modern species is informative are based upon contemporary observations, coupled with our accepted background theories about evolution, the relatedness of organisms, and so forth. While the fossil evidence helps structure our understanding of the primate lineage, it does little confirmatory work in our understanding of the primate adaptive suite.

7.4 Summary

The important lesson from this case study is how theories about how the world works drive our understanding of the past. As we understand more, our understanding of the past, our interpretation of evidence, and our awareness of new sources of evidence, changes.

A change in background knowledge potentially changes our understanding of the past in three ways. It changes which hypotheses are worth investigating. Sussman's hypothesis became worth investigating as we understood more about the co-evolutionary relationships of organisms. It justifies our observations as observations of evidence. Cartmill and Szalay had different justifications for their evidence. And models or theories when acting as hypotheses about the past, make predictions, and which point we can utilise Cleland's machinery. Cartmill's hypotheses made predictions about modern primates, which in this case, were not seen.

On occasion, all this can change in interpretations can be without discovering any further traces from the past. I would suggest that a century of changes in views about primate origins could have occurred without any new fossil finds of primate ancestors, nor any new primate descendents being discovered. While there has been increasing fossil evidence, some surprising, the main changes in interpretation have been driven by changes in our understanding of contemporary organisms and their habitats. Observations of contemporary organisms have acted as tests of theories about primate origins, and have been part of the confirmatory apparatus deployed by primatologists seeking to understand the past.

Geologists study contemporary processes, generate models and theories about how those processes work, and then apply them to the past. This research strategy is known as uniformitarianism. The study of primates, and the application of contemporary findings to the past, is much like the uniformitarian strategy in geology. From observations of contemporary phenomena, primatologists generate theories about primates. These theories are then applied to the past.

8. Configurations of Evidence

This chapter begins to discuss the practitioners of the historical sciences as narrators: scientists who put together causal histories of the past. We move from the problem of testing hypotheses, to the problem of constructing and ultimately testing narratives.

Narratives, as we saw in previous chapters, are collections of processes in a temporal sequence, operating on a central subject. Because a narrative includes multiple events and processes, our evidence for a narrative must provide information on changes in a central subject over time. The historical scientist's first task is thus to isolate the relevant evidence for a narrative from an undifferentiated mass of traces from the past: they need to construct a chronicle of changes in a central subject from a variety of evidential sources. Before one can test a narrative, one must construct a chronicle that the narrative then explains.

The construction of a chronicle is not straightforward. We have to determine which historical traces is in fact evidence; evidence that bears on the central subject. We must have confidence that the chronicle itself is accurate. Take a simplistic example: the evolution of hominin teeth. Perhaps we want to construct a narrative of changes in hominin dentition leading to *Homo sapiens.* The evidence we use to test our narrative is finds of fossilised teeth. But a bunch of teeth by themselves don't tell us much. Prior to testing our narrative, we need to construct a chronicle; we need to determine whether the teeth are teeth of human ancestors and we also need to be sure that the temporal order we put the teeth in is correct. Should some of the teeth *not* be of human ancestors, they would be misleading sources of evidence. Should our dates be wrong, we might think our narrative needs to explain a transition from teeth adapted for seed eating, to teeth adapted to fruit eating, rather than the other way around. Each individual piece of evidence, in this case, each tooth we find, needs to be placed within a framework, a temporal context. So we need accounting claims that not only justify seeing the teeth as teeth of hominins, we also need accounting claims that justify a temporal ordering for our chronicle.

The push here is to understand multiple pieces of evidence as configurations of evidence that provide insights into trends, lineages and other patterns that the historical scientist finds of interest. These secure configurations of evidence are not explanations in themselves; they are rather the raw data that may then need further elucidation. This is particularly true of archaeology where the configurations of evidence, and the chronicles that emerge from them, are chronicles of past human activities, and not a history of those humans, stricto sensu. The construction of narratives from these chronicles is the subject of the subsequent chapter. Because archaeology has been particularly aware of these difficulties, we will once again rely on archaeological examples.

The first section of this chapter will expand on this need for configurations of evidence through time and space. Single pieces of evidence are not enough. In order to construct chronicles, and from chronicles, to construct narratives, archaeologists require sets of evidence. These sets of evidence act as a chronicle that the narrative then explains.

The second section raises the challenge in providing this evidential context for historical science claims. In chapter 5, I outlined the concerns of Derek Turner, who emphasised the fact that some events in the past may not leave traces of the right sort, and that our hypotheses about the past may be underdetermined by evidence. This section suggests a variation on this challenge: Evidential traces may not preserve in the right configurations. Misleading dates can causes confusion in the construction of a chronicle, and misleading relationships between evidence can hinder the application of Cleland's machinery. We will look at this problem by looking at the problems associated with archaeological sites, and how processes that occur *after* the events of interest can distort or erase evidence.

The third section details how archaeologists deal with this problem. They do this by understanding historical degradation as a variety of processes. In effect, they propose hypotheses about how evidence from the past might be degraded, and test these hypotheses. Consequently, they can utilise the strategies outlined in the first half of this thesis: They utilise there understanding of processes to propose hypotheses about the past, and they test them against the physical evidence.

In the concluding section, I will try to move beyond the 'earth sciences' broadly conceived[18] to suggest that similar data collection and analysis methodologies are part of the historical sciences generally.

8.1 The Need for Context

Many of us have found ourselves in the position of moving into a new property, and finding the detritus of previous tenants. Clearing away an overgrown suburban backyard might reveal strange odds and ends; half-buried teaspoons, empty bottles and indefinable rusty objects. Even offices or graduate study rooms can have the cast-offs of the previous inhabitants; anything from pens with no ink, to outdated texts, to abandoned snack foods of some long forgotten graduate who worked late at night.

Archaeology deals with this kind of detritus from human lives. Archaeological sites are merely such human leftovers scaled up in terms of age and occasionally in size. Not just the refuse of the previous few tenants, but the remains of entire cultures who once inhabited the land. Archaeologists can be confronted with the remains of villages, with the foundations of buildings, places that looked like workshops, dwellings, temples, and the gravesites, grave goods of cultures and rubbish dumps of generations.

Sometimes the nature of an archaeological site will not be as recognisable as the remains of a village or settled community. Nomadic peoples also leave traces of their activities that can make it into the archaeological record. A rocky overhang can be reused by different groups over the centuries for animal butchery and tool making in stone and bone; activities with by-products that can survive to our times.

For instance, when Ralph Solecki excavated the Shanidar caves of Iraq he found evidence of its use by 'people' back to Neanderthal times. In fact, during the excavation Solecki had to work round the nomadic people who still used the cave as a seasonal shelter in the 1960's (Solecki

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[18] Strictly speaking, archaeology is not an 'earth science.' It is probably best characterised as a social science, although even that is contentious (See Jeffares 2008). However archaeology's data gathering/data measurement sub-discipline, archaeometry, does borrow ideas and terminology, if occasionally indiscriminately, from geology, and certainly shares a good deal of methodological problems with paleobiology and paleoecology.

1971). In parts, the sediment that Solecki and his team had to dig through was 10 meters thick. And all through that sediment, at various layers, were objects: Broken pottery, flints, bones, things made by humans and discarded or lost. Solecki was confronted with the results of more than 50,000 years of people breaking things, burying objects and loosing stuff.

Along with the manufactured remains of past cultural groups, archaeologists have access to various other sources of information. As early as the 1940s archaeologists had begun to use evidence such as seeds, pollen and other biological remains to reconstruct important ecological information about past groups (Trigger 1990). Developments in techniques over recent decades have seen increasingly arcane information being utilised by researchers. Soil micro-morphology can reveal whether ploughing has disturbed sediment. Proton-Induced X-ray Emission (PIXE) can determine the mix of trace elements in artefacts, which can then be matched against possible raw material sources. The "science" of evidence gathering has greatly the increased the potential sources of information available to archaeologists.

8.1.1 From Evidence to Context

In archaeology, the recovery of a mass of material by an archaeologist makes little sense without a context. A jumble of objects will not tell us much at all. Archaeologists are interested in the configuration of objects, and the configuration of traces. What sits next to what matters, and these relationships between finds provides the basis for further interpretation.

This is a subtly different use of configurations of evidence than that argued for by Carol Cleland. Cleland was interested in using multiple lines of evidence to choose between hypothesised processes. While the mass of evidence that archaeologists confront can be used in this fashion, the configurations of evidence we are interested in here is evidence that reveals the presence of a central subject for a narrative. We infer the presence of a community of people from the remains of their material culture: the pots, building remains and other detritus of their lives. We infer the presence of an ecological community from fossils, paleo-botanical remains and other evidence. A paleobiologist may even want to reconstruct an organism from remains that have been scattered across the landscape. So historical scientists are trying to reconstruct the *subject* for a narrative from multiple lines of evidence, rather than choosing between multiple hypotheses about a past event.

Archaeologists face two types of configurations in their archaeological data. On the one hand, there are the configurations through the various layers of deposits. As time passes debris, dirt and sediment is laid down, creating a series of deposits where older layers are beneath more recent.[19] This can be a matter of thousands of years as in the case of the caves of Shanidar, or shorter time-scales. As the archaeologists progress through the layers, so the artefacts change. The excavations of a sequence of archaeological remains at a site in Hissarlik, Turkey, got the designations Troy I through to Troy VII because of changes in artefacts at different depths: There was a discernible difference in the artefact types at each layer over hundreds of years (Daniel 1981 p158). The changes in artefacts documented the changes in the central subject: the city-state of Troy.

So archaeological data can appear to have temporal relations. It can look as though the different artefacts at different layers show sequences and changes in artefact types from the more recent layers on the surface to the older layers beneath. It is this temporal context that we can use to construct a chronicle. Such a chronicle potentially represents the continuity and change of a central subject through time.

However, such a chronicle also raises questions: Does a change in the morphology of tools found at a site represent a technological development within a group, or does it represent a migration event, with a new group of people arriving from another location? Thus, a chronicle might be of a single culture that innovates, or two successive cultures that have settled a particular region. Part of the archaeologist's task is to determine which of these possible chronicles is the correct one.

8.1.2 Configurations in Space

The second kind of evidence that archaeologists deal with is the spatial patterns of evidence. Again, an example may help. When Graham Avery and his associates excavated through the layers at Die Kelders cave in southern Africa, they unearthed various stone artefacts and the flakes associated with the manufacturing process at a layer they attribute to the Middle Stone Age. These were associated with the remains of what looked to be dismembered carcasses with bone surface modifications, and

[19] A regularity that geologists refer to as the law of superposition.

a few comparatively robust hominin teeth (Avery, Cruz-Uribe et al. 1997). This range of artefacts shared a distinctive spatial relationship that was the basis of an interpretation: Hominins with robust teeth dismembered carcasses at this location with stone tools they made or modified on site. The finds were related to one another in ways that were potentially meaningful: They appeared to be evidence for the presence of a tool using hominin.

Thus, archaeologists deal with objects and their configurations through time and space. From scattered objects and a variety of evidence, it may be possible to see patterns, which enable the archaeologist to talk of cultures, peoples, settlements and groups, as they change through time and move through the landscape.

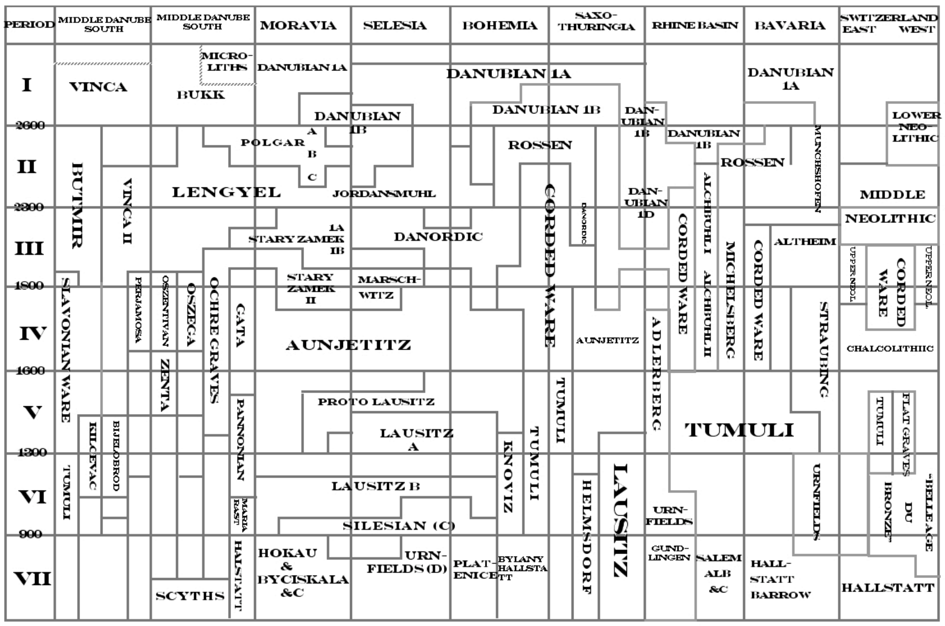

Figure 8-1 A classic collection of evidence through time and space due to Gordon Childe. The horizontal axis documents the spatial locations of various cultures across a section of Europe. From top to bottom are temporal relationships, potential chronicles of changes within an area, with the most recent at the bottom. There is no interpretation at this stage, merely temporal and geographical ordering of distinctive archaeological finds that are presumed to represent the cultural products of distinct groups. (Diagram re-drawn from (Childe 1929))

However, it is not always a straightforward process to identify meaningful relations between artefacts. Finding a group of artefacts in a contiguous location might mean they were contiguous in use, but not necessarily. How they ended up in the same place becomes an issue. Archaeologists must also determine when temporal relations indicated by depth or dating are genuinely representative. Because the context of a find is important in constructing an account of a the past, that context needs to be reliable.

This is not to say that single pieces of evidence are not informative. Clearly, some singular finds are spectacular insights into past cultures, lineages and so forth. The history of paleoanthropology, the study of the hominin lineage, is replete with spectacular one-of-a-kind finds that have overturned previously accepted accounts of the past. But these one-of-a-kind are important because they change the way we view a larger chronicle or narrative. They are important within the context of a broader history.

We can see this with the response to finds that challenge accepted chronologies. The find of a dwarfed hominin, *Homo Floriensis* (Brown, Sutikna et al. 2004), challenged the chronologies and narratives of some paleoanthropologists. One response is to try and argue that *Homo Floriensis* is a random mutant or freak (See for instance Martin, MacLarnon et al. 2006). Sceptics about such finds are attempting to preserve a previously accepted historical narrative. The sceptic doubts the relevance of a find *within* the context of a previously accepted set of evidence. Any particular piece of evidence is always judged by how it fits into a historical pattern or context.

The obvious exemplar of why the configuration of evidence matters is clearly dating. To construct a reliable chronicle, we need to get a reliable ordering of changes; we need to be sure *this* really did come before *that*. When a site is undisturbed and sediments have built up slowly in a regular fashion, an archaeologist might be confident that they have a good temporal sequence. However, across larger sites, or between sites, an archaeologist must rely on some kind of absolute dating method such as carbon dating. Frequently however, the object of direct interest that archaeologist wants to locate in time cannot itself be dated. In such a case, she might hope to find material within the same layer that can be dated. So, the archaeologist must ensure that the datable evidence and the un-datable evidence are not accidentally in the same layer, as this could lead to misleading chronologies across sites.

Archaeologists subsequently face the rather difficult task of determining the informative relationships in their evidence from the merely coincidental. The task for archaeologists, and other historical scientists, is one of constructing reliable chronicles — the sequences of evidence— and reliable relationships between temporally contiguous evidence, which can be used to track a subject of interest through time.

8.2 The problems of sites

It is one thing to acknowledge that archaeologists and other historical scientists utilise patterns, and are interested in the configurations of evidence. However, how reliable are the configurations? Our methodological uniformitarianism research strategy may well provide us with good reasons to think that a hominin has been the cause of cut marks on a particular bone. However, do we also have good reason to think that a stone tool close by the tool made the mark? Or even, that it was a tool of a similar type to the one that made the marks? This is potentially a useful relationship between observations if we can establish that the two things are related in the right kinds of way. We may be in a position to make claims about a tool and its deployment. But we need good reasons to think that the two objects are in fact related. Perhaps the stone tools and the de-fleshed bones have ended up in the same place because of the activity of a watercourse and are fact unrelated to each other?

In the second half of chapter 5, I outlined the concern of Derek Turner that evidence would decay in such a fashion that it would make the past unrecoverable. According to Turner, there are situations where local underdetermination of evidence would mean that we would be unable to choose between alternative hypotheses. We are examining here an extension of this problem: If we want to use configurations of traces — whether they be temporal or spatial— its possible that these configurations could decay or distort in ways that could underdetermine our hypothesised chronicle, or simply mislead us.

For many historical scientists, particularly ones concerned with evidence 'dug up' from the past, determining meaningful configurations from perhaps meaningless associations is not straightforward. We saw in chapter 4 that in archaeology this realisation resulted in middle range research: the study of the relationships between observable evidence and past causes. In what follows, we are interested in that area of middle range theorising and research that deals with configurations of evidence, rather than a single piece of evidence. Just as a single piece of evidence can decay, a pattern of evidence can also decay. We are interested in the processes that distort configurations of evidence, even if they do not destroy them outright. Geological processes alter configurations of material; intrusions into the sediment by various organisms, including humans, mix and distort it. Post-depositional processes —things that happen after the events we are interested in— overlay the signals of human behaviour, organisms, and past ecosystems with a layer of distorting noise. Some of this noise removes evidence, some of it distorts it, some of it mixes with the signal. In determining which traces count as evidence from an archaeological site or a paleontological dig, the field worker needs to be able to secure the configurations of evidence in the face of such information destroying processes.

Crucially, some of these noise-making processes are regularities; they are causal processes in their own right. So, the informative configurations can sometimes be reconstructed by understanding how they have been distorted. If we can understand the noise, the relevant signals of the past might be detectable from the noise of depositional and post-depositional processes.

Understanding the formation processes that underlie configurations of traces, and understanding the various disruptive processes associated with site formation with the aim of being able to determine signal and noise, is the province of taphonomy.

8.2.1 Taphonomy

The term taphonomy was originally used to describe the study of processes associated with turning material from the "biosphere" to the "lithosphere;" i.e., from living organisms to fossils. Over time however, the use of the term has broadened to include all the processes of site formation and transformation (Gifford 1981). This is partly the result of the term moving from paleobiology to archaeology. For paleobiology, the ultimate target is the reconstruction of the biosphere from the remains of organisms that have mineralised and become part of the lithosphere. For paleobiologists, understanding the transformation from living organism to fossilise remains is important.[20] Humans however manipulate the lithosphere as well. Humans reshape rocks for tools, move them for

[20] The recognition that fossils are in fact the remains of organisms that have mineralised was an important development in paleobiology's history. Previous theories from times that divided matter into the organic and inorganic suggested that fossils were the result of misdirected creative energy; the forms of organisms were being misplaced into stone instead of organic matter (Rudwick 1972).

campfires, construct things using stone, and also manipulate the sediment during activities related to agriculture such as ploughing and drainage, and digging for a variety of purposes. Consequently, for the archaeologist the target signal can be a range of material that includes stone, ceramics, and manipulated biological material. It is this broader archaeological sense of the term taphonomy that I will use here.

Taphonomy then is interested in the way that deposition and post-depositional factors alter the configurations of material evidence, and potentially distorts or destroys individual pieces of evidence or its relationship to other material. Clearly dating is an important piece of configurative information in the construction of chronicles and narratives, and thus taphonomy broadly construed has as one of its areas of study the way post-depositional processes impact upon dates, both relative dates and absolute dates.

One could conceive of taphonomy as a filtering process; gathering signals for further analysis. However, this separation into an evidence gathering phase and an analysis phase is artificial. The processes that disrupt evidence may well be of interest to the archaeologist. For instance, a burial by a later culture may well disrupt the evidence of a prior culture. *Both* sets of evidence are of interest, as both are the results of human activity, despite the later activity disrupting evidence of the former. Because human practices impact on depositions, there is always the possibility of an overlap between taphonomic claims and anthropological claims. The same goes for paleobiological sites. In reconstructing an ecosystem or a habitat of an extinct organism, knowing that the living organism inhabited a certain type of sediment, and manipulated that sediment through burrowing, can assist in making sense of the material one confronts. The noise making processes may well be causal processes that are of interest to the researcher.

8.2.2 A taxonomy of noise: the empirical problem

Taphonomy then is the science of detecting configurations that tell us something about the subject of interest in amongst the noise of post-depositional processes. To demonstrate just how much noise there is, we will briefly outline potential distorting processes. The point here is to demonstrate what sorts of sciences are involved in sorting through the mass of historical material, but equally, to demonstrate that the disruption to evidence is not purely random: There are regularities that can be understood by historical scientists. The disruptions to patterns of evidence are causal processes: processes that can be understood by a range of sciences.

Taphonomy is divisible into two broad areas of research: the science of how organisms decompose, and the post-depositional processes that affect a site (Mignon 1993 p336). The study of decomposition is of particular interest to paleobiologists, and is an area of research concerned with "the remains of an organism and final burial." Its concern is the process of turning a recently dead organism into something found much later. Anything else that happens after burial to disturb remains is included as part of the study of post-depositional processes.

For most of human pre-history, tectonic processes play little role, although erosion and deposition of sediments and the movement of ground water through sediments are important factors to consider. However, within the historical sciences generally, particularly palaeontology, the deformation of objects through the movements of the earths crust is clearly an issue. These processes can distort sediments, or rearrange geological layers. So too, the process of mineralization that creates a fossil. It is important to understand how this affects the remains of an organism that the palaeontologist confronts.

Other post depositional factors are concerned with various interactions of organisms with deposits, or agent based processes. Michael Schiffer divided these into two distinct sets of processes: those associated with non-human organisms that are "noise" to the archaeologist, such as the activities of rodents, and those associated with human activity which are potentially of interest (Schiffer 1981). Human activities such as reburial, rubbish pits, and other intrusions can upset the stratigraphy of a site, but are also potential signals. Thus, we can arrange site formation processes into a taxonomy of potential disruptive processes.

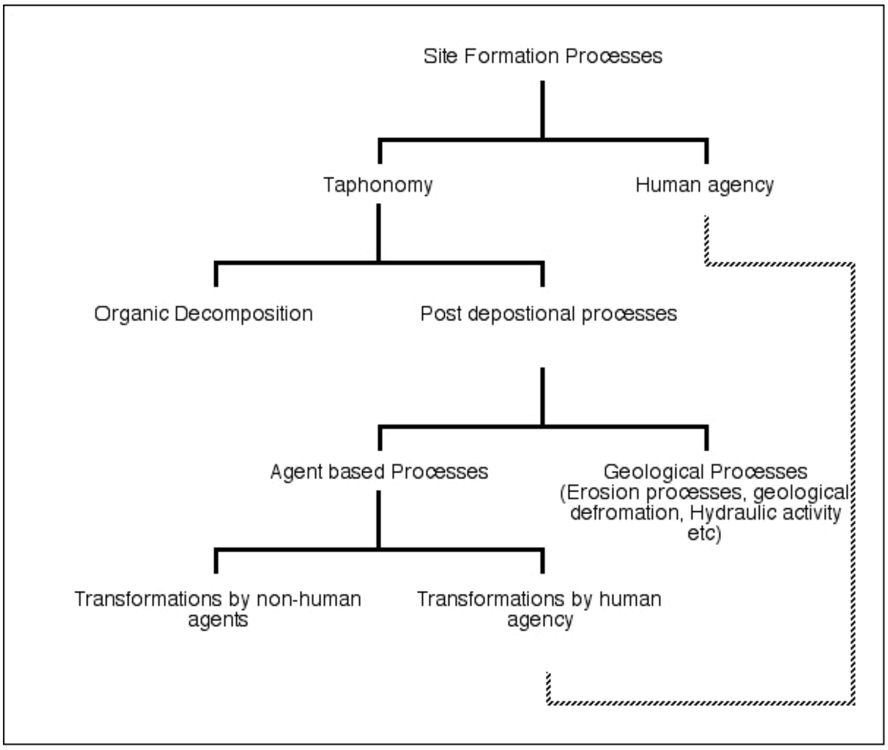

Figure 8-2 A Taxonomy of Taphonomic processes. This is hardly exhaustive, and the configuration is somewhat arbitrary. However, it does get across the range of sciences involved in assessing a site. Geology in particular, frequently plays a role in determining how mixed deposits are. However, understanding the habits of underground living organisms can also be useful. Archaeologists are typically interested in isolating human agency, but this in itself may be a response to other factors, and other organisms may of course operate in response to human activities.

The project of studying site formation processes is to investigate all of this. It is an ongoing effort to sort out the relevant processes that are of interest from this mass of potential causes. An archaeologist requires tools to determine the history of the things in front of them. Palaeontology too faces difficulties in assessing finds, particularly in cases where it is attempting to reconstruct the ecological setting. The differential survival of some species in catastrophic events, the dismemberment and disturbance of dead organisms by scavengers, the reuse of shells by hermit crabs[21] and so forth; these can all skew the samples for the reconstruction of ecological systems (Gifford 1981 p372).

[21] Discussions with Katherine Szabo of Australian National University's department of Archaeology and Natural History provided insight into the difficulties hermit crab activities pose to unwary archaeologists.

The problem that is faced by the historical scientist is that she may not know whether an assemblage, a wear pattern, a disturbance, or some other trace or find are the result of human activities, and therefore of interest, or have been caused by something else. Because of this, we risk missing important information about the past. Without being able to reliably detect human causes from non-human causes, we may over interpret the things we see. Archaeology has had its moments in the regard: from controversies over eoliths, to controversies over the lost builders of the mounds of New Caledonia.[22]

8.3 Pattern Detection in Evidence

In chapter 3, we saw how a historical scientist can use multiple strands of evidence to choose between hypotheses. A hypothesis that accounts for one piece of evidence makes additional predictions about other pieces of evidence the researcher should see. This additional observation, or observations, acts as a test, and allows us to choose between competing hypotheses. The question now is whether this methodology can be used to disentangle the signals of interest from the noise of post-depositional processes. In amongst the collected traces of the past, there are some signals that may be of interest.

In chapter 4, I argued that in order too be able to use multiple lines of evidence, historical scientists required background theories. These background theories play two roles: They secure the historical inference, from a single piece of evidence to it's past cause, and they make predictions about additional down stream consequences that we can use to confirm our hypotheses. In some cases, a historical scientist may even have to construct a narrative about transformations of evidence. So how, given the idiosyncrasies of site formation, do background theories play a role in extracting information that is of interest from the noise of history? This doesn't just impact upon archaeologists. Tools for understanding sites and depositions are also required in geology and paleobiology (Gifford 1981; Behrensmeyer, Damuth et al. 1992; Behrensmeyer and Hook 1992).

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[22] In the eolith case naturally occurring breaks in rocks were being interpreted as early stone tools. In the New Caledonia case, the "mounds" dotted across the landscape that led people to speculate about a lost civilisation were in fact the remains of the incubating nests of an extinct giant megapode (Green 1988).

As we saw in the previous section, the processes that disrupt patterns of evidence are potentially regularities. As such, they are amenable to study. The historical scientist can understand how organisms decay. They can research how water moving through a site may disturb sediments and artefacts. From this research, historical scientists can generate models of how various processes will disrupt and rearrange evidence.

8.3.1 Models in Taphonomy

In chapter 4, I suggested that the historical sciences used models to understand processes in the past. In this section we will see how models work, and more importantly, how they can go from being representations of general processes, to being representations of specific target systems. The models utilised in taphonomy and in other areas of the historical sciences are representations of causal processes that generate configurations of evidence. These models are representations of possible processes and the way they leave evidence. To show how this works, we will look at a standard informal model that is a general model, and underpins an area of research in paleobiology.

Plants disperse pollen, some of which lands upon still bodies of water such as lakes and swamps. Over time, this pollen sinks and mingles with the sediment at the bottom of these water bodies. Changes in the local flora will change the pollen in the sediment. A core of the sediment will document these changes in pollen over time, with the most recent pollen being at the top of the slice through the sediment. Assuming that pollen morphology is reasonably constant, and that pollen is diagnostic of particular species[23], paleobiologists can construct a chronicle of flora changes through time.

To test the validity of this model, historical scientists can engage in research in contemporary settings. A researcher can put up pollen traps in an area of interest and calibrate the pollen deposition to a known flora. They can even use reliable historical testimonies of flora changes (Dimbleby 1985). Thus, the historical records of plantations of trees or crops in an area can be used to check the model in local situations.

Pollen libraries can also be constructed with pollen gathered from

[23] Pollen morphology is typically distinctive the level of the genus, and in some cases, to the level of species.

contemporary plants, and used as a guide to understand historical flora. Thus, this *general* model can be checked by contemporary observations.

This general model is an account of how a process leaves traces. In this case, it is not a mathematical model, and it contains little in the way of laws as such. It does however utilise a number of regularities that can be checked and calibrated using a methodological uniformitarian research strategy. We assume that pollen morphology is the same now as in the past, so that it is diagnostic of particular plant types. We assume that pollen behaves the way it does now: It is dispersed by winds, it settles on lakes, bogs and swamps, it then drifts to the bottom of these water bodies and settles in the sediment. While the model has limits in its applicability, —it doesn't describe situations where water energy disturbs sediments, or inflows of water brings in water and pollen from outside regions— the model does provide us with an interpretive framework, and gives us confidence in a chronicle of changes in local flora. As a model, it captures regularities in how processes in the past will determine the configuration of observable traces.

When trying to piece together the complexities of an archaeological site, an archaeologist will use a variety of such models as starting point to understand the configurations of traces. For instance, an archaeologist may carbon date materials in a site, which in turn provides approximate dates for other finds in contiguous layers. In some cases, the distribution of dates within a site can look odd. They can look out of sync with the physical evidence, or look unevenly spaced within the layers of a site. This can raise doubts about whether the datable material is a reliable indicator of the age of contiguous finds.

This kind of difficulty can often arise in a site where sediment fills in a depression in a location such as a cave or cliff overhang. Dates from regular intervals from within the sediment can look unevenly spaced. In order to make sense of a sequence of such dates, a researcher might start with a simple model of sediment deposition that accounts for differing thicknesses of layers, utilising a model such as the shape-filling model of Tony Barham and Matiu Prebble outlined below (See Figure 8-3 re-drawn from (Barham and Prebble 2005)). In this case, the model is a visual one, although, clearly, one could work through the relationships mathematically. What's more, it is clearly a very abstract representation of a complex and occasionally chaotic process. No one expects sediments to fill a depression in a constant manner, nor does anyone expect to find depressions with such a precise geometric shape. It is a representation of the key variables that matter for this particular process. The model is a way of understanding how one might find objects deposited in such a way that the physical relation of the objects is not linearly related to the actual age of the objects. The model represents an important relationship between the shape of a depression, and the subsequent spacing of objects within that depression, given a constant deposition of sediment. In making sense of the relationships between data points, the model acts as a guide to further investigation, and a way of understanding the distorting processes involved.

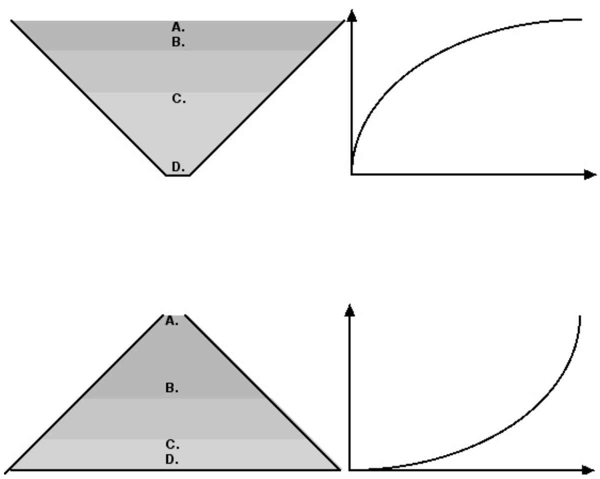

Figure 8-3 A very simple model of how the changes in shape of a depression affect the distribution of sediments. Assuming a constant rate of sediment deposition, depressions with narrow bases will have thicker sediment layers lower down, and thinner higher up, changing the physical spacing of any finds in the site. A, B, C, and D are locations of artefacts or materials for dating. The simplified graphs represent the speed of at which the space fills. The vertical axis represents the rate the spaces fill on the assumption of a constant sediment influx. Should we be in a position to accurately date finds at A, C and D, and then wish to assign a relative date to the find B, the model suggests that in the top case, B will be closer in age to A, while in the bottom case, it will be intermediate between A and C.

Some models that archaeologists use to understand sites are even less specified than the shape filling one. Archaeologists frequently have a set of background assumptions that they utilise prior to a dig that guides their research and investigative strategy. Background theories provide a context for investigations.

An important source of models and background theories for archaeology is contemporary groups. These provide important analogues for making claims about past groups. The archaeologist Lewis Binford studied contemporary hunting groups and their butchery practices in order understand what one should expect to find in archaeological sites (Binford 1981). He built informal models as a means of guiding research.

Binford's motivation for this research was archaeological work that claimed some archaeological sites were the results of concentrated human activity. The archaeologists were interpreting the relationships between pieces of evidence as "home bases" for hunting hominins. By investigating the behaviours of modern groups, and investigating alternatives mechanisms that shaped archaeological sites, Binford was in a position to dispute the claims of other archaeologists. Binford argued that the sites were the result of a number of non-human activities, and were being over-interpreted (Binford 1981).

The question that now confronts us, is how to turn these general models of processes into specific accounts of a target system.

8.3.2 Modifying Models through Tacking

One of the topics that we have been dealing with over the course of this thesis is the notion that history is particular, and our theories about the world are general. So, having these abstract representations of general processes is not enough. The investigative process is to take these general abstract representations of possible causal processes, and turn them into a localised and specific account of the target system. To see this how this process of works, I will introduce the notion of tacking between hypotheses and evidence outlined by Alison Wylie (Wylie 1989; Wylie 2002).

We can frame our view of the investigative process as one where the researcher starts with a general representation or set of ideas about what *might* be happening, and then she particularises these ideas to the target system she is investigating, by bringing her models into contact with the physical evidence of the past. The insight that Wylie provides is that this process is not a *single* test; where a model or hypothesis is tested, found wanting, and then discarded in favour of another hypothesis. Rather, Wylie sees the process of deploying models or hypotheses as a dynamic process, where a hypothesis, or perhaps a range of hypotheses, is modified over time in response to information. In the archaeological cases that Wylie is interested in, archaeologists draw upon

> …a range of background information about source contexts that then serves as a basis for reconstructive inference: information from ethnohistoric, sociological, and psychological sources, as well as from the natural and life sciences that deal with the ecological and physical conditions of human, cultural life. (Wylie 2002 p165)

This background 'source' information is not always a single hypothesis; it can be a set of background theories. This set of possible processes acts as the initial templates or models for understanding the physical evidence to which researchers have access. However, as noted, these are not hypotheses to be tested; rather they are working hypotheses that guide initial research, and are modified in response to evidence.

In chapter 4, I argued that background theories play an important role in limiting the space of potential hypotheses, and in making predictions about further lines of evidence. Models play these roles here. Models act as constraints and guides for the researcher. And models potentially make predictions, which can then be confirmed by physical evidence.

For example, an archaeologist may investigate a particular location with a starting hypothesis that the site is the result of nomadic hunter-gatherer activity. Given what the researcher knows of the region and time period, this may well be a reasonable choice. Background knowledge plays a role in the initial choice of a hunter-gatherer model of human behaviour. Now, to choose nomadic hunter gathers as an initial model for investigation suggests certain evidence should be found at the site; one being that the habitation site will show low amounts of investment in habitations and material goods, commensurate with a semi-nomadic pattern of resource exploitation. On finding evidence of some investment in structures in the form of building remains, rather than discarding the hunter-gatherer hypothesis, the researcher has the option of modifying it. The researcher can modify the hypothesis from nomadic to semi-nomadic; from permanently mobile hunter-gatherers to something more complex, perhaps a group that exploited resources on a fixed seasonal rotation, from site to site, perhaps with extended periods at the particular location under investigation.

The archaeologist can then *re*-test their modified hypothesis. After all, this modified hypothesis makes new predictions; if the group were semi-nomadic with a fixed seasonal rotation, evidence of resource use found at the site will have a distinctly seasonal bias, as permanent year round habitations will contain evidence of resource exploitation from all seasons. The researcher can then look for evidence of seasonality of resource use, perhaps in the form of pollen evidence or by some other means.

This method of refinement of a general model to a particular case is common within the historical sciences and in the sciences generally. As I noted in chapter 4, A. Brad Murray suggests a similar process of modifying general models in geomorphology. There is a continuum of models, based on the model's purpose (Murray 2003). At one end of the continuum are simulation models…

At the one end of the spectrum are general exploratory models…

> …distinguished by a high degree of simplification... In this case a modeler leaves out as many processes as possible, in an attempt to determine the mechanisms that are essential in producing the basic behavior in question. … Exploratory models are not intended to reproduce specific cases, but to investigate general behaviors. (Murray 2003 p2)

These exploratory models are general in scope, and are targeted at understanding underlying mechanisms across cases. As such, they apply across a variety of real world situations, and they isolate the key variables that generate differences in particular cases. Such models are a representation of robust processes that allow for comparison and contrast across cases. Stripped of the minutiae of a particular case, a model can suggest commonalities across tokens that unify them as instances of a common process.

However, while we can strip out variables, we can also put them back in. With variables fleshed out, and the details of a particular case included within the model, the model can become predictive. So, a general model of flood plains can suggest commonalities across a number of river systems, from the Nile Delta to the Yangtze to the Mississippi.

General models also provide contrastive information and isolate variables that need to be quantified for a particular system. For such a model to become fully predictive, variables that can be treated as averages or constants in a general model may require information that is more precise. With the addition of local contingent details such as expected rainfall patterns in tributaries, local manmade structures such as levies and dams, and substrate dependent factors such as silt build up, the model can become a detailed representation of a particular target system.

When applied to historical cases, geomorphologists can use the contrastive information provided by the model. The contrastive information provides the necessary variables that a worker needs to precede further. Again, the Barham and Prebble model provides a good example. It is unlikely that any cave floor is going to have a nice simple geometric shape; nevertheless, the model provides a good tool for thinking through how the shape of a depression is likely to influence the configurations of the site. As a model, it isolates what further information is required: the shape of the depression. The model plays the role of a working hypothesis, a way of guiding the early phases of investigation, and making sense of some of the initial data.

Models act as initial hypotheses that are then modified in response to the physical evidence to generate a more comprehensive and detailed picture. The process of research in such cases is a dynamic one, involving constant modification of hypotheses and the assessment of these hypotheses in relation to evidence. The tacking analogy here is one where there is a constant reassessment of data and hypothesis. This movement between data and hypothesis results in overall movement towards an increasingly refined account of the past.

Like the testing of hypotheses, the tacking procedure uses multiple lines of evidence. A model accommodates one piece of evidence first, but just as importantly, it also makes predictions about additional pieces of evidence. These additional pieces of evidence can show that a model is an inappropriate choice if they are not present, or, it can show how a general model needs to be adjusted to account for a particular target system. The tacking procedure can provide the precise variables need to make further predictions.

It is not always the case that models make predictions. Sometimes the models and background theories utilised are straightforward limitations on possibilities. Land organisms are limited as to their migrations and expansions, humans at various points in history are limited in their technologies, and physics provides limits on cosmology. Thus, our background theories about how the world works define a space of possible models to choose from.

Nevertheless, models of general processes can provide crucial insights into what to look for to turn a representation of a general process into an account of a particular situation.

8.3.3 Signal, Noise and Multiple Models

In trying to determine a signal from noise, two sorts of models would interact. In the archaeological case, the first sort of model is an expected signal based on background knowledge of the site under investigation. So, as outlined above, the researcher might expect that hunter-gatherers have used a particular cave site, and this model might be refined according to the evidence they find.

The second sorts of models are those of potential distorting or decaying processes. In investigating the site, and in trying to construct a chronology, the archaeologist might bear in mind the Barham and Prebble shape filling model. As they excavate, this model might be refined to take into account the shape of the depression in the cave floor, and how this might affect the relative dates of the finds.

So, we have here models of how a subject of interest might leave evidence of a particular sort, and models of how distorting processes might disrupt that evidence. To determine how these two models interact in any particular case, we can make comparisons across sites. Ideally, we check our model of sedimentary depositions from a nearby locale with no known human influence. For instance, if we had two islands that were similar in all the relevant non-human variables, but distinct in the human variable, with one uninhabited and one inhabited, we could compare the sedimentary records of the two islands.

We can also do this with the human signal of interest. Within a region, there may be a number of archaeological sites of a particular cultural group. This allows comparisons of the signal of interest across sites, and the identification of distorting features within a particular site. If across a number of sites, we get a pattern of human occupation, but at one site this pattern is disrupted, then the archaeologist can look for potential causes for this disruption.

This is why background knowledge of what to expect from an archaeological site is important. The archaeologist selects different models of processes she thinks might be appropriate —both processes that document the subject of interest and processes that distort those signals— and refines them to the circumstances at hand. The process is a reflexive, recursive process, where alternatives are tried, modified, and refined in relation to the evidence at hand, and in relation to expected patterns.

Models then can interact with one another in a kind of refinement processes. The evidence of an archaeological site and its configurations can be repeatedly filtered for noise and interpreted with a variety of models. One might start with the refinement of a geological model of a site; making sense of its formation, erosion, and any water movement through the site. Then one might utilise an informal model of hunter-gatherer activity to isolate the human impacts upon the site.

An analogous methodology to this process of using interacting models is it that of recovering the text of a document that has been has been shredded. An individual can approach the shredded remains of a document with a model or set of ideas about the original text; there will presumably be orderly page numbering, with page numbers in a consistent place on the page; lines of text will run across the page, and may be justified on one or both margins. There is also a model of how the language of the text should appear: there will be sentences that made sense in the language of the writer; the writer will deal with one topic at a time and so forth.

These models of the original text —both its formatting and its language— make predictions about how the final document should look when reassembled and suggest important links to look for in the evidence at hand. For instance, should the researcher find a piece of a shredded page with a number at the bottom, offset from other lines of text, these are presumably page numbers. There will be only one page number per page, so getting these numbers in an orderly sequence may well be a good first step. It will highlight missing pages, and provide an idea of the documents original length. Shredded pieces may have the edge of the body of the text, and again, there will be only one per page. Our model of how language works also make predictions about how individual pages should fit together, making coherent sentences.

The shredding process, the noise making process, is also a model that can be refined so that it too provides clues. A mechanical document shredder slices the pages in a uniform and predictable fashion. A slightly blunted cutter on the particular shredder used in a particular case may mean that some of the shredded pieces have one edge that is more ripped than sliced. This again provides an observable clue, and there will be only two such shredded pieces per page, that match each other along one edge.

With a model of what to expect from the recovered document in its formatting and its text, coupled with a model of the shredding process as a starting point, an individual can, with a tacking process of trial and error, paste the document back together and read the text. The tacking process turns local idiosyncrasies, such a slightly blunt cutter, into an important clue.

The picture that emerges is one of multiple models interacting: Models of what the researcher expects to find given their background knowledge, models of potential distorting processes and models of how various processes of the past are likely to leave traces. The models make predictions about what to see, and constantly test and refine what was a general model into a localised account of a processes or processes.

Consequently, with patience and time, a historical scientist can piece together the evidence from an undifferentiated mass of material in such a way that patterns can be detected. She can do this by treating the disruptive processes as historical causes in their own right. She can posit a hypothesis of a past disruptive cause that makes predictions about additional downstream consequences, and test this hypothesis. Moreover, this additional evidence can be used to particularise a model; move it from being a robust model that applies across cases, to a representation of a particular case by a tacking procedure.

8.4 Summary

Because historical scientists frequently have a wealth of material, not all of which is relevant, and not all of which is appropriate 'signal,' they utilise their understanding of a variety of processes to filter the signal of processes of interest from the noise of physical evidence. In so doing, they exploit their understanding of processes hard won from experience, but also borrowed from the general pool of scientific knowledge.

The disentangling is done by using models of noise making processes which can be used to reverse engineer the configurations of evidence of that are of interest. By understanding how a configuration of evidence of the past may be degraded or distorted, an informative signal can be recovered. In effect, a distorting process is itself a historical process, and like other historical processes, so long as it is not genuinely chaotic, it can be understood, tested, and refined to a particular case.

The models used by archaeologists in this process play the role of a starting point in the investigative strategy, and as guides to how a model needs to be modified. This methodology refines a general model, a set of starting assumptions— into a localised account and highly specific representation of the processes that have occurred in the past. The models used are representations of how a process in the past leaves evidence. It is a hypothesis that can be tested.

Making sense of a mass of undifferentiated material is then a process of utilising well-understood models of processes that make predictions about consequences. The models used may well be testable in their general form; they represent robust processes.

While this chapter has primarily focussed on archaeology, other historical sciences clearly engage in a similar process of filtering signals of interest from the noise of the past. Cosmologists too face a mass of evidence. In trying to understand the history of a solar system or a galaxy, they must adjudicate between sources of information. The way to do this is through models that act as hypotheses of how a process will leave evidence, and models that act as hypotheses of how processes will disrupt that evidence. The disruptive process can then reverse engineer the signal of interest.

The question for this chapter is how we can make sense of a mass of evidential material. I have argued that in order to do this, historical scientists use a variety of informal background theories and models as a means to extract configurations of evidence from undifferentiated traces. The aim of this process is to find those configurations of evidence that isolate a central subject and its movements in time and in space. So the very disruptive processes that suggested to Turner that the past might well be difficult to reconstruct, are processes that we can understand and account for utilising their consequences.

However, for an archaeologist or any other historical scientist, tracking a central subject through time and space is only the first step. There also needs to be an interpretation of the past built from this data. This is the construction of a causal narrative; an account that explains how the changes in a central subject came about. It is to this that we now turn.

9. Constructing Narratives

In the last chapter, we looked at how archaeologists use hypotheses of processes that generate noise to develop chronicles. This chapter looks at work that constructs narratives from this data. It would be misleading however, to think that these are two separate processes of enquiry that work completely independently of each other, and as we shall see, the distinction between a chronicle and a narrative is somewhat arbitrary. This is because the process of positing general models of processes, and then tacking between the model and the evidence outlined in the previous chapter occurs when constructing a narrative as well. The same methodology allows the historical scientist to construct and test a narrative. Nevertheless, a distinct set of problems comes with the construction of narratives.

These problems come from the historical context required for a narrative. Because narratives are causal histories, prior events shape subsequent events. In turn, processes later in a causal sequence potentially distort or in some cases erase traces of prior events. The problem is one of historicity. Two processes can be of the same general type, but how the tokens manifest themselves will be conditional on a historical context: events prior to a process constrain a process and may set its key variables. Processes that occur subsequently will determine the availability and form of the physical consequences of a process.

On top of this problem of historical contingencies, we can also expect multiple processes to be operative on a narrative's central subject. The *Homo* lineage has been shaped by multiple evolutionary processes, some acting simultaneously. We can expect history to be causally messy, and one of the tasks is to work out which processes are operative and how they interact. The issue is how to identify the interaction of multiple processes, rather than identifying a single process. I will argue that this is done in a similar fashion to the way that historical scientists deal with the problems of noise: multiple processes are proposed as hypotheses, and interactions between various processes are worked through.

The problems in the construction of a narrative are then very much the same problems as the historical scientist faces in the construction of a chronicle. First, there may well be multiple processes that interact that transform the central subject of a narrative. A narrative of a landscape is not a simple ordered list of processes that occur one after another. Rather, at any time there is a unique confluence of processes; erosion and deposition working at different rates; stabilisation of sediments through forestation breaking up soil and clays previously deposited and a variety of other processes all in operation. Thus, Cleland's machinery and the utilisation of downstream evidence of the past must be extended to not only choose between hypotheses; it must also be in a position to determine the right mix of processes operative at any one time.

The second problem is the historicity of processes. In the previous chapter, we looked at the issue of detecting signals from noise. Much of that noise was from subsequent processes that erased and transformed the signal of interest. However, by their very nature as transformations of a central subject within a narrative, such transformations are of relevance when constructing a narrative. They are noise at one point in a historical sequence, and signal at another. The cumulative results of processes must be disentangled during the construction of a narrative.

We will start this chapter by briefly looking at the distinction between chronicles and narratives. We will set out clearly the distinction between the two so that we can see why they are different, but just as importantly, how in practice they are difficult to separate.

In the second section, we look at the problem of multiple processes and how we can determine the mix of processes operative at any one point. To do this we will utilise the work of Jared Diamond, and in particular his approach to understanding the past deployed in his book "Collapse: How Societies Choose to fail or Succeed" (Diamond 2006). I will frame Diamond's approach to his subjects using the metaphor of "tacking" outlined by Alison Wylie that we discussed in the previous chapter (Wylie 2002). In the Diamond case, a number of possible processes may be at work, and through assessing the evidence; the right mix of processes is chosen. Like Cleland's machinery, the historical scientist uses multiple lines of evidence for this task. However, rather than choosing between hypotheses, the historical scientist is determining the mix of processes operative in a particular instance.

The third section will look at the problem of the historicity of processes. Again, multiple lines of evidence are used, and again, there is a range of hypothesised processes. However, in this section we are emphasising the need to pay close attention to the temporal sequence of the processes, and how prior processes set the starting conditions for subsequent processes. I argue that to account for this historicity of processes, historical scientists actually must engage in a *three* place tacking procedure: from evidence, to hypothesised process, to a broader narrative context.

I will argue throughout this chapter that as elsewhere, historical scientists deploy general models of regularities to understand the transformative processes. However, because processes are highly dependent upon what has gone before, the regularities we posit must account for the idiosyncratic results of history. The result of the deployment of these models is the construction of a particular kind of explanation: A narrative.

9.1 Chronicles to Narratives

Narratives are different from chronicles, in that a historical narrative includes the transformative processes that change a central subject of a narrative. A chronicle is a temporally and spatially ordered catalogue of data, while a narrative includes transformative information that explains the chronicle. However, the methodology used to construct narratives is the same as that used to construct a chronicle, and in practice, the two tasks overlap substantially.

Over extended time frames, historical scientists are interested in trends or changes in systems or subjects. A paleoanthropologist is not just interested in the event of the emergence of the *Homo* lineage out of Africa; they are interested in what this implies within the context of Hominin evolution; what process underlay this event, and what this presages for later events in the lineage. An oceanographer or climate historian is not just interested in documenting when the northern and southern landmasses of the American continent joined; they are interested in this event as a node in a sequence of events that have shaped certain features of past climates, and the tectonic processes that drove this event. Historical scientists are interested in constructing historical narratives that include explanations for a chronicle. The construction of a narrative is one of the goals of the historical sciences.

Narratives clearly play a role in the investigative process. They structure investigation, and act as hypotheses to be tested. So during the process of investigation, narratives and chronicles are entwined. However, for the sake of exposition in this chapter, we will treat the construction and testing of a narrative as a separate endeavour from the construction of a chronicle. Nevertheless, it is important to emphasise that narratives act as hypotheses. Consequently, they shape the investigative project. And like all hypotheses, narratives are statements about the world that require testing.

We start then with a well documented chronicle: A collection of evidence with the 'noise' of post depositional processes removed. What remains is temporal data for a central subject (it is well dated and has a good sequence) and spatial data (we have accounted for any movements of data by various 'noisy' processes.) An example of such a chronicle might be a well dated series of finds within a particular region that seem to show an initial period of settlement, the emergence of a structured agricultural society, and it gradual decline and disappearance. A particular society within a region acts as the central subject that unifies the data. (See Figure 9-1)

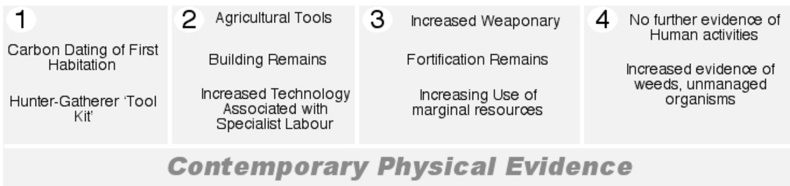

Figure 9-1. In this hypothetical example, we have an ordered collection of physical traces that act as a chronicle for a particular region. Through dating and other assessment techniques, the individual pieces of physical evidence have a temporal and spatial context. Each piece of evidence needs to have some reason for thinking it is reliable, and part of a meaningful set of data, and secure dating in order to get a sufficiently robust "chronicle" for the region.

As we can see here, the separation of chronicle and narrative really is artificial. The very fact that we see this data as linked implies that some transformative processes have been at work, and the very use of the words settlement, consolidation, decline, and abandonment provide a narrative structure to the data the researcher has assembled. Nevertheless, such a chronicle lacks the full and rich causal information we would like. It remains a chronicle of changes in the central subject without any information about *why* those changes came about.

The evidence for the initial chronicle is a variety of physical evidence with the 'noise' removed. So we have lines of evidence for this chronicle such as the ceramics, building remains, changing patterns of paleobotanical evidence, artefacts, burials, and other debris, all of which we can securely date. The contemporary physical evidence, coupled with background theories that relate the physical evidence to the past, provides us with reasons for thinking that our chronicle is correct.

The notion of constructing a chronicle is common within archaeology. The work of Kirch and Green, (2001) is a good example of utilising various lines of evidence that are commensurate with a particular chronicle of Pacific settlement. Kirch and Green use evidence from contemporary ethnography, linguistics and archaeology to refine a sequence of Pacific settlement. The *cause* of the sequence —the underlying reasons for the settlement pattern— are not the immediate issue. Rather, Kirch and Green attempt to track the settlement of the Pacific through time and space, without going into too much detail about the reasons that underpin the migrations.[24] As we saw in the previous chapter, the construction of a chronicle utilises multiple lines of evidence that track a central subject through time and space. Cleland's machinery helps us isolate these states, as multiple lines of evidence are used to construct our chronicle.

However, in many cases within the historical sciences, we also want to go beyond this. We want to turn our chronicle, our sequence of dates and configurations of artefacts, into a history for a region. So our physical evidence has to do double duty: it also has to act as evidence for a narrative. Again, it is worth noting that our 'chronicle' and our narrative are importantly entwined. The chronicle constrains the eventual narrative, and is the explanatory target for the narrative. Nevertheless, from our collection of temporal and spatial data we have collated from the physical evidence, we want to get something more closely resembling a "history."

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[24] In fact, Kirch and Green don't venture much in the way of explanations *for* the settlement of the Pacific. There seems to be a tacit assumption that demographic pressure on settled islands encouraged a sea faring people to expand further a field.

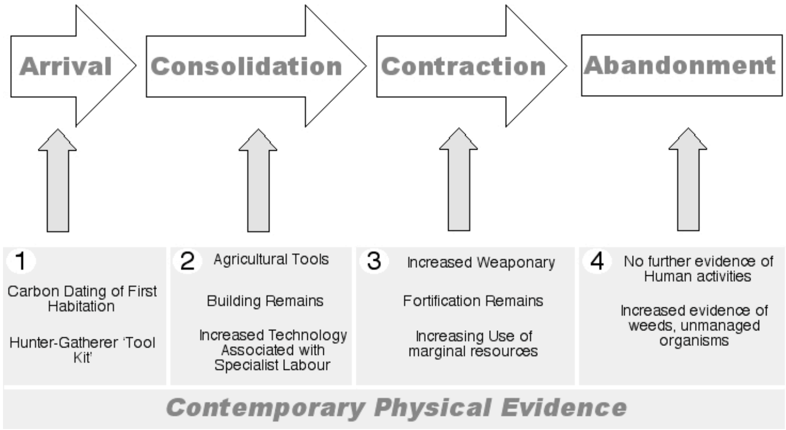

Figure 9-2 From our 'chronicle' —stages 1-4 below— we might wish to construct a narrative. So our physical evidence has to inform that narrative, constrain it, and act as a test for our story. In this hypothetical case, our narrative is one of the arrival of a group, the consolidation and expansion of their society, its contraction, and the eventual abandonment of the area. A complete narrative should, ideally, provide the reasons for these changes. Thus, a full narrative explains a chronicle.

Thus far, our discussion has really focused on the relationship of physical evidence to hypotheses in two forms. Firstly, we have talked about a single piece of evidence and its past cause. This kind of work requires research by actualistic studies, or reference to background theories. This research generates a model that accounts for the relationship between an observation and a part cause. So, we have to have a model of the relationship between an observation of shattered quartz and the historical event of an impact. This is the basic "historical inference." To use Peter Kosso's phrase, background theories play a role as "accounting claims," linking the observation to the past event.

We require these basic historical inferences to assist us with the second use of physical evidence: that of set of evidence that provides a distinctive signature for an event. We have models that suggest *sets* of evidence and make predictions about evidence not yet seen. This is Cleland's machinery, where we use the ramifications of a past event to choose between hypotheses. We utilise multiple lines of evidence to secure our hypothesis about a particular event within a chronicle. So we determine that a settlement was more socially stratified than previously through a variety of evidential traces: Increased numbers of dwellings, increased specialisation of crafts indicating a cohesive social structure, expansion of centralised storage facilities for crops, and other forms of evidence.

What I want to suggest here is that there is a third use of physical evidence. Traces of the past are also used as evidence for the transformative processes that cause changes in the states of central subjects. Traces of the past do not just provide evidence for chronicles; they do additional duty as sources of evidence for transformative processes that explain changes within a chronicle.

In effect, we want the physical evidence to act as tests of hypotheses about transformations *between* states, and not just act as evidence *for* states. We want to give an account of the processes that underlie changes in data, over and above the existence of any particular set, or piece of data. For instance, we may have a well-dated sequence of fossil finds. In addition, our background theories about changes in lineages may well give us good reason to think that there is an ancestor-descendent relationship between these fossils. Consequently, we have a good chronicle for a particular lineage. However, we would also like those fossils, along with additional information, to provide evidence for why the transition between forms occurred. We would like a good causal story, a narrative, for why the lineage changed the way it did. As with other areas of research in the historical sciences, the way to do this is by formulating hypotheses, and testing them.

9.2 Positing Processes and Building Narratives

History is frequently messy, with multiple processes in operation. An evolutionary lineage may be shaped by a combination of drift in some traits, and natural selection in others. Consequently, we need ways to disentangle the particular mix of processes involved in a narrative. What's more, history is frequently cumulative, and processes build upon and are constrained by their history: Processes are embedded in historical situations. The Polynesians that settled New Zealand had lost ceramic technologies as they traversed the clay free atolls and islands of the Pacific. Consequently, despite New Zealand having the right raw materials, the Polynesian settlers didn't have ceramic technologies before the arrival of Europeans. Later European settlers arrived with ceramics technology, and quickly exploited the local clays. Part of the explanation for the presence or absence of ceramic technology comes from the historical precedents of the settlers. The same historicity also plays a role in biological lineages. Historical adaptations constrain and shape later adaptations.

The difficulty for the construction of a narrative is then two fold: disentangling multiple processes that may be co-occurring on the one hand, and understanding how prior processes and states have shaped subsequent processes on the other. We will examine these two problems separately, although it is worth bearing in mind that more often than not they overlap. A confluence of factors may transform a central subject of a narrative, but only in the context of a particular historical setting.

9.2.1 Disentangling Co-occurrent processes

What I want to outline here is a broad view of how a historical scientist can disentangle a number of processes. To illustrate this, I will look briefly at the work of Jared Diamond, and in particular his book "Collapse: How Societies choose to Fail or Succeed" (Diamond 2006). Although controversial, Diamond's work is provides a good example of an approach that I think many historical scientists engage in. His work illustrates of a way of coming to grips with a number of concurrent processes.

In his book "Collapse," Jared Diamond documents a number of cases that he considers exemplars of the collapse of social groups. However, Diamond does not think there is a single cause for the collapse of the societies he examines. Rather, he thinks there a mix of processes, with that mix being unique in each case he examines. In his book, Diamond outlines five factors that he thinks are potentially operative in the collapse of historical societies, and explores whether these factors are operative in particular cases, and how they combine. The factors he thinks are potentially operative in any case are environmental damage, climate change, hostile neighbours, friendly trade partners and a societies cultural response to environmental change. Thus, a mix of external environmental factors, economic factors, and factors internal to the society.

In any particular case, it is quite possible that no single factor is sufficient cause for a societies collapse in itself, although it might be a necessary contributing cause. The conclusion that Diamond comes to is that while there are commonalities in the cases that he explores, there are differences too. There is no single model of a cultural collapse. The nature of contingent history is such that no two cases of the collapse of a social group will have the same mix of causes.

Interestingly, Diamond did not start with this five-point framework for investigating societal collapse.

> When I began to plan [the book Collapse], I didn't appreciate those complications, and I naively thought that the book would be just about environmental damage. (Diamond 2006 p11)

Diamond thus started with a single cause for the collapse of societies —environmental damage— but his review of the physical evidence forced him to include a number of other factors. The result is a menu of potential causes for the collapse of historical groups. Each potential cause is plausible in its own right, and can be modelled, examined in contemporary groups, and thoroughly understood. However, the mix of causes in any particular historical instance is unique.

Diamond's starting point in any particular case under examination is a chronicle. And we will quickly run through a chronicle for Easter Island. The chronicle documents the settlement and changes in the material culture of a group of Polynesians who settled the volcanic Easter Island in the eastern south Pacific around 1000AD. As usual in these cases, there is some dispute over the precise dates. However, carbon dating of charcoal associated with remains of native birds that were quickly hunted to extinction seems to suggest permanent settlement around 900AD.

Evidence for the initial phase of settlement comes from storage pits, rubbish dumps and stone chicken runs associated with the remains of houses. For the first 500 years or so, agriculture seems to have been restricted to the lowland areas. As population numbers increased, as evidenced by increased dwellings and increased agricultural intensity, upland areas were also transformed into gardens for crop production, although the population appears to have continued to live close to the coast. Commensurate with this need to commute from dwelling to upland farming plot is evidence of well-maintained pathways.

This increased agriculture appears to have been co-occurrent with increased social stratification as there is an increase of the number of larger dwellings compared with the standard sized "commoner" dwellings. Knowledge of modern Polynesian groups, also descendents of the initial Polynesian societies, plus ethnographic evidence gives us good reasons to believe that the social and political structure was based around an elite set of chiefs and their immediate families who controlled an area, and commoners who worked the land.

The famous Easter Island statues, the *Moai*, are impossible to date, so archaeologists must rely on indirect methods and additional lines of evidence. Dating of material from manufacturing sites and erected statues, coupled with stylistic changes in the statues, seems to suggest that the main period of statue construction was between 1000 and 1600 AD.

Over time, the statues became larger and consequently required the investment of much more human labour. This has been shown by actualistic studies, with members of the current population of Easter Island assisting in determining just how many people were required to shift some of the larger Moai. They also demonstrated how the statues could be erected (Diamond 2006). From these studies, archaeologists can make good estimates of how many people were required to move a statue from its quarry location to its erection site, their nutritional requirements, and so forth. Among the engineering requirements a good supply of wood to provide rollers, levers and bark for ropes for manipulating the statues into position was necessary.

The paleobotanical evidence in the form of pollen counts reveals that when Polynesians first arrived on Easter Island, there were abundant trees, notably a now extinct variety of palm. Recovery of some fossilised samples of the palm's nut revealed that this palm was a close relative of a Chilean species, and was probably slightly larger. Alternative evidence for the initial supplies of wood on the Island consists of burnt charcoal that can be dated, and also that can be analysed for the variety and size of the plant. The paleobotanical evidence shows a decrease in the amount of woody plants available over the course of the Polynesian occupation of Easter Island: from initial abundance to dramatic shortage by the time of the arrival of European explorers.

Coupled with the paleobotanical evidence, there is also paleobiological evidence of the exploitation and eventual extinction of a number of native bird species including 2 parrots, 2 rails, a heron and an owl. The chronology of these ecological resources on Easter Island, both wild foods such as seabirds, native birds, and plant resources, show an initial abundance followed by decline and extinction of local species.

By the 1700s and the arrival of Europeans, the local population had decreased markedly. From estimated peak numbers of anything up to 30 000 people[25], by the 1700s housing sites had decreased by 70%, and

[25] Estimates for the highest population levels in Easter Island history range from 6 000 to 30 000 individuals. 6 000 is probably far too low however, given the food

when missionaries documented the population after a small pox epidemic in the 1860s, there were approximately 2 000 individuals.

Post the peak occupation period and the end of the Moai construction period, with local resources running short, archaeological evidence reveals increased signs of cannibalism among the island's inhabitants, with long bones cracked open for the extraction of marrow. This is also back up by ethnographic evidence in the form of interviews with Easter Island inhabitants at various points.

Thus, from a number of sources of evidence, we have a chronicle for Easter Island; A settlement period; A consolidation period with an increase in population and social stratification; A Moai building period, with increased pressures on resources leading to the extinction of various local species; and finally a period of lower population numbers, cannibalism and dietary stress.

This is the physical evidence from Easter Island as a chronicle. As we can see, we use a variety of models to construct this chronicle.

However, we now want to explain this chronicle. We have a series of transformations in the central subject, the culture of Easter Island, and what we want now is an explanation for those transformations.

9.2.2 From potential processes to narrative

Upon examining evidence from Easter Island, a variety of initial hypotheses might be proposed for why it changed over time: Endemic warfare, environmental overexploitation, environmental change or some other reason. We can presume that all of the hypotheses are reasonable, given our understanding of contemporary and historical societies, and in many cases, all these hypotheses have been proposed by researchers at one time or another. They are part of our general social science background theories.

What is interesting about Diamond's approach is that he argues that these hypotheses may not be mutually exclusive. They may in fact be mutually reinforcing, and the historical scientist must instead disentangle the relative timings and weighting of the processes involved. Was the

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available during the peak occupation period and the manpower required for the construction of Moai.

environmental exploitation of Easter Island such that it promoted inter-group rivalry? Alternatively, did a prolonged inter-group rivalry promote environmental over-exploitation? Was environmental change a significant factor, or was environmental change merely the coup de grace for a culture already in decline anyway?

Historians dealing with the complex interplay of events, processes and a variety of timings must put together an account that can accommodate the intricacies of quite contingent processes. In effect, while lots of the individual contributing processes may be well-understood components of our body of scientific knowledge, the particular mix for any situation may well be unique. This is the nature of a complex history.

In constructing a narrative, our historical hypotheses may well focus on processes or trends in history, rather than historical events. A historical scientist may frequently propose as a hypothesis a complex interplay of processes, with multiple starting conditions, causes, and downstream consequences.

Now, each of these hypotheses will have expected consequences which may be detectable; changing environmental data for environmental change and overexploitation, increased fortification and weapon production as archaeological signatures of endemic warfare. A hypothesis for changes between states identified in our chronicle should make predictions about what sort of evidence we should see in a particular case.

Thus, we have differing types of hypotheses, all of which we assess on the basis of evidential traces. We have hypotheses about what happened that we test using multiple lines of evidence. We also have hypotheses about *why* things happened and the underlying causal processes that transform the central subject of a narrative. These second kinds of hypotheses also use multiple lines of evidence. Both sorts of hypotheses use regularities and models that we can study in contemporary contexts.

However, regularities are generalities: they apply across multiple situations. To turn these generalities into specific accounts of the past, and to construct our unique narrative, we must work out the unique mix of processes involved.

9.2.3 Tacking

In the Diamond case, there is a tacking procedure similar to that outlined in the previous chapter. A general regularity is suggested as a possible cause for a change in states in a chronicle. This regularity makes predictions about further evidence.

However, rather than a single hypothesis, Diamond outlines five potential factors that he thinks operate to collapse, or stabilise, societies: Environmental Damage, Climate Change, Hostile Neighbours, Friendly Trading Partners and Cultural Responses (Diamond 2006 p11). Diamond in essence starts with these as possible contributing factors as a set of background theories. His initial hypothesis is that all, or a combination of these processes, will be operative in all cases, and a distinctive mix of processes in a particular case.

In the past, debates have been about which one of these five factors accounted for the disappearance or decline of a society. Diamond asks a different question; he asks about the relative role of the factor, and assumes that all five may or may not be operative in any particular case. The choice of which factors might be involved in any particular case come down to how well they fit the evidence.

That evidence is what one would expect to see, given the factor involved. In effect, a hypothesis that a particular process may have been operative predicts new lines of evidence. The factor of climate change makes claims that one should see physical consequences of climate change such as changing pollen in the sediments of swamps. Increased warfare among neighbouring groups should have physical consequences in the form of increased fortification and tools associated with warfare in the archaeological remains.

Diamond can work through the full range of physical evidence, refining the role of each of the potential transformative processes in the particular case under study. He is quite happy to downgrade, or even discard in some cases, one of his potential processes should evidence for it not appear. He is even prepared to mix and match the causes, giving priority to one cause over another in one situation, and reversing that priority in another.

In the Easter Island case, Diamond can see clear evidence of one of his five factors: environmental damage in the form of decreases in local resources through over-exploitation. However, he also sees this as working hand in hand with another factor, the cultural response of the Easter Islanders. Diamond sees the construction of Moai as contributing to environmental damage because of its demands upon supplies of wood for construction which led to the loss of habitat for native species, and also the loss of materials for other subsistence needs such as wood for fires and the construction of canoes for fishing. Easter Islanders could not then shift their subsistence strategy from exploiting land based wild foods to offshore fishing, aggravating their impoverished position.

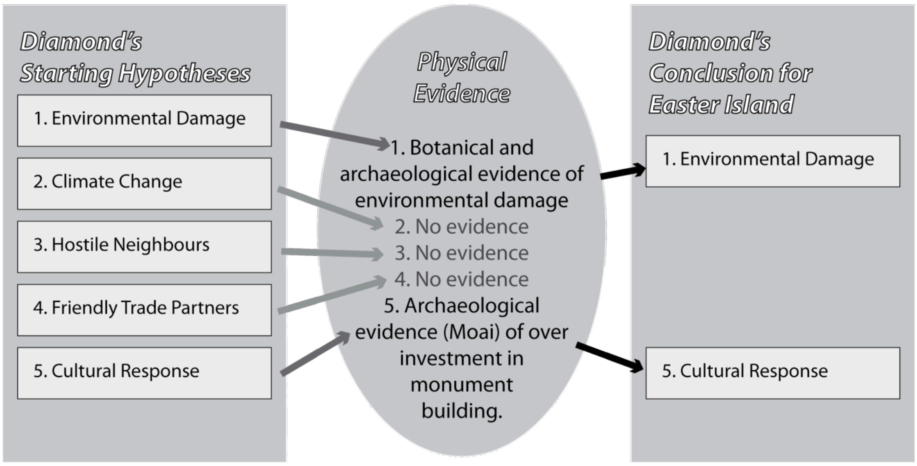

Figure 9-3 The research process in the Diamond case. The column in the far left is the list of factors that Diamond thinks could be relevant to any case of societal collapse. The central oval is what we see as evidence in the target system, in this case, the Easter Island evidence. The column to the right is the conclusion Diamond reaches once the tacking procedure is worked through. In this case, the final account only includes 2 of the 5 possible factors.

The tacking procedure transforms Diamonds initial set of 'possible' processes of change into an idiosyncratic and unique account of a particular situation. Of course, some researchers may dispute the evidence he has for some processes, and his interpretation on some occasions is open to question. Nevertheless, it is the physical evidence that is used to confirm or question his hypothesised process.

Thus far, we have been interested in how we can determine the relevant mix of transformative processes operative on a central subject, and how we can turn generalities into the specifics of a historical case. As we have seen, the procedure is the same as that of determining signal from noise that we discussed in the previous chapter. We start with multiple models, and adjust or discard them based on how the measure up to the physical evidence.

However, this is only one of two problems to be dealt with. A narrative is a causal history, and as such, it must take into account the temporal order of processes. A process may well be operative initially, but decrease over time, and be replaced by another process. To illustrate this temporal aspect of constructing narratives, I am going to turn to a different science for a source of examples.

9.3 The complexity of history

Geomorphology is the geological study of landscapes, both the processes that change them, and how they form. Geomorphology has an applied contemporary component, where geologists look at landscapes in relation to large projects like dams, or potential flood zones and other interactions between human activities and the geological world. However, it also has an historical project; explaining landscapes and how they came to be the way they are.

Because of these two distinctive strands of research, there is a division of labour between those who understand active contemporary processes of landscapes, and those that look at the past. Some see this division of labour as a distinctive split in the discipline:

> Geomorphology [as a practice has] split into two mutually exclusive strands: mechanistic process studies and qualitative narratives of landscape development which stress the role of contingency. (Harrison 2001 p335)

To make the distinction of Harrison clear, I will talk of mechanistic geomorphology, and historical geomorphology.

The problem confronting historical geomorphologists, and other historical sciences, is the construction of narratives that account for historical contingencies —those "accidents of history"— that generate unique results. These processes do not overlay each other in a straightforward fashion. Subsequent processes erase and distort prior processes. Prior processes set key variables for subsequent processes. As we saw in the previous chapter, post-depositional factors are a source of noise for the signal of interest. However, in this case, the post-depositional 'noise' that obscures or distorts a signal of interest, is in fact a signal of a later process that is of interest in its own right. Later erosion may well destroy prior deposition, but erosion is of interest to the historical geomorphologist.

However, prior processes of deposition of sediment may have consequences on the rate of erosion. The sedimentary substrate may have certain properties that impinge upon the rate of erosion. As the erosion process proceeds, it may work through a softer substrate, to a substrate that is hard, changing the rate of sedimentation lower in the system, as well as its nutrient levels, and so forth.

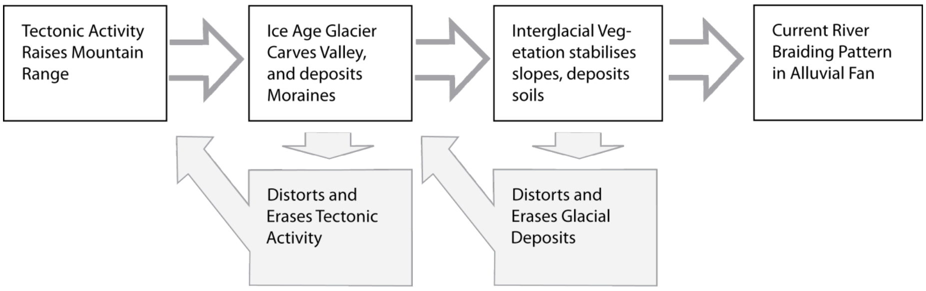

Figure 9-4 As in archaeology, historical geomorphology is interested in constructing a narrative for the past that explains the contemporary evidence, in this case, the braiding pattern in an alluvial fan. Various prior processes, from the initial tectonic activity that raises seabed sediments, to ice ages and glacial valleys, and vegetation changes, change the composition, shape and other qualitative features of the landscape. Each process also distorts and erases the prior process, and sets variables for subsequent processes.

Mechanistic geomorphologists have developed a number of tools to understand contemporary processes. So, historical geomorphologists have available well-confirmed general models of geological processes. However, for the historical geomorphologist, these models have to be applied within a quite distinctive historical context. The sediments of an alluvial fan are evidence of erosion, but equally, they are ramifications and downstream consequences of glaciation earlier in the geological sequence: They are traces an even earlier event. Consequently, in understanding erosion, the geomorphologist has to account for the prior glaciation process, and how that process created boulders, clays and other sediments that later eroded during the interglacial.

The tacking procedure we outlined in the previous chapter presumed that the researcher moved between a general model as a starting hypothesis and the physical evidence for that model. The general model made predictions about what to expect, and what variables to look for. On examination of the physical evidence, the observed variables are used to refine the model and to turn it into a particular account. The process localises the model.

However, in some cases, the general model of a process won't provide all the variables necessary to account for a particular transformation. However, a refined model from an earlier process may well provide some key variables for a subsequent process. A localised model of prior deposition may well provide important information that helps localise a model of subsequent erosion. We can better modify a general model to account for historical contingencies by taking into account the historical context of the process of interest.

Making sense of the context of a particular event by accounting for what has come before is particularly important in evolutionary scenarios. Evolution by natural selection modifies a species, and does not design new species from scratch. In explaining an adaptation, we often provide an account of a transition between states but saying how the subsequent state improved fitness in comparison with the prior state: We compare the fitness of organisms without an adaptation to organisms with an adaptation. By this means, we can explain why an adaptive trait spreads through a population and displaces the prior population.

The arrival of Polynesians in New Zealand outlined earlier is another case where the historical context matters. A model of resource exploitation that did not account for the prior history of Polynesians being on Islands without clay, would fail to understand why Maori did not exploit ceramics in a country rich in the relevant resources.

In building a historical narrative, what has gone before is an important consideration in understanding any particular node in a series of transformations. So in the construction of a narrative, the tacking procedure is not just one of moving between physical evidence and a particular cause: it is one of tacking between a hypotheses about a particular event and its evidence, and further tacking between a hypothesis and its place within a historical sequence. The model of a particular process has to be modified in accordance with the physical evidence *for* that process, and models of prior and subsequent processes.

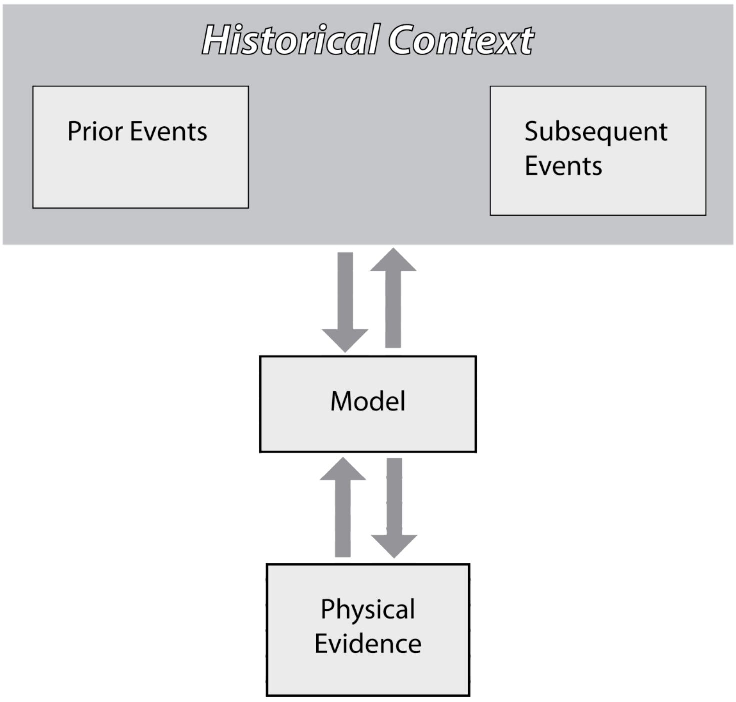

Figure 9-5 In the process of using a model within a narrative, there is two tacking procedures. One is modification of a model in response to physical evidence. This takes a general model, and turns it into a localised account. However, there is also a tacking procedure between a model and the historical context. The historical context provides historical "set up" conditions, and takes into account subsequent processes that may erase prior traces. The historical context may well be evidence of various sorts.

There are then two refinement processes going on in the construction of a narrative: The relationship between different processes, and the refinement of individual processes to account for its context within the larger narrative. It is a three-place movement between physical evidence, a model that accounts for a particular subset of evidence, and a narrative that encompasses the transformations that link sub-sets of evidence.

9.4 Summary

The testing of narratives is closely allied to the construction of chronicles that we saw in the previous chapter. The historical scientist starts with general models of regularities that may well be testable as tokens of standard processes. These models are then tested against the contingencies of history via the physical evidence available, through a process of tacking between evidence and hypothesised processes. The tacking process generates a particular account of a process.

In many situations, there may well multiple processes operative. Some of these processes may well be noise, obscuring processes of interest, or there may be multiple processes operative on a single subject of interest in a narrative. For instance, there may be an underlying trend that is aggravated or retarded by another process. In either case, the methodology is the same. The researcher can select a range of general models that can act as hypotheses, and through a tacking procedure, transform the initial models to a unique mix of processes.

Further, where prior processes set key variables for subsequent processes, the historical scientist must include within the reflexive process of tacking prior processes that shape the starting conditions for subsequent processes and account for the historicity of the process.

The result is that a complete narrative of a particular historical subject will document a unique combination of processes, a unique temporal ordering for processes, with each individual process within the narrative being a highly localised model of a particular process.

In the next chapter, I will work through a contentious case in historical science: the extinction of the North American megafauna. In my view, the reason this case is contentious is because the historicity of processes and the need for context has been ignored. A single model has been applied without recognising the need to include additional models that account for underlying trends and additional contributing factors.

10. In Through the Out Door: The Extinction of the North American Megafauna

This chapter will examine a case study where historical and local contexts matter to the application of a model to a past event. The idea here is that we can have a good model that has a great deal of support from contemporary sources, but that a simple application of the model to a particular case, an application that ignores historical and local context, can get things wrong. Or at least, it can be misleading.

In the last chapter, we saw how Jared Diamond started his investigation of the collapse of cultures with a number of possible causes. Through a tacking procedure, he was able to come to conclusions about the relevant mix of causes that were operative in any particular situation. In the Easter Island case, he came to the conclusion of five possible contributing factors —environmental damage, climate change, hostile neighbours, friendly trade partners and cultural responses— only two, environmental damage and cultural responses, were contributing causes to the decline of the Easter Island population.

In the case study we are going to examine here, I argue that the debate over the North American megafauna extinction has foundered because on one side of the argument, only *one* potential cause has been examined. The presumption of the people arguing for a human caused extinction has been to focus explicitly on one model as a possible explanation, ignoring the historical context other contributing causes.

In contrast, those who have difficulty with the human cause model do not doubt that human hunting played a role. They do however take into account other possible causes, and attempt to integrate these into their understanding of the past. The megafauna extinction debate provides us with a good case study of the importance of historical context, and multiple causal factors that interact.

To start the chapter, I will set up the problem to be explained: the extinction of various megafauna. I will then outline a particular model that is supposed to account for this extinction, a model based on the arrival of human's to new areas of the globe: I will call this the overkill model.[26]

I will then focus on a particular application of this model to a particular case: the extinction of the North American megafauna at the end of the last Ice Age. The reason to focus on this case is two fold. First, there is simply more evidence available than elsewhere. The Australian case in particular is hampered by a paucity of physical data, and controversy over what is available.[27] More importantly for our purposes, the point of this chapter is to illustrate why historical context and geographical particularity matters when constructing narratives of the past. We want to explain a particular set of evidence, and a particular past event, so in working through a case study, we need to have a particular historical event to work through.

Once the North American case and its problems are presented, I will work through some of the sceptic's arguments against the North American case. In contrast to Diamond's investigation of social collapse, where many models have been proposed, much of the debate over the Megafaunal extinctions have been overly focussed on one model —the role of humans— ignoring the historical and particular contexts that provide the complete picture. In response to this scepticism, the human cause model has not remained static. In fact, a tacking procedure has modified the model substantially. However, because *context* has not been taken into account, and because other contributing causes have not been considered, the model still does not accommodate all the particularities we would wish.

We will discuss an alternative view that accommodates multiple causal factors in the fourth section. This multiple view takes human's as one of a

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[26] The term overkill is not mine, and as far as I can tell, emerged in the1984 Martin and Klein edited collection Quaternary Extinctions: A Prehistoric Revolution (Martin and Klein 1984). The Martin and Klein volume remains an important reference work in this debate.

[27] In fact, the small size of the Australian research community, coupled with the lack of evidence, has meant that the Australian debate is heated, and at times rather personal. An good insight into the Australian debate, an some of the leading players, can be found in the extended abstracts for the 2007 Selwyn lectures for the Victoria Division of the Geological Society of Australia (Cupper and Gallagher 2007).

number of contributing factors. It may well be true that human's killed the last remaining breeding pair of mammoths, but this should be seen in context of climatic and faunal changes that lead to the decline of mammoth and other megafauna.

History, in my view, is frequently messy. In real world systems, multiple processes interact in complex ways. Single cause models of change are unlikely to capture this complexity. This leaves them open to questioning, as they frequently cannot account for all the physical traces of the past. While the extinction of the megafauna in different continents and at different times may have much in common, a single model does not work at a fine enough grain of detail to account for the idiosyncrasies of particular cases.

The lesson we learn from the megafauna extinction debate is the importance of historical context and background conditions. To understand what has gone on in the past, we need to place an event within a historical context, and understand how a particular causal process interacts with other causal processes. My view is that the fundamental error made by partisans in the megafauna extinction debate is to argue for a single causal process across a number of cases, and to ignore the idiosyncrasies of particular cases.

10.1 The explanatory problem: The end of an Era

The first thing that we need to set out is the explanatory target that the model of change is supposed to explain. In this case, the explanatory target is a chronicle of changes in fauna.

Fossils and the rock they are embedded in are diagnostic of a geological period. Historically, this is because during geology's formative years researchers attempted to reconcile changes in geological strata with religious texts, and the thought was that such changes represented repeated catastrophes, floods or other events that decimated older populations of organisms, to make way for new populations. Catastrophism maintained that the history of life on earth was the story of numerous catastrophes, the extinction of monsters and dragons and the repopulation of the earth by new species (Rudwick 1972). Thus, like other geological eras, the end of the Pleistocene and the beginning of the Holocene, approximately 10000 years ago (10kya), is marked by a change in the fossil fauna in the geological strata.

However, unlike a faunal change evidenced in major geological eras such as the Cretaceous-Tertiary boundary, where there is a turnover of organisms, some going extinct, and novel species emerging, the characteristics of the Pleistocene-Holocene boundary is a number of extinctions, but nothing much that was new. It was a faunal change with little, if any, new species to replace the old. It appears that it was less of a turnover of species, and more of a selective culling. What is more, the culling was disproportionate in different regions: very few in Africa, some in Eurasia, but notable disappearances in the Americas and Australia.

The terminal Pleistocene and beginning of the Holocene also represents the time since the last glacial maxima (LGM), one of a series of cooling events throughout the Pleistocene with accompanying ice ages. Approximately 10kya during this glaciation, the polar ice caps of the northern hemisphere extended well south, locking up vast quantities of water. Because these increased masses of ice locked up vast quantities of water, there were lowered sea levels, with various land bridges that linked Eurasia and North America, England with the continental mainland, and Tasmania, mainland Australia and New Guinea, into larger landmasses.

At first blush, the extinctions that occurred appear to be rather odd. They tend to be big organisms, and mammals. In fact, precisely the kinds of large homeotherms —large animals that can regulate their own body temperature— that one would think would be in a position to survive an ice age, the last glacial maxima, that marked the terminal Pleistocene, and be ready to flourish once its over. In fact, many of the creatures that went extinct had already survived the turbulent changes of the Pleistocene with its repeated expansion and contraction of the polar ice sheets.

After surviving repeated glaciations throughout the Pleistocene, a whole raft of large North American and Australian mammals went extinct, and no new things emerged. What made the last Ice Age so different? Well, it is not quite true that no new creatures emerged. One organism did start to appear in North America and Australia around the same time: *Homo sapiens.* Throughout this period, the range of *H. sapiens* was expanding. Emerging in Africa approximately 100kya, *Sapiens* pushed into Europe, displacing other Hominins in Eurasia. They then moved into precisely those continental landmasses where the extinctions occurred: The Americas, and Australia.

And therein lies the controversy. Given that these events all occurred at approximately the same time —the extinction events, the arrival of

Homo sapiens, and a major climate shift in the form of the last glacial maxima (LGM)— we are entitled to think that these events are not unrelated. The question is; how are they related? There are two camps. One says that the LGM was something of a red herring. It might well have helped *Homo sapiens* get to these new continents, but the LGM was not the direct cause of the extinctions. After all, these large organisms that went extinct had already survived numerous glaciations throughout the pleistocene. No, the real cause for the extinctions was the arrival of human beings. Others are not so sure. The alternative to the human caused view says that its human arrival that is the red herring. The extinctions are tied to the ice age of the last glacial maxima.

10.1.1 A Caveat and the topic

I want to examine the megafaunal extinction debate in the light of just one continent, North America. The reason for this is partly to reduce complexity, and as we shall see, complexity has an important role in all this. However, it is also fair to say that the North American case has more evidence than elsewhere, and certainly more evidence than the other touted great megafaunal extinction, that of Australia.

The strategy will be to set up the human cause "model" initially, a model we shall come to know as "overkill." As an account of the megafaunal extinctions in North America and Australia, the model has a great deal of indirect support from various sources. I say indirect support, as the model itself seems to apply to a number of cases outside of North America and Australia, and it is a variant of another model of biological interactions. Intuitively the model seems right.

The problem is, however, that it has difficulty matching up with the evidence. The only piece of evidence that it matches up with is the disappearance of various organisms.

10.2 The Model and its Support

In 1967, Paul Martin and Herbert Wright edited the proceedings from a conference that was to be very influential (Martin and Wright 1967). The topic of the conference was the Pleistocene extinctions, and in it, Martin argued that the primary cause of these extinctions were *Homo sapiens.*

The idea here is simple enough. Humans would effectively prey on certain larger organisms to the point of extinction. This model has two advantages. One is that the model is fairly well understood. Essentially, it is a modified predator/prey model. In the standard situation, the predator population is supposed to decrease as encounter rates with a decreasing prey population go down. The result is an offset oscillation of predator and prey population levels.

The modification to this standard model comes because humans are supposed to be able to alter their behaviour in ways that ameliorate the decreasing encounter rates. An increase in the efficiency of resource use means an increased return from successful encounters. Increased diet breadth reduces dependence upon a single prey species. Improved technology and co-operation increases both encounter rates and capture rates. These factors in concert may drive a species to extinction.

As Donald Grayson points out that this is what we would expect given an application of foraging theory to humans moving into a new area.

> Foraging theory thus predicts that, in the face of heavy human hunting pressure and in the absence of conservation practices, larger vertebrates will in general decline in abundance more rapidly than smaller ones, and that as this occurs, greater numbers of smaller vertebrates will enter the diet. (Grayson 2001 p7)

The model then in its own right is straightforward, and not too difficult to understand. In its application to extinction events, it frequently gets the title "overkill." It is the standard predator-prey dynamic gone wrong.

10.2.1 The Overkill Model

Now, we can state all this as an abstract model, and we can see how it is a variant of a predator-prey model. But how much support does the Overkill model have?

In fact, a good deal of support comes from observations of contemporary, and documented, extinctions. These are mostly Island extinctions of various fauna, in particular, the extinction of Moa (The *Diornis* genus) in New Zealand and extinctions on Madagascar. New Zealand is particularly important for two reasons. Firstly, there is very good evidence of the process of the extinction of the Moas. Secondly, New Zealand is comparatively large, so can more closely model a continental extinction.

However, the model has other forms of support as a general process. A great number of extinctions have occurred on Islands, or in situations where humans have overexploited various organisms. There are well-documented cases of human's driving large organisms extinct.

10.2.2 From Overkill to Blitzkrieg

The Overkill hypothesis in its extreme form has gone under the nickname "Blitzkrieg." In this version, humans encounter a naive fauna, with no natural fear of human beings.

> Critical to almost all models that support human causation is a prominent role for prey ‘naivety’. Because they lacked human-specific anti-predator responses, naive species fell easy prey to human colonisers. Analogy is drawn with remote island taxa that are indeed pathetically vulnerable to human predation. (Wroe 2005 p10)

Consequently, humans armed with a reasonable technology wipe out a naive fauna extraordinarily quickly. The humans, arriving in a new continent, form an advancing wave of extinctions.

So, that’s the general model; Overkill, and an extreme version, Blitzkrieg, which makes additional assumptions a naive fauna that allows the extinctions to take place rapidly. Now to the details of the North American case.

10.3 The Hunter's and the Hunted

In North America, the Overkill model in its extreme form, Blitzkrieg, is supposed to account for 35 genera of organisms: including herbivores and their associated predators (Grayson 2001 p35).[28] This includes Mammoths, a North American Bison (*Bison Priscus*), Giant Ground Sloth, and various other large, and seemingly edible organisms, plus their predators such as Sabre Tooth tigers and Dire Wolves (*Canis Dirus*). A not insignificant number of species, and as one would expect given this is a

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[28] In north America, the extinctions were four genera of giant ground sloth, two genera of tapirs and horses, two genera of peccaries, two genera of deer-like animals, two of pronghorns, a rabbit, the various species of mammoth and mastodon, and the associated predators of these species; the dhole, two bears, three cats, and a skunk. (Grayson 2001 p35) And note; this is a count at the genus level, not at the level of individual species.

debate about the extinction of the megafauna; most of the relevant organisms are large mammals, typically above 100kgs in estimated body weight.[29]

So, who were the putative killers of this ensemble of creatures?

The Clovis culture is so named because of some distinctive tools, essentially heavy spear points, found near Clovis, New Mexico, in the 1930s (MacNeish and Kislak 1973). The initial impression of the Clovis culture is one that supports the Overkill Hypothesis. Clovis is a brief culture of tools, only lasting some 300 years, but for all its brevity, it is widespread across the North American continent. Typically, the dates for Clovis are around 13.2 kya to 12.9 kya.

Clovis points look like killing points. They are heavy, crude, but possess fluting on the sides that would allow for the profuse bleeding of an injured animal. In short, the wielders of Clovis points look like the guilty party for overkill. They had the technology, they were at the scene of the crime, and they represented a brief flurry of activity.

Tim Flannery certainly sees Clovis culture as having the hallmarks of a group responsible for the megafauna extinctions. The evidence as he sees it

> …suggests that the Clovis people occupied the entire continent within a century or two and that the impetus for manufacturing their spear points existed over the entire region. This and the brevity of the culture's existence strongly suggest that Clovis was a pioneer culture. The fact that functional tools comprise almost all that Clovis people left to posterity also speaks eloquently of a frontier existence. (Flannery 2001 p183)

Flannery interprets the Clovis culture as one engaging in a highly

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[29] The precise definition of the term 'Megafauna' does not matter too much in the context of the subsequent discussion here. In fact, what counts as "Megafauna" differs somewhat from author to author. 100kgs is an arbitrary figure that probably represents a good average definition, and makes the megafauna typically bigger than average *Homo sapiens,* and captures within the definition primarily the large glamorous extinct organisms such as Mammoths. Others have put the figure lower at 44kgs, and others have suggested that the definition be on a within genera basis; Megafauna are the biggest members of a closely related group.

mobile lifestyle, leaving little behind but their hunting tools. He interprets this against a backdrop of other pioneer cultures, with little time for the settled luxuries of art, agriculture and adornment.

10.3.1 The Sceptics

The North American overkill model remains to many *the* explanation of the extinction of the mammoth, mastodon, and various other large organisms. Ask someone with even a vague professional awareness of the issues, and they will tend to back the Overkill hypothesis, and probably the extreme Blitzkrieg version. Partly this is because it is memorable, and partly due to the fact that the model, this modified predator-prey model, is well known, and once people think in populations, fairly intuitive. On first blush, it makes sense. Humans are one of the current causes of animal extinctions, and we can expect them to have been historically as well. There is no reason for them not to be a prehistoric cause of extinctions.[30]

However, as Grayson notes, the overkill hypothesis is…

> …widely accepted by superb ecologists whose research focuses on contemporary organisms, often in other parts of the world. It is also an argument that most scientists fully versed in the relevant archaeology and paleontology firmly reject. (Grayson 2001 p35)

According to Grayson at least, despite the plausibility of the model, and its applicability to various cases, it is not a widely accepted account in the North American case. Why? And in the face of this general scepticism, how is it that the overkill hypothesis stays on the table? Clearly, the simplicity of it plays a role. Overkill is 'psychologically plausible,' memorable, and in other contexts, the best explanation on offer, and probably the right one in many contexts. The extinction of various Island species by 18th and 19th century Europeans arriving on Islands are testament to that.

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[30] I have to count myself among the individuals who accepted this model of extinctions. It was only interactions with the Australian archaeological community that made me aware that there were real problems. My acquaintance with a historian of Australian geology, Kirsty Douglas, finally convinced me that the megafauna extinction debate was worth examining in detail.

To explain what is going on, it is worth going into some detail with the Overkill model's problems, and comparing this with its strengths. Its strength is its simplicity, its unity and its accessibility. Its weaknesses are its failure to account for the fine grain details of a particular historical case.

10.3.2 Problems with Blitzkrieg

The first piece of actual history that raises problems for the Blitzkrieg model is that it is not quite so clear that the Clovis people are the first human beings in the Americas. The defenders of the Overkill hypothesis, and the Blitzkrieg version in particular have remained sceptical about the evidence for pre-Clovis settlement of the Americas. Nevertheless, the evidence is accumulating that there were people in North America before the Clovis culture, possibly as early as 2000 years before the Clovis culture emerged (Marshall 2001; Falk 2004). If this is right, then some of the intuitive force of the Overkill hypothesis is now missing. This is certainly the case for the extreme Blitzkrieg version of the hypothesis. The naive fauna that the blitzkrieg model requires might not be quite so naive. Admittedly, the existence of a pre-Clovis culture isn't a knock down argument against the Overkill hypothesis. One can still argue that is it was a culture armed with a particular technology that invaded North America, rather than humans per se. Nevertheless, if humans were around for two millennia before the Clovis culture, that seems plenty of time for them to do some damage to the local fauna, and certainly time for a naive fauna to get used to *Homo sapiens* wandering around the place trying to eat them.

Further, evidence for even the Clovis culture butchering the megafauna is slight. There is some evidence for the hunting of mammoth and mastodon, but little or none for anything else. The continent wide case is circumstantial rather than a direct evidential "smoking gun." Some take this as evidence of the Overkill hypotheses weakness. In the New Zealand example there is ample evidence of butchering activities.

However, in the North American case it has been argued that a fast Overkill process won't leave much evidence. Part of the motivation for the strong version of Overkill in the form of Blitzkrieg is in part to suggest that a quick and rapid extinction will not leave much in the way of evidence. Archaeological preservation is patchy, and brief periods of time mean less chances of good preservation of evidence. Plus, the highly mobile Clovis people were not the sort to leave much evidence anyway.

On this view, the lack of evidence is a good thing; lack of evidence is strength of the Blitzkrieg hypothesis, not a weakness.

At this point, some people would baulk, and suggest that the overkill hypothesis is in trouble. Suffice to say at this point that lack of clear evidence is seen as problematic by people on one side of the debate, as it smacks of an ad hoc hypothesis, but is argued away on the other.

There are other problems with the human element of the overkill hypothesis as well. Take into consideration the following bit of everyday data we have about the world; bullfighting. Bullfighters typically enrage the bulls through wounding them first. This is the picador's job: A man on horseback who wounds the bull with a metal tipped weapon. The bull doesn't die. It does however get very annoyed. And a bull might weigh a ton or so, and in such an enraged state, is undoubtedly dangerous. Now take this piece of information gained from contemporary observations, and apply this to the past. A mammoth might weigh anything up to 6 tons. And its not metal tipped spears being thrown from horseback that are annoying it, it stone tipped spears thrown by men on foot. And this is not happening in a bullring, where there are many places for the men to hide from their enraged prey. As Wroe et al note, that while we might accept Overkill in the New Zealand case..

> Its one thing to efficiently dispatch 75kg moas with wholly wooden spears or clubs, its another to kill 6,000kg *Mammathus primigenius*. (Wroe, Field et al. 2004 p306)

For a group of humans, even with extraordinary co-operation, taking down a large elephant like organism is not a nice safe option. Its dangerous with very real risks involved. The history of human warfare suggests that the best strategy when faced with elephants on a battlefield is not to try to annoy the elephant; rather, the best strategy is to take out the human drivers (Archer, Ferris et al. 2003). Organisms that big are just hard to kill. What's more, mammoth and other megafauna would be used to protecting their offspring against various specialist predators, and would be hard organisms to take down.

There is no guarantee that the life of Overkill is a life of ease and luxury, as humans happily barbecued their way through dumb livestock. And given the difficulty of large game hunting, it seems unlikely that they would neglect other options. There is evidence to suggest that in North America that they did hunt large game, but other organisms are going to look attractive options for hunting as well. Indeed, another model comes into play at this point, and that is the optimal prey size model. As prey gets larger, the risks of damage to the predator is such that the additional pay off of increased size decreases. Optimal prey size models make good predictions about predators preferred prey, but the human case is slightly different. Humans have nasty habits about not picking the optimal size, or engaging in the optimal strategy. Nevertheless, the increased protein acquisition of a six tonne organism is offset by the risks: increased payoffs have decreasing utility for predators with no storage facilities.[31]

The final point to make here is the naive fauna component of the hypothesis. It assumes that megafauna are naive about humans. Possibly, but the theme emerging here is whether they are naive about human technology. A group of humans engaging in a close quarters attack is one thing. A group of humans utilising fire, ranged weapons, and potentially metal tipped weapons with poison, is an entirely different kettle of fish.

The point to take away here is simply this; the overkill hypothesis gets some of its plausibility by not taking into account the detail of what is required to make it work in any particular case. A great deal of the overkill hypothesis makes assumptions about the technology and culture of the humans concerned. Humans did slaughter the American Bison in large numbers: When armed with guns. And humans continue to slaughter various species in large numbers over and above their immediate need: When armed with preservative technology that meant that immediate excess is not wasted. Humans have to possess the means and the motivation to engage in overkill on a continent wide scale. Humans can do this, but they need time, and they also need the technology. Whether they had the time and technology to do this in the North American case is unclear. The initial psychological plausibility breaks down once we think through the process in more detail. The applicability of the model is less clear.

10.3.3 The problem of dates

There is also a lurking technical problem with our chronology that should be noted: that of dates for events. Most dating methods come with margins of error, so their resolution is not great. However, nuclear dating

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[31] Storage technologies such as refrigeration and preserving make larger organisms much more attractive options.

methods such as Carbon dating have a greater margin of error the older they are. Because they rely on the half-life of a particular element, older samples tend to have barely detectable quantities of the relevant element. At these lower levels of concentration, random statistical variation starts to play a greater role in how much of the relevant material is present. The result being that *any* presence of a trace element can provide a date, and that older dates with the lowest levels of concentration tend to cluster around the maximum date possible, but with very wide margins of error. And for Carbon dating, at what period is the margin for error the greatest, and where are dates most likely to cluster? The terminal Pleistocene.

What this means is that there is good reason to be sceptical about chronologies based on carbon dating. The dating technology currently available is not fine grained enough to provide the accuracy required. Precise chronologies then, are not going to help one side or another, although improvements in technologies might help in the long term. In the North American case, the chronology is not as detailed as we would like. The explanatory target is a crude chronology that clusters the terminal pleistocene, the arrival of humans in North America and the extinction of the megafauna within the same time frame.

10.3.4 Overkill, Blitzkrieg and Sitzkrieg

The Overkill hypothesis, and the extreme version, Blitzkrieg, does not really stand up to close scrutiny. It does not account for details of particular situations. And in point of fact, the exemplar case, New Zealand, doesn't really look like a blitzkrieg case either. The problem is that if we only focus on the megafauna, we miss what is going on a wider scale.

In New Zealand, it is clear that the Moas were wiped out by humans, and that they left extensive evidence of this fact. But its important to note that at the same time as the Moas were going extinct, so too were a number of other organisms. Ironically, one of the chief supporters of a blitzkrieg model for North America, points this out in the New Zealand case.

> Recent fossil finds indicate that as little as 800 years ago frogs were once the dominant life form in many of New Zealand's forests, for their fossilised bones have been found by the tens of thousands in some fossil deposits. The mossy forest floor probably crawled with them. It seems probable that their dramatic decline was brought about by the kiore (the Maori name for *Rattus exulans*), which reached New Zealand with the first Maori. …some New Zealand biologists think that soon after their arrival, kiore may have formed plagues of such vastness that they have never since been rivalled. In a few brief years they may have stripped the forests of their frogs and other fauna. (Flannery 1994 p54)

The picture that emerges here is that the exemplar case of overkill, is a more complicated picture in its own right. It not just the large flightless Moa that is going extinct, it is also smaller organisms too. In fact, the Early Polynesian settlers of New Zealand brought not only rats, they also brought dogs, and they probably engaged in a reasonable amount of fire assisted land clearance to prepare the way for crops. The New Zealand case then looks far more complex. Diamond argues that the New Zealand case is the result of an interaction of processes he describes as Sitzkrieg: a general human caused impact, rather than the early Polynesian settlers eating their way through the megafauna.

10.4 Finding the Difference Maker

Part of the difficulty here is that of finding the difference maker. To see this, take a case put forth by Stephen Wroe, whose concern is that disentangling causes for the megafauna extinction might well be rather difficult.

> To illustrate this argument I refer to a study of localised extinction among Caribbean island lizard populations. In 2001, Schoener et al. found that a naturally invading predatory lizard (*Leiocephalus carinatus*) increased the risk of extinction for a smaller species (*Anolis sagrei*) in the wake of a major hurricane. Despite serious population crashes, *A. sagrei* invariably survived where the invasive predator was absent, but went locally extinct on most islands occupied by the new predator. (Wroe 2005 p8)

The example that Wroe provides here is analogous to a modified account of the Overkill. An environmental factor, in this case hurricanes, seriously impacted on a population of organisms. A novel predator, in this case a lizard, caused the extinction of these organisms where it was present.

But note that while the presence of the larger lizard *L. carinatus* is a necessary condition for the extinction of the smaller *A. sagrei,* it is not actually clear from the information above whether it is a sufficient condition, and that matters if the general model of Large Lizard causes Small Lizard to go extinct is to hold. After all, the counterfactual, 'If there wasn't *L. carinatus* on the Island, then *A. sagrei* would not be extinct' is true for all Islands where *L. carinatus* is present. It is just not a sufficient condition. A disturbance, in this case a hurricane, is required as well.

So might it be the case that there is a similar necessary condition over and above human hunting for extinction to occur in the North American case?

10.4.1 Faunal Interchange and Changing Environments

In fact, two possible contributing factors may have provided the necessary background for the megafauna extinctions.

The first factor is the event that got the Clovis hunters to North America in the first place. In periods of glacial maxima, a land bridge, (Beringia) exists between Eurasia and North America and then a corridor between the Laurentide and Cordelleran ice sheets connects Alaska with the rest of North America. This has been a route for many faunal exchanges between the continents. For instance, in previous glacial maxima, horses and other North American fauna colonised Eurasia.

At the last glacial maxima, a number of Eurasian species entered the continent; Wapiti (*Cervus Canadensi*), Moose (*Alces alces*), Grey Wolves (*Canis Lupus*), Bison (*Bison bison*), and tailing along behind, humans. Given the large range of new species that entered North America, it is fair to say that any species with a contracted ecological setting because of the ice ages could well be in difficulty. During this faunal interchange, modern bison actually replaced previous North American species such as *Bison Priscus*. The arrival of competitive browsers in the form of large ungulates like Moose and Wapiti could disrupt existing North American browsers. And on top of all this competition, the North American fauna faced the arrival of not one, but two novel predators: Humans, and modern wolves.

Modern Grey Wolves in particular may well have been a novel predator just as much as *Homo sapiens*. There is some suggestion that the North American Dire Wolf (*Canis Dirus*) may have been a solo predator, and Grey Wolves are group hunters, possibly the first the North American fauna had encountered.

This view of the North American extinctions sees humans as just another invading species, part of a general faunal interchange. Faunal interchanges are not well understood, but no one doubts that they happen, and that there are winners and losers during such events. Some species outperform others, invading their ecosystem, or perhaps just disrupting it, driving a resident species extinct. Humans on this view, while they may have played a role, are part of an ensemble cast invading the North American ecological stage.

The second background factor is related to the first. Here, the idea is that at the end of the last glacial maxima the habitats of the North American biome were disrupted in ways that made the megafauna in particular susceptible to a human coup de grace. This is the climate change alternative to the Overkill model. However, determining the role of climate change is problematic.

> Many suspect that episodes of rapidly changing climate could cause extinctions, but the links between such an episode and the extinctions—be they gestation time, diet, temperature tolerance, or something else— are complex indeed, and the kinds of links required are not the kind that leave obvious traces in the ground. Unfortunately, ambiguity and diffuseness in the proximate coup have come with the territory. Biologists lack theory that can predict the responses of organisms to climatic change in general, and because they lack such theory, it does not seem surprising that many climatic accounts of Pleistocene extinctions have focused on the search for provocative correlations. (Grayson 1984 p819)

Interestingly, the problem that Grayson notes here is a problem we should recognise; we don't have a good enough understanding of the relationship between climate change and extinctions to reliably pinpoint physical evidence. In short, our theories or models are not good enough. Ideally, like the Ice Age example we examined in chapter 6, we would like a model that we could scale to accommodate the intensity of this past event. As yet, we simply do not have a good model.

However, Dale Guthrie has more recently suggested that there is some evidence for a distinctive change in the eco-system. Guthrie suggests that the terminal pleistocene marked the end of a unique ecological zone; the mammoth steppe. This model allows us to treat North America as an island subject to disruption of ecological balances by invasions of new flora and fauna.

The general idea is a trend in tertiary period leading up to the last glacial maxima that lead to increased seasonality and a shorter "growing season" for plants. Under these conditions, there is a move from a flora dominated by woody plants that require longer growing seasons, to herbs. This trend initially helps large ungulates:

> In the tertiary large ungulate increase in diversity and biomass with the spread of shrubs, herbs and trees. (Guthrie 1984 p261)

Initially in the Pleistocene, the trend favours mammoths as generalised browsers. The vegetation is a mosaic, without distinctive ecological "zones" of vegetation, so some ungulates survive in this mosaic "plaid" environment alongside generalists and woody plant browsers. However, over time the number of plant species contracts, and the vegetation zones simplify. The growing season gets even shorter, favouring grasses, this penalises some varieties of large ungulates. Vegetation specialists do better within this emergent, zoned ecology than mosaic generalists.

The result was a trend to homogenous vegetation that suited deer and other large ungulates, but did not suit mammoths and other organisms of the "Mammoth Steppe" (Guthrie 1990). For Guthrie, the ecological disruption is enough…

> …to argue that both the megafaunal extinctions and the expansion of humans are features of the same climatic event, an event that opened the door in the artic to human expansion while at the same time bringing the environmental changes that led to extinctions. (Guthrie 1984 p290)

If Guthrie is right, there was a distinct change in the North American Biome, particularly in Eurasia and Alaska. One that was disruptive enough to favour Wapiti and Moose over Mammoth. Wolves and humans followed these prey species across the Eurasian steppe into North America, providing just the sort of disruption necessary for the eventual extinction of a marginalised megafauna.

The important point about these alternatives is that they provide an important context for the application of an overkill model. We need to identify the mix between the various potential causes: Overkill, Faunal Interchange, and climate induced habitat disruption. Just as Jared Diamond suggested that there were a number of possible factors underlying the collapse of different societies, and then tried to isolate those important in any particular case (Diamond 2006), so too a procedure of tacking between a set of potential causes and evidence would best serve us in the case of North American Megafaunal extinction. It may even turn out that the particular mix of causes differs from region to region within North America, which is, after all, a continent which contains a range of habitats, from forested woodlands, plains, and more mountainous regions. For instance, Guthrie's models of habitat change are focused on Alaska, and shaped in part by the fact that at various points in the glacial-Interglacial cycle it is part of the Eurasian Biome, and thus it may not translate to other areas of the North American Continent.

10.5 Models and Narratives

In the previous chapter, I put forth the idea that individual stages within a narrative may well have starting conditions and key variables set by prior stages within the narrative. History is such that events are shaped by prior occurrences, and understanding these prior occurrences can matter.

The megafauna extinction debates generally have been polarised between those who argue for a human caused extinction, and those that argue for climate change. In fact, this characterisation on closer examination is subtler. No one, on either side, doubts the role of climate or humans. The debate is actually about a single counterfactual; If human's had not arrived, then the megafauna would not have gone extinct. Everyone allows a role for humans in the extinctions, but some people are sceptical of the truth of the counterfactual.

The scepticism about the overkill model has various bases. Paucity of evidence is at least part of the problem, particularly in the Australian case. In the North American case, there is little in the way of direct 'smoking gun' evidence, and the situation is clearly complex. There was a faunal interchange, it occurred at the end of an ice age that may well have been distinctive both in the preceding interglacial and the intensity of the glacial period itself (Guthrie 2006). There may well have been humans in North America before the Clovis people. Scepticism about the Overkill model is justified.

However, there is another way to interpret the debate. Overkill is one of a number of contributing causes, and partisans have ignored other possible causes. They have been too reliant on a single explanatory factor: the arrival of *Homo sapiens*, to do all the explanatory work.

Stephen Wroe's example of the extinction of the small lizard *A. sagrei* is a particularly apt counter to these single cause models, for it acknowledges historical contingencies in just the right way. It may well not enough to have the presence of the predator *L. carinatus*, it also matters to have the historical event of a hurricane as well.

Diamond's account of Easter Island is similar. Cultural practices associated with mound building aggravate resource over exploitation.

Good accounts of the past, good narratives that explain, include variables that matter, and those variables may be the result of historically contingent prior processes, and mixes of processes, and not just a single process. Guthrie's use of the human predation model considers these past variables. They set the background conditions for a more limited, but still potentially important role, for human predation.

The human predation model is not wrong. It is a good model, and there is no doubt that it is a useful model as a starting point for investigations. However, as we have seen in prior chapters, researchers must modify a model through contact with the physical evidence available on one hand, and the broader historical context on the other. Geomorphologists, when applying a process model in a historical context must acknowledge the role of prior and subsequent processes in order to make the model 'fit' the particular case they are looking at. The human predation model also requires modification, from a working hypothesis to an account of the past by contact with data and accounting for the particulars of a situation.

In fact, this has been the case. People have modified the Overkill model and its stronger form, Blitzkrieg, over time. The application of the term 'Sitzkrieg,' a slow war of attrition, by Jared Diamond to the New Zealand case being an example of a variant on a model (Diamond 1989).

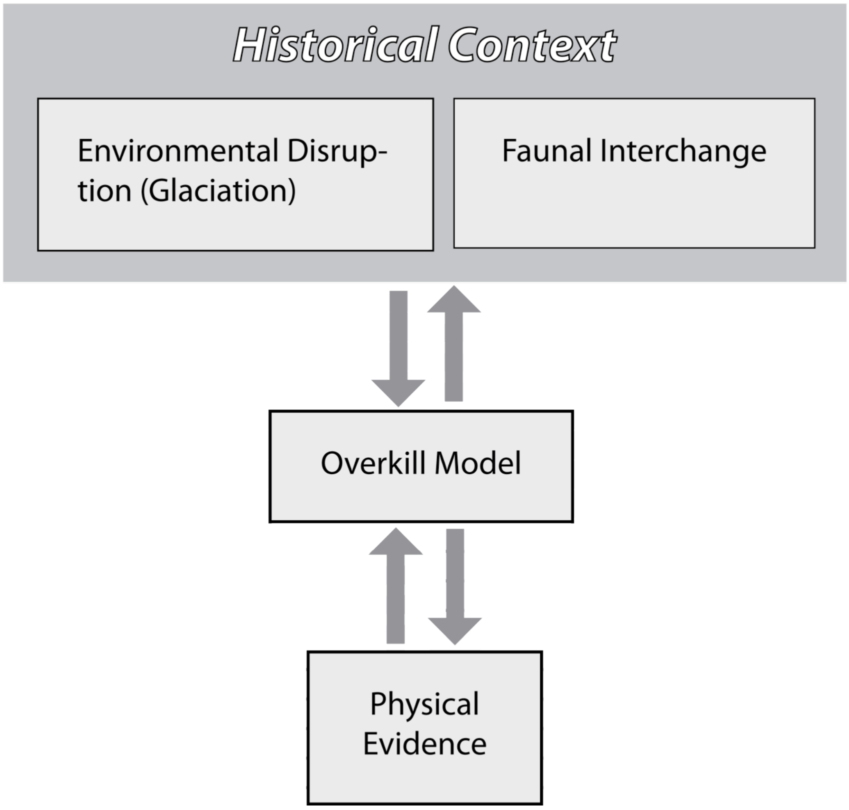

Figure 10-1 The three place tacking procedure of the prior chapter applied to the megafaunal extinction case. Through modification, the overkill model may decrease in importance as a factor over time. It may even disappear altogether.

As noted in the introduction, the take home message for this chapter is simply that models cannot be uncritically applied to past contexts without further information. The human predation model is a good model, and a good starting hypothesis across a number of situations. It is potentially a robust model; it provides good comparative information across cases. However, in any particular situation, it cannot serve as a complete account of the past. The role of Humans, and the extent of their role, will be very different in different historical and geographical contexts. Therefore, while as a model it can be 'true' in any particular case, it can also be incomplete, and potentially misleading in any particular case, if it does not include the relevant historical factors.

11. Cognitive Archaeology

In chapters 3-10, I have shown that in favourable cases, the historical sciences can construct well confirmed, explanatory narratives both of present traces of past phenomena, and of the past itself. I have also shown that the conditions that allow for the successful practice of the historical sciences are not rare, exceptional or very restricted. Although on occasions making claims about the past can be difficult and time consuming work, surprisingly obscure things can be discovered.

However, there are situations where hypotheses about the past can be undetermined by the evidence available, and the project of building a narrative becomes difficult. How widespread are those situations? Do they block historical sciences of great intrinsic importance? In this chapter we attack this problem by considering what would appear to be difficult cases for the historical sciences: the reconstruction of human belief systems. How can intentional agency be accommodated within the historical sciences?

Intentional agency raises an interesting problem for the historical sciences, in that the link between the past and present is likely to be tenuous. However, the beliefs of agents do structure behaviours. And the behaviours of agents do have consequences that can leave evidential traces. Tools, for instance, are a consequence of tool making behaviour, which in turn is a consequence of a tool making capacity. The remains of a meal are the consequence of behaviours associated with obtaining and preparing a foodstuff. For some behaviour, there is frequently a correlate or proxy that does preserve that will allow us to detect behaviour. Nevertheless, there is a long chain of inference here: From a belief to a behaviour, from a behaviour to physical consequences that leave traces that we can detect.

According to Derek Turner's argument that I outlined in chapter 5, events in the past can disperse beyond recovery. They simply do not leave traces that can act as evidence for choosing between alternative hypotheses. If Turner's criticism of the historical sciences bites anywhere, it is surely here, with this long causal chain from beliefs to a final observation. Each step in this causal chain can disperse. Even if they do not disperse, we need to secure the interferential link between each transformation. As I argued in chapter 4: we have to justify the link between our observation and a past cause. To use Peter Kosso's phrase from chapter 4, we require accounting claims to secure this inferential chain.

This inferential chain also points to an important constraint when talking about beliefs: we will have evidence for belief systems only if they have impacts upon the physical world that preserve as traces. The behaviours we can talk reliably about will have to have consequences that preserve in the historical record.

This chapter is going to look at applying the ideas developed in previous chapters about how historical science works to the archaeology of modern humans. While many of the examples given thus far have been from archaeology, archaeology itself has a particular and unique problem that deserves attention in its own right; human behaviours, and in particular, human behaviours associated with beliefs. On occasion, we do have access to the thoughts of literate groups. However, the prospect of showing how reliable such statements are moves us towards the provenance of history as a discipline, and the philosophy of history, and we will not explore that here.

For most of the human and pre-human past, however, we do not have access to texts. We do however have the results of behaviours that are shaped by beliefs: from ceremonial objects, through to the large temple complexes of semi-literate peoples. We also have the results of political processes that are themselves driven by views of the world. In the archaeological setting of a village, the remains of a house that is larger, more ornate, and more richly endowed with worldly goods, raises the possibility that it belonged to a political leader or high status individual within the community. This suggests that such a community had a hierarchy, and perhaps a semi-professional leadership. However, while its clear that a group believed that a temple, or having a leader was important; why they believed that is unclear.

The belief systems of groups and individuals are then historically causal: They shape the evidence from the past that archaeologists deal with. And therein lies the problem. In order to talk about elements of the human past, we need to be able to make reliable inferences about the belief systems of past social groups. The historical inference in this case looks particularly difficult, because human belief systems are not something we understand well. We lack the regularities or mechanisms that would allow us to connect belief systems to distinctive traces. The first section of this chapter will outline this problem in more detail and give the problem more structure.

We will then briefly deal with one potential response to the challenge of human beliefs systems that arose in archaeology, that of processual archaeology. The processual archaeology response was sceptical. It thought that there were quite defined limits on just how much we could know about the belief systems of past human groups. While we can find out a great deal about the human past, it was its technological and material culture we could find out about.

The final section is a response to this sceptical and limited appeal, and in it, I outline two strategies that make some headway against the sceptics.

The issue then this chapter is the extent to which we can make sense of the human past, and the political, economic and religious practices of a group that shape the physical traces that we see before us.

11.1 Hawkes' Hierarchy

To frame the discussion of archaeology, I am going to present the ideas of Christopher Hawkes. The 'hierarchy' of explanatory difficulty he presents is problematic, as we shall see. Nevertheless, it is a good starting point for our discussion of cognitive archaeology. Before we outline Hawkes' ideas, I will provide a brief historical background to set the stage.

By the 1950s, archaeology was coming to grips with the prospects of being a science. New techniques such as carbon dating were overturning previous ideas. Carbon dating in particular was restructuring previous chronologies in remarkable ways, and the narratives that archaeologists had built on the back of old chronologies were thus faltering, and in many cases were worthless (Renfrew 1973). Other techniques and insights were also making an impact on archaeology. For instance, views from the air were revealing archaeological sites and landscapes shaped by previous inhabitants that were previously un-noticed (Trigger 1990 p249). In effect, by the 1950s the epistemological possibilities of archaeology were expanding rapidly.

The influx of new techniques went hand in hand with an increasing focus upon the economic and subsistence history of past peoples. While archaeology had for a long time been closely akin to treasure hunting,[32] it was becoming increasingly historical and anthropological in its focus. The archaeology of collecting, where precious objects were recovered and sent back to museums, was being replaced by archaeological practice associated with anthropology, and the recovery of the life ways —the economic, subsistence and social practices— of past groups. In particular, the environmental and economic bases of past social groups were becoming of increasing interest.

Thus, by 1953, when Christopher Hawkes of Oxford University gave a talk for a Werner-Gren supper conference at Harvard University,[33] he was in a position to look back over almost 20 years of change within archaeology. The older cataloguing and treasure hunting aspects of archaeology was changing into something akin to historical anthropology. Professional archaeologists were increasingly seeing the construction of chronologies and the cataloguing of artefacts as a first step in archaeological practice, a means to an end, rather than the end in itself. Coupled with new techniques, and new insights, archaeology was changing into a discipline that provided narratives of development and change about the past.

In his paper, Hawkes makes a number of points about the state of archaeology. However, for our purposes, the point of interest is his views on the new archaeological project of explaining cultures, rather than talking and documenting artefacts. Hawkes' concern was that much of this was effectively educated guesswork. In essence, Hawkes was concerned about what I have referred to as the historical inference: from

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[32] The treasure hunting continues to this day. The black market in archaeological finds, and the destructive practices that serve these markets is of continuing concern. However, "treasure hunting" archaeology was a practice tacitly sanctioned by academia and governments until the mid 1950s, and such institutions as the British Museum are a collection of semi-legitimate pillage, as controversies over the Elgin Marbles testify. For all that, the tales of these early archaeologists, their sometimes hair-raising adventures in exotic locales, are of course, rather amusing at times (See for instance Daniel 1967; Daniel 1975; Adkins 2003).

[33] Hawkes' presentation was later published as a revised paper in *American Anthropologist* under the title "Archaeological Theory and Method: Some suggestions from the Old World" (Hawkes 1954). All page references in subsequent quotes are to this published version of the paper.

observations of physical traces to claims about the past. When it came to the historical inference in archaeology, Hawkes suggested that there was an ascending level of difficulty in making inferences from the physical remains available to different aspects of a culture.

The lowest level of what we will call "Hawkes' Hierarchy" is the techniques of manufacture for objects. Inferences regarding techniques of manufacture should be relatively easy. How something is made, and its constituent materials, can be learnt from the object itself. This is, after all, the direct physical evidence available to the researcher.

The next level of difficulty in Hawkes' Hierarchy is the "subsistence-economics of the human groups concerned" (Hawkes 1954 p161). To discover this requires information about the economic (or subsistence) use of the artefacts discovered and information about the physical environment of the group concerned. As awareness of ecological factors had come to the fore prior to the 1950s, Hawkes was very clear about the role of the physical environment in determining the nature of a group's material remains as they bore on questions about what they ate, and where they got their food from.

After this point however, things become more difficult. The claims move from inferences from single finds, to inferences about larger patterns of data. The next level was the socio-political institutions of a group. This is always going to be difficult. While various contexts within which things are found may provide some clues, the inferences from artefact to the behaviour that produced it will always be a matter of conjecture. No one object is going to provide the information required. Relationships between objects might provide more information, in that a 'site' rather than a single artefact may provide some ideas, but this will always be open to multiple interpretations.

The final level of difficulty is the religious and spiritual life of a group. Inferring religious ideals from a group's artefacts without any external reference will be very difficult indeed. The find of a sculptural object may provide information about how it was made and what it was made from. But can we know why it was made and the role that it played within the social, cultural and political life of the group?

Hawkes Hierarchy is then a catalogue of difficulty in making the historical inference about the human past. The hierarchy depends on increasing amounts of context and the configuration of finds.

To see this, take the following toy example. To understand the physical properties of a find of a pot, I just need the pot. It alone will provide information about what it is made of, whether it was thrown on a potting wheel, fired and glazed, and other features directly discernable by an experienced observer simply by looking at the object. One may even be able to go beyond unaided observation, and detect traces of materials stored within the pot using a variety of techniques. Nevertheless, at base, this is all physical information about a find. This is the lowest level of Hawkes Hierarchy. To go further, and understand the economic and subsistence role of the pot, I need both physical evidence about the environment and other finds related to subsistence technologies. The pot alone will not provide enough information. To progress even further up the hierarchy, and understand the political and cultural dynamics of the pots production, I need yet more evidence about specialisation within a community, and other demographic and economic clues such as population size, political structure, and means of exchange. Finally, to understand the religious significance of the pot, I need to make even more inferences across even more data, and perhaps utilise even more models to understand the religious and ideological role of the pot. The first dynamic that underpins Hawkes' Hierarchy is the increasing need for further information for interpretation.

The second factor that underlies the hierarchy is slightly different, and the one that Hawkes himself was interested in. Hawkes saw the lower levels of his hierarchy as comparable to animal adaptations. In this, he was influenced by the increasing awareness of environmental factors that was apparent in the work of people like Grahame Clark (Trigger 1990). Although Hawkes does not explicitly talk in adaptive terms, he does suggest that the cultural behaviours of humans, and their tools and artefacts…

> …differ from animal only in the use of extra-corporeal limbs, namely tools, instead of corporeal ones only. (Hawkes 1954 p162)

The idea here is that archaeologists can interpret tools functionally, in the same way that a biologist interprets an organism's adaptations. We can utilise something resembling the Design stance of Daniel Dennett, (1999) and rightly presume that an object was made to fulfil a particular purpose. Viewing the environmental context for an artefact can provide some of this purpose.

For the next level, there were again parallels with animals. Human subsistence-economics are comparable to animal subsistence needs. For modern archaeologists, there thus arises the possibility of utilising optimal foraging models, or simple subsistence economics to explain the resultant behaviour. However, for Hawkes there was something of a noticeable difference in human subsistence behaviours which is the "degree of forethought which they involve" (Hawkes 1954 p162). On this view, humans can be understood to be engaging in practices that are not just responses to the environment, but anticipations of it. For Hawkes then, the higher up the hierarchy we progress, the less like animals humans become. What separates the easily explicable from the difficult to explain is just how obvious something is as a function in a material culture geared to survival.

> The critical factor, standing between fair intelligibility and stark unintelligibility, is surely ecology, the study of the physical environment. So long as you can depend on that, as you can for the material aspect of man's life, his technological and his economic existence… Archaeology is rewarding. (Hawkes 1954 p162-163)

So, the physical existence of humans, the way they made tools and their subsistence activities, their economic life; for these activities we do have something approaching regularities that we can utilise to understand the physical evidence.

Where things get harder, and where regularities in human behaviour become increasingly less obvious, is when we approach the mental life of humans; their belief systems, ontological systems, and the way they see the world. It is worth while quoting Hawkes in order to gain the full flavour of his claims.

> …human communal institutions next transcend the animal level very considerably; and human spiritual life transcends it altogether. So the result appears to be that the more specifically human are men's activities, the harder they are to infer by this method of Archaeology. What it seems to offer us is positively an anticlimax: the more human, the less intelligible. (Hawkes 1954 p162)

The key point to extract out of Hawkes' Hierarchy is that Hawkes believes that important features of human life transcend animal features, in that they are not purely functional in ways that we can reconstruct utilising ecological and environmental information.

If then we want to base interpretation of human artefacts solely based on ecological and economic factors, we are going to have difficulty with coming to any kind of understanding of the areas of a culture that are shaped not by functional requirements, but by the mental life, the belief systems of those who artefacts are under study. In trying to reconstruct human behaviour then the more closely related to the subsistence needs of a group an artefact or collection of artefacts are, then the more easily explainable they are. Once archaeologists attempt to identify or explain socio-political structures or religious beliefs they are bereft of interpretative tools, and such hypotheses do not have the same security of inference of lower levels. The assumption here is that there is something arbitrary and irregular about the upper parts of the hierarchy. We have no mechanisms or models of the dispersal of consequences from social-political structures and religious beliefs to the physical traces we can observe.

Hawkes hierarchy highlights the need for, and the problems of making inferences about, the cultural and mental lives of extinct peoples from the raw data of Archaeology. Even finds that unambiguously pointed to the existence of social activities and the social life of people such as ruins of villages or multiple dwellings, could not with equal clarity point to the specific kind of social life that created them.

11.1.1 Regularities and Beliefs

The problem that Hawkes' is identifying is actually two entwined problems common within the social sciences generally. On the one hand, there is the problem of rational agency. The lower levels of Hawkes Hierarchy, the mechanics of objects, and the subsistence economics of social groups, are behaviours that we can presume with some confidence are based on some level of rational behaviour. So, in making the historical inference from evidence to cause, humans, at least on average, are rational agents when it comes to behaviours with potential impacts upon their survivability.

Religious behaviours on the other hand, the upper reaches of Hawkes' Hierarchy, are notoriously less rational. System of beliefs, systems of symbols, can be much more arbitrary, and our inability to ask questions of the makers or artefacts makes our interpretations problematic. We don't have good background theories to secure our historical inference at this point. What's more, some of these rather arbitrary beliefs may well have impacts on lower levels of the hierarchy as well. While there will be some adaptive pressures on these activities to conform to reasonably rational and economic forms, religious prohibitions on foods eaten, divisions of labour and subsistence activities are well known.

The second part of the problem is that of functional explanations in the social sciences. Economic and subsistence explanations are functional explanations of behaviours. However, functional explanations of social behaviours frequently do not capture the beliefs associated with such behaviours (See for instance the discussions in Martin and McIntyre 1994 chapter 22-26). While a social behaviour may be economically rational, and we can explain it by reference to this rationality, this may not capture why the individuals engaged in the behaviour. They may simply be engaging in this behaviour because it was the social norm. People may maintain a socially or economically advantageous behaviour because they believe the gods will punish them, it’s the social norm, or a religious doctrine, and not because it is inherently socially or economically advantageous. The functional advantages of a behaviour may not be the motivation.

And yet its precisely the motivations that archaeologists are looking for when faced with archaeological sites such as Stonehenge, or other economically costly, yet clearly significant remains. The belief systems of the group plays the motivational role in the construction of such artefacts, and not economic or subsistence rationalism.

Thus, Hawkes Hierarchy captures the epistemic problem of the historical inference for archaeologists in two ways: the need for context from more data, but more importantly, the ambition to understand the motivational beliefs that drive the upper levels of Hawkes hierarchy: The religious behaviours and social norms that prompted people to leave behind the artefacts they did.

11.2 The flight from intentionality

In this section, we are going to briefly look at one attempt to come to grips with Hawkes' Hierarchy by looking at Processual Archaeology. Processual Archaeology's response to the problem is important, for it was an attempt to be rigorous and scientific about the archaeological project. Equally, it represents a response that later workers at the end of the 20th century were reacting against; Processual archaeology shapes an explicit

Cognitive Archaeology.

In text-aided archaeology, we have access to the public thoughts of individuals, and occasionally the private thoughts, in the form of texts. Such texts can provide insights into what the archaeologist Lewis Binford describes as the "ideosphere" of a culture: its ideology, its belief systems and so forth (Binford 1962). Without such texts, Binford argued, archaeology is constrained to talking about the "technosphere" of a culture: its technology, the functional aspects of the material stuff of a culture. We might still be able to tell a great deal from the technosphere. But, we would not be in a position to talk confidently about the religious beliefs and so forth, of a group, its ideosphere. Inferences from material remains would be constrained to be about those material remains themselves, and the role that they played in a cultural system.

Lewis Binford was part of a young generation of American archaeologists in the 1960s that took this difficulty very seriously. In particular, Lewis Binford articulated an approach to Archaeology known as Processual Archaeology,[34] and it took a hard line on this problem. Archaeology, said Binford and the Processual Archaeologists, has to be a science. The central tenet of Processual Archaeology is that the material remains of groups are a systemic adaptation to the environment (Binford 1962). The remains an archaeologist is confronted with are the material leftovers of an adaptive system: a culture. In the same way that the fossilised hard parts of an organism, its bones, teeth, shells and so forth, can be used to infer various functional aspects of the living organism, so too can a culture be reconstructed as a interdependent functioning system. To explain an artefact was to elucidate its role within an integrated systemic and adaptive system that was culture.

Now for Lewis Binford, the material culture of past groups is the sole basis for historical inferences. For Binford, the new archaeology of the 1960s was to be a rigorously materialistic archaeology, with all causes being physical causes; either changes in the external environment, or subsequent changes in the integrated cultural system that had flow on effects within the system. One upshot of this was that psychological or intentional causes of archaeological data became out of bounds as explanations.

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[34] For some time, this was also known as the "New" archaeology.

> [Processual] Archaeologists have continued to condemn explanations of change that invoke either conscious or unconscious psychological factors. Instead they have identified relations between technology and the environment as the key factors determining cultural systems, and through them, human behaviour. (Trigger 1990 p302)

Effectively, the processual position appears to drop the intentional actor out of the explanatory equation by assuming all changes within a cultural system are the result of environmental, and therefore physical, factors. A cultural system is an adaptation to the environment, an integrated functional system. An agent's beliefs or desires seemingly play no role in this system.

There are two ways to take the processual archaeology concerning Hawkes' hierarchy. On the one hand, we can interpret Processual Archaeology as saying that the higher levels of the hierarchy, religious and political institutions, were of no explanatory import to anthropologists, or at least, they were not within the purview of a rigorous scientific archaeology. They are epiphenomenal by-products of culture, and not the things of interest.

It is certainly true that adopting what was in effect a materialist stance, rejecting ideational explanations and talking in terms of culture being an adaptive complex we can interpret Processual Archaeology this way. On this view, Processual Archaeology simply says that the upper bounds of Hawkes' Hierarchy are not within the bounds of a rigorous and scientific archaeological practice. However, I do not believe that that is the case with Processual Archaeology. Rather Binford and other processual archaeologists believed that all aspects of cultural groups can be explained.

> He [Binford] maintained that the archaeologist's primary duty is to explain the relations that are extant in the archaeological record. In particular he repudiated the idea that it was inherently more difficult to re-construct social organisation or religious beliefs than it was to infer economic behaviour. (Trigger 1990 p298)

The idea here is that all objects found within the archaeological record, and their relations to other objects, should be informative regarding the culture that produced them. Binford and Processual archaeology's real response to Hawkes' Hierarchy is an archaeological holism. All components of an extinct culture are the result of, and can be explained by, ecological factors and the systemic nature of a culture. Cultures as a whole are highly inter-dependent systems that as adaptations to the environment included all elements of a socio-cultural complex. Material culture *didn't* arbitrarily vary in ways that Hawkes' hierarchy seemed to imply: there were regularities that could potentially be recovered. Processual Archaeology in effect assumes that components of a cultural system are not independently variable of one another; any change in one variable should be indicative of change elsewhere.

The French anatomist Georges Cuvier maintained that the components of living organisms are so tightly interconnected that the whole organism can be reconstructed from any part (Rudwick 1972 p130). So long as you understand how the parts of an organism are integrally related, in effect, one knows the regularities that underpin the construction of organisms, one can reconstruct a complete organism from any piece. In something of a similar fashion, Binford maintained that the inter-relationships between the various components of the archaeological record were such that one can infer the entire system, including the upper reaches of Hawkes' Hierarchy, from its parts. Systems can be re-constructed in their entirety once one possesses regularities about the relationships between parts. The idea here is that any change in a particular variable would result in changes elsewhere in the cultural system in a way that was regular and predictable. To remain systemically viable as an environmental adaptation, the entire system would have to change in regular ways.

So long as there was some means of testing it, a hypothesis about the ideological beliefs of a people was acceptable. Given the implied dependence on all aspects of culture on environmental or demographic factors, there was no real reason to suppose that religious or political systems are beyond explanation. Given the right theories, enough data points, and tight enough connections between the components of a culture, Hawkes' Hierarchy was collapsible, and one could reliably infer even the non-material aspects of a culture.

> 'Non-material' aspects of culture are accessible in direct measure with the testability of propositions being advanced about them. (Binford and Binford 1968)

Binford believes that whatever shaped a society and changed it through time was measurable, and the causes were regular and predictable. Binford in particular has spent much of his career attempting to investigate and document regularities in contemporary and past cultures for use in making claims about past societies. (See for instance the rather encyclopaedic Binford (2001))

The important take home message of the processual project is simply this: According to rigorous Processual Archaeology, the relation between the archaeological record and the behaviour that produced it could be determined. Processual Archaeology took behavioural reconstruction to be achievable in a controlled manner without positing unverifiable causes, and it took the primary causes to be external physical conditions to which a culture was an adaptation, rather than the internal dynamics of cultures.

The upshot is that Processual Archaeology sees human material culture as the explanatory target, and also sees human material culture from an externalist and adaptationist view point: Its all an adaptation to the environment. If this were true, we could collapse Hawkes' Hierarchy. However, it is not true.

11.2.1 The variability of cultural practice

The processual approach to archaeology, and in particular its belief that cultural systems are integrated wholes is problematic. Its underlying assumption is that all parts of a culture are functional, and respond to adaptive functional demands on a cultural group; even religion on this view was importantly shaped by the technological and subsistence needs of a people.

By the 1980s, there was an increasing discontent with the Processual view. For a start, many felt the overemphasis on material culture as an adaptation missed things of interest in archaeology. The symbolic systems of culture, the belief systems, are of interest, and when confronted by a construction such as Stonehenge, or elaborate burials, processual archaeology potentially had little to say. What's more, even if a functional account can be given, and the artefacts reveal information about the technologies and capabilities of a group, the psychological motivations or meanings behind a series of acts remain elusive. An entirely functional account of a cultural system, even if it could account for the functional aspects of the "ideosphere," still missed something that matters; the content of the ideosphere in the form of beliefs, ideas and so forth. It is quite possible that I can explain a small communities regular church attendance as serving a social function of maintaining community networks, and providing a regular meeting place for negotiation and so forth. But, the ideology, religious iconography, and other features of the church, and the role it plays within the belief system of individuals are not explicable by a functional account.

The second concern was a concern about regularities. As more and more ethnography became available to the archaeological community, it became increasingly clear that human culture is characterised by a wide variety of responses, rather than variations on a single theme. Just as increased observations of contemporary primates changed how we understood the primate adaptive complex, increased ethnography changed how we understand cultural variability. The complicated, highly variable mass of cultures that archaeologists, anthropologists, ethnographers and others were seeing seemed to be more variable than alike. The notion that there was underlying commonalities across cultures that could do real explanatory work at the upper reaches of Hawkes Hierarchy, particular the varied religious and ontological belief systems of groups, seemed hopelessly optimistic.

Perhaps more to the point, it is not clear that such social practices vary in regular ways that are predictable across cultures. One thing that we do know about cultures is that there is frequently a lag behind the functional demand and adaptive response of a cultural practice. Cultural practices frequently persist long after they are no longer functional. Judge's wigs in the British court system are an example of a behaviour that has stayed in place for cultural reasons. The forward pointing bow of some modern ships are an example of a practice that has stayed in place for aesthetic reasons (Gordon 1978).[35]

There is little reason to suppose that the ideational components of a culture will shift in response to changing ecological demands. Cultural practices with clear success or failure criteria respond quickly to changes in conditions, while practices with less clear feedback respond in a much

[35] The original functional reason for prominent bows on sailing ships was to increase the potential surface area of sails attached to the bow. Modern, non-planing motorised craft have no need of this, but some designs retain the projecting bow for aesthetic reasons, notably on cruise ships, and motorised luxury yachts.

slower fashion if at all (Jeffares 2005; Sterelny forthcoming). Religious practices in particular seem to be immune to real epistemic feedback, and stay in place long after they make functional sense.

Part of the driver for this change of views was the idea that parts of cultures vary independently of each other. The rise of evolutionary views in archaeology suggested that different components of a culture changed at different rates. Symbolic and decorative parts of a culture tended to remain remarkably stable, while functional aspects of culture were highly labile and responsive to environmental changes and opportunities (Dunnell 1996). This makes sense; symbol systems and decorations are arbitrary systems, while the parts of cultural with direct impacts on survival have obvious success or failure conditions that cultural actors can see and make judgements on. So, when the European musket and some European crops were made available to the New Zealand Maori they quickly adopted these new technologies. However, many of their political systems and their decorative arts, stayed remarkably stable. The result were European muskets with the stocks carved in a very Polynesian manner reminiscent of various other Maori weapons and tools.

The result then is that the processual methodology was under threat. Practicing archaeologist felt that Processual was unduly materialistic, ignoring the mental life of past groups. It also ignored the highly variable aspects of human culture. Most crucially, cultures were not systemic and integrated in the ways that Processual presumed. It seems quite possible that the upper reaches of Hawkes Hierarchy varied in ways that were not dependent upon the lower material and subsistence aspects, and that the physical information would not specify information about the political and religious life of groups.

11.2.2 The Archaeology of Beliefs

The upper reaches of Hawkes' Hierarchy, particularly religion, is concerned with the beliefs of individuals and groups, and how those beliefs shape their attitudes to the world, and their desires that drive their behaviours. For instance, an ontological system that identifies individuals of certain lineages as possessing special status changes how members of their community will interact with them, with resultant social consequences.

Clearly, we need more than talk about environments and behavioural outputs if we want to talk of human beliefs. To merely talk of environmental inputs, and behavioural outputs, is to make the study of past humans, and pre-humans, a behaviourist exercise, with the underlying systems of belief that drive these behaviours as an inaccessible "black box." David Whitley, in the introduction to a collection on post-processual and cognitive archaeology suggested that much processual archaeology of the 1960s through to the early 1908s, was implicitly behaviourist in this regard (Whitley 1998). Quoting Howard Gardner, Whitely notes…

> [Behaviourists] eschew such topics as mind, thinking, or imagination and such concepts as plans, desires, or intentions. Nor ought they to countenance hypothetical mental constructs like symbols, ideas, schemas, or other possible forms of representation … According to behaviourists, all psychological activity can be adequately explained without resorting to these mysterious mentalistic entities. (Howard Gardener quoted in Whitley 1998 p5)

If we look at archaeological practice, archaeologists are rarely the behaviourists sketched here. Even the minimal claims of processual archaeology have psychological elements. The mere act of saying that something was functional to a group, that it played some role in their life, coupled with the notion of a manufacturer of an artefact immediately suggests an intention; the intention of the maker to fulfil a certain functional requirement.

> Anything made or done by a human being reflects the mental processes that lie behind its production and use. Some artefacts not only thus *reflect* cognition but also intentionally *represent* mental content. (Frake 1994)

While the function of an artefact may well be adaptive, and perhaps ultimately caused by an environmental need, the proximal cause is the intentions of an agent. We may wish to frame the actor behind an artefact as a biological actor, an enculturated organism engaging in an adaptive activity. Nevertheless, there is an agent, and we can with some confidence assign subsistence level economic motives to that agent. The agent desires a certain end, and the agent believes that that end can be achieved through certain behaviour.

As I noted in the introduction to this chapter, there is a long causal chain from beliefs to traces of behaviours. There are lots of opportunities for dispersal of this causal chain; perhaps beyond recovery in some cases. However, the notion that we can utilise physical by-products of behaviour as tools for understanding minds should not alarm us. We as individuals do this everyday when we interpret the desires and beliefs of our fellow agents through the consequences of their actions. We are quite comfortable inferring a set of beliefs and desires about an individual when arriving at a shared office to find an office mates computer on, and a warm cup of coffee and a scatter of articles on their desk, despite their temporary absence. Forensic scientists routinely re-construct behaviours and motives from physical evidence in ways that juries find convincing. There is no principled reason to think agents in the past leave less evidence.

However, the beliefs and desires a historical agent has may well be different than what we expect. An actor may manufacture a tool because that is the social norm; as a member of a group, the group may have expectations about what to manufacture and how to deploy the results. We can readily interpret the behaviours and beliefs of office mates, family members, and perhaps members of our own culture because we share norms and similar belief systems. Even if elements of that shared belief system is arbitrary, we can understand it because it is shared. We are unlikely to be able to so readily interpret the arbitrary elements of a different culture. We don't know the system of beliefs that guides action.

Despite this, the attribution of intentions to make the artefact in question, and the attributions of desires to make an artefact, are commonplace in archaeology. Functional accounts of artefacts implicitly rely upon some kind of background theory about beliefs and desires. A human made something, and a human *intended* to make it. Its not always noticed, but our everyday Folk Psychology, our attribution of mental states —beliefs and desires— to agents is in fact playing a role here.

The question is then, how can we go beyond functional ascriptions to artefacts, to explicitly assert beliefs and desires about, and meanings to, artefacts. And, if we do this, how tight can our ascriptions be?

11.3 Cognitive Archaeology

The Cambridge archaeologist Colin Renfrew suggested that there might be options for a "cognitive" archaeology in a lecture in 1982 (Renfrew 1982). (Renfrew 1994; Renfrew and Zubrow 1994). Renfrew was actively involved in Processual archaeology, but he was well aware of its limits.

The resulting theoretical work has gained the tag Cognitive Archaeology, and the aim is to put the inferences made by archaeologists about the upper reaches of Hawkes' hierarchy on a more secure footing. This is an ongoing project. What I want to suggest in this section is that like other sciences, it relies how well we understand the present.

To demonstrate this, I am going to briefly work through an approach to cognitive archaeology by James Hill, part of a collection of work looking at cognitive archaeology's possibilities (Hill 1994; Renfrew and Zubrow 1994). Hill is interesting because he too is someone with a processual archaeology background, is something of a sceptic of the prospects for cognitive archaeology, but nevertheless, thinks that within limits, cognitive archaeology can make reliable inferences about the political and religious systems of past cultures.

11.3.1 Building on what we know

Hill distinguishes between two distinct approaches to reconstructing the "ideosphere," the political and religious belief systems of past Cultures. The first he dubs the Tight Local Analogy, (TLA) and the second the Established Generality Testing (EGT). There is in fact a spectrum between these two extremes, but nevertheless, they capture the possibilities, and the working practice, of archaeology. As we shall see, Hill is essentially arguing for a local model that can then be applied to the past. Given that cultures are rather localised and idiosyncratic, the key that Hill suggests is to utilise contemporary descendents of past groups to provide a model to understand the past.

Underpinning this use of local models is the fact that a culture's descendents act as an additional line of evidence. After all, part of the reason that a contemporary cultural group has the distinctive cultural traits that it does is because of its history. Thus, contemporary groups serve two purposes; they provide a highly localised initial model for the past, and they also supply downstream evidence. To see how this works, we will look at the two strategies that Hill outlines.

11.3.2 The simple case: Tight Local Analogy

The example of the "Tight Local Analogy" is in some ways obvious. Confronted with an unknown burial of an early settler, and discovering the Lords Prayer, or other Christian symbolism on the tombstone, we would not hesitate to ascribe some ideologies and beliefs to the actors who buried such an individual. Such cultural practices are still around today. The TLA is simply a method that uses the fact that many groups in the past have modern descendents, and that there is some level of continuity between the cultures of the present and the cultures of the past. The assumption here is that cultural groups change slowly, and do not change radically without some kind of evidence of that change.

In effect, there is a substantive uniformitarian assumption that cultures change slowly, and if there are sudden changes, this will show in the physical evidence. We can use observations of contemporary groups provided this assumption holds.

The example that Hill explores is that of early Iroquoian village sites in South West Ontario. Archaeologists investigating these sites have found deliberately interred collections of seemingly enigmatic artefacts; bones of an extinct parakeet, stone pipe-bowls, slate tools and antlers. Dating of these sites suggests they are pre-European contact, and some are much older. Such sites are prime examples of an activity that persists, but not because of any ecological function.

Hill points out that ethnographic records from the 19th century reveal that these artefacts are typically associated with Iroquoian Shamanic activity, so we can infer that such finds have similar associations. We can assume that while cultures change, they change slowly. Over time of course this is problematic. The deeper in the past we go, the less like 19th Century Iroquoians their ancestors are. Nevertheless, we can view this as a decrease in confidence. With documented access to the beliefs and religious systems of modern, or near modern individuals, we can make reliable inferences about past systems of belief with some level of confidence, and that confidence decreasing the deeper into the past we go.

Ethnographic Evidence		**Increasing Age of Archaeological Finds ➔**							
Stone Pipe-bowls	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No
Parakeet Remains	Yes	Yes	Yes	Yes	Yes	No	No	No	No
Antler Remains	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No
Slate Tools	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Deliberate Joint Interment of Artefacts	Yes	Yes	Yes	Yes	Yes	No	No	No	No

Associated Stylistic Continuity	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Similar Subsistence Economy	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No
Tally of cultural traits	7	7	7	7	7	5	2	2	1

Table 1: A sketch of how reliable inferences about the past may be, and how they decrease in reliability in the case of the Tight Local Analogy. As the investigators go deeper into the past, the configurations of finds may change from that recorded in ethnographic accounts. So in the above table, the starting point is the Ethnographic case, (the grey column) and elements in the finds either occur or not. The "tally" of traits below shows the decreasing overlap in traits the deeper we go into the past, and suggest and decreasing reliability.

The strategy of using a tight local analogy counters the problem of arbitrariness by utilising a highly local model to understand the past. In effect, the contemporary group act as an additional line of evidence: an additional consequence of the past actions of a group. Consequently, the archaeologist in such cases has more than just the physical evidence to go on. They also have the testimony of the descendents of a culture, whose beliefs systems have been shaped by the various historical contingencies of their ancestors.

If there is a more general assumption being made, it is that groups retain a level of continuity over time that makes them credible guides to the past. There has been some dispute about which elements of a culture change more quickly than others. As noted above, Dunnell notes that symbolic systems tend to be more stable, while functional systems tend to respond more to environmental contingencies (Dunnell 1996). If that is right, then it is precisely those aspects of culture that we cannot reconstruct using adaptationist ideas that the TLA strategy gets us access to. The present, in the form of the testimony of descendents, might well be a good guide to the past.

There are limits to this strategy. Historians are reluctant to infer the nuances of 20th century morality or ideologies to individuals of even 100 years ago, let alone further back in time. In part, this is because historians are interested in the literate members of society and very aware of subtle changes in intellectual history. However, the truth is that cultures do change. What was once believed becomes a polite fiction. The divine right of kings becomes a constitutional monarchy.

The strategy has limits then. But nevertheless, utilising the additional line of evidence that the testimony of modern descendents provides, we can at least choose between hypotheses. The tight local analogy provides us with enough additional evidence to get Cleland's machinery in play. We may then be able to choose between competing hypotheses such as whether a group were monotheists or polytheists by utilising the additional line of evidence that descendent testimony provides.

11.3.3 Cultural Discontinuity

Clearly, the more complex cases are where there are cultural discontinuities. Where a culture is very old, or there are no living descendents of recent cultures, analogies become less secure. Hill suggests that the alternative is to use Established Generalities.

The example that Hill uses of an Established Generality is an economic one. In the Indian Knoll culture of Western Kentucky of approximately 2 000 years ago, a variety of artefacts were found in different contexts and placed into three categories: general utility instruments such as tools, ceremonial equipment or artefacts, and a third category of "ornaments." Archaeologists assessed the difference between ornaments and artefacts on the basis of where they were found, and their economic value. Finds of artefacts of general utility and everyday functional use were predominantly in middens, essentially trash heaps, along with other refuse. High value imported goods, copper and conch shells, were being buried with people, but not found in middens. Ornaments were found both with burials and in middens.

The copper used in various objects and conch shells are not local, but are imported materials. At the very least, we can reasonably attribute to the Indian Knoll culture a belief in the value of these artefacts. There is a reasonable inference here that because they were imported and rare, they had a high value. We can treat the people of the Indian Knoll culture as rational economic agents in this regard. Like us, they would be reluctant to throw away high value goods.

What's more, archaeologists only found the material in association with "high status" burials. Here, high status is the low ratio of individuals who have such burial accoutrements. So by comparing the grave goods across a range of individuals, and noting that a minority have grave goods that are rare, imported luxuries, we can reasonably assume that these were individuals of wealth and status within a society. The families of these individuals could bury luxuries with their loved ones.

Note here though that the economic context of the group is providing insight into the social and political context. It also seems reasonable that we can apply these ideas to other aspects of people's lives outside of burial rituals. Individuals who possessed copper and conch shells would be engaging in costly signalling, demonstrating that they can afford to dispose of expensive goods in burials, and also that their relatives have status that requires the deposition of goods, and that descendents deserve to inherit this status.

But we haven't proceeded that far up Hawkes Hierarchy in the application of these ideas. While we have gained some insight into potential social and political structures, and certainly gained some insight into the economics of the group beyond that of subsistence, we lack insight into the religious aspects of the Knoll cultures burials.

Nevertheless, by presuming that members of the Knoll culture are economically rational, in that they value the rare imported materials we have begun to make headway into understanding the culture.

The regularity in play in established generalities then are not the quite the ones that Binford foresaw: a systemic adaptation to the environment. Rather, the regularity is that of human rational agency, coupled with local information. In the Indian Knoll case, the geographical context of a group away from sources of copper and conch shells, provides an insight into a unique economy. But once we recognise this fact, a more general insight into economic behaviour provides us with insight into a particular historical case.

The Established Regularities of James Hill are then a mix of general regularities in human behaviour, and local variables. Essentially, it is the deployment of general social science models in a historical context. They don't tell us *much*, so we might not be in a position to say why the people of the Indian Knoll culture valued conch shells, but they do make some kind of headway. Certainly, we can again choose between hypotheses. The Indian Knoll culture was socially stratified and not egalitarian. That in itself will provide context for other claims about the Indian Knoll culture, and will provide insights into other areas of their life. And the use of context is important. Although Binford was wrong to think that cultural systems are so interdependent that a change in one component necessitated a change throughout the system, he we was right to think that cultural systems as a whole matter. They do provide an important context. It is to this background context that we now turn.

11.4 Regularities and Historically Situated Cultures

We can, with some reasonableness, assume that human agents are rational agents. This has limits, but nevertheless, it is a reasonable assumption. We can expect a certain amount of commonality across human beings. Other members of *Homo sapiens* will be "like us" enough to warrant assuming basic rationality. But this basic assumption is not all that is required.

The key here is to understand cultures in the same way that we understand other aspects of history; they are temporally and spatially situated. When Polynesians settled New Zealand, they did not re-invent their culture from scratch: rather the culture they brought with them from a different geographical location was adapted to suit the new environment they found themselves in. Things that worked in the new environment stayed the same or changed slowly, things that didn't work had to change, and if necessary for survival change rapidly, and things with little or no direct or obvious benefit, drifted as they always do. Thus, to understand why the New Zealand Maori or any other Pacific group changed the way it did in response to the new environment it faced, we have to understand the culture they were descendents of (Kirch and Green 2001).

Just as in other areas of history, prior processes and prior states set key variables for changes. It matters if a group that has a metal working technology settles an area. If not, no amount of available resources is going to guarantee that the culture develops it. History and pre-history is littered with examples of groups being displaced by rival groups with cultural traits better able to extract resources from an environment. Jared Diamond argues that this is one of the reasons why the Norse settlement of Greenland failed while the Inuit continued, as the Norse cultural system was unsuited to extracting resources from a particular ecological setting (Diamond 2006).

One issue is of course whether we can extend our understanding beyond the economic rationality that underpinned our insights into the Knoll Culture Indians. Economic rationality is of course a habit of mind that we take it underpins human cultural activities. But an alternative way of seeing it is the way that we think about, reason about, plan for, and engage with, the physical world.

The key to developing cognitive archaeology lies with developing better models of the interactions of agents with their physical world. Any insights that psychology might provide us on regularities in the way that human beings interact with their world will provide some kind of insight into the past as well. The work of people like Andy Clark, who has speculated on the role of the external environment in cognition, could well be a fertile ground for insights into how humans in the past have cognised their environment and interacted with it (Clark 1997; Clark 2003). For Clark, the interaction is of two sorts; embodied agents, and as agents that use the environment as a tool in cognition and guiding behaviour. It is the second of these strands of work, the investigation of how humans use the environment that probably has the most potential to offer. Ironically, given that we are looking at the minds of past agents, Clark's book on this area of his research is subtitled "Minds, Technologies, and the Future of Human Intelligence" (Clark 2003). The irony comes from the focus on the future, for humans always have used their external environment in a variety of ways. When Colin Renfrew introduced the idea of a cognitive archaeology, one his suggestions was that the introduction of weights and measures in pre-historic societies represented an important breakthrough as a technology, but also an important change in the way people thought about the world (Renfrew 1982). Other workers took up this idea (Frake 1994), and the changes in thinking patterns that technology enables are an important area of research.

The possibility is then that the more we know about how environments structure human minds, their beliefs and behaviours and the more we understand how human behaviours shape their environments, the better off our understanding of the past will be. Once again, active research in the present into how humans interact with their physical environment should provide more tools for understanding the past.

Whether the models developed by further research in psychology will answer all our questions about the past, remains to be seen. The argument I made in earlier chapters about dinosaur colouration applies here as well: We might discover surprising things about the past with the sciences of the future. As with all historical sciences, our understanding of the past is only as good as contemporary science. The difficulty for a cognitive archaeology is simply the fact that if there is an incomplete science, it is the science of human minds. We cannot expect to understand the behaviours and belief systems of past agents, if we don't have regularities or models of contemporary agents. To turn the question on its head; Just how far can we ascend Hawkes Hierarchy with modern groups? Think beyond the borders of one's own country and culture, and we can at times be at a loss to understand how these systems work. We can understand people with whom we share norms and belief systems. However, even with access to contemporary agents and their statements about their beliefs, their ontological systems and their religions, we can still be struck by a culture's oddness, and still be confounded by its inexplicability.

11.5 Conclusion

The results of this foray into cognitive archaeology are tentative, yet encouraging. The social sciences broadly construed utilise intentional explanations, and it seems reasonable that archaeology can too.

Broadly, there are two strategies available. The first is to use the additional line of evidence that descendents provide. Because cultural changes are prescribed by what has gone before, contemporary cultures are consequences of the past, and as such, can be utilised as models, and within limits, can act as a "smoking gun" to choose between hypotheses. This clearly has its limits, and the deeper we go in time the more removed from their ancestors the practice of a group is. However, radical discontinuities in cultural lineages should be detectable within a historical chronicle provided by the physical evidence. There should be good signals as to when the model provided by a contemporary culture is no longer reliable.

The cognitive archaeology sketched here takes seriously the idea that cultures are responses to, and shaped by, their past, and that this can at times over-ride rational responses to the environment. History matters for cultures in important ways, for cultures are cumulative, and discard and retain elements within a culture differentially. This does not mean that this differential retention of cultures is beyond reconstruction.

We can expand on the use of these tight local analogies, and in cases of discontinuity will be forced to do so, by looking to regularities in human responses. The failure of the Processual project was that it looked for regularities purely in response to the immediate environment, and as a monolithic system. But that failure should not blind us to the possibility that there are some regularities in cultures, or individual cultural responses. Views of the mind that include artefacts as extensions of minds, and views of cognitive processing that include the role of the world, will make a difference to the future of cognitive archaeology.

Cognitive archaeology is in its infancy, and it relies upon a still developing cognitive science, so in my view the future is positive. There will be limits to how much we can reconstruct. The genuinely arbitrary nature of symbol systems and language, coupled with complete discontinuities will make some past cultures un-recoverable. There will be pockets of genuine mystery. We may never know what motivated an individual to contribute time and effort to the construction of a particular historical edifice such as Stonehenge. Nevertheless, the key lesson from this chapter is a re-statement of the general thrust of this thesis: We can only know as much about the past as we know about the present.

12. Conclusion

Narratives are explanations. Moreover, they serve in part to explain contemporary phenomena. We explain the current ethnic makeup of New Zealand by reference to historical processes of settlement and immigration. We explain the current form of an organism by reference to its evolutionary history. Geological processes explain the current form of a landscape. The narratives constructed by historical scientists serve to account for the current state of things by reference to a causal history.

However, such explanations explain historically contingent facts. They are not general explanations that explain types of processes. Unlike the experimental sciences, they do not typically explain an event by showing it to be an instance of a regularity. Rather, they explain a particular set of traces of the past by referencing unique configurations of history. The question that emerges is how to confirm these causal histories with observations made in the present.

Carol Cleland provides a strategy for historical scientists to choose between hypotheses using unique signatures of the evidence available. Her strategy allows us to confirm a hypothesis about an event in the past. However, we can extend Cleland's idea to encompass the confirmation of chronologies and narratives. All historical sciences use the downstream consequences of past events; they all depend epistemically on causal dispersion. Causal dispersion helps us solve the problem of the unobservability of processes in the past. Utilising unique configurations of traces, we can reconstruct past events.

However, to use causal dispersion, we must understand the causal relationship between an event and its consequences. Cleland's machinery relies upon both the logic of unique configurations of traces, and background theories to secure the component inferences that make up a distinctive set of ramifications. This is where regularities, and models, play an important role. Regularities secure the inference from observation to past cause.

Regularities, models and the like gain credence from testing and deployment in contemporary settings. They are robust precisely because they can be deployed over many cases. Hence, all historical science relies on some form of the idea first made famous in geology, that of uniformitarianism. The uniformitarian assumption is the idea that the past can be investigated by applying our understanding of the processes of the present. We can take our general well-confirmed models and apply them to historical settings. We may have to modify variables to account for the scale, context and historicity of past instances of these processes, but this can often be done with some degree of confidence.

The uniformitarian assumption thus implies that changes in our contemporary understanding of processes, and the development of new models, will change our understanding of the past. We saw how this happened in the understanding of the primate adaptive suite. Changes in contemporary knowledge drove changes in interpretation. The lack of clear regularities in human cultural systems makes inferences about past human agents difficult.

This also draws attention to limits on the historical sciences. They are constrained in principle to processes we can model in the present. If we cannot construct models of regularities of contemporary processes because they are arbitrary or genuinely chaotic, then we will have no traction in understanding past tokens of these processes. In practice, the historical sciences are constrained by our current competence in understanding mechanisms that are important both now and in the past. We saw in chapter 11 with the discussion of cognitive archaeology the potential importance of this practical limit. Our understanding of human psychology is not yet good enough to provide models for determining whether human belief systems do in fact share underlying regularities. It is impossible to tell at this juncture whether there are features of human belief systems that are regular, or whether they are all arbitrary. I suspect a mix of both.

This then is the theoretical background for the historical sciences. When successful those sciences deploy well-understood models and theories to past situations, using the dispersal of ramifications of past events as evidence. The historical sciences thus use the models and theories of the experimental sciences, and deploy them in the past. They are symmetrical to the future directed sciences, which try to predict the future.

There is a question of the reliability of this process of dispersion. Consequences of past events must be observable to act as tests of our hypotheses. Turner argues that some events, or some facts in the past, might disperse beyond our capability for observation. This lack of traces will consequently underdetermine our choice of hypotheses. However, the events of interest to the historical sciences structure causal histories in important ways. Events that matter to the sciences will have important consequences. These consequences may not be currently obvious, but this is a weakness in our current understanding of how the world works, and not a weakness of the historical project per se. As the sciences develop new insights, we may discover new causal ramifications that we can detect and use as evidence.

The second section of the thesis explores how historical scientists go about deploying these models in the construction of narratives. Our models of general robust processes have to be modified to account for the particulars of the case we are investigating. Models are modified to account for the contingencies of particular historical trajectories, and particular explanations.

This modification of models must account for the messiness of history. Some processes of the past are noisy, as they disrupt or degrade configurations of physical traces. Physical traces of past events can have their own causal histories. Processes of interest may interact, work in tandem, or cancel each other out. In order to accommodate these interactions, researchers propose multiple models, and modify them in accordance with the physical evidence, and with models of other processes crucial to the final narrative.

The result is that there is a three place series of movements: from a model of a process to potential evidence, and from the model to the broader narrative. The general requirement is one of coherence. Does the evidence, the model of the process, and the broader historical context of the narrative all make sense?

The North American megafaunal extinction is a case study showing the importance of context. Constructing a good narrative of a particular megafaunal extinction event, in this case that of North America, was not just a matter of applying a simple model. It is a matter of modifying it to take into account a number of variables, and including different processes that provide an important background upon which the process can play out. In the North American case, this included additional models of a general faunal interchange between the Eurasia and the Americas, and models of long-term changes in the ecological structure of the Mammoth Steppe.

The first 10 chapters of this thesis showed that the project of the historical sciences is viable. The final chapter looked at a particular challenge: That of reconstructing human cultural behaviours. Hawkes Hierarchy highlighted the fact that when explaining the human past we want insights into the belief systems and the ideologies of individuals. If Turner is ever right about the underdetermination of hypotheses by observable traces, it would surely be here. Sometimes we can use the contemporary descendents of past people, to gain traction on the belief systems of the past. Descendents of past people and their belief systems are downstream consequences that can be used to choose between alternative hypotheses. However, where there are no descendents, and no texts that record their beliefs, we are forced to rely on regularities in human behaviours. Genuinely idiosyncratic and contingent belief systems may well be too arbitrary for reconstruction.

However, this supports the general point of the thesis. Our understanding of the past is only ever as good as our confidence in the regularities that we think obtain. Uniformitarianism assumes regularities in processes. Human's ability to generate unique and seemingly arbitrary systems of beliefs make the uniformitarian assumption problematic in this instance. Language too has that property. A language that leaves literally no traces, either in the form of descendent languages, or in texts, is probably beyond reconstruction. All we can posit in such cases is that a group had a language, even if we cannot reconstruct the semantic and syntactic details.

12.1 Good Historical Science

To conclude, I will outline what I take to be the confirmation strategies of a good historical science.

Firstly, a good historical science will utilise the understandings of regularities that the sciences in general use. These regularities will be well tested, using all the apparatus of experimentation, repeated observations, and intervention in processes that allow us to understand the relevant variables. They will be regularities that are well confirmed.

Secondly, historical scientists will engage in research to determine how these regularities leave traces that can act as evidence for their occurrence. This dispersal of consequences also utilises regularities that can be tested, observed, and understood.

Utilising these regularities in dispersal allows researchers to choose between alternative hypotheses. Hypotheses about the past should have distinct signatures of downstream consequences. They should also make predictions about additional lines of evidence.

The general models of historical processes must be modified to capture the idiosyncrasies of history. To check that these are not ad hoc modifications to account for problematic evidence, these models and regularities should make additional predictions about further lines of evidence, or cohere with models of prior, synchronous or subsequent processes. They must account for the historical context.

These tools can be deployed to construct chronicles of the past, and from these, causal narratives, that are reliable accounts of the past.

The upshot of the thesis is then on the whole a positive one. The historical sciences can provide good and reliable information about the past. We can investigate the present, assume that the regularities that we see around us now obtain in the past, and then modify general models of processes to account for a complex and messy history. In practice, there are limits to this process; evidence from the past may on occasions disperse in such a way that we cannot isolate the details of a process precisely. However, the only in principle part of the past that we cannot account for is the genuinely idiosyncratic, and the genuinely arbitrary. However, this is a problem that all the sciences share, as all sciences rely upon a regular, and hence predictable, universe.

Bibliography

Adkins, L. (2003). Empires of the plain : Henry Rawlinson and the lost languages of Babylon. London, Harper Collins.

Alvarez, W. (1998). T. Rex and the Crater of Doom. New York, Vintage Books.

Archer, C. I., J. R. Ferris, et al. (2003). World History of Warfare. London, University of Nebraska Press.

Avery, G., K. Cruz-Uribe, et al. (1997). "The 1992-1993 Excavations at the Die Kelders Middle and Later Stone Age Cave Site, South Africa." Journal of field archaeology 24(3): 263.

Barham, A. J. and M. Prebble (2005). Taphonomic Modes, Sediment Storage and Regional Archaeology. Australasian Archaeometry Conference. Canberra, Australia.

Behrensmeyer, A. K., J. D. Damuth, et al., Eds. (1992). Terrestrial ecosystems through time : evolutionary paleoecology of terrestrial plants and animals. Chicago, University of Chicago Press.

Behrensmeyer, A. K. and R. W. Hook (1992). Paleoenvironmental Contexts and Taphonomic Modes. Terrestrial ecosystems through time : evolutionary paleoecology of terrestrial plants and animals. A. K. Behrensmeyer, J. D. Damuth, W. A. Dimichele et al. Chicago, University of Chicago Press: 15-93.

Binford, L. R. (1962). "Archaeology as Anthropology." American Antiquity 28: 217-225.

Binford, L. R. (1972). An Archaeological Perspective. New York, Seminar Press.

Binford, L. R. (1981). Bones: Ancient Men and Modern Myths. New York, Academic Press.

Binford, L. R. (2001). Constructing frames of reference : an analytical method for archaeological theory building using hunter-gatherer and environmental data sets. Berkeley, University of California Press.

Binford, S. R. and L. R. Binford, Eds. (1968). New Perspectives in Archaeology. Chicago, Aldine.

Brown, P., T. Sutikna, et al. (2004). "A new small-bodied hominin from the Late Pleistocene of Flores, Indonesia." Nature 431: 1055-1060.

Cartmill, M. (1992). "New views on primate origins." Evolutionary Anthropology: Issues, News, and Reviews 1(3): 105-111.

Childe, V. G. (1929). The Danube in prehistory. Oxford, Clarendon Press.

Clark, A. (1997). Being there : putting brain, body, and world together again. Cambridge, Mass., MIT Press.

Clark, A. (2003). Natural-born cyborgs: Minds, Technologies, and the Future of Human Intelligence. Oxford; New York, Oxford University Press.

Cleland, C. E. (2001). "Historical Science, experimental science, and the scientific method." Geology 29(11): 987-990.

Cleland, C. E. (2002). "Methodological and Epistemic Differences between Historical Science and Experimental Science." Philosophy of Science 69: 474-496.

Cupper, M. L. and S. J. Gallagher, Eds. (2007). Climate Change or Human Impact? Australia's Megafaunal Extinction, Selwyn Symposium of the GSA Victoria Division. Geological Society of Australia Extended Abstracts. Melbourne, Geological Society of Australia.

Daniel, G. E. (1967). The origins and growth of archaeology. Penguin.

Daniel, G. E. (1975). A hundred and fifty years of archaeology. London, Duckworth.

Daniel, G. E. (1981). A short history of archaeology. Ancient peoples and places ; v. 100. London, Thames and Hudson.

Dennett, D. C. (1999). True believers: The Intentional Strategy and Why it Works. Mind and Cognition: An Anthology. W. G. Lycan. Oxford, Blackwell: 75-86.

Diamond, J. M. (1989). "Quaternary megafaunal extinctions: Variations on a theme by Paganini." Journal of Archaeological Science 16(2): 167-175.

Diamond, J. M. (2006). Collapse: How societies choose to fail or succeed. London, Penguin.

Dimbleby, G. W. (1985). The Palynology of archaeological sites. Studies in archaeological science. London; Orlando, Fla, Academic Press.

Dunnell, R. C. (1996). Style and Function: A Fundamental Dichotomy. Evolutionary archaeology : theory and application. M. J. O'Brien. Salt Lake City, University of Utah Press: 112-122.

Falk, T. (2004). "ARCHAEOLOGY: Wisconsin Dig Seeks to Confirm Pre-Clovis Americans." Science 305(5684): 590a-.

Flannery, T. F. (1994). The future eaters : an ecological history of the Australasian lands and people. Chatswood, N.S.W., Reed Books.

Flannery, T. F. (2001). The eternal frontier : an ecological history of North America and its peoples. New York, Atlantic Monthly Press.

Frake, C. O. (1994). Dials: A Study in the Physical Representation of Cognitive Systems. The ancient mind : elements of cognitive archaeology. C. Renfrew and E. B. W. Zubrow. Cambridge [England] ; New York, Cambridge University Press: 119-132.

Frigg, R. and S. Hartmann (2008). Models in Science. The Stanford Encyclopedia of Philosophy. E. N. Zalta.

Giere, R. N. (1988). Explaining science : a cognitive approach. Science and its conceptual foundations Chicago, University of Chicago Press.

Giere, R. N. (1999). Science Without Laws. Science and Its Conceptual Foundations Chicago, The University of Chicago Press.

Gifford, D. P. (1981). Taphonomy and Paleoecology: A Critical Review of Archaeology's Sister Disciplines. Advances in Archaeological Method and Theory. M. B. Schiffer. New York, Academic Press. 4.

Godfrey-Smith, P. (2006). "The Strategy of Model-based Science." Biology and Philosophy 21: 725-740.

Gordon, J. E. (1978). Structures, or, Why things don't fall down. New York, Da Capo Press.

Gordon, J. E. (1984). The new science of strong materials, or, Why you don't fall through the floor. Penguin.

Gould, S. J. (1965). "Is Uniformitarianism Necessary?" American Journal of Science

263: 223-228.

Grayson, D. K. (1984). Explaining Pleistocene Extinctions: Thoughts on the Structure of the Debate. Quaternary extinctions : a prehistoric revolution. P. S. Martin and R. G. Klein. Tucson, Ariz, University of Arizona Press: 807-823.

Grayson, D. K. (2001). "The Archaeological Record of Human Impacts on Animal Populations." Journal of World Prehistory 15(1): 1-68.

Green, R. (1988). "Those Mysterious Mounds are for the Birds." Archaeology in New Zealand 32(3): 153-158.

Gribbin, J. R. (2002). Science: a history, 1543-2001. London, Allen Lane.

Guthrie, R. D. (1984). Mosaics, Allelochemics and Nutrients: An Ecological Theory of Late Pleistocene Megafaunal Extinctions. Quaternary extinctions : a prehistoric revolution. P. S. Martin and R. G. Klein. Tucson, Ariz, University of Arizona Press: 259-298.

Guthrie, R. D. (1990). Frozen fauna of the Mammoth Steppe : the story of Blue Babe. Chicago, University of Chicago Press.

Guthrie, R. D. (2006). "New Carbon Dates Link Climatic Change with Human Colonization and Pleistocene Extinctions." Nature 441: 207-209.

Harrison, S. (2001). "On reductionism and emergence in geomorphology." Transactions of the Institute of British Geographers 26(3): 327-339.

Hawkes, C. F. (1954). "Archaeological theory and method: some suggestions from the old world." American Anthropologist 56: 155-168.

Hill, J. N. (1994). Prehistoric Cognition and the Science of Archaeology. The ancient mind : elements of cognitive archaeology. C. Renfrew and E. B. W. Zubrow. Cambridge [England] ; New York, Cambridge University Press: 83-92.

Holland, T. (2005). Rubicon : the last years of the Roman Republic. New York, Anchor Books.

Hooykaas, R. (1975). Catastrophism in Geology, Its Scientific Character in Relation to Actualism and Uniformitarianism. Philosophy of Geohistory: 1785-1970. C. C. Albritton. Stroudsburg, Pa.,, Dowden. 13: 310-356.

Hull, D. L. (1975). "Central Subjects and Historical Narratives." History and Theory 14(3): 253-274.

Jeffares, B. (2005). Cultural Variation and Epistemic Access. International Society for the History, Philosophy, and Social Studies of Biology, University of Guelph, Ontario, Canada.

Jeffares, B. (2008). Philosophy of Archaeology. The Blackwell Companion to the Philosophies of History and Historiography. A. Tucker. New York, Blackwell.

Jones, A. (2002). Archaeological theory and scientific practice. Topics in contemporary archaeology Cambridge ; New York, Cambridge University Press.

Kirch, P. V. and R. C. Green (2001). Hawaiki, ancestral Polynesia: An Essay in Historical Anthropology. Cambridge, U.K. ; New York, Cambridge University Press.

Kitcher, P. (1995). The advancement of science: science without legend, objectivity without illusions. New York ; Oxford, Oxford University Press.

Kosso, P. (1993). "Middle-Range Theory in Historical Archaeology." Studies in history and philosophy of science 24(2): 163-184.

Kosso, P. (2001). Knowing the past: philosophical issues of history and archaeology. Amherst, NY, Humanity Books.

Kosso, P. and C. Kosso (1995). "Central Place Theory and the Reciprocity between Theory and Evidence." Philosophy of science 62(4): 581-598.

Kutach, D. N. (2002). "The Entropy Theory of Counterfactuals." Philosophy of Science 69: 82-104.

Landau, M. (1984). "Human Evolution as Narrative." American Scientist 72: 262-268.

Landau, M. (1987). Paradise Lost: The Theme of Terristoriality in Human Evolution. The Rhetoric of the Human Sciences. J. S. Nelson, A. McGill and D. N. McClosky. Madison, WI, University of Wisconsin Press: 111-124.

Landau, M. (1991). Narratives of human evolution. New Haven, Yale University Press.

Lewis, D. (1979). "Counterfactual Dependence and Time's Arrow." Nous 13(4): 455-476.

MacNeish, R. S. and J. I. Kislak (1973). Early man in America; readings from Scientific American. San Francisco,, W. H. Freeman.

Marshall, E. (2001). "Pre-Clovis Sites Fight for Acceptance." Science 291(5509): 1730-1732.

Martin, M. and L. C. McIntyre (1994). Readings in the philosophy of social science. Cambridge, Mass., MIT Press.

Martin, P. S. and R. G. Klein (1984). Quaternary extinctions : a prehistoric revolution. Tucson, Ariz, University of Arizona Press.

Martin, P. S. and H. E. Wright, Eds. (1967). Pleistocene extinctions: the search for a cause. International Association for Quaternary Research, 7th conference, Boulder, Colo., 1965, Proceedings ; v.6.

Martin, R. D., A. M. MacLarnon, et al. (2006). "Flores hominid: New species or microcephalic dwarf?" The Anatomical Record Part A: Discoveries in Molecular, Cellular, and Evolutionary Biology 288A(11): 1123-1145.

Mignon, M. R. (1993). Dictionary of concepts in archaeology. Reference sources for the social sciences and humanities, no. 13 Westport, Conn., Greenwood Press.

Murray, A. B. (2003). Contrasting the Goals, Strategies, and Predictions Associated with Simplified Numerical Models and Detailed Simulations. Prediction in Geomorphology. P. R. Wilcock and R. M. Iverson, American Geophysical Union. 135: 151-165.

Odenbaugh, J. (Forthcoming). Models. A Companion to the Philosophy of Biology. S. Sarkar and A. Plutynksi, Wiley-Blackwell.

Oldroyd, D. R. (1996). Thinking about the Earth: A History of Ideas in Geology. Studies in the history and philosophy of the earth sciences. Cambridge, Mass., Harvard University Press.

Petroski, H. (1992). To engineer is human : the role of failure in successful design. New York, Vintage Books.

Petroski, H. (1994). Design paradigms : case histories of error and judgment in engineering. Cambridge [England] ; New York, N.Y., Cambridge University Press.

Petroski, H. (1994). The evolution of useful things. New York, Vintage Books.

Petroski, H. (2006). Success through failure : the paradox of design. Princeton,

Princeton University Press.

Raab, L. M. and A. C. Goodyear (1984). "Middle Range Theory in Archaeology: A critical review of origins and applications." American Antiquity 49(2): 255-268.

Renfrew, C. (1973). Before civilization : the radiocarbon revolution and prehistoric Europe. London, Cape.

Renfrew, C. (1982). Towards an archaeology of mind : an inaugural lecture delivered before the University of Cambridge on 30 November 1982. Cambridge, Cambridge University Press.

Renfrew, C. (1994). Towards a cognitive archaeology. The ancient mind : elements of cognitive archaeology. C. Renfrew and E. B. W. Zubrow. Cambridge [England] ; New York, Cambridge University Press: xiv, 195.

Renfrew, C. and E. B. W. Zubrow, Eds. (1994). The ancient mind : elements of cognitive archaeology. New directions in archaeology. Cambridge [England] ; New York, Cambridge University Press.

Rudwick, M. J. S. (1969). "The Glacial Theory." History of Science 8: 136-157.

Rudwick, M. J. S. (1970). "The Strategy of Lyell's Principles of Geology
." Isis 61(1): 4-33.

Rudwick, M. J. S. (1972). The Meaning of Fossils: Episodes in the History of Palaeontology. History of science library. London, Macdonald.

Salmon, M. H. (1982). Philosophy and archaeology. Studies in archaeology. New York, N.Y., Academic Press.

Schick, K. D. and N. P. Toth (1993). Making Silent Stones Speak: Human Evolution and the Dawn of Technology. London, Weidenfeld and Nicholson.

Schiffer, M. B., Ed. (1981). Advances in Archaeological Method and Theory. Advances in Archaeological Method and Theory. New York, Academic Press.

Scullard, H. H. (1986). From the Gracchi to Nero: A History of Rome from 133 B.C. to A.D. 68.

Seddon, G. (1996). "Thinking like a geologist: The culture of geology. Mawson Lecture 1996." Australian Journal of Earth Sciences 43(5): 487-495.

Simpson, G. G. (1975). Uniformitarianism: An Enquiry into Principle, Theory, and Method in Geohistory and Biohistory. Philosophy of Geohistory: 1785-1970. C. C. Albritton. Stroudsburg, Pa.,, Dowden. 13: 310-356.

Sober, E. (1991). Reconstructing the past : parsimony, evolution, and inference. Cambridge, Mass., MIT Press.

Solecki, R. S. (1971). Shanidar, the first flower people. New York,, Knopf.

Stanley, S. M. (1992). "An Ecological Theory for the Evolution of Homo." Paleobiology 18: 237 - 257.

Stanley, S. M. (1996). Children of the Ice Age: How a Global Catastrophe Allowed Humans to Evolve., Random House.

Sterelny, K. (2001). Explanatory Pluralism in Evolutionary Biology. The evolution of agency and other essays. Cambridge, UK ; New York, Cambridge University Press: 129-151.

Sterelny, K. (forthcoming). "SNAFUS: An Evolutionary Perspective." Biological Theory.

Sterelny, K. and P. E. Griffiths (1999). Sex and death: An Introduction to Philosophy of Biology. Science and its conceptual foundations Chicago, Ill., University of Chicago Press.

Sussman, R. W. (1991). "Primate Origins and the evolution of angiosperms." American Journal of Primatology 23(4): 209-223.

Trigger, B. G. (1990). A history of archaeological thought. Cambridge ; New York, Cambridge University Press.

Tucker, A. (2004). Our knowledge of the Past: A Philosophy of Historiography. Cambridge ; New York, Cambridge University Press.

Turner, D. (2005). "Local Underdetermination in Historical Science." Philosophy of Science 72: 209-230.

Walker, A. (1981). The Koobi Fora Hominids and their bearing on the Origins of the genus *Homo*. *Homo Erectus:* Papers in Honor of Davidson Black. B. A. Sigmon and J. S. Cybulski, University of Toronto Press: 193-215.

Whitley, D. S., Ed. (1998). Reader in archaeological theory: post-processual and cognitive approaches. Routledge readers in archaeology;. London ; New York, Routledge.

Wroe, S. (2005). "On Little Lizards and the Big Extinction Blame Game." Quarternary Australasia 23(1): 8-12.

Wroe, S., J. Field, et al. (2004). "Megafaunal Extinction in the late Quaternary and the Global Overkill Hypothesis." Alcheringa 28: 291-331.

Wylie, A. (1989). "Archaeological Cables and Tacking: The Implications of Practice for Bernstein's 'Options Beyond Objectivism and Relativism'." Philosophy of Social Sciences 19(1): 1-18.

Wylie, A. (2002). Archaeological Cables and Tacking: Beyond Objectivity and Relativism. Thinking from things : essays in the philosophy of archaeology. A. Wylie. Berkeley ; London, University of California Press: 161-168.

Wylie, A. (2002). Thinking from things: Essays in the Philosophy of Archaeology. Berkeley ; London, University of California Press.